The Real Costs of Credit Access: Evidence from the Payday Lending Market

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Abstract

I estimate the real effects of credit access among low-income households by exploiting geographic and temporal variation in the availability of payday loans. Payday loans, which are small, short-term consumer loans that carry comparatively high interest rates, constitute the marginal source of credit for many high risk borrowers. I find no evidence that payday loans alleviate economic hardship. To the contrary, I find that loan access leads to increased difficulty paying mortgage, rent and utilities bills, and delay of needed health care. 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. Through further analysis of differences in loan access – over time and across income groups – I rule 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.

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Introduction

Historically, consumer lending markets have been highly regulated, subject to state-imposed usury and small loan laws that limit loan interest rates and principal amounts, among other terms and conditions. Among high credit-risk individuals, of whom lenders require high interest rates, 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 who, due to misinformation or self-control problems, borrow to increase current consumption and then face reduced financial flexibility due to a large, ongoing debt service burden (Ausubel 1991; Laibson 1997; Bond, Musto and Yilmaz 2008).

In this paper, I make use of the emergence and development of the payday lending industry, which provides short-term consumer loans at high interest rates, to study this issue empirically. By focusing on payday loans, which likely constitute the marginal source of loans for high risk borrowers,[1] I aim to identify the impact of credit access on the margin for the group that is most likely to be constrained when credit is rationed.

In the empirical design, I exploit geographic and temporal variation in the availability of payday loans in order to estimate the effects of 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 fairly broad selection of outcomes on which we

[1] Using a differences-in-differences approach, Morgan (2007) finds that individuals with high uncertainty about future income and low education are less likely to report being denied credit when they live in states with fewer restrictions on payday lenders. In a survey of payday borrowers, Elliehausen and Lawrence (2001) also find payday borrowers frequently report being turned down for credit (73 percent), and being constrained by their credit card borrowing limit (61 percent).

might observe the effects of borrowing on financial distress. Importantly, the likelihood of these events is also plausibly influenced by a fairly small, short-term loan.

The empirical investigation is complicated by the fact that variation in loan access is influenced by the location decisions of households and lenders, as well as the regulatory decisions of state legislators, who oversee these businesses. The latter two decisions, on the part of store operators and legislators, are likely made in response to the characteristics of potential borrowers. Additionally, payday lending regulations are unlikely to be independent of state-level policies impacting welfare programs and health care coverage for poor populations, which exert an independent influence on many outcomes of interest.[2] 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, I utilize an empirical design that isolates variation in loan access that is independent of store location decisions and state-level policy decisions. First, I focus the analysis on households within states that prohibit payday loans. These households cannot obtain payday advances without leaving their home state.[3] Individuals living near a state that allows payday lending, however, can cross the border to obtain a loan. Conversely, individuals 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 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.[4]

[2] Consistent with the concern that cross-state differences in payday lending laws are confounded with other variation across states, Benmelech and Moskowitz (2009) find considerable evidence that usury laws in the 19th century are influenced by economic conditions (financial crises), as well as political and economic policies.
[3] 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.
[4] In a somewhat similar identification strategy, Pence (2006) uses discontinuities in state foreclosure laws to estimate the effect of foreclosure laws on the supply of mortgage credit. Though I also utilize cross-state regulatory differences, my empirical design relies on within-state variation in loan access as opposed to the discontinuities imposed by cross-state law differences within a local market.

There is considerable anecdotal evidence documenting the practice of individuals crossing into payday-allowing states to obtain loans.[5] Using geographic data on payday loan store locations that I compiled from state regulators, I offer further support for this view. I show that, conditional on zip code-level observables and a general effect of border proximity, the number of store locations is almost 20 percent higher in zip codes close to payday-prohibiting states. Furthermore, I show that this effect is stronger in areas where, judging by the income distribution, there are more potential payday loan customers across the border. This evidence suggests that there is substantial additional loan demand from residents of payday-prohibiting states.

In the main analysis, I find no evidence that payday loan access mitigates financial distress along the dimensions that I observe. In fact, I find that loan access 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. I estimate that among families with \$15,000 to \$50,000 in annual income, loan access increases the incidence of difficulty paying bills by 25 percent. I also find that among adults in these families, loan access increases the delay of needed medical care, dental care and prescription drug purchases by roughly 25 percent. The estimates are robust to the inclusion of extensive individual-level and county-level control variables as well as a measure of proximity to any state border. These controlled specifications are important in confirming that the estimated effects are not driven by differences between sampled individuals or geographic areas that are unrelated to loan access.

Beyond the main analysis, I evaluate three additional empirical specifications in order to confirm that the measured effects are due to payday loan access and not some other factor. First, I isolate temporal change in loan access within a difference-in-difference model that includes county fixed effects. The resulting variation in loan access, which derives from changes in the availability of payday loans in bordering states, allows me to assess whether the main findings are influenced by omitted cross-sectional variables. Omitted variables of this type do not seem to be an issue, as the difference-in-

[5] 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.

difference results generally confirm the sign and magnitude of the main findings, albeit with less inferential weight.

Second, I investigate the possibility that differences in county-level financial safety net and welfare services are driving the estimated effect of loan access. Specifically, I identify the effect of loan access by comparing the outcomes of individuals in the $15,000 to $50,000 income group, who represent the vast majority of payday borrowers, to outcomes of individuals in the below $15,000 income group, who are largely screened out of the payday loan market. 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 among those with access, but show little effect of loan access on health-related hardship.

Finally, I investigate whether the effects of loan access are stronger in counties where a greater proportion of workers commute to payday-allowing states. Individuals who regularly commute to a payday-allowing area face a lower cost of accessing loans, so we expect a larger effect of payday loan access in counties with a greater proportion of such commuters, even after conditioning on proximity to a payday-allowing state. In a test of this hypothesis, I find that the effects of loan access on difficulty paying bills and the other non-health related hardship are indeed larger in areas with more cross-border commuters. Loan access effects for the health-related outcomes, on the other hand, are not concentrated in areas with greater 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. In the interpretation of the results, I discuss this issue further and conjecture about the implied effects of borrowing.

By offering an empirical analysis of the effects of payday lending, my 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 2008; Morgan and Strain 2008; Morse 2009; Skiba and Tobacman 2008; Zinman 2009). My 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[6] and for more specific populations.[7] 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,[8] and others suggesting that loan access provides benefits.[9] Accordingly, additional research is valuable in furthering our understanding. In subsequent discussion and interpretation of the results, I will delve further into the conclusions of these papers.

The following section offers an overview of the conceptual framework underlying the hypotheses tested in this paper. Section II highlights relevant background material on the nature of payday loan transactions as well as the regulation and development of the industry. Sections III and IV cover the data, empirical methodology and results. Finally, sections V and VI offer further discussion and interpretation of the results along with concluding thoughts.

I. Theories on Consumer Borrowing

A. Borrowing to Smooth Current Income or Consumption Shocks

Credit access can alleviate hardship by expanding a household's options in managing consumption over time. If an otherwise credit-constrained household can

[6] Morse (2009) identifies the effect of borrowing after natural disasters.

[7] Skiba and Tobacman (2008) estimate the effects of borrowing for the payday borrowers that are most likely to default (based on a credit score), and Carrell and Zinman (2008) estimate loan access effects among Air Force members.

[8] 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 in areas with payday loan availability. Zinman (2009) shows some evidence that consumers are less likely to experience late bill payments after Oregon law restricted payday lending.

[9] In areas with payday loan access, Morse (2009) finds lower foreclosures following natural disasters. Morgan and Strain (2008) find increases in the volume of bounced checks after Georgia and North Carolina ban payday lending. Zinman (2009) identifies deterioration in Oregon consumers' subjective assessment of their financial well-being after payday lending is restricted. In a field experiment in South Africa, Karlan and Zinman (2008) randomize access to 4-month loans with 200% annualized interest rates (longer maturity and lower APR than a payday loan), and find that borrowing results in greater rates of employment and better food security.

borrow, even for a short period, the household can potentially smooth expenditures around periods of income or consumption shocks, which in the absence of borrowing would lead to adverse events like delinquency on rent payments, eviction, or forgone health care. Under such 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. Furthermore, competition in credit markets can benefit consumers; if payday loans offer clear financial benefits over a consumer's next best borrowing option, payday loan access can be beneficial.[10] 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.

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

While loans provide flexibility in managing consumption over time, they can raise an individual's risk of hardship in the future by imposing a substantial debt service burden. When consumers underestimate the future debt service burden or are unable to commit themselves when they plan to repay the loan promptly, the future costs of borrowing can outweigh the initial benefits, even from an *ex ante* perspective.

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 plan to borrow, even at high interest rates, and repay the loan in one period. However, they cannot commit to this plan, and 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

[10] 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 avert a delinquency fee that exceeds the loan's interest charge (see Community Financial Services Association of America 2007).

1991).[11] 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 test, strictly speaking, will not determine whether payday loans are welfare increasing or decreasing, but rather whether they facilitate the smoothing of important expenditures.

II. Payday Lending Background

Payday advance loans offer a short-term source of liquidity to a low- to moderate-income customer base. Loans typically have terms of two to four weeks, 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.[12] 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

[11] Another possibility, put forth in Bond et al. (2005), is that borrowers are misinformed about their ability to repay loans in the future, and consequently underestimate the costs of borrowing.
[12] Barr (2004) and Caskey (2005) discuss the basic features of payday loan transactions and the industry more broadly.

$15,000 and $50,000, while only seven percent of borrowers report family incomes below $15,000.[13]

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. In parallel, annual loan volume is estimated to have grown from about $8 billion in 1999 to between $40 and $50 billion in 2004.[14] High interest rates and rapid industry growth have piqued the attention of consumer advocates, the popular press and state legislators, 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.[15] Delaware, New Hampshire, Pennsylvania and Rhode Island are the payday-allowing states that border Massachusetts, New Jersey and New York.[16] During the sample period, New Hampshire and Rhode Island experienced a change in payday lending laws. New Hampshire's small loan interest rate cap, which effectively prohibited direct payday lending, was removed in January 2000, facilitating entry of a number of payday lenders. Similarly, Rhode Island amended its check cashing statutes to allow payday lending via deferred deposit check cashing transactions, effective July 2001. Payday lending also emerged in Delaware and Pennsylvania over the sample period, entering Delaware as non-depository licensed lenders in 1998 and Pennsylvania as agents for bank lenders in 1997. Accordingly, I consider payday loans to be available in Delaware and Pennsylvania in the latter two

[13]See Elliehausen (2006), p. 19, which relies on data from Elliehausen and Lawrence's (2001) survey of payday borrowers.
[14] Stegman (2007), p. 169-170.
[15] 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.
[16] 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 inter-national 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.

years covered by the survey, 1998 and 2001, and in New Hampshire and Rhode Island in the final year covered by the survey, 2001. More thorough discussion of the relevant state regulations is provided in an appendix to this document.

