

Spillovers from Costly Credit*

Brian T. Melzer

December 2016

Abstract

Recent studies of credit access among low-income households find that high-cost payday loans can exacerbate, rather than alleviate, financial distress. I find that households with payday loan access are also more likely to use food assistance benefits and less likely to make child support payments required of non-resident parents. I use an identification strategy built around cross-border access to loans, and supplemented with a variety of additional tests, to establish that payday lending plays a causal role in worsening financial distress. The findings suggest that borrowers in distress turn to transfer programs to supplement the household's resources and prioritize payday loan payments over other liabilities like child support. In that way, payday lending produces negative externalities—costs imposed on taxpayers that fund transfer programs and non-resident family members that fail to receive child support.

JEL classification: D14 (Personal Finance), G2 (Financial Institutions and Services), I38 (Government Policy; Provision and Effects of Welfare Programs)

Keywords: Household finance, consumer credit, credit access, payday loan, predatory lending, usury, child support, food stamps

*Kellogg School of Management, Northwestern University, 2001 Sheridan Road, Evanston, IL, 60208 (phone: 847-491-5053; email: b-melzer@kellogg.northwestern.edu). I thank Tom DeLeire, Lindsey Leininger, Annamaria Lusardi, David Matsa, and Justin Sydnor for their helpful comments and suggestions, Frank Limehouse for his assistance at the Chicago Census Research Data Center, and Heather Sieber for help with the manuscript. I received valuable feedback from seminar audiences at the Duisenberg School of Finance, the University of California, Davis Center for Poverty Research, and the University of Chicago Center for Human Potential and Public Policy. The University of California, Davis Center for Poverty Research provided financial support for this project through their Small Grants competition. The analysis presented in this paper was conducted while Melzer was a Special Sworn Status researcher of the U.S. Census Bureau at the Chicago Census Research Data Center. Research results and conclusions expressed are those of the author and do not necessarily reflect the views of the Census Bureau. This paper has also been screened by the Census Bureau to ensure that no confidential data are revealed.

1 Introduction

The expansion of credit to low- and moderate-income households in the United States was a notable development of the 2000s, with substantial growth not only in mortgage credit but also in short-term, unsecured credit such as overdraft loans provided by banks and cash advances provided by payday lenders. Subprime mortgage originations roughly tripled, growing from 7.5% of annual mortgage originations in 2001 to over 20% by 2006 (Furlong and Krainer 2007). Similarly, payday loan volume grew six-fold in the first half of the decade, increasing from \$8 billion in 1999 (Stegman 2007) to an estimated \$48.5 billion in 2006 (Stephens Inc. 2011).

When assessing the economic efficiency of these credit expansions, it is important to consider whether the increase in household indebtedness entailed social costs or benefits that borrowers and lenders failed to internalize in making their choices.¹ Recent research on the housing market highlights substantial negative externalities related to homeowners' increased leverage and risk of default, costs borne by taxpayers who have funded mortgage modification subsidies (Congressional Budget Office 2013), and by neighbors whose home values have declined with nearby foreclosures.² The goal of this paper is to examine external costs in short-term credit markets by testing whether the rise of payday lending affected child support payments to non-resident family members and participation in transfer programs funded by taxpayers.

There has been considerable debate about whether payday lending alleviates or exacerbates household financial distress. In principle, access to credit can improve welfare by allowing households to smooth expenditures through periods of income and consumption shocks. Under this view, payday loans facilitate important expenditures and prevent distress, which may also reduce child support delinquency and participation in transfer programs. However, payday loans are also quite expensive, with a typical bi-weekly interest charge of \$15 per \$100 borrowed. Given these high costs, loans that increase current consumption can also create hardship in the future, especially among individuals who underestimate or discount such risks due to cognitive biases, forecasting problems (Ausubel 1991; Brunnermeier and Parker 2005; Bond, Musto, and Yilmaz 2009) or self-

¹Externalities are just one consideration when assessing the efficiency of credit supply. As Zinman (2014) highlights, a complete evaluation requires examination of lenders' market power, borrowers' preferences and the impact of information differences between borrowers and lenders.

²A number of papers document price declines resulting from nearby foreclosures (Immergluck and Smith 2006; Harding, Rosenblatt, and Yao 2009; Campbell, Giglio, and Pathak 2011; Mian, Sufi, and Trebbi 2015; Gerardi, Rosenblatt, Willen, and Yao 2015).

control problems (Laibson 1997). This perspective yields the opposite predictions, namely that payday lending aggravates distress and increases child support delinquency and participation in transfer programs.

I test these predictions using the U.S. Census Bureau’s Survey of Income and Program Participation (SIPP). For a large national sample of households, the SIPP provides a comprehensive view of economic well-being, including information on employment status and income, financial hardship, transfer program participation, and child support payments. The data also cover a long time period—1991 to 2007—that encompasses the early 1990s, when payday loans were not readily available, as well as the mid-2000s, when payday loan stores were widespread. To measure geographic access to payday lending, I use county identifiers suppressed in the public SIPP data, but made available for this study through a secure Census Research Data Center. I follow the research design that Melzer (2011) used to identify the causal impact of payday lending among households in the National Survey of America’s Families (NSAF).

Identifying the effect of payday lending on household financial distress is challenging. Households seek payday loans particularly when they have fallen behind on other debt payments (Bhutta 2014), so it is difficult to disentangle whether borrowing is the cause of financial distress or the result. Furthermore, even after ignoring variation in loan *take-up* and focusing instead on loan *access*, the problem remains that lenders may choose where to locate in response to borrower demand and creditworthiness, resulting in correlation between borrower distress and loan access even in the absence of a causal relationship.

The empirical strategy addresses these issues by examining differences in loan access that result from households’ ability to borrow across state borders. Focusing on states that prohibit payday loans, I compare households that live close to payday-allowing states, where they can readily borrow by crossing the border, to households that live farther from such borders and have more limited access.³ This approach has two important features. First, the empirical measure of loan access is unaffected by lenders’ location choices, and instead depends only on a household’s proximity to the border and the prevailing lending regulations across the border. By construction, therefore,

³Industry data reveals substantial cross-border borrowing among residents of Georgia, where payday loans are prohibited (see “Georgia Border Residents Going out of State to Acquire Legal Short-term Cash Advances” 2007). Payday lenders also locate in greater numbers at the borders of prohibiting states, consistent with lenders serving cross-border borrowers (Melzer 2011; Bhutta 2014).

lenders' location choices will not cause bias. Second, the measure varies among residents of the same state, which provides identifying variation even after controlling flexibly for state outreach and enforcement policies that are known to influence food assistance and child support payments.