III. Data and Outcome Measures

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 intended to facilitate the study of welfare programs targeting the poor, particularly as fiscal responsibility for these programs transferred from federal to state government in 1996.[17]

In total, the NSAF data constitute a repeated cross-section of roughly 42,000 households per year during 1997, 1999 and 2002.[18] The data are nationally representative, and are also representative at the state level for 13 selected "focal states".[19] 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 my study. Furthermore, the survey's inclusion of county-level geographic identifiers facilitates the measurement of household location relative to state borders and payday loan store locations.

In addition to person-level and family-level control variables sourced from the NSAF, I also make use of county-level unemployment data from the Bureau of Labor Statistics, and county-level economic, demographic and workflow data from the 2000 Census. I have also collected the names and addresses of licensed payday lending branch

[17] See Abi-Habib, et al. 2004.
[18] Following the Urban Institute's convention, I refer to the waves of data based on the year in which the survey was conducted rather than the year to which the survey responses pertain. 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.
[19] The 13 focal states are: AL, CA, CO, FL, MA, MI, MN, MS, NJ, NY, TX, WA and WI.

locations as of July 2007 from state banking regulators in 10 states.[20] I use these data to determine whether the supply of store locations depends on the distance to payday-prohibiting states.

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 Table 1. Four health care-related measures are taken at the person level: *Medical Care Postponed*, *Dental Care Postponed*, and *Drug Purchase Postponed* are indicators for whether an individual has forgone or postponed needed care of each type due to lack of insurance or money. From these three components, I form a single binary measure, *Any Care Postponed*, of the postponement or delay of any health care. Other hardship measures, taken at the family level, include: 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*). Finally, I summarize these four family-level measures in a single binary variable, *Any Family Hardship*, which takes the value of one if a family experiences any form hardship, excluding the health measures.[21] Since many of the specific hardship measures depend on other shocks in addition to underlying financial distress, the summary hardship measures provide additional statistical power in detecting financial distress.

IV. Does Access to Payday Loans affect Economic Hardship?

A. Defining Payday Loan Access

As described in the introduction, the empirical design relies on within-state variation in loan access that is unaffected by store location decisions and home-state

[20] The states for which I have store location data are AL, DE, FL, KY, NH, OH, RI, SC, TN and VA.

[21] Since the NSAF does not report health measures for all individuals within a sampled family, I cannot include the health measures in the summary measure of family hardship.

regulations. Among families in payday-prohibiting states, I define access to loans based on the family's distance to the nearest payday-allowing state. In practice, since I know a family's county of residence rather than its precise location, I use distance from the county center to the border in place of actual distance. Specifically, I define *PaydayAccess*, a binary measure of geographic access to payday loan stores, which is 1 if the center of the family's county is within 25 miles of a payday-allowing state in that survey year and 0 otherwise. *PaydayAccess* varies both in the cross-section and over time, due to changes in border-state loan availability over the sample period. 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 family's county is within 25 miles of a state that ultimately allowed payday lending, regardless of whether it was allowed at the time of the observation.

The goal in defining *PaydayAccess* as a binary measure is to separate counties which are within reasonable driving distance of a payday-allowing state from those which are not. With the boundary set at 25 miles, counties are *de facto* separated into those which border payday-allowing states and those which do not. This binary measure introduces some measurement error. For example, in extreme cases some individuals living in counties with *PaydayAccess* of zero might be closer to payday-allowing states than individuals living in counties with *PaydayAccess* of one. In robustness exercises, I consider two alternatives to the binary measure of geographic access. I define *LogDistance*, the natural logarithm of the distance from a family's county to the nearest payday-allowing state, which does not assert a discontinuity in geographic access at 25 miles. I also define *Pct Pop < 15 miles*, a continuous measure of geographic access ranging from zero to one. This variable measures 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.

B. Do Individuals from Payday-Prohibiting States Visit Other States to Obtain Loans?

To buttress the anecdotal evidence that individuals cross state borders to obtain payday loans, I analyze the relationship between the number of payday loan stores within a zip code and the proximity of payday-prohibiting states. If the practice of crossing borders to get loans is common, we would expect the supply of store locations near payday-prohibiting states to increase in response to this additional demand. To test this hypothesis, 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 (*X*)[22] and an indicator for the proximity of any state border (*Dist. Any State < 25 Miles*):

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

As shown in column (1) of Table 2, I find suggestive evidence that store locations respond to demand from payday-prohibiting states, as there are roughly 16 percent more stores (a 0.25 increase over an average of 1.50) in zip codes within 25 miles of payday-prohibiting states.

If this relationship is truly driven by demand for payday loans and not some other unobserved factor, we expect the effect to be stronger in zip codes that border areas with more potential payday borrowers. To test this additional hypothesis, I add to the model 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.[23] Recall that the $15,000 to $50,000 income category encompasses the vast majority of payday borrowers. Results from this analysis, displayed in column (2) of Table 2, show that the coefficient on the interaction term of interest 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 larger pools of potential customers across the border. From these results, I conclude that there is considerable evidence that customers

[22] The content of this vector is enumerated in the description of Table 2.

[23] In computing the distribution of households by income category in the nearby payday-prohibiting area, I use zip code tabulation area (ZCTA5) data from the 2000 Census. I define the nearby area to be the closest zip code as well as any other zip code that is within 10 miles of the closest zip code.

travel across borders to get loans, and that the practice is fairly extensive, as the supply response (measured in number of locations) is quite large.

C. Regression Sample, 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. Since all models include state-year fixed effects, these observations do not contribute directly to the identification of *PaydayAccess* coefficients, but are included to improve precision in the estimation of county-level and individual-level covariates.

In an attempt to limit the analysis to the population that uses payday loans, I stratify the sample by family income. I limit the regression sample to individuals in the low- to moderate-income range of $15,000 to $50,000, which captures the vast majority of borrowers.[24] In a falsification exercise, I also estimate the effect of loan access on individuals outside of this income range. Finally, the sample excludes observations from counties with populations below 250,000, for which county identifiers are unavailable.[25]

The summary statistics of the regression sample, limited to individuals in payday-prohibiting states and stratified by *PaydayAccess*, are displayed in Table 3. Treatment and control groups differ. At the county level, areas with payday loan access are higher income, more populous and more urban. As measured in the person-level regression sample, individuals with payday loan access have, on average, higher family incomes, higher asset ownership (home and car), more education, and higher rates of health insurance. Demographically, they are more likely to be white, and less likely to be

[24] Roughly 70 percent 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.

[25] To preserve respondent confidentiality, the Urban Institute does not release geographic information beneath the county level, nor does it release county identifiers for households living in counties with population less than 250,000.

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. It is worth noting, however, that 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. Nevertheless, some differences remain statistically significant. For example, individuals with loan access remain more likely to be white, less likely to be Hispanic and less likely to be foreign born. In a robustness exercise, I explore the effect of these sample imbalances on estimation results by estimating regressions on sub-samples stratified by race and immigrant status.

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

The general regression model I estimate is of the following form:

$$(2) \qquad \Pr\left(Outcome_{ijt}\right) = \Phi(\alpha + \beta PaydayAccess_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$$

Within this equation "i" indexes person or family, "j" indexes county and "t" indexes time. X and Z are vectors containing relevant household-level and county-level controls, respectively.[26] All specifications include state-year fixed effects. I also define the dummy variable *Border,* which is 1 if the individual's county is within 25 miles of any state border, and 0 otherwise.[27] This control, which accounts for a general border effect, is included in the fully-controlled specification. The identifying variation in *PaydayAccess* in this model includes a cross-sectional component, determined jointly by

[26] The vector Z contains the following 2000 Census measures at the county level: cubics in county median income, population and percent urban population; percent unemployment; percent home ownership; percent foreign born; and racial composition. In the family-level regressions, the vector X contains: log family income, number of family members, age (average for adults), dummies for home ownership, car ownership, past year unemployment spell (any adult), 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, college and/or graduate degree). In the person-level regressions, the vector X contains: log family income, dummies for home ownership, car ownership, past year unemployment, past year health un-insurance spell, sex, marital status, race (white, African-American, Hispanic, Asian/other), immigrant status and education (no high school degree, high school degree, college and/or graduate degree).

[27] Because I observe households near state borders without differential access to payday stores, I am able to separately identify a border effect.

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. A key assumption of this identification strategy is that individuals do not choose their location based on their access to payday loans, or based on characteristics that happen to be correlated with payday loan access after conditioning out observables. In subsequent analysis of differences in access, over time and across income groups, I relax this identification assumption.

D.1 Regression Results, Non-Health Outcomes

Since the outcomes of interest are low probability, binary events, I employ probit estimation in the main set of results. Estimation results for the non-health outcomes are presented in Table 4, with control variables layered into the model as one moves from column (1) through column (4). In this table, I present point estimates, standard errors and average incremental effects for the parameter of interest, which is the coefficient on *PaydayAccess*, and suppress estimation results for other covariates in the model.[28]

The specification in column (1) includes limited controls – state-year fixed effects alone. The estimated coefficient on *PaydayAccess* in this model is positive for four of the five dependent variables, indicating that loan access raises the likelihood of these outcomes. *Difficulty Paying Bills* and *Moved Out* show the greatest sensitivity to loan access, with average incremental effects of 3.2 and 1.0 percentage points, respectively. For both of these outcomes, the coefficients underlying the estimated incremental effects are significant at the 10 percent level. While the estimated coefficients on *PaydayAccess* are also positive for *Any Family Hardship* and *No Phone*, these estimates are not statistically significant. *Cut Meals* shows a negative, but statistically insignificant, relationship to loan access in this specification.

Since *PaydayAccess* varies at the county level, it is important to control for potential confounding variables that also vary at the county level. The specification in column (2) adds a number of county-level controls to the model. The introduction of

[28] To calculate the "average incremental effect," I compute the change in the predicted probability of the outcome due to a discrete change in *PaydayAccess* for each sample member, and then average the change in predicted probability across all sample members.

these controls raises *PaydayAccess* coefficients for *Family Hardship* and *Difficulty Paying Bills*. The average incremental effect of loan access on *Family Hardship* rises to 3.6 percentage points (from 2.4) and the effect on *Difficulty Paying Bills* rises to 4.0 percentage points (from 3.2). The former is significant at the 5 percent level, while the latter is significant at the 1% level. County-level controls reduce the estimated effect on *Moved Out* to 0.7 percentage points and renders it statistically insignificant. Neither *Cut Meals* nor *No Phone* shows any statistically significant relationship to loan access in this specification.

Aside from the possibility that county differences confound the *PaydayAccess* effect, there remains the possibility that sampled families, stratified by *PaydayAccess*, differ in ways that obscure the effect of loan access. The specification in column (3) adds a number of family-level controls to the previous specification in order to address this issue. The inclusion of these controls raises the estimated *PaydayAccess* effects for all five outcomes. *Any Family Hardship* and *Difficulty Paying Bills* remain the only outcomes showing statistically significant effects of loan access, with each significant at the one percent level. The average incremental effects of loan access on these two outcomes are 4.2 and 4.4 percentage points, respectively. The effect of loan access on *Moved Out*, remains 0.7 percentage points, but is not quite significant. The point estimates for *Cut Meals* and *No Phone*, while also positive, are quite imprecisely estimated.

Finally, one way in which treatment and control observations are certainly different is in their location relative to state borders. To the extent that border areas are unique, the coefficient on *PaydayAccess* might be measuring some other factor unrelated to loan access. Since my sample includes a number of counties bordering other states which do not offer differential loan access, I am able to control for a general border effect when estimating the coefficient on *PaydayAccess*. Column (4) displays estimation results for a specification using all previously discussed control variables in addition to a border dummy. The border control proves to be quite important; coefficients on *PaydayAccess* rise for all five outcomes. In this fully controlled specification, the average incremental effects of *PaydayAccess* on *Any Family Hardship* and *Difficulty Paying Bills* are largest, at 5.1 percentage points and 4.9 percentage points, respectively. The average incremental effects for *Moved Out* and *No Phone* are both 0.7 percentage points, but neither is statistically significant.

The magnitudes of these effects are substantial. Average incremental effects of loan access represent a 7 percent increase over the unconditional likelihood for *Cut Meals* (1.2 percentage point increase over a 16.9 percent unconditional likelihood), a 25 percent increase for *Difficulty Paying Bills* (4.9 percentage point increase over 20.3 percent) and a 17 percent increase for *Any Family Hardship* (5.1 percentage point increase over 29.2 percent). The effects on *No Phone* and *Moved Out* are large, at 40 percent for *No Phone* (0.7 percentage point increase over 1.7 percent) and 60 percent for *Moved Out* (0.7 percentage point increase over 1.2 percent), but these estimates are quite imprecise.

D.2 Regression Results, Health Outcomes

In Table 5, I present the estimation results for the health outcomes. These results follow the same template as Table 4, with increasing controls layered into the model as one moves from column (1) through column (4). Due to the nature of the NSAF survey design, the health outcomes are measured at the person level rather than the family level. Because child heath utilization is likely to be quite different from adult utilization, I restrict the sample to individuals greater than 18 years of age. Additionally, because the NSAF questionnaire for 1997 did not inquire about the reason for delayed health care (i.e., was delay due to lack of insurance or money), the four health outcomes of interest are undefined for 1997 data, and the regression sample is therefore limited to 1999 and 2002 data.[29]

Results for the specification including only state-year fixed effects, displayed in column (1), show positive coefficients on *PaydayAccess* for each of the four dependent variables. *PaydayAccess* coefficients are strongly statistically significant for *Any Care Postponed* and *Medical Care Postponed*, and are significant at the 10 percent level for *Drug Purchase Postponed*. The implied average incremental effect of loan access is 4.6 percentage points for *Any Care Postponed*, 3.0 percentage points for *Dental Care*

[29] In principle, one could analyze variables defined on postponement of care without knowing the reason for delay. This would introduce measurement error in the left hand side variable, reducing precision of the *PaydayAccess* estimates.

Postponed, 1.9 percentage points for *Medical Care Postponed*, and 1.3 percentage points for *Drug Purchase Postponed*.

The inclusion of county-level controls reduces the *PaydayAccess* point estimates modestly for *Drug Purchase Postponed*, and substantially for the other three dependent variables. County controls also improve the precision of the estimated effects on all four outcomes. The results, which are given in column (2), suggest that loan access has a statistically significant effect on the likelihood of *Any Care Postponed* and *Drug Purchased Postponed*, raising the former by 3.7 percentage points and the latter by 1.2 percentage points. The estimated effects on *Dental Care Postponed* and *Medical Care Postponed*, though not quite significant, are 2.2 and 0.8 percentage points, respectively.

Person-level control variables are potentially quite important in this model. Illustratively, whether an individual had a spell without health insurance in the prior year proves to be an important control variable, and its inclusion causes *PaydayAccess* coefficients to rise for each of the four outcomes. As shown in column (3), the specification including person-level controls confirms the finding that *PaydayAccess* increases the likelihood of *Any Care Postponed* and *Drug Purchase Postponed*; average incremental effects are 4.2 and 1.5 percentage points, respectively. Relative to the prior specification, *PaydayAccess* coefficients for *Medical Care Postponed* and *Dental Care Postponed* rise slightly; the positive coefficient for the former is significant at the 10 percent level, while the coefficient for the latter is not quite significant.

Finally, results for the fully controlled specification are given in column (4). As in the case of the non-health outcomes, adding a border dummy to the model increases the estimated effect of loan access. *PaydayAccess* coefficients in this specification are positive and strongly significant for *Any Care Postponed* (4.5 percentage point effect) and *Drug Purchase Postponed* (1.8 percentage point effect), and significant at the 10 percent level for *Medical Care Postponed* (1.3 percentage point effect) and *Dental Care Postponed* (2.6 percentage point effect).

Across the four specifications, the magnitudes of the estimated *PaydayAccess* effects are substantial. Average incremental effects imply roughly 25 percent increases in the likelihood of delayed care for each category. The unconditional likelihood of *Any Care Postponed* is 17.9 percent, and the estimated increase due to payday loan access is

4.5 percentage points. *Dental Care Postponed* shows an increase due to loan access of 2.6 percentage points, which is roughly 20 percent of the outcome's unconditional likelihood of 13.2 percent. *Medical Care Postponed* shows an increase of 1.3 percentage points over a 5.7 percent unconditional likelihood. Finally, *Drug Purchase Postponed*, which occurs among 6.6 percent of sampled individuals, is estimated to increase by 1.8 percentage points due to loan access.

D.3 Falsification Exercises

To further evaluate the model results, I perform three falsification exercises, which are presented in Table 6. First, I offer further confirmation that the effect of loan access is not confounded with an effect due to state border proximity. I estimate the coefficient on a border dummy (county within 25 miles of a border) in the sample of payday-allowing states, which excludes observations from Massachusetts, New Jersey and New York. Results are reported in column (1) of Panels A and B. Point estimates for the *Border* coefficient are generally negative, and are in no instances positive and significant, indicating that the positive effects of loan access are likely not border related.

In justifying the choice of a regression sample stratified by income, I hypothesized that geographic access to payday loans ought to have no effect on the outcomes of 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. I find support for this hypothesis among both sets of outcomes, as the results in column (2) of panel indicate. As shown in Panel A, I estimate small, slightly negative coefficients on *PaydayAccess* for each of the non-health outcomes when I restrict the sample to the pooled group of families with less than $15,000 or greater than $50,000 in income. Estimation results for the health outcomes, given in Panel B, offer further confirmation of the hypothesized null effect. The *PaydayAccess* point estimate for each health outcome is quite a bit smaller in this excluded income sample than it is in the main sample. Out of the nine outcomes, I find one significant result; the 1.3 percentage point negative effect of loan access on *Any Family Distress* is significant at the 10 percent level. The null findings are not estimated precisely enough to constitute strong evidence in support of the hypothesized null effect, but standard errors are generally smaller in magnitude as in the comparable specification for the main sample, so the primary determinants of the null results are lower point estimates on *PaydayAccess*. Furthermore, this exercise does not reveal a broad set of positive coefficients, as one would expect if there were some unobservable characteristic common to *PaydayAccess* areas, but unrelated to payday loan access, that also causes economic hardship.

I have also argued that payday loan stores were not accessible from New Jersey and New York in the 1997 survey year or from Massachusetts in the 1997 and 1999 survey years. Geographic access to the nearby states that eventually allowed payday loans should have no effect before loans were available. In the third falsification exercise, I test this hypothesis by restricting the sample to observations from the above state-years and regressing the outcome variables on *PaydayBorder*, the cross-sectional measure of access to payday-allowing states.[30] Results from this exercise are given in column (3) of Panels A and B. With the exception of *Cut Meals*, the non-health outcomes show small and insignificant coefficients on *PaydayBorder*, consistent with the hypothesized null effect. The null findings are driven mainly by lower point estimates, which are fairly small for each outcome. The only significant result is a negative effect on *Cut Meals*. For the health outcomes, I find a positive *PaydayBorder* coefficient for *Dental Care Postponed* (significant at the 5% level) and *Any Care Postponed* (significant at the 10% level), and small, statistically insignificant *PaydayBorder* coefficients for *Medical Care Postponed* and *Drug Purchase Postponed*. The positive finding on postponement of dental care raises the concern that for this outcome, there is some unobserved factor causing postponement of care that is unrelated to loan access. On the whole, however, this exercise does not show signs of systematically higher levels of hardship in *PaydayBorder* areas. In the next section, I will attempt to address this concern more formally.

[30] As in the main specification, I also include observations from payday-allowing states in the estimation sample. These observations do not contribute to the identification of the *PaydayBorder* coefficient but add precision in the estimation of county- and individual-level covariates.

E. Identification using Temporal Variation in Payday Loan Access

To further address the problem of confounding variation at the county level, I isolate temporal variation in *PaydayAccess* by estimating the following difference-in-difference model.

$$(3) \qquad \Pr\left(Outcome_{ijt}\right) = \Phi\begin{pmatrix} \alpha + \beta PaydayAccess_{jt} + \theta PaydayBorder_j + \varphi Post_t \\ +\gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt} \end{pmatrix}$$

In this model, *PaydayAccess* remains the independent variable of interest, and has the same definition and content as in the main specification. However, it is also identical to *PaydayBorder*Post*, the interaction of the static *PaydayBorder* variable and the time-changing *Post* variable. *Post* is a dummy variable that takes on a value of one if payday lenders were operating in the relevant bordering states during the sample year under consideration.[31] The resulting model is in the canonical form for a difference-in-difference analysis over time, with *PaydayAccess* as the treatment-post interaction and *PaydayBorder* as the treatment variable.