The model's identifying assumption is that, in the absence of payday lending, households close to a "payday border" would display similar rates of hardship as those living far from a payday border. This assumption might be violated if, for example, different types of households located in border areas or if economic conditions differed between border and non-border areas. The main analysis includes a variety of control variables to relax this assumption. These controls distinguish hardship at payday borders from hardship common to all border areas and from hardship explained by differences in income, employment, health insurance status, and demographic characteristics. The empirical strategy includes three additional tests to examine and further relax the identifying assumption. First, I restrict the comparison group to near-border counties, which are potentially more similar to payday-border areas in unobservable characteristics due to their proximity. Second, I carry out falsification tests to confirm that rates of hardship, program participation and child support delinquency among border and non-border residents are similar before payday loans become available at payday borders. Third, I use a differences-in-differences specification that isolates time-series variation in lending at payday borders due to the emergence or prohibition of lending in the nearby state. This model eschews the cross-county variation in loan access between border and non-border residents, and instead pins down the effect of payday lending by comparing payday-border residents to their geographic counterparts who lived near the border before payday credit became available.

Confirming results from Melzer (2011) within the larger SIPP sample, I find that lower-income households with proximate access to payday loans are more likely to experience economic hardship, particularly in the years during which payday loans were prevalent. The probability of hardship—as measured by the failure to pay important bills (mortgage, rent and utilities) or obtain needed medical care—increases by 4 percentage points where payday loans are available between 1997 and 2007. This estimate is not only statistically significant at the 5% level, it is also economically meaningful: it implies a 16% proportional increase relative to the 25% average rate of hardship in the sample. The estimate is nearly unchanged in the more narrow comparison of border and near-border residents. The falsification test shows no difference in hardship at payday borders between

1991 and 1996, which provides support for the main specification’s identifying assumption: risk of hardship is indeed similar at payday borders in the absence of payday lending. Finally, the differences-in-differences model implies a 3.1 percentage point ($p < 0.10$) increase in hardship when border residents gain access to loans.

These increases in hardship are also manifest in households’ use of food stamps. Among households with income below the Food Stamp Program’s eligibility cut-off, the probability of participation is 5.8 percentage points ($p < 0.01$) higher in areas with loan access, a 19% increase relative to the average participation rate in the sample. This estimate does not reflect general economic disadvantage, as households with proximate loan access have similar risk of unemployment and slightly higher incomes, on average. This estimate is also robust to excluding states’ interior residents from the comparison group. Digging more deeply into the cross-sectional correlation between payday lending and program participation reveals a time pattern consistent with a causal relationship. Specifically, the correlation between payday lending and food stamp receipt is absent in the early- to mid-1990s, before payday loans were available, and the correlation strengthens through the 2000s as payday lending gains further penetration. Within the formal differences-in-differences model, I find no significant difference in food stamps utilization at payday borders prior to payday lending—a coefficient of 1.5 percentage points ($p = 0.20$)—followed by an increase of 4.7 percentage points ($p < 0.01$) once loans become available.

Turning to child support payments, I find greater incidence of payment delinquency among lower-income households with proximate access to payday loans. Although households with loan access are no more likely to have child support obligations, they are nearly 10 percentage points ($p < 0.01$) less likely to make a payment conditional on having an obligation, which represents a 12% decline relative to the average payment rate in the sample. Reports by child support recipients confirm the same pattern: recipients living close to payday lending receive less support than they are owed—a 10 percentage point ($p < 0.10$) increase in delinquency—particularly when the payer lives nearby and therefore also has access to payday lending. Both of these findings hold, with similar or even greater statistical significance, in the restricted sample of border and near-border counties and in the differences-in-differences model. Delinquency rates are no higher at payday borders when loans are unavailable nearby, but then increase when loans become available, by an estimated 9 percentage points ($p < 0.05$) in data reported by recipients and 17 percentage points

($p < 0.01$) in data reported by payers. The data on child support recipients enables a further, quite stringent test to distinguish the effects of payday lending from potential omitted variables. In cases where a recipient lives at a payday border but the payer lives elsewhere in the state, with more limited access to loans, there should be no difference in payments received; it is only when the payer has access to loans that delinquency should arise. The evidence fits this pattern, as child support recipients at payday borders experience an increase in delinquency—by 6.9 percentage points ($p < 0.10$)—when the payer lives in the same county (proximate to payday lending), but no difference—a 0.63 percentage point ($p = 0.85$) estimate—when the payer lives elsewhere in the state. Payment delinquency, therefore, is not explained by the unobserved economic conditions or the demographic make-up of payday border counties. While the magnitude of the estimates vary somewhat across models and subsamples, collectively these findings show that child support delinquency increases where payday loans are available.

This study’s findings imply that payday lending aggravates distress. The findings furthermore suggest that as borrowers accommodate interest and principal payments on payday loan debt, they turn to transfer programs like food stamps to supplement the household’s resources and they prioritize loan payments over other liabilities like child support payments. In that way, payday lending entails negative externalities that result in excess supply of credit, unless offset by other market distortions.⁴

By characterizing externalities from borrowing, this study extends the academic literature on payday lending, which has largely focused on the costs and benefits of loan access for borrowers themselves. Only Morse (2011) and Cuffe (2013) consider externalities of payday lending on crime: Morse (2011) finds that payday lending reduces crime following natural disasters and Cuffe (2013) finds that payday lending leads to higher crime rates. Studies of payday lending and household well-being also show mixed results.⁵ A number of studies find that payday credit aggravates financial difficulties, leaving borrowers less able to pay important bills (Melzer 2011), at greater risk of credit default (Gathergood, Guttman-Kenney, and Hunt 2015) and bankruptcy (Skiba and Tobacman 2015), and more likely to underperform at work (Carrell and Zinman 2014). Behavioral

⁴Dobbie and Skiba (2013) find ambiguous effects of information asymmetry on credit supply in the payday loan market. They find evidence of adverse selection—borrowers seeking larger loans default more often—which may lead to credit rationing. Yet they also find that increasing the size of a loan for a given borrower reduces his default risk (the opposite of moral hazard), which induces the opposite effect, pushing lenders to supply more credit.

⁵Caskey (2012) provides an insightful overview of this literature.

biases may contribute to sub-optimal borrowing choices, as financial literacy is particularly low among payday borrowers (Lusardi and Tufano 2009; Lusardi and de Bassa Scheresberg 2013), and simple information disclosures designed to lessen cognitive biases limitations result in significant reductions in payday borrowing (Bertrand and Morse 2011). On the other hand, many studies find beneficial effects of credit access, such as reductions in checking account overdrafts (Morgan, Strain, and Seblani 2012; Bhutta, Skiba, and Tobacman 2015; Bhutta, Goldin, and Homonoff 2015), declines in foreclosures following natural disasters (Morse 2011), improvement to perceived financial well-being (Zinman 2010), and improvements in consumption smoothing between paychecks (Zaki 2014; Parsons and Van Wesep 2013). Two studies find no effect of payday lending on credit scores (Bhutta 2014; Bhutta, Skiba, and Tobacman 2015). The disparity in findings across studies may reflect heterogeneity in the effects of loan access, both across borrowers, for whom intensity of usage varies widely, and across states of the world.

This study also complements research on child support payments by showing that household borrowing influences payment delinquency. Prior research finds that payment delinquency becomes more prevalent as child support debt increases (Heinrich, Burkhardt, and Shager 2011; Cancian, Heinrich, and Chung 2013). My findings indicate that other types of indebtedness can have the same effect.