For example, in the cross-section loan access might correlate with the availability of other goods and services across state borders, or with county-level characteristics that influence household location decisions.

E.1 Difference-in-Difference Results, Non-Health Outcomes

Difference-in-difference results for the non-health outcomes are given in Table 7, Panel A. The first specification of this model, reported in column (1), includes state-year fixed effects, as well as family-level and county-level controls. The identifying assumption in this model is that, conditional on observables, outcomes in *PaydayBorder* areas would have trended similarly to non-*PaydayBorder* areas absent the emergence of payday lending. *PaydayAccess* coefficient estimates for this model are positive for each outcome, suggesting that improved access to payday loans over time is associated with a

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

greater likelihood of hardship. In this model, *Family Hardship* shows a 5.9 percentage point effect, which is significant at the 1 percent level. The effects of loan access on *Difficulty Paying Bills* (3.3 percentage points), *Moved Out* (1.0 percentage point) and *Cut Meals* (3.5 percentage points) are significant at the 10 percent level, while the effect on *No Phone* (0.5 percentage points) is not statistically significant.

The second specification, in column (2), weakens the model's identifying assumption by including county fixed effects in place of county-level control variables. In this case, only unobserved variables that exhibit change over time, in the same pattern as *PaydayAccess*, can bias the estimated effect of loan access. *PaydayAccess* point estimates for this specification are positive for all variables except *No Phone*. The effect of loan access on *Any Family Hardship* (4.1 percentage points) and *Moved Out* (2.2 percentage points) are both significant at the 10 percent level. The null effect on *No Phone*, and the 1.6 percentage point effect on *Difficulty Paying Bills* are quite a bit lower than the effects found in the main specification.

Because temporal variation in payday loan access is fairly limited, inferences are somewhat weaker compared to the main specification. Overall, the results provide modest confirmation that *PaydayAccess* increases the likelihood of the non-health outcomes, as found in the main specification.

E.2 Difference-in-Difference Results, Health Outcomes

To estimate difference-in-difference specifications for the health variables, I must slightly alter the outcome measures and incorporate the 1997 data.[32] Since I do not know the reason for postponement of health care in the 1997 data, I redefine each variable based on whether or not care was postponed or foregone, regardless of the reason.

Difference-in-difference results for the altered health measures (denoted with asterisks) are given in Table 7, Panel B. In column (1), I present the results for the specification including county-level controls rather than county fixed effects. The *PaydayAccess* coefficients for *Any Care Postponed**, *Dental Care Postponed** and *Drug*

[32] Temporal variation in *PaydayAccess* in the 1999 and 2002 data is too limited to form useful estimates for the unaltered health measures.

*Purchase Postponed** are positive, but none of these estimates are statistically significant. Notably, *Medical Care Postponed** shows a decline in likelihood due to temporal changes in loan access, which is the opposite sign of the effect found in the main specification.

Substituting county fixed effects in place of county-level controls does not have much effect on *PaydayAccess* estimates. *PaydayAccess* coefficients, as shown in column (2), remain statistically insignificant for each outcome. With the exception of *Medical Care Postponed** these results show a pattern of greater delay of care due to loan access, but all of the effects are quite imprecisely estimated, so I hesitate to draw strong conclusions from this evidence. The estimates for *Any Care Postponed** (3.3 percentage points) and *Dental Care Postponed** (1.5 percentage points) are somewhat smaller than the effects found in the main specification, indicating that county-level unobservables might be inducing some bias in *PaydayAccess* coefficients for these outcomes in the main specification. On the other hand, the effect on *Drug Purchase Postponed** (1.6 percentage points) is quite similar to the finding in the main specification.

F. Identification using Variation in Payday Loan Access across Family Income Groups

An important concern to address is the possibility that counties with loan access, as defined by *PaydayAccess*, might differ in the provision of safety net and welfare services to low-income groups, as compared to counties without access. Since control variables that measure these differences are lacking, I explore a further identification strategy that permits simultaneous estimation of the loan access effect with county-year fixed effects. As discussed earlier, use of payday loans is quite limited among individuals with family incomes below \$15,000, as individuals without bank accounts and steady employment are screened out of the market. Therefore, I propose isolating variation in loan access between those with incomes of \$15,000 to \$50,000 and those with incomes below \$15,000. 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, if anything, overcompensate for this potential source of bias.

$$(4) \qquad \Pr\left(Outcome_{ijt}\right) = \Phi\begin{pmatrix} \alpha + \beta PaydayAccess * Income15to50 + \theta PaydayAccess_{jt} \\ +\varphi Income15to50_{it} + \gamma X_{it} + \delta Z_j + \eta_{jt} + \varepsilon_{ijt} \end{pmatrix}$$

PaydayAccess has the same definition and content as in the main specification, and the regression sample is restricted to individuals with less than $50,000 in family income. *Income15to50* is a dummy for the $15,000 to $50,000 family income category. The independent variable of interest is *PaydayAccess* Income15to50*, which isolates differences in loan access between those in the two income categories. Estimation results for this model are given in Table 8, Panels A and B.

F.1 Results, Difference across Income Categories, Non-Health Outcomes

Results for the non-health outcomes are given in Panel A. The first specification includes county fixed effects, while the second specification includes county-year fixed effects. This change in specifications has little effect on the results. Therefore, I focus on the results, reported in column (2), from the version that includes county-year fixed effects. The effect of loan access is positive for each of the outcomes, but is strongest for *Family Hardship* (5.2 percentage points), *Difficulty Paying Bills* (4.7 percentage points), *Moved Out* (4.0 percentage points) and *Cut Meals* (3.8 percentage points). The underlying coefficient on *PaydayAccess* Income15to50* is statistically significant at the 10% level for *Difficulty Paying Bills*, and at the 5% level for *Moved Out*. These results indicate that even after differencing out the effect of *PaydayAccess* on the lower-income group, the effect of loan access remains positive.

F.2 Results, Difference across Income Categories, Health Outcomes

Results for the health outcomes, which are given in Panel B, show smaller effects of loan access in this differenced specification than in the main specification. Notably, all the coefficients are very imprecisely estimated. The implied effect on *Any Care*

Postponed (0.1 percentage points) and *Dental Care Postponed* (-1.1 percentage points) are quite a bit lower than in the main specification. The point estimates for the effects on *Medical Care Postponed* (0.8 percentage points) and *Drug Purchase Postponed* (1.1 percentage points) are only slightly below the estimates from the main specification. Lack of precision in estimation suggests that the health-related results from this model are not very informative.

G. County Work Flow Interactions

I also test whether *PaydayAccess* effects depend on the proportion of workers that commute to work in nearby payday-allowing states. Since individuals that regularly commute to a payday-allowing area face a lower cost of accessing loans, we would expect loan access to have a larger effect in counties with a larger proportion of such commuters, even after conditioning on proximity to a payday-allowing area. Using county-to-county workflow data collected by the Census, I define *Pct Workflow*, the proportion of workers in a county that commute to a payday-allowing state. I then estimate the model:

$$(5)\quad \Pr\left(Outcome_{ijt}\right) = \Phi\begin{pmatrix} \alpha + \beta PaydayAccess * Pct\ Workflow + \theta PaydayAccess_{jt} \\ +\varphi Pct\ Workflow_j + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt} \end{pmatrix}$$

In this specification, the parameter of interest is the coefficient on the interaction term *PaydayAccess*Pct WorkFlow*. Estimation results are given in Table 9. Results for the non-health hardship measures, shown in Panel A, 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 each outcome except *No Phone*. *Cut Meals*, *Any Family Hardship* and *Difficulty Paying Bills* show positive *PaydayAccess*Pct WorkFlow* coefficients that are significant at the 1 percent, 5 percent and 10 percent levels, respectively. *Moved Out* also shows a positive coefficient on the interaction term; this estimate is not quite significant at the 10 percent level. 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.

H. Further Robustness Checks

In Tables 10 and 11, I present robustness checks of the main specification for each set of outcomes. First, I assess robustness relative to functional form, by estimating a linear probability specification. Second, I estimate the model with sampling weights to confirm that survey design effects and survey response bias are not confounded with effects due to payday loan access.[33] Finally, I analyze two alternative measures of loan access, *LogDistance* and *Pct Pop < 15 miles* (defined in section IV.A). To address the concern that loans might have been available in bordering states due to lax regulatory oversight of payday loan companies in 1996, I limit the regression sample to 1999 and 2002 data. This specification does not require any assumptions about loan availability for the 1997 data.

Table 10 contains results for the non-health outcomes. Estimates from the linear probability specification, displayed in column (1), confirm the coefficient magnitudes and inferences of the probit estimates for each variable. Likewise, estimation results for the sample that excludes 1997 data, shown in column (2), largely confirm the direction and magnitude of the effects in the main specification. The main difference is that the effect on *Moved Out* becomes marginally significant when the 1997 data is excluded. The specification using regression weights, reported in column (3), confirms the positive and

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

statistically significant effect of *PaydayAccess* on *Any Family Hardship* and *Difficulty Paying Bills*, and shows a larger effect on *Moved Out*. In the specification using *LogDistance*, reported in column (4), I confirm the finding that easier access implies a greater likelihood of negative outcomes. That is, greater distance from payday-allowing states implies a lower probability of each outcome, with strongly statistically significant effects on *Any Family Hardship* and *Difficulty Paying Bills*. Finally, the model that uses *Pct Pop < 15 miles* in place of *PaydayAccess* also yields results qualitatively similar to the main findings, with slightly larger effects on *Any Family Hardship* (6.9 percentage points) and *Difficulty Paying Bills* (6.6 percentage points).