The rest of the paper proceeds as follows. Section 2 provides background on payday lending, food stamps and child support. Section 3 introduces the data and describes the research design, and Section 4 presents the findings. Section 5 concludes.

2 Background on Payday Lending, Nutrition Assistance Programs and Child Support

2.1 Payday Lending

Payday loans are small, short-term loans that evolved from check cashing transactions as check cashers began to advance funds against personal checks (Stegman 2007). In a typical transaction, the borrower receives \$350 of cash in exchange for a promise to repay \$400 in two weeks. The transaction fee—in this example, \$50—is typically around 15% of the loan amount. Underwriting

is minimal: borrowers must have a bank account and must show a recent pay stub as evidence of employment.

A recent study by the Consumer Financial Protection Bureau (Consumer Financial Protection Bureau 2013) uses administrative records for a sample of 15 million payday loans to characterize borrowers and their usage patterns. Borrowers have low to moderate incomes: the median borrower has annual income of \$22,500 and the vast majority of borrowers have income below \$50,000. Most borrowers also have weak credit histories and limited access to credit from cheaper sources such as credit card loans (Bhutta 2014; Elliehausen and Lawrence 2001). The median loan provides \$350 of credit for two weeks in exchange for a fee of \$52.50, or 15% of the loan amount (Consumer Financial Protection Bureau 2013). In practice, however, payday loans are used for much longer than two weeks, as borrowers commonly renew or roll over loans (Carter, Skiba, and Sydnor 2013). Almost 50% of borrowers use 11 or more loans per year and 14% of borrowers take out 20 or more loans per year (Consumer Financial Protection Bureau 2013). Annual interest payments are substantial, especially for heavy users: the median borrower pays \$460 in fees per year and the top quartile of borrowers pay more than \$780 in fees per year (Consumer Financial Protection Bureau 2013).

The payday lending industry emerged in the mid- to late-1990s and grew substantially, peaking at 25,000 store locations in 2006 before declining to 19,600 locations in 2010. Annual loan volume is estimated to have grown in parallel, from about \$8 billion in 1999 (Stegman 2007) to \$48.5 billion in 2006, before declining to \$40 billion in 2010 (Stephens Inc. 2011). The industry is regulated at the state level, through check cashing, small loan and usury laws. Restrictions on fees, loan amounts and loan rollovers are typical, and several states have taken a more extreme position, enacting or enforcing a prohibition on payday lending (McKernan, Ratcliffe, and Kuehn 2013). Prohibiting states are crucial in this study, which makes use of cross-border access within states that disallow payday lending as well as changes in cross-border access due to bordering state regulations. Over the study period, eleven states maintained prohibitions for some period of time. Connecticut, Massachusetts, New Jersey, New York and Vermont prohibited payday loans for the entire sample period. Maryland, Georgia, North Carolina and West Virginia enacted prohibitions within the sample period, as of June 2002, May 2004, April 2006 and August 2006, respectively. New Hampshire and Rhode Island sanctioned payday lending in January 2000 and July 2001, respectively. Online Appendix A1 documents the relevant laws and law changes in detail.

2.2 Nutrition Assistance

The Supplemental Nutrition Assistance Program (SNAP) is a federally funded transfer program that assists qualifying households in purchasing food. The program began in 1964 as the Food Stamp Program (FSP) and was re-titled as SNAP in 2008. I refer to SNAP and FSP benefits interchangeably as “food stamps.” To qualify for food stamps, a household must pass income and asset tests: those with income below 130% of the Federal Poverty Level (FPL) qualify, as long as their assets (excluding primary residence and retirement savings) do not exceed \$2,000. Currently, the poverty level for a family of four is \$23,850 per year, which means that households with annual income below \$31,005 qualify for food stamps as long as they lack \$2,000 in assets. The set of food stamp-eligible households overlaps strongly with the set of payday borrowers: more than two thirds of payday borrowers are below the income cutoff for food stamp eligibility.⁶

Food stamps are an important component of the social safety net for low-income households.⁷ Participation in the program is widespread—an average of 23 million people per year, or roughly 10% of the U.S. adult population, participated during this study’s sample period (1991 to 2007). And, for participating households, food stamps make up a significant share of their income. The SNAP benefit schedule prescribes payments that increase with household size and decrease with income. Using the current benefit schedule as a representative example, a family of four with monthly income of 100% FPL (\$1,988) would be entitled to \$204 in monthly benefits, which is more than 10% of its earned income.

Participation in the food stamps program varies considerably over time and across states, due to both economic factors and policy differences (Ganong and Liebman 2013). For example, national caseloads fell dramatically in the late 1990s, from 25.5 million people in 1996 to 17.2 million people in 2000, due to a strengthening economy and changes to welfare policies (Currie and Grogger 2001; Ziliak, Gundersen, and Figlio 2003). Though program funding and benefit levels are set federally, state food assistance agencies also play an important role in administering the program. States conduct marketing outreach, collect and certify application, and disburse benefits. Across states, estimated participation rates vary markedly, at least in part because of state-level differences

⁶ Author’s calculations using Consumer Financial Protection Bureau (2013).

⁷ Blundell and Pistaferri (2003) and Gundersen and Ziliak (2003) find that food stamps facilitate consumption smoothing for low-income households, especially those that experience a persistent income shock.

in program administration (Ratcliffe, McKernan, and Finegold 2008). In 2011, for example, the proportion of eligible individuals collecting benefits ranged from a low of 57% in California to a high of nearly 100% in Oregon, Washington and Maine (Cunningham 2014). An important feature of this study’s empirical strategy, which exploits within-state variation in loan access while controlling for state-by-time fixed effects, is that it avoids bias due to unobserved heterogeneity in program participation over time and across states.

2.3 Child Support

Child support orders are legal directives, established through a court or administrative hearing process, that mandate the financial support of children by nonresident parents. The rationale behind requiring child support is to ensure the well-being of children in single-parent households. Especially for low-income recipients, support payments are an important source of income: for the average recipient, support payments constitute 16% of income (Grall 2013), and for recipients with income below the poverty level, support payments constitute one third of income (Sorensen and Oliver 2002).

Despite being legal obligations that are not dischargeable in bankruptcy, child support obligations routinely go unpaid. Of the 6.3 million custodial parents due child support in 2011, only 43% received full payment, while 26% received no payment and 31% received partial payment (Grall 2013). In dollar terms, the \$23.6 billion of support that was paid in 2011 constituted less than two thirds of the \$37.9 billion that was due. This shortfall contributed to accumulated child support debt of \$111 billion by the end of 2011 (Office of Child Support Enforcement 2014).

Policies governing award determination and enforcement differ substantially across states (Heim 2003), which results in stark differences in payment delinquency. In 2011, for example, only 51% of obligations were paid in Alabama, whereas 84% of obligations were paid in Pennsylvania (Office of Child Support Enforcement 2014). As discussed above, the empirical design will isolate within-state variation in loan access to avoid bias due to omitted state-level factors that might influence both child support enforcement and payday lending regulations.