In Table 11, I repeat the same robustness checks for the health outcomes, with the exception of dropping the 1997 data, since the health-related analysis already excludes these observations. As with the non-health outcomes, the results from a linear probability specification are very similar to those of a probit specification. The weighted probit specification, reported in column (2), confirms the positive effect of *PaydayAccess* on *Any Care Postponed* and also shows a significant effect on *Dental Care Postponed*. Notably, regression weights reduce the *PaydayAccess* coefficient on *Medical Care Postponed*, and reduce the precision of the *PaydayAccess* coefficient on *Drug Purchase Postponed*, rendering each statistically insignificant. In the specification using *LogDistance*, reported in column (3), I find negative point estimates, confirming that areas closer to payday-allowing states have higher postponement of needed health care. In this specification, the only significant effects are on *Any Care Postponed* and *Dental Care Postponed*. Finally, the specification in column (4), using *Pct Pop < 15 miles* in place of *PaydayAccess*, confirms that loan access increases the likelihood of *Any Care Postponed* and *Medical Care Postponed*.

H.1 Addressing Sample Imbalance

As a final robustness exercise, I investigate whether the estimated effects of loan access are driven by sample imbalance across treatment and control groups. Immigrant status and race are the two key dimensions along which average characteristics differ among individuals with and without loan access, even after controlling for basic county-level observables. To assess the impact of these differences I estimate the main regression model among sub-samples, splitting the sample by race and immigrant status; results are displayed in Table 12. Estimated *PaydayAccess* coefficients among native-born individuals, shown in column (1) of each panel, are consistent with the main findings. Loan access increases hardship, with strongly statistically significant effects on *Any Family Hardship*, *Difficulty Paying Bills*, *Any Care Postponed* and *Drug Purchase Postponed*. The estimation results by racial sub-samples, given in columns (2) through (4) also generally support the conclusion that loan access increases hardship. Among whites I find statistically significant effects of loan access on *Any Family Hardship* (4.0 percentage point increase), *Any Care Postponed* (6.0 percentage point increase), *Medical Care Postponed* (2.5 percentage point increase) and *Drug Purchase Postponed* (3.4 percentage point increase). For African-Americans and Hispanics, point estimates of the coefficient on *PaydayAccess* suggest that loan access increases non-health hardship, but has little effect on health-related hardship. *PaydayAccess* coefficients are estimated very imprecisely in these regressions, however, so this evidence does not support strong conclusions about differential effects across racial categories. Overall, the results from this exercise suggest that sample imbalance, in racial composition and immigrant status, is not driving *PaydayAccess* estimates.

Finally, based on income, assets, insurance status and education, individuals with payday access are better off, which suggests that they should have a lower likelihood of negative outcomes in the absence of a direct *PaydayAccess* effect. If the differences in unobservable characteristics follow the same pattern, with treatment group members being better off than comparison group members (and less likely to experience negative outcomes), then the corresponding bias would be negative, implying that the true effect of *PaydayAccess* is at least as large as what I estimate.

V. Discussion and Interpretation of Results

A. Implied Effects of Borrowing

The incremental effects discussed previously represent averages across all individuals in the sample who have proximate geographic 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. Table 13 shows a series of calculations that provide a rough estimate of the implied effects of borrowing. These calculations are based on historical estimates of the number of households that borrowed at payday loan stores in 2001 (Fox and Mierzwinski 2001), adjusted to account for two important factors. First, cross-border geographic access is imperfect. Second, the historical estimate of payday borrowing covers a year-long period, but the hardship measures taken in the survey might reflect costs or benefits of borrowing not just in the past year, but over multiple years. Duration of exposure therefore matters, with a larger proportion of households borrowing at longer intervals. These calculations produce the following estimates: roughly 10 percent of sample households borrow and 6 percent of sample adults borrow.

Table 14 illustrates 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 in spite of their borrowing, so they should not be considered as contributing to the marginal effect of loan access. As Table 14 shows, an estimated 4.0 percentage point increase in *Difficulty Paying Bills* among households with loan access implies a 57 percentage point increase among borrowing households. This implies a substantial increase in distress over the baseline likelihood of 20 percent.

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 15, attests to this fact. Frequency of usage across borrowers is quite heterogeneous, with a substantial mass (around 25 percent) of borrowers using 1-2 loans per year, but also 30 percent 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. Under these assumptions, around 40 percent of borrowers face an annual interest burden of at least $500, while 10 percent of borrowers pay upwards of

$1000 in interest annually. This is a substantial commitment of resources relative to the uncommitted income of households with between 15 and 50 thousand of total income.

The estimated effects are intended to 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 (2009) 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.

B. Reconciling with Previous Findings

Consistent with this study's results, Skiba and Tobacman (2008) find evidence that payday borrowing substantially increases the likelihood of Chapter 13 bankruptcy filing. Their methodology, regression discontinuity based on a credit score threshold, identifies effects particularly for the least creditworthy payday borrowers. Zinman (2009) examines an outcome – late bill payments – that is closely related to the central dependent variable in this study. The results offer weak confirmation of the conclusion of this study; individuals are less likely to report late bill payments after payday lending is restricted.[34] On the contrary, Morse (2009) and Morgan and Strain (2008) find that payday loan access can be beneficial. One way in which these findings can be reconciled with this study's findings is by recognizing that the effects of loan access might be heterogeneous, both across consumers and across states of the world. Morse (2009) finds that in periods after natural disasters, payday loan availability benefits communities by increasing birth rates, and reducing mortgage foreclosures, death rates, and drug and

[34] The estimated fall in late bill payments is significant at the five percent level in the main differences-in-differences regression, but the estimated effect drops in magnitude and is statistically insignificant in a weighted specification that attempts to correct for sample attrition and sample imbalance. It is important to note that Zinman also finds, with more robust results, that consumers' subjective assessment differs; they perceive deterioration in their financial situation after payday lending is restricted.

alcohol clinic admissions. Though Morse's results seem to run counter to my findings, it is quite likely that the influence of payday loans in post-disaster periods differs from their influence in general, either because borrowers' circumstances differ or because the composition of borrowers differs after a disaster. Morgan and Strain (2008) find evidence that bounced check levels fall when payday loans are available; some individuals, therefore, seem to benefit from payday loan access by avoiding the costs of bounced checks. In light of this evidence, it is possible that the current study's hardship measures fail to detect small financial gains (e.g., financial benefits from avoiding bounced checks) experienced by some payday loan users, while detecting hardship costs of a smaller number of heavy borrowers.

VI. 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 form of consumer debt, since for many individuals they constitute the marginal source of credit**.** The effects of borrowing in this manner 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.

In light of other evidence that payday loans provide benefits in particular circumstances, the ideal policy response is uncertain. Recent state legislation governing short-term loans sanctions payday lending, but limits the duration and intensity of usage (i.e., the number of concurrent and consecutive loans).[35] Such policy offers promise of striking an effective balance: allowing individuals to avoid costly mistakes in short-term financial planning that lead to high delinquency fees, but also preventing individuals from borrowing repeatedly and bearing the negative effects of borrowing as identified in this study. Further research ought to investigate the basis for heterogeneity in the use and consequences of payday loans, and thereby provide the basis for more nuanced policy.[36]

[35] Florida, Illinois, Michigan, New Mexico, North Dakota and Oklahoma have implemented statewide databases tracking all payday loan transactions in order to enforce these restrictions.

[36] As an example, Bertrand and Morse (2009) investigate the impact of framing and disclosure of interest rates on payday borrowing.

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Appendix on Payday Loan Regulations

Regulatory Environment 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 percent 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.

Regulatory Environment in States Bordering Massachusetts, New Jersey and New York

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. Through a conversation with the Staff Attorney of the Consumer Credit Division, New Hampshire Department of Banking, I have 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 percent of those locations were not making loans in 1996.[37]

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 percent *per annum* by Conn. Gen. Stat. 36a-563). Vermont prohibited lending through an interest rate cap of 18 percent *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.

[37] 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).

Table 1: 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, 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.

Table 2: Effect of Distance to Payday-Prohibiting State on Number of Payday Loan Locations

In column (1) are OLS estimation results for the regression of the number of payday loan stores in zip code i on a dummy for the proximity of the nearest payday-prohibiting state. In column (2), I test whether this effect is stronger where the bordering zip codes contain a higher proportion of households in the $15,000 to $50,000 income category, from which most payday loan users are drawn. Specifically, I interact the key coefficient of interest with the proportion of bordering zip codes' population in the $15,000 to $50,000 category. Included in both regressions are state fixed effects, a control for the proximity of any state border, and a set of zip code-level controls sourced from the 2000 Census. These controls are: cubics in median income, population and land area; the proportion of the population in five racial/ethnic categories and five education categories; and the proportion in the following categories: foreign born, unemployed, living in an urban area, living in poverty, owning a home and owning a home mortgage.

$$Stores_i = \alpha + \beta\, Dist.Prohibiting\ State < 25\,miles_i + \theta\, Dist.Any\ State < 25\,miles_i + \delta X_i + \varepsilon_i$$

	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

** Significant at 5% level

Table 3: Sample Summary Statistics, Stratified by *PaydayAccess*

Summary statistics, stratified by *PaydayAccess*, are given for counties (Panel A) and individuals (Panel B) from payday-prohibiting states. The sample in Panel B is restricted to adults with family income of $15,000 to $50,000, as in person-level regressions. In each panel, the column "Diff." displays the unconditional mean difference across *PaydayAccess* status. Within Panel B, I explore whether individual-level differences are explained by basic county-level observables. Specifically, I regress the individual-level characteristics on cubics in county-level median income, population and percent urban population. The column "Adj. Diff" displays the result of this exercise, which is a difference in conditional means across *PaydayAccess* status. The final column indicates whether this adjusted difference is statistically significant at the 5% level.