3 Data and Research Design

3.1 Data

The U.S. Census Bureau’s Survey of Income and Program Participation (SIPP) is the primary data source for this analysis. The SIPP is a longitudinal survey that tracks household well-being and public program participation over a period ranging from 32 to 48 months. While in the survey, participants report their monthly income, employment and transfer program participation through “core wave” interviews that recur every four months. Participants report additional information on economic hardship, wealth and child support payments, among other topics, at lower frequency. Samples in the SIPP panels range from 14,000 to 43,000 households (with smaller samples in earlier panels) and are designed to oversample low-income households for whom program participation is most likely, but are otherwise nationally representative and cover all 50 states.

This analysis uses the 1991, 1992, 1993, 1996, 2001 and 2004 SIPP Panels, which provide data for 1991 through the end of 2007. The earlier SIPP panels (1984–1990) were excluded because they lack information on economic hardship and the later panel (2008) was excluded because it was not yet published when the Census authorized this project. The analyses of economic hardship and child support use the Adult Well-being, Child Support Paid and Child Support Agreements Topical Modules, while the analysis of food stamps uses information from the higher frequency “core wave” interviews. Detailed geographic information, which is used to measure distance to payday lending, is unavailable in the SIPP public use files, so the analysis uses confidential data warehoused in the Census Research Data Center.

The SIPP data are merged with the following county-level data: economic and demographic information from the 2000 Census, monthly unemployment rates from the Bureau of Labor statistics and annual personal income information from the Bureau of Economic Analysis.

3.2 Research Design

The goal of the research design is to compare households that differ in payday loan access but otherwise have similar incidence of economic hardship, food stamp participation and child support delinquency. To approximate such quasi-random variation, I focus on cross-border borrowing,

exploiting the fact that residents of prohibiting states retain geographic access to payday loan stores if they happen to live near a state that allows lending.

I measure proximity to state borders using the household’s county of residence, the finest geographic area available for all SIPP respondents. To proxy for loan access, I define an indicator variable, $PaydayAccess_{ct}$, which is one for all residents of a county c that is less than 25 miles from a state that allows payday lending at time t , and is zero otherwise. In robustness exercises I use a continuous measure of payday loan access, $LogDistancePayday_{ct}$, which is the natural logarithm of the distance from the household’s county of residence to the nearest payday allowing state at time t . For falsification tests, I also define $PaydayBorder_c$, an indicator for whether a household is within 25 miles of a state that allows payday lending at any time between 1996 and 2007.

I estimate the relationship between each dependent variable and loan access using the model:

$$y_{icst} = \alpha + \beta PaydayAccess_{ct} + \gamma Border_c + \delta' X_{it} + \theta' Z_{ct} + \lambda_{st} + \varepsilon_{icst}, \quad (1)$$

where the unit of observation is household i in county c and state s at time t . All specifications include state-year fixed effects, denoted by λ , so that the effect of loan access is not identified from cross-state differences in hardship or state-level changes in hardship over time. All models also include $Border$, an indicator for whether the household lives within 25 miles of any state border, and vectors of household and county controls denoted by X and Z , respectively. The household-level controls are: natural logarithm of monthly income; number of household members; number of children; age of the head of household; and indicator variables for unemployment, lack of health insurance, home ownership, educational attainment (5 categories) and race (4 categories). The county-level controls include three static measures from the 2000 Census—cubics in county median income, population and percent urban population—as well as two time-varying controls, the county’s unemployment rate and the natural logarithm of per capita personal income in the year that the outcome is measured.

For the analysis of economic hardship and child support payments, the regression sample is limited to households with \$15,000 to \$50,000 of annual income, the range that encompasses the vast majority of borrowers. For the analysis of food stamp usage, the regression sample is limited to households with income below 130% of the FPL. In each analysis, the sample includes

households from all states, with identifying variation in *Payday Access* coming exclusively from payday-prohibiting states, but identifying variation in other covariates like log income and *Border* coming from payday-allowing states as well. In the Online Appendix, I show that the estimates for β are similar when estimated strictly among states that prohibit payday lending.

I estimate the model using ordinary least squares, with observations clustered by state-*Payday Border* combination in calculating standard errors.⁸ Allowing for county clusters is important, since hardship may be correlated within local areas and *Payday Access* is, by construction, correlated among residents of the same county and persistent within a county over time. The broader state-*Payday Border* clusters subsume county clusters and allow for further correlation in residuals among households that live in separate counties within the state but have similar proximity to a payday border. While in principle regional economic conditions may be correlated among payday border areas, in practice clustering at this broader level generates similar inferences as clustering at the county level.

The model's identifying assumption is that, in the absence of payday lending, areas with and without proximate loan access would have similar rates of hardship after controlling for observable differences. Importantly, *Payday Access* varies based on household location and the credit laws of bordering states, but is not affected by the location decisions of payday lenders and the regulatory decisions of the home-state lending supervisor. The endogenous choices of lenders and home-state regulators, therefore, do not cause bias in the estimated effect of loan access.

The identifying assumption might nevertheless be violated in certain cases. First, employment conditions and safety-net programs might differ between payday border and non-border areas. Second, households might migrate toward or away from payday borders depending on their risk of financial distress. The control variables for income, employment, health insurance, and family structure diminish both of these concerns but do not completely eliminate them. Third, the income-related sample restrictions might introduce bias if payday lending affects household earnings. Specifically, the experiences of households near the income cut-offs will not contribute to the estimate for β when a rise or decline in income pushes those households out of the sample. Previous

⁸In each state that prohibits payday lending between 1997 and 2007, there are two unique clustering units—households that live at current or future payday borders (for whom the *Payday Border* indicator is one) and households that do not (for whom the *Payday Border* indicator is zero). In the remaining states, which never prohibit payday lending, there is a single clustering unit per state.

studies offer no specific evidence on how payday loan access affects income, but Carrell and Zinman (2014) shows that payday loans worsen job performance and readiness among military personnel. This finding suggests that, if anything, the sample selection would exclude lower-income individuals for whom loans reduced income, leading the model to understate the negative consequences of loan access.

The empirical strategy supplements the evidence from Equation 1 with further tests to either relax or validate the identifying assumption. First, I narrow the geographic scope of comparison to identify differences in hardship at payday borders relative to “near-border” areas that are between 25 and 50 miles from a state border. The motivation for this test is that near-border residents are more likely than interior residents to face similar safety-net programs and economic conditions as their peers at payday borders. Second, I carry out falsification tests to confirm that proximity to payday borders has no effect on hardship, program participation and child support delinquency before payday lending emerges across the border. For these tests, I restrict the sample to the time period 1991–1996, before payday lending emerged, and estimate Equation 1 with *Payday Border* in place of *Payday Access*. Third, I use the full sample period of 1991–2007 to estimate a differences-in-differences specification that isolates time-series variation in lending at payday borders:

$$y_{icst} = \alpha + \beta \text{PaydayAccess}_{ct} + \eta \text{PaydayBorder}_c + \gamma \text{Border}_c + \delta' \mathbf{X}_{it} + \theta' \mathbf{Z}_{ct} + \lambda_{st} + \varepsilon_{icst}, \quad (2)$$

As in the falsification tests, the coefficient on *Payday Border* captures differences in hardship associated with proximity to payday borders irrespective of whether loans are available across the border. After controlling for *Payday Border*, the remaining identifying variation in *Payday Access* results from changes in cross-border loan access as payday lending emerges after 1996 and as bordering states pass laws to sanction or prohibit payday lending.⁹ The coefficient β therefore measures any differences in hardship that emerge at payday borders particularly when loans are available. The identifying assumption is weaker than in Equation 1, since it requires only that changes in cross-border loan access are uncorrelated with changes in hardship.

⁹*Payday Border* and *Payday Access* are analogous to the treatment and treatment-post interaction variables, respectively, in a canonical differences-in-differences model. Time effects typical of a differences-in-differences model are absorbed by the state-year fixed effects here.

3.3 Economic and Demographic Characteristics of Areas with Payday Loan Access

Table 1 displays sample statistics for a variety of economic and demographic variables, stratified by *Payday Access*. Counties at payday borders are, on average, less populous and more rural. The average population is 160,851, compared to 263,106 in areas without loan access. The share of urban population averages 50.8% in counties with loan access and 62.6% in counties without. Time-varying economic measures, summarized in Panel B, indicate that areas with loan access are slightly more prosperous, with lower rates of unemployment and higher per capita income. During the main sample period of 1997–2007, the average unemployment rate and per capita income are 4.5% and \$35,158 at payday borders, compared to 4.9% and \$33,995 in areas without loan access.

Summary statistics for SIPP households are shown in Panel C. The sample includes all low- to moderate-income households—\$15,000 and \$50,000 in annualized income—between 1997 and 2007. Within this sample, households with loan access have slightly higher monthly income (\$2,623 vs. \$2,569), higher rates of home ownership (62.6% vs. 52.9%), and lower risk of being without health insurance (18.4% vs. 21.3%), but they show no difference in unemployment risk (4.7% vs. 4.8%, with no statistically significant difference). Educational attainment is modestly higher for residents at payday borders, as a smaller proportion fall short of a high school diploma (9.4% vs. 12.2%) and a larger proportion graduate and attend some college classes (35.7% vs. 31.9%). Disparities in racial and ethnic composition are more striking, with fewer minorities residing at payday borders. The proportions of black and Hispanic households are 13.1% and 2.8% in counties with loan access, compared to 15.6% and 10.7% in counties without loan access. Heads of household, on average, are older in areas with loan access (53.1 years vs. 51.7 years). The overall household size and number of children are modestly lower in areas with loan access.

To summarize, areas with payday loan access appear to be more rural, less racially diverse and slightly more prosperous, which highlights the need for county-level and individual-level controls in the regression analysis that follows. Beyond controlling for these variables in the main analysis, I will also estimate the effect of loan access within subsets of the main sample (by racial categories and for the subsets of more populous and heavily urban areas) to ensure a closer match between the treatment and comparison groups. Finally, I will use falsification exercises—testing for the absence

of a *Payday Border* effect before loans are available—to evaluate whether county-level omitted variables lead to bias in the estimated effects of loan access.

4 Empirical Findings

4.1 Payday Loan Access and Economic Hardship

The first phase of the analysis examines the relationship between payday loan access and economic hardship, replicating results from Melzer (2011) using the SIPP sample. The SIPP’s survey questions are quite similar to those of the National Survey of America’s Families (NSAF), which is studied in Melzer (2011). However, the SIPP includes a longer time series and greater geographic coverage, which increases the number of prohibiting states analyzed from three in that study to eleven in this study.¹⁰

To measure economic hardship, the SIPP includes a battery of questions about the household’s ability to afford essential items and pay important bills over the prior four or twelve months. The questions about food-related hardship, which are of particular interest in conjunction with the analysis of food stamps, are coded into two indicator variables. *Cut Meals (adult)* takes the value of one if over the prior four months the adults cut the size of meals or skipped meals due to lack of money. Analogously, *Cut Meals (children)* is one if the children were not eating enough because the household could not afford more food, or zero otherwise. The questions about hardship on non-food expenditures are summarized in a single indicator variable, *Any Hardship*, which takes the value of one if the household was unable to pay its mortgage, rent or utilities bills or if anyone in the household failed to get needed dental or medical care over the prior twelve months.

The regression results, shown in Table 2, reveal higher incidence of economic hardship in areas proximate to payday lending. Households with cross-border loan access are 4.03 percentage points more likely to report hardship on the summary measure. This difference is statistically significant at the 5% level and represents a 16% increase relative to the average incidence of 24.9% among all households in the sample. In contrast, the measures of food-related hardship display smaller and statistically insignificant coefficients on the indicator for payday loan access. Food-related

¹⁰The SIPP sample covers 1991 to 2007 whereas the NSAF sample covered 1996 to 2002. The SIPP survey also samples households in all states, whereas the NSAF sampled primarily households in thirteen “focal” states.

hardship among adults is 41 basis points more prevalent in areas with loan access, a 9% proportional increase relative to the average rate of hardship in the sample (4.6%). Food-related hardship among children shows little relationship with loan access—a decrease of 1 basis point, or 3%. Such hardship, however, is exceedingly rare and the model therefore has limited statistical power. These findings regarding payday loan access—greater difficulty in paying important bills, but smaller and statistically insignificant differences in food-related hardship—match the results of Melzer (2011) in the NSAF sample.

The elevated rate of hardship in payday access areas is neither driven by general economic disadvantage nor common to all border areas. The estimates reported above are conditional on a broad set of demographic and economic controls, which include controls for household income and spells of unemployment and uninsurance. As illustrated by the coefficients reported in Table 2, these three control variables absorb substantial differences in hardship without eliminating the portion of hardship explained by *Payday Access*. The signs of these coefficients are as one would expect: households are more likely to report economic hardship when they have low income or experience spells without a job or health insurance. Each model in Table 2 also includes the *Border* indicator variable, which absorbs variation common to all border counties. The negative coefficients on the border indicator, which are statistically insignificant for broad hardship, but significant at the 10% level for food-related hardship, imply that hardship is, if anything, less prevalent at borders. The increase in hardship attributable to payday loan access is therefore very specific to borders between payday-prohibiting and payday-allowing states.

A further test, reported in the second column of Table 3, narrows the geographic scope of comparison used to identify the effect of loan access. The estimation sample excludes households residing in counties that are 50 miles or further from a state border. The goal of this test is to create treatment and comparison groups that are similar in unobserved economic conditions. Such unobserved differences are likely to be smaller among geographically proximate border and near-border counties than they are between border counties and more distant counties at the interior of the state. This model's *Payday Access* coefficient of 3.74 percentage points ($p < 0.05$) is very close to the estimate of 4.03 percentage points in the full sample.