	PaydayAccess = 0		*PaydayAccess = 1*		Diff.	Adj. Diff.	Adj. Diff. significant at 5% level
	obs	mean	obs	mean			
PANEL A:							
County-Level Characteristics							
Median Income	27	52,200	10	53,700	-	-	
Population	27	824,200	10	600,400	-	-	
Percent urban	27	0.955	10	0.912	-	-	
Unemployment	27	0.062	10	0.050	-	-	
Home ownership	27	0.591	10	0.682	-	-	
Percent white	27	0.646	10	0.802	-	-	
Percent black	27	0.138	10	0.082	-	-	
Percent hispanic	27	0.136	10	0.062	-	-	
Percent foreign born	27	0.19	10	0.097	-	-	
PANEL B:							
Individual-level Characteristics							
Income/Assets							
Family income	4181	31,500	1062	32,700	1,200	376	
Home owner	4181	0.397	1062	0.493	0.10	0.03	
Car owner	4175	0.749	1062	0.885	0.14	0.04	*
Employment/Insurance							
Collected unemployment last yr	4181	0.081	1062	0.087	0.01	-0.02	
Health insurance for past year	4181	0.710	1062	0.781	0.07	0.04	
Education							
No high school degree	4181	0.180	1062	0.140	-0.04	-0.02	
High school degree only	4181	0.617	1062	0.669	0.05	0.03	
College degree	4181	0.204	1062	0.190	-0.01	-0.01	
Race/Ethnicity							
White	4181	0.530	1062	0.706	0.18	0.05	*
Black	4181	0.203	1062	0.131	-0.07	0.01	
Hispanic	4181	0.208	1062	0.110	-0.10	-0.07	*
Asian/other	4181	0.059	1062	0.053	-0.01	0.00	
Other							
Age	4181	39.5	1062	40.3	0.80	0.02	
Male	4181	0.393	1062	0.397	0.00	0.01	
Married	4181	0.479	1062	0.477	0.00	0.00	
Foreign born	4181	0.308	1062	0.182	-0.13	-0.06	*

Table 4: Main Specification, Non-Health Outcomes

Below are estimation results from 20 separate probit regressions of hardship indicators (for family i, in county j, and year t) on *PaydayAccess* and a set of controls. The table is structured so that the left hand side variables differ across block rows and the right hand side variables differ across columns. Control variables, including state by year fixed effects, county-level controls (Z), family-level controls (X) and a general border control are layered into the model moving from left to right. Estimates are reported for the coefficient on *PaydayAccess*, but are suppressed for other right hand side variables. In each regression cell, I report the average incremental effect (in brackets), followed by the underlying probit coefficient, the probit coefficient standard error (in parentheses), the number of observations and a measure of model fit. In each specification, observations are grouped by county when calculating standard errors.

$$\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, PaydayAccess_{jt} + \gamma\, X_{it} + \delta\, Z_j + \eta_t + \varepsilon_{ijt})$$

		----------------Coefficient on *PaydayAccess*----------------			
	Mean	(1)	(2)	(3)	(4)
Any Family Hardship	**0.292**	**[0.024]**	**[0.036]**	**[0.042]**	**[0.051]**
		0.069	0.102**	0.128***	0.154***
		(0.048)	(0.049)	(0.049)	(0.048)
N		25038	25038	24998	24998
Pseudo R^2		0.00	0.00	0.07	0.07
Difficulty Paying Bills	**0.203**	**[0.032]**	**[0.040]**	**[0.044]**	**[0.049]**
		0.104*	0.130***	0.150***	0.167***
		(0.057)	(0.049)	(0.050)	(0.050)
N		25012	25012	24973	24973
Pseudo R^2		0.00	0.01	0.06	0.06
Moved Out	**0.012**	**[0.010]**	**[0.007]**	**[0.007]**	**[0.007]**
		0.273*	0.207	0.223	0.231
		(0.150)	(0.147)	(0.150)	(0.153)
N		25012	25012	24973	24973
Pseudo R^2		0.02	0.02	0.09	0.09
Cut Meals	**0.169**	**[-0.008]**	**[0.001]**	**[0.007]**	**[0.012]**
		-0.035	0.004	0.03	0.052
		(0.044)	(0.055)	(0.061)	(0.061)
N		24866	24866	24835	24835
Pseudo R^2		0.00	0.00	0.05	0.05
No Phone	**0.017**	**[0.006]**	**[0.005]**	**[0.006]**	**[0.007]**
		0.132	0.127	0.154	0.186
		(0.145)	(0.153)	(0.163)	(0.160)
N		24456	24456	24424	24424
Pseudo R^2		0.01	0.02	0.11	0.11
State X Year FEs?		Y	Y	Y	Y
County-level Controls?		N	Y	Y	Y
Family-level Controls?		N	N	Y	Y
Border Control?		N	N	N	Y

* Significant at 10% level   ** Significant at 5% level   *** Significant at 1% level

Table 5: Main Specification, Health Outcomes

Below are estimation results from 20 separate probit regressions of hardship indicators (for family i, in county j, and year t) on *PaydayAccess* and a set of controls. The table is structured so that the left hand side variables differ across block rows and the right hand side variables differ across columns. Control variables, including state by year fixed effects, county-level controls (Z), family-level controls (X) and a general border control are layered into the model moving from left to right. Estimates are reported for the coefficient on *PaydayAccess*, but are suppressed for other right hand side variables. In each regression cell, I report the average incremental effect (in brackets), followed by the underlying probit coefficient, the probit coefficient standard error (in parentheses), the number of observations and a measure of model fit. In each specification, observations are grouped by county when calculating standard errors.

$$\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, PaydayAccess_{jt} + \gamma\, X_{it} + \delta\, Z_j + \eta_t + \varepsilon_{ijt})$$

		----------------Coefficient on *PaydayAccess*----------------			
	Mean	(1)	(2)	(3)	(4)
Any Care Postponed	**0.179**	**[0.046]**	**[0.037]**	**[0.042]**	**[0.045]**
		0.178**	0.146**	0.175**	0.189***
		(0.080)	(0.071)	(0.070)	(0.069)
N		17601	17601	17581	17581
Pseudo R^2		0.01	0.01	0.09	0.09
Dental Care Postponed	**0.132**	**[0.030]**	**[0.022]**	**[0.026]**	**[0.026]**
		0.144	0.107	0.137	0.137*
		(0.099)	(0.086)	(0.084)	(0.081)
N		17608	17608	17588	17588
Pseudo R^2		0.01	0.01	0.08	0.08
Medical Care Postponed	**0.057**	**[0.019]**	**[0.008]**	**[0.011]**	**[0.013]**
		0.182**	0.082	0.120*	0.145*
		(0.071)	(0.064)	(0.068)	(0.075)
N		17607	17607	17587	17587
Pseudo R^2		0.01	0.01	0.14	0.14
Drug Purchase Postponed	**0.066**	**[0.013]**	**[0.012]**	**[0.015]**	**[0.018]**
		0.096*	0.093*	0.117**	0.140**
		(0.056)	(0.051)	(0.053)	(0.057)
N		17612	17612	17592	17592
Pseudo R^2		0.01	0.01	0.07	0.07
State X Year FEs?		Y	Y	Y	Y
County-level Controls?		N	Y	Y	Y
Person-level Controls?		N	N	Y	Y
Border Control?		N	N	N	Y

* Significant at 10% level   ** Significant at 5% level   *** Significant at 1% level

Table 6: Falsification Exercises

Below are results from 27 separate probit regressions. In each panel, the left hand side variables differ across block rows, and the right hand side variables differ across columns. Column (1) regressions investigate whether *PaydayAccess* estimates are confounded with a general border effect. I report the coefficient on *Border* in the model: $\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, Border_j + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$, which is estimated on the sample of payday-allowing states. Column (2) regressions test for a null effect of *PaydayAccess* among those who are outside of the \$15,000 to \$50,000 family income range that ecompasses most payday borrowers. I report the *PaydayAccess* coefficient in: $\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, PaydayAccess_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$, which is estimated on the pooled sample of observations with family income below \$15,000 or above \$50,000. Column (3) regressions test for a null effect of loan access in the time period before loans were available in the states bordering MA, NJ and NY. I report the *PaydayBorder* coefficient in: $\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, PaydayBorder_j + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$ which is estimated on a sample that excludes the 1997 and 1999 survey years for MA observations, and the 2002 survey years for NY and NJ observations (when loans were available). In each regression cell, the average incremental effect is given in brackets, followed by the underlying probit coefficient, the probit coefficient standard error (in parentheses), the number of observations and a measure of model fit. In each specification, observations are grouped by county when calculating standard errors.

Panel A				***Panel B***			
	Border (1)	*Excluded Income Categories Only* (2)	*Before Loan Avail.* (3)		*Border* (1)	*Excluded Income Categories Only* (2)	*Before Loan Avail.* (3)
Any Family	**[-0.019]**	**[-0.013]**	**[-0.004]**	*Any Care*	**[-0.016]**	**[0.007]**	**[0.020]**
Hardship	-0.060	-0.066*	-0.012	*Postponed*	-0.068	0.042	0.069*
	(0.058)	(0.039)	(0.051)		(0.054)	(0.046)	(0.041)
N	17918	36339	21477	N	12705	29650	25352
R^2	0.07	0.21	0.07	R^2	0.09	0.11	0.05
Difficulty	**[-0.012]**	**[-0.013]**	**[0.016]**	*Dental Care*	**[-0.011]**	**[0.003]**	**[0.031]**
Paying Bills	-0.050	-0.081	0.06	*Postponed*	-0.053	0.022	0.133**
	(0.058)	(0.050)	(0.046)		(0.057)	(0.057)	(0.058)
N	17904	36295	21458	N	12709	29655	25366
R^2	0.06	0.16	0.06	R^2	0.08	0.10	0.05
Moved Out	**[0.004]**	**[-0.004]**	**[-0.003]**	*Medical Care*	**[-0.002]**	**[0.002]**	**[0.004]**
	0.115	-0.246	-0.154	*Postponed*	-0.020	0.035	0.027
	(0.113)	(0.196)	(0.120)		(0.085)	(0.068)	(0.058)
N	17904	36295	21458	N	12706	29662	25364
R^2	0.09	0.17	0.09	R^2	0.14	0.18	0.07
Cut Meals	**[0.002]**	**[-0.006]**	**[-0.025]**	*Drug Purchase*	**[-0.015]**	**[0.002]**	**[-0.001]**
	0.008	-0.045	-0.111*	*Postponed*	-0.135**	0.024	-0.006
	(0.062)	(0.071)	(0.060)		(0.064)	(0.075)	(0.068)
N	17816	36180	21325	N	12711	29662	25368
R^2	0.05	0.22	0.05	R^2	0.08	0.12	0.06
No Phone	**[-0.005]**	**[-0.002]**	**[0.003]**				
	-0.149	-0.088	0.104				
	(0.126)	(0.107)	(0.128)				
N	17466	35430	20957				
R^2	0.11	0.23	0.11				
State X Year FEs?	Y	Y	Y	State X Year FEs?	Y	Y	Y
County Controls?	Y	Y	Y	County Controls?	Y	Y	Y
Family Controls?	Y	Y	Y	Person Controls?	Y	Y	Y
Border Control?	-	Y	Y	Border Control?	-	Y	Y

* Significant at 10% level ** Significant at 5% level *** Significant at 1% level

Table 7: Difference Over Time

Below are probit estimation results from 18 separate regressions. In each panel, the left hand side variables differ across block rows, and the right hand side variables differ across columns. All specifications include state by year fixed effects and individual-level controls. Column (1) specifications include county-level Census controls, while column (2) specifications include county fixed effects. I report the estimated coefficient on *PaydayAccess* and suppress the coefficient estimates for the other right hand side variables. The inclusion of *PaydayBorder* as a control variable isolates temporal variation in *PaydayAccess* in the estimation of β. Within each regression cell, I report the average incremental effect (in brackets), followed by the underlying probit coefficient, the probit coefficient standard error (in parentheses), the number of observations and a measure of model fit. In each specification, observations are grouped by county when calculating standard errors.

$$\Pr(Outcome_{ijt}) = \Phi\begin{pmatrix} \alpha + \beta\, PaydayAccess_{jt} + \theta\, PaydayBorder_j + \varphi\, Post_t \\ + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt} \end{pmatrix}$$

Panel A

	County-level Controls (1)	County FEs (2)
Any Family Hardship	**[0.059]**	**[0.041]**
	0.176***	0.123*
	(0.063)	(0.065)
N	24998	24998
R^2	0.07	0.07
Difficulty Paying Bills	**[0.033]**	**[0.016]**
	0.114*	0.055
	(0.062)	(0.063)
N	24973	24973
R^2	0.06	0.07
Moved Out	**[0.011]**	**[0.022]**
	0.313*	0.508*
	(0.183)	(0.291)
N	24973	22877
R^2	0.09	0.10
Cut Meals	**[0.035]**	**[0.025]**
	0.146*	0.105
	(0.082)	(0.095)
N	24835	24835
R^2	0.05	0.06
No Phone	**[0.005]**	**[0.000]**
	0.124	-0.003
	(0.184)	(0.171)
N	24424	23582
R^2	0.11	0.13
State X Year FEs?	Y	Y
County-level Controls?	Y	Y
Family-level Controls?	Y	Y
County FEs?	N	Y

Panel B

	County-level Controls (1)	County FEs (2)
*Any Care Postponed**	**[0.030]**	**[0.033]**
	0.102	0.113
	(0.078)	(0.081)
N	29502	29502
R^2	0.05	0.05
*Dental Care Postponed**	**[0.014]**	**[0.015]**
	0.059	0.063
	(0.108)	(0.111)
N	29516	29516
R^2	0.05	0.05
*Medical Care Postponed**	**[-0.004]**	**[-0.007]**
	-0.028	-0.05
	(0.087)	(0.103)
N	29514	29514
R^2	0.07	0.07
*Drug Purchase Postponed**	**[0.014]**	**[0.016]**
	0.103	0.113
	(0.074)	(0.078)
N	29518	29518
R^2	0.06	0.06
State X Year FEs?	Y	Y
County-level Controls?	Y	Y
Person-level Controls?	Y	Y
County FEs?	N	Y

* Significant at 10% level ** Significant at 5% level *** Significant at 1% level

Table 8: Difference Over Income Categories

Below are probit estimation results from 18 separate regressions. In each panel, the left hand side variables differ across block rows, and the right hand side variables differ across columns. All specifications include state by year fixed effects and individual-level controls. Column (1) specifications include county fixed effects, while column (2) specifications include county by year fixed effects. I report the estimated coefficient on the interaction term of interest, *PaydayAccess*Income15to50* , and suppress coefficient estimates for the other right hand side variables. Within each regression cell, I report the average incremental effect (in brackets), followed by the underlying probit coefficient, the probit coefficient standard error (in parentheses), the number of observations and a measure of model fit. In each specification, observations are grouped by county when calculating standard errors.

$$\Pr\left(Outcome_{ijt}\right)=\Phi\left(\begin{array}{l}\alpha+\beta\, PaydayAccess * Income15to50+\theta\, PaydayAccess_{jt} \\ +\varphi\, Income15to50_{it}+\gamma\, X_{it}+\eta_{jt}+\varepsilon_{ijt}\end{array}\right)$$

Panel A

	County FEs (1)	*County-year FEs* (2)
Any Family Hardship	**[0.053]**	**[0.052]**
	0.152	0.149
	(0.109)	(0.112)
N	34513	34497
R^2	0.07	0.07
Difficulty Paying Bills	**[0.049]**	**[0.047]**
	0.157*	0.153*
	(0.088)	(0.091)
N	34464	34398
R^2	0.06	0.06
Moved Out	**[0.040]**	**[0.040]**
	0.608**	0.572**
	(0.243)	(0.267)
N	33004	28793
R^2	0.10	0.11
Cut Meals	**[0.035]**	**[0.038]**
	0.126	0.139
	(0.142)	(0.144)
N	34259	34232
R^2	0.06	0.07
No Phone	**[0.001]**	**[0.002]**
	0.014	0.032
	(0.092)	(0.094)
N	32833	29630
R^2	0.12	0.13
State X Year FEs?	Y	Y
Family-level Controls?	Y	Y
County FEs?	Y	-
County-year FEs?	N	Y

Panel B

	County FEs (1)	*County-year FEs* (2)
Any Care Postponed	**[0.001]**	**[0.001]**
	0.005	0.003
	(0.154)	(0.156)
N	23201	23201
R^2	0.09	0.09
Dental Care Postponed	**[-0.011]**	**[-0.011]**
	-0.061	-0.059
	(0.151)	(0.153)
N	23179	23154
R^2	0.08	0.09
Medical Care Postponed	**[0.009]**	**[0.008]**
	0.09	0.084
	(0.162)	(0.167)
N	23022	22711
R^2	0.14	0.15
Drug Purchase Postponed	**[0.011]**	**[0.011]**
	0.086	0.09
	(0.122)	(0.117)
N	23187	23082
R^2	0.07	0.08
State X Year FEs?	Y	Y
Person-level Controls?	Y	Y
County FEs?	Y	-
County-year FEs?	N	Y

* Significant at 10% level   ** Significant at 5% level   *** Significant at 1% level

Table 9: County Workflow Interactions

Below are the results from 9 separate regressions that investigate whether the effect of loan access is stronger in counties from which a larger percentage of workers commute to a payday-allowing state. Probit coefficients and standard errors are reported for the interaction between *PaydayAccess* and *Pct Workflow Payday*, the percentage of workers commuting to a payday state, as well as for the main effect on *PaydayAccess*. Observations are grouped by county when calculating standard errors. Each specification includes state by year fixed effects, individual-level controls and county-level controls.

$$\Pr(Outcome_{ijt}) = \Phi\begin{pmatrix} \alpha + \beta\, PaydayAccess * PctWorkflow + \theta\, PaydayAccess_{jt} \\ + \varphi\, PctWorkflow_{j} + \gamma\, X_{it} + \delta\, Z_{j} + \eta_{t} + \varepsilon_{ijt} \end{pmatrix}$$

Panel A			***Panel B***		
Any Family Hardship	*PaydayAccess X Pct Workflow*	2.179** (0.865)	*Any Care Postponed*	*PaydayAccess X Pct Workflow*	-1.085 (1.166)
	PaydayAccess	-0.005 (0.073)		*PaydayAccess*	0.186* (0.112)
N		24998	N		17581
R^2		0.07	R^2		0.09
Difficulty Paying Bills	*PaydayAccess X Pct Workflow*	1.502* (0.850)	*Dental Care Postponed*	*PaydayAccess X Pct Workflow*	-2.45 (1.805)
	PaydayAccess	0.026 (0.088)		*PaydayAccess*	0.230** (0.109)
N		24973	N		17588
R^2		0.06	R^2		0.08
Moved Out	*PaydayAccess X Pct Workflow*	2.308 (1.936)	*Medical Care Postponed*	*PaydayAccess X Pct Workflow*	0.56 (1.071)
	PaydayAccess	0.335 (0.224)		*PaydayAccess*	0.037 (0.091)
N		24973	N		17587
R^2		0.09	R^2		0.14
Cut Meals	*PaydayAccess X Pct Workflow*	2.067*** (0.555)	*Drug Purchase Postponed*	*PaydayAccess X Pct Workflow*	-1.452 (1.197)
	PaydayAccess	-0.049 (0.086)		*PaydayAccess*	0.143* (0.086)
N		24835	N		17592
R^2		0.05	R^2		0.07
No Phone	*PaydayAccess X Pct Workflow*	-2.406 (1.584)			
	PaydayAccess	0.299 (0.254)			
N		24424			
R^2		0.11			
State X Year FEs?		Y	State X Year FEs?		Y
County-level Controls?		Y	County-level Controls?		Y
Family-level Controls?		Y	Person-level Controls?		Y

* Significant at 10% level ** Significant at 5% level *** Significant at 1% level

Table 10: Robustness Checks, Non-Health Outcomes

Below are results from 25 separate regressions of family hardship on measures of loan access and controls. Column (1) displays OLS estimation results for the coefficient on PaydayAccess in a linear probability model: $Outcome_{ijt} = \alpha + \beta\, PaydayAccess_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt}$. Columns (2) and (3) display probit estimates for the *PaydayAccess* coefficient in: $\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, PaydayAccess_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$
The estimation sample in the column (2) specification excludes 1997 data, while column (3) is estimated using sampling weights. The specifications in columns (4) and (5) use alternative definitions of loan access. Column (4) evaluates *LogDistance,* the log distance between a family's county and the nearest payday-allowing state: $\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, LogDistance_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$. Column (5) evaluates *Pct Pop < 15 miles,* which, for each family, measures the percentage of their county's population living within 15 miles of a payday-allowing state (this percentage is calculated using the location and population of the census tracts that compose each county): $\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, Pct\,Pop < 15\,miles_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$
Average incremental effects, where relevant, are given in brackets, followed by the underlyign probit (or OLS) coefficients and standard errors. Observations are grouped by county when calculating standard errors.