The final set of tests examine time variation in economic hardship at payday borders. These tests show that economic hardship increases particularly when payday lending becomes prevalent

across the border. As shown in the third column of Table 3, geographic proximity to payday borders has a small and statistically insignificant effect on hardship between 1991 and 1996, before payday loans were available. This falsification test is quite informative, as it confirms that the main findings do not reflect a persistent difference in hardship in these particular border areas that pre-dates payday lending. Only in the mid-2000s, when payday loan stores were prevalent, does loan access predict greater rates of economic hardship. In the differences-in-differences model, which isolates time-series variation in loan access, I find a *Payday Access* coefficient of 3.14 percentage points ($p < 0.10$). This time variation in payday-access effects represents compelling evidence that payday lending causes hardship.

4.2 Payday Loan Access and Food Stamp Participation

The second phase of the analysis examines the relationship between payday loan access and food stamp utilization. In the main tests I measure utilization with an indicator variable for food stamp receipt and in further tests I consider the dollar amount of benefits received. To avoid measurement error caused by “seam bias,” I follow common practice among others using the SIPP by dropping observations outside of the interview month (Moore 2008).¹¹ As a result, the dependent variable is a measure of monthly food stamp usage at a frequency of every four months.

The main regression results, which are shown in Table 4, indicate that food stamp usage is higher among food-stamp eligible households that have payday loan access. Food stamp receipt is 5.80 percentage points ($p < 0.01$) more prevalent at payday borders between 1997 and 2007, a 19% increase relative to the average take-up rate of 30.8%.¹² The value of benefits received shows a similar difference, as reported in Appendix Table A2: monthly benefits are higher by \$12.07 for households with proximate loan access, a 20% increase over the \$60.03 monthly average in the regression sample. As with economic hardship, these findings are conditional on individual-level controls and county-level economic controls, as well as the *Border* control. The estimated effect of

¹¹Transitions in employment and program participation are reported at much greater frequency at seams between interviews than during interview reference periods, perhaps because households recall the recent month most clearly and repeat their response for prior months for which their memory is less clear.

¹²The food stamp participation rate within this sample is similar to the participation rate calculated by others using household survey data (Gundersen and Ziliak 2003). Consistent with Meyer, Mok and Sullivan (2009), who document systematic underreporting of transfer program participation in household surveys, I find lower rates of participation and lower average benefit levels than administrative data imply.

loan access is similar within the restricted sample of border and near-border counties. Within this specification, food stamp utilization is 6.52 percentage points ($p < 0.01$) higher in counties with loan access compared to those without.

The effect of loan access on food stamp receipt is smaller and statistically insignificant in the early years of the sample, before payday loans are available, and strong and statistically significant in the later years, once payday loans are widely available. The third model in Table 4 is a placebo test meant to evaluate whether proximity to a payday border predicts food stamp utilization even before loans become available across the border. The coefficient on *Payday Border* is 1.78 percentage points, which is both statistically insignificant and substantially smaller than the 5.80 percentage point *Payday Access* coefficient during the time period when payday loans are available. The fourth and final column of Table 4 displays the estimates for the differences-in-differences specification. In this model, loan access is associated with a 4.73 percentage point ($p < 0.01$) increase in food stamp usage. The coefficient on the *Payday Border* indicator remains insignificant at 1.45 percentage points ($p = 0.20$).

The analysis presented in Table 5 confirms the robustness of these results within demographic subsamples that were chosen to limit variation in characteristics that differ, on average, between areas with and without cross-border loan access. Within racial sub-samples, food stamp receipt increases with loan access, by 2.85 percentage points for whites and by 8.25 percentage points for African Americans. In the subset of counties with greater than 90% urban population, the estimated effect of loan access on food stamp participation remains positive and significant. The effects of loan access are also positive and significant, and even larger in magnitude, after dropping observations from smaller counties (those with population less than 500,000). Further analysis, reported in Online Appendix Table A2, also confirms the robustness of the main findings to sample weighting, probit estimation, clustering at the county level and using a continuous measure of proximity to payday lending, *Log Distance Payday*.

These findings for food stamp receipt may help to explain why measures of food-related hardship—cutting back on quantity or quality of meals due to lack of money—show no significant increase with

payday lending despite increased hardship along other dimensions; borrowers that would otherwise cut back on food consumption may avoid doing so by taking up food stamps.¹³

4.3 Payday Loan Access and Child Support

The final stage of the analysis studies the effects of payday loan access on child support payments, from the perspectives of both payers and recipients. The aim of this analysis is to judge whether payday loans facilitate payments of child support or instead whether debt service on payday loans crowds out support payments. For these tests, it should be the payer's proximity to payday lending that matters. The first portion of the analysis thus focuses on whether SIPP respondents who have child support obligations follow through by making a payment. The rest of the analysis evaluates whether child support recipients receive less money than they are due. Studying recipients serves two purposes. First, recipients are likely to report delinquency rates more accurately than payers, who may feel stigmatized for failing to pay their obligations. Second, the experience of recipients provides additional variation that strengthens the identification strategy, as described in Section 4.3.2 below.

4.3.1 Child Support Paid

In periodic topical module interviews SIPP respondents report whether they owe child support and whether they have made support payments over the past four months. I code an indicator for whether a household made any child support payment. The complement to this variable is full delinquency; that is, failing to make a payment. Since the survey does not ask about the size of the obligation, we cannot identify partial delinquency, or a payment less than the amount due.

Table 6 presents the regression results for child support payments as reported by payers. Roughly 4% of sample households report that they are required to make child support payments, and there no is significant difference in prevalence between areas with and without payday loan access. Though the requirement to pay child support does not vary with loan access, delinquency does. As shown in the second specification, households with proximate access to payday loans are 9.75 percentage points ($p < 0.01$) less likely to make a payment, a 12% decline relative to the

¹³Hoyne and Schanzenbach (2009) find that food consumption increases with food stamp take-up. Ratcliffe, McKernan, and Zhang (2011) and Mabli, Ohls, Dragoset, Castner, and Santos (2013) find that food stamp take-up reduces food-related hardship.

sample average of 79.1% that report making their required payments. The estimated effect of loan access remains relatively similar, at -8.97 percentage points ($p < 0.01$), within the sub-sample of border and near-border counties. Robustness tests reported in Online Appendix Table A3 also confirm the main findings in a sample restricted to payday-prohibiting states and in models that use sample weighting, probit estimation, clustering at the county level and *Log Distance Payday* as a measure of proximity to payday lending.

The SIPP data on child support payments unfortunately do not allow for the falsification using data from 1991 to 1996; only in the 1996 panel (with most interviews conducted in 1997 or later) did the SIPP questionnaire begin asking whether respondents were required to pay child support. The differences-in-differences estimation, however, is still feasible using data from 1997 through 2007, although it relies on substantially less time-series variation in *Payday Access* than the comparable models for economic hardship and food stamp utilization. The time-series variation after 1996 is caused by changes in payday loan laws in bordering states, but excludes variation due to the nationwide emergence of payday lending. The estimated coefficients on *Payday Access* and *Payday Border*, accordingly, are less precise than the differences-in-differences models for the other outcomes. Nevertheless, the point estimates are consistent with payday lending increasing child support delinquency at payday borders specifically when loans are available and not when loans are unavailable. The estimated effect of loan access is -17.22 percentage points ($p < 0.01$) and the estimated coefficient on *Payday Border* is 8.24 percentage points ($p = 0.18$).

4.3.2 Child Support Received

In periodic topical module interviews SIPP respondents report whether they are owed child support, the amount they are due and the actual payment received. As the main dependent variable, I use an indicator for delinquency—whether the recipient received less child support than owed. On average, roughly 61% of recipients receive less support than they are owed between 1997 and 2007. Respondents also provide partial information about the payer’s location—whether the payer lives in the same state and the same county as the recipient—which I use to measure the child support payer’s proximity to payday lending.

The first set of tests, reported in Table 7, examine payment delinquency for child support recipients that live in the same county as the payer. The child support payer’s proximity to

payday lending is known precisely in this sample since it is the same as the recipient’s. The regression results show that payment delinquency increases where payers have proximate access to payday loans. The first model’s *Payday Access* coefficient of 10.34 percentage points ($p < 0.10$) implies a nearly 17% increase in delinquency relative to the average rate in the sample. The second specification in Table 7, estimated within the subsample of border and near-border counties, shows a similar coefficient with slightly stronger statistical significance—11.75 percentage points ($p < 0.05$). This finding holds, as well, in the restricted sample of payday-prohibiting states and in models that use sample weighting, probit estimation and clustering at the county level, however the specification using *Log Distance Payday* shows no statistically significant negative relationship with payment delinquency (see Online Appendix Table A4). The placebo test, reported in the third column, reveals no relationship between child support delinquency and proximity to payday borders before 1997. The estimated coefficient of 1.38 percentage points ($p = 0.77$) is both small and statistically insignificant. Finally, the differences-in-differences model shows a strong positive relationship between payday loan access and child support delinquency. Delinquency rises by 9.02 percentage points ($p < 0.05$) when payday loans become available across the border. In contrast, the model reveals no difference in delinquency at payday borders before loans are available—a coefficient of 0.77 percentage points ($p = 0.81$). These results almost uniformly support the conclusion that payday loans increase child support delinquency.

The second set of tests, reported in Table 8, refines the identification strategy by using variation in payers’ locations among child support recipients in a given county. As in the prior specification, I define *Payday Access* based on the location of the recipient. To allow variation in payer location relative to recipient location, I expand the sample to include all recipients for whom the payer lives in the same state. I estimate the following model, where *PayerSameCounty* indicates whether recipient i ’s child support payer resides in the same county:

$$\begin{aligned}
 y_{icst} = & \alpha + \beta \text{PaydayAccess}_{ct} + \phi \text{PaydayAccess}_{ct} \times \text{PayerSameCounty}_{it} \\
 & + \mu \text{PayerSameCounty}_{it} + \gamma \text{Border}_c + \delta \mathbf{X}_{it} + \theta \mathbf{Z}_{ct} + \lambda_{st} + \varepsilon_{icst},
 \end{aligned} \tag{3}$$

The coefficient β measures differences in payment delinquency common to all child support recipients in counties with loan access, irrespective of the location of the payer. The coefficient ϕ

measures any further differences in payment delinquency for recipients with proximate loan access whose payer lives in the same county. Such payers also have proximate loan access, whereas payers that live in the same state but a different county typically do not: on average, only 30% of the population of payday-prohibiting states lives within 25 miles of a payday-allowing state. If payday lending has a causal impact on delinquency, we would therefore expect the interaction coefficient ϕ to be positive, reflecting co-located payers' increased access to loans at payday borders. On the other hand, if elevated child support delinquency at payday borders were instead driven by an omitted characteristic of those areas, we would expect such differences to be common to all recipients in the county and therefore to load through β rather than ϕ .¹⁴

The regression estimates from this model, reported in Table 8, support the conclusion that child support delinquency is tied to payday lending *per se*, rather than to an omitted characteristic of payday-border counties. Specifically, I find little difference in delinquency when child support recipients have loan access but their payers live elsewhere—the estimate for β is small and insignificant, at 0.63 percentage points ($p = 0.85$). The estimate for the interaction coefficient ϕ , on the other hand, is both positive and significant at the 10% level. The coefficient of 6.91 percentage points ($p < 0.10$) implies more than a 10% increase in child support delinquency among child support payers that have access to payday lending. This pattern is even stronger in the subsample of border and near-border counties, as the estimated interaction coefficient is 11.20 percentage points ($p < 0.01$). Finally, the falsification exercise using data from 1991 to 1996 shows no corresponding difference in delinquency attributable to *Payday Border* or its interaction with *Payer Same County*. These differences emerge only after payday loans become available. Overall, these additional tests provide compelling evidence that payday lending plays a causal role in reducing child support payments: delinquency appears within the county precisely among those for which the child support payer has access to loans and precisely during the time periods when payday loans are available.

¹⁴The coefficient on β would also be positive if child support payers were likely to locate at another payday-border county even when they do not co-locate in recipient's payday-border county. If payers that do not co-locate are evenly distributed within the state, on the other hand, then β should be zero.

5 Conclusion

Recent studies of credit access among low-income households find that high-cost payday loans can exacerbate financial distress, leading to increased difficulty paying mortgage, rent and utilities bills (Melzer 2011), increased risk of bankruptcy and credit default (Skiba and Tobacman 2015; Gathergood, Guttman-Kenney, and Hunt 2015), and performance declines on the job (Carrell and Zinman 2014). This paper confirms, within a large national sample of households, that payday lending aggravates economic hardship, and provides further analysis of whether payday lending affects others outside the borrowing household. To gauge such externalities, I examine child support payments and food stamp participation, and find that households with proximate access to payday lending are less likely to pay required child support and more likely to use food stamps, particularly during the time period when payday lending is prevalent.

This evidence suggests that households struggling to meet debt payments on payday loans adapt by reducing child support payments and by seeking transfer benefits to supplement household income. In both ways, the impact of payday lending spreads beyond the borrowing household and results in negative externalities. These social costs should be weighed alongside the direct costs and benefits of credit access for borrowers when evaluating policies that impact the supply of short-term credit.