	---------------------*PaydayAccess*	---------------------	---------------------	*LogDistance*	*Pct Pop < 15 miles*
	OLS, Linear Probability Model	*Probit, Without 1997 data*	*Probit, National Weights*	*Probit*	*Probit*
	(1)	(2)	(3)	(4)	(5)
Any Family Hardship		**[0.055]**	**[0.051]**	**[-0.023]**	**[0.069]**
	0.051***	0.168***	0.159**	-0.070**	0.215***
	(0.016)	(0.047)	(0.068)	(0.028)	(0.056)
N	24998	14960	21100	3521	24998
R^2 or Pseudo-R^2	0.08	0.07	0.08	0.06	0.07
Difficulty Paying Bills		**[0.051]**	**[0.058]**	**[-0.021]**	**[0.066]**
	0.048***	0.175***	0.204**	-0.074***	0.235***
	(0.015)	(0.057)	(0.085)	(0.022)	(0.043)
N	24973	14935	21081	3515	24973
R^2 or Pseudo-R^2	0.06	0.06	0.08	0.06	0.06
Moved Out		**[0.009]**	**[0.018]**	**[-0.003]**	**[0.004]**
	0.010	0.270*	0.495**	-0.083	0.132
	(0.006)	(0.160)	(0.195)	(0.118)	(0.192)
N	24973	14935	21081	3312	24973
R^2 or Pseudo-R^2	0.01	0.09	0.14	0.14	0.09
Cut Meals		**[0.018]**	**[-0.023]**	**[-0.009]**	**[0.019]**
	0.011	0.08	-0.108	-0.038	0.082
	(0.014)	(0.060)	(0.111)	(0.024)	(0.067)
N	24835	14919	20963	3510	24835
R^2 or Pseudo-R^2	0.05	0.05	0.07	0.06	0.05
No Phone		**[0.008]**	**[0.011]**	**[-0.006]**	**[0.009]**
	0.008	0.189	0.255	-0.171*	0.269
	(0.007)	(0.180)	(0.161)	(0.103)	(0.214)
N	24424	14660	20649	3467	24424
R^2 or Pseudo-R^2	0.02	0.12	0.13	0.17	0.11
State X Year FEs?	Y	Y	Y	Y	Y
County-level Controls?	Y	Y	Y	Y	Y
Family-level Controls?	Y	Y	Y	Y	Y
With Border Control?	Y	Y	Y	N	Y

* Significant at 10% level   ** Significant at 5% level   *** Significant at 1% level

Table 11: Robustness Checks, Health Outcomes

Below are results from 16 separate regressions of health-related hardship on measures of loan access and controls. Column (1) displays OLS estimation results for the *PaydayAccess* coefficient in a linear probability model: $Outcome_{ijt} = \alpha + \beta\, PaydayAccess_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt}$. Column (2) displays probit estimates for the *PaydayAccess* coefficient in: $\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, PaydayAccess_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$, which is estimated using sampling weights. The specifications in columns (3) and (4) use alternative definitions of loan access. Column (3) evaluates *LogDistance*, the log distance between an individual's county and the nearest payday-allowing state: $\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, LogDistance_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$. Column (4) evaluates *Pct Pop < 15 miles*, which, for each individual, measures the percentage of their county's population living within 15 miles of a payday-allowing state (this percentage is calculated using the locationand population of the census tracts that compose each county):
$\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, Pct\,Pop < 15\,miles_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$
Average incremental effects, where relevant, are given in brackets, followed by the underlyign probit (or OLS) coefficients and standard errors. Observations are grouped by county when calculating standard errors.

	--------*PaydayAccess*--------		*LogDistance*	*Pct population within 15 miles*
	OLS, Linear Probability Model	*Probit, National Weights*	*Probit*	*Probit*
	(1)	(2)	(3)	(4)
Any Care Postponed		**[0.042]**	**[-0.017]**	**[0.041]**
	0.047***	0.179*	-0.073***	0.180**
	(0.017)	(0.103)	(0.025)	(0.081)
N	17581	17213	4144	17581
R^2	0.08	0.10	0.08	0.09
Dental Care Postponed		**[0.051]**	**[-0.011]**	**[0.022]**
	0.024	0.257**	-0.059*	0.121
	(0.016)	(0.113)	(0.031)	(0.094)
N	17588	17220	4147	17588
R^2	0.06	0.08	0.09	0.08
Medical Care Postponed		**[0.004]**	**[-0.001]**	**[0.017]**
	0.014*	0.042	-0.017	0.201**
	(0.007)	(0.125)	(0.052)	(0.079)
N	17587	17219	4148	17587
R^2	0.07	0.16	0.14	0.14
Drug Purchase Postponed		**[0.019]**	**[-0.002]**	**[0.012]**
	0.019***	0.158	-0.015	0.103
	(0.007)	(0.112)	(0.045)	(0.072)
N	17592	17224	4148	17592
R^2	0.04	0.10	0.07	0.07
State X Year FEs?	Y	Y	Y	Y
County-level Controls?	Y	Y	Y	Y
Person-level Controls?	Y	Y	Y	Y
With Border Control?	Y	Y	N	Y

* Significant at 10% level   ** Significant at 5% level   *** Significant at 1% level

Table 12: Estimation Sub-samples by Race and Immigrant Status

To investigate whether differences in racial and immigrant composition across treatment status are confounding the loan access effect, I estimate *PaydayAccess* coefficients within subsets of the main sample, and report the results of those 36 separate regressions below. The table is structured so that in each panel, the left hand side variables differ across block rows and the estimation sub-samples differ across columns. The specification in column (1) restricts the sample to US-born individuals, while the specifications in columns (2) through (4) restrict the sample to whites, african-americans and hispanics, respectively. All specifications include state by year fixed effects, county-level controls (Z), family- or individual-level controls (X) and a general border control. In each regression cell, I report the average incremental effect (in brackets), followed by the underlying probit coefficient, the probit coefficient standard error (in parentheses), the number of observations and a measure of model fit. Observations are grouped by county when calculating standard errors.

$$\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta\, PaydayAccess_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$$

Panel A

	Native born only (1)	*White only* (2)	*African-American only* (3)	*Hispanic only* (4)
Any Family Hardship	**[0.055]**	**[0.040]**	**[0.080]**	**[0.109]**
	0.169***	0.132**	0.216	0.292***
	(0.054)	(0.067)	(0.157)	(0.110)
N	20878	14596	3773	4218
R^2	0.07	0.09	0.04	0.04
Difficulty Paying Bills	**[0.049]**	**[0.035]**	**[0.068]**	**[0.104]**
	0.170***	0.137	0.194	0.314
	(0.047)	(0.085)	(0.181)	(0.192)
N	20857	14580	3768	4214
R^2	0.07	0.08	0.04	0.04
Moved Out	**[0.008]**	**[0.002]**	**[0.029]**	**[0.052]**
	0.264	0.094	0.473	0.745***
	(0.167)	(0.216)	(0.304)	(0.236)
N	20857	13790	3635	3958
R^2	0.10	0.13	0.12	0.11
Cut Meals	**[0.019]**	**[0.012]**	**[-0.027]**	**[0.041]**
	0.086	0.059	-0.111	0.136
	(0.066)	(0.075)	(0.165)	(0.123)
N	20744	14499	3739	4201
R^2	0.06	0.07	0.04	0.05
No Phone	**[0.007]**	**[0.004]**	**[0.013]**	**[0.038]**
	0.199	0.174	0.229	0.488
	(0.162)	(0.222)	(0.300)	(0.352)
N	20387	14072	3565	3939
R^2	0.13	0.14	0.13	0.09
State X Year FEs?	Y	Y	Y	Y
County Controls?	Y	Y	Y	Y
Family Controls?	Y	Y	Y	Y
Border Control?	Y	Y	Y	Y

Panel B

	Native born only (1)	*White only* (2)	*African-American only* (3)	*Hispanic only* (4)
Any Care Postponed	**[0.054]**	**[0.060]**	**[0.003]**	**[0.002]**
	0.213***	0.237***	0.015	0.008
	(0.072)	(0.073)	(0.205)	(0.128)
N	13603	10215	2882	3594
R^2	0.09	0.10	0.08	0.05
Dental Care Postponed	**[0.020]**	**[0.025]**	**[0.017]**	**[0.030]**
	0.099	0.118	0.117	0.186
	(0.088)	(0.102)	(0.197)	(0.136)
N	13610	10221	2883	3594
R^2	0.08	0.09	0.09	0.06
Medical Care Postponed	**[0.016]**	**[0.025]**	**[0.003]**	**[-0.013]**
	0.160*	0.249**	0.036	-0.275
	(0.088)	(0.108)	(0.224)	(0.209)
N	13606	10219	2839	3595
R^2	0.15	0.16	0.14	0.12
Drug Purchase Postponed	**[0.035]**	**[0.034]**	**[-0.010]**	**[-0.012]**
	0.258***	0.267***	-0.093	-0.105
	(0.062)	(0.092)	(0.191)	(0.248)
N	13611	10222	2886	3565
R^2	0.08	0.09	0.07	0.09
State X Year FEs?	Y	Y	Y	Y
County Controls?	Y	Y	Y	Y
Person Controls?	Y	Y	Y	Y
Border Control?	Y	Y	Y	Y

* Significant at 10% level   ** Significant at 5% level   *** Significant at 1% level

Table 13: Estimating Proportion of Borrowers within Sample

Households

1) Households, annual income $15,000 and $50,000, that used payday loans in 2001: 5.6 to 7 million (70%* of 8 to 10 million**)
2) Total households, annual income $15,000 and $50,000, in payday allowing states: 39.4 mil***
3) Proportion borrowing in 2001: 14% to 18% (from #2 and #3)
4) Proportion borrowing cross-border: 7% to 9% (assuming 50% fewer borrow cross-border)
5) Proportion borrowing over prior two years: 9% to 11.5% (assuming 30% more housholds borrow over longer period)

Adults

1) Proportion of adults with cross-border access borrowing: 5% to 7% (assuming 1.2 borrowing adults per HH and 2 adults per HH)

* Elliehausen and Lawrence 2001
** Fox and Mierzwinski 2001
*** U.S. Census 2000

Table 14: Treatment on the Treated

Below is a hypothetical decomposition of the estimated average treatment effect into a treatment effect on non-borrowers (on whom there is no effect of loan access), borrowers who would have already reported distress (so there is 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 50% higher than the proportion of sampled individuals reporting distress (30% v. 20% for *Difficulty Paying Bills* and 10% v. 6.6% for *Drug Purchase Postponed*).

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	3.0	0%	0.0%
Borrowers not reporting distress	7.0	57%	4.0%
			4.0%

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

	Percent of sample	Group effect	Contribution to avg. effect
Non-borrowers	90	0	0
Borrowers	5.4		
Borrowers already reporting distress	0.5	0%	0.0%
Borrowers not reporting distress	4.9	30%	1.5%
			1.5%

Table 15: Frequency of Payday Borrowing

Payday borrowing data from Florida and Oklahoma, compiled by Veritec Solutions Inc., show that loan usage is quite heterogeneous across borrowers, with a substantial proportion of borrowers using more than a dozen loans per year.

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.