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Table 1: Average Economic and Demographic Characteristics, by Payday Loan Access

	Mean <i>Payday Access</i> = 0	Mean <i>Payday Access</i> = 1	Difference Significant at 5%
<i>Panel A: County (Census)</i>	N = 154	N = 104	
Median annual income	43,431	42,156	
Population	263,106	160,851	*
Urban population (%)	62.6	50.8	*
<i>Panel B: County (BLS and BEA)</i>	N = 6,586	N = 2,592	
Unemployment rate (%)	4.9	4.5	*
Annual income per capita	33,995	35,158	*
<i>Panel C: Household (SIPP)</i>	N = 44,000	N = 14,000	
Income/Assets			
Monthly income	2,569	2,623	*
Homeowner? (%)	52.9	62.6	*
Employment/Health Insurance			
Unemployment spell? (%)	4.8	4.7	
Uninsurance spell?	21.3	18.4	*
Education			
Less than HS (%)	12.2	9.4	*
HS diploma	33.3	32.9	
Some college	31.9	35.7	*
College degree	14.7	14.1	
Graduate degree	7.8	7.9	
Race/Ethnicity			
White (%)	69.3	80.7	*
Black	15.6	13.1	*
Hispanic	10.7	2.8	*
Asian/Other	4.4	3.4	*
Other			
Age	51.7	53.1	*
Household size	2.4	2.2	*
Number of children	0.6	0.5	*

Notes: This table presents county-level and household-level summary statistics, stratified by *Payday Access*, an indicator for proximity to payday lending. The underlying sample includes monthly observations for households in the Survey of Income and Program Participation (SIPP) between 1997 and 2007 that have annual income between \$15,000 and \$50,000 and reside in a state that prohibits payday lending. Panel A reports summary measures from the 2000 decennial Census after collapsing the data to the county level. Panel B reports the average unemployment rate from the Bureau of Labor Statistics and per capita income from the Bureau of Economic Analysis after collapsing the data to the county-year level. Panel C reports the average economic and demographic characteristics, as reported in the SIPP, for the full household sample. Due to Census disclosure constraints, the number of observations in the household sample is rounded to the nearest thousand.

Table 2: Economic Hardship, Food Insecurity and Payday Loan Access

Dependent Variable:	Any Hardship	Cut Meals (Adults)	Cut Meals (Children)
Mean (%):	24.9	4.6	0.3
Payday Access	4.03** (1.97)	0.41 (0.78)	-0.01 (0.12)
Border	-0.81 (0.77)	-0.52* (0.29)	-0.12* (0.07)
Log of Household Income	-13.05*** (0.74)	-4.95*** (0.35)	-0.30*** (0.09)
Unemployment Spell	12.47*** (0.96)	4.36*** (0.56)	1.09*** (0.25)
Uninsured Spell	15.76*** (0.60)	2.56*** (0.30)	0.13 (0.08)
N	37,000	41,000	41,000
R^2	0.15	0.04	0.01
Household controls?	Y	Y	Y
County controls?	Y	Y	Y
State-year FEs?	Y	Y	Y

Notes: This table reports estimation results for regressions of economic hardship or food insecurity on an indicator for proximity to payday lending (*Payday Access*) and control variables. All specifications include state-year fixed effects, household-level controls and county-level controls. In addition to the covariates listed in the table, the household-level controls are: number of household members; number of children; age of the head of household; and indicator variables for home ownership, educational attainment (5 categories) and race (4 categories). The additional county-level controls are: cubics in median income, population and percent urban population from the 2000 Census; county unemployment rate from the Bureau of Labor Statistics; and the natural logarithm of county per capita personal income from the Bureau of Economic Analysis. The regression sample includes annual observations for all households in the Survey of Income and Program Participation (SIPP) between 1997 and 2007 that have annual income between \$15,000 and \$50,000. Standard errors, which are reported in parentheses, are calculated with observations clustered by state-*Payday Border* combination. Due to Census disclosure constraints, the number of observations in each regression is rounded to the nearest thousand.

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

Table 3: Economic Hardship and Payday Loan Access, Additional Tests

	Dependent Variable: Any Hardship			
	Main	Border and Near- Border Counties (< 50 miles)	Falsification	Diff-in-Diff
Time Period:	1997–2007	1997–2007	1991–1996	1991–2007
Mean (%):	24.9	24.2	20.8	23.3
Payday Access	4.03** (1.97)	3.74** (1.86)		3.14* (1.88)
Payday Border			-0.43 (1.20)	0.67 (0.91)
N	37,000	21,000	23,000	60,000
R^2	0.15	0.16	0.15	0.15
Household controls?	Y	Y	Y	Y
County controls?	Y	Y	Y	Y
State-year FEs?	Y	Y	Y	Y

Notes: This table reports estimation results for regressions of economic hardship on an indicator for proximity to payday lending (*Payday Access* or *Payday Border*) and control variables. All specifications include the same control variables used in the regressions reported in Table 2. The baseline specification in the first column is identical to the first column of Table 2. The second specification is estimated within a restricted sample: households that live within 50 miles of a state border. The third specification provides a falsification test by estimating the coefficient on *Payday Border*—an indicator for future proximity to payday lending—in the time period, 1991–1996, when payday loans were not readily available. The final specification is a differences-in-differences model estimated within the full sample period 1991–2007. It isolates time-series variation in loan access by estimating the coefficient on *Payday Access*, a measure of current proximity to payday lending, while controlling for *Payday Border*. Standard errors, which are reported in parentheses, are calculated with observations clustered by state-*Payday Border* combination. Due to Census disclosure constraints, the number of observations in each regression is rounded to the nearest thousand.

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

Table 4: Food Stamp Usage and Payday Loan Access

Dependent Variable: Any Food Stamps Used				
	Main	Border and Near- Border Counties (< 50 miles)	Falsification	Diff-in-Diff
Time Period:	1997–2007	1997–2007	1991–1996	1991–2007
Mean (%):	30.8	32.8	35.0	32.4
Payday Access	5.80*** (1.20)	6.52*** (1.65)		4.73*** (1.46)
Payday Border			1.78 (1.25)	1.45 (1.11)
N	154,000	87,000	101,000	255,000
R^2	0.19	0.19	0.25	0.21
Household controls?	Y	Y	Y	Y
County controls?	Y	Y	Y	Y
State-year FEs?	Y	Y	Y	Y

Notes: This table reports estimation results for regressions of an indicator for food stamp usage on an indicator for proximity to payday lending (*Payday Access* or *Payday Border*) and control variables. All specifications include the same control variables used in Table 2. The regression sample includes monthly observations for all households in the Survey of Income and Program Participation (SIPP) that are eligible for food stamps based on income (monthly income below 130% of the Federal Poverty Level). The baseline specification in the first column is estimated within the time period 1997–2007. The second specification is estimated in the same time period but within a restricted sample: households that live within 50 miles of a state border. The third specification provides a falsification test by estimating the coefficient on *Payday Border*—an indicator for future proximity to payday lending—in the time period 1991–1996, when payday loans were not readily available. The final specification is a differences-in-differences model estimated within the full sample period 1991–2007. It isolates time-series variation in loan access by estimating the coefficient on *Payday Access*, a measure of current proximity to payday lending, while controlling for *Payday Border*. Standard errors, which are reported in parentheses, are calculated with observations clustered by state-*Payday Border* combination. Due to Census disclosure constraints, the number of observations in each regression is rounded to the nearest thousand.

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

Table 5: Food Stamp Usage and Payday Loan Access in Demographic Subsamples

Dependent Variable: Any Food Stamps Used				
Sample:	Whites	African Americans	Counties with 90% or more urban population	Counties with 500,000 or more population
Mean (%):	23.7	45.5	30.4	30.7
Payday Access	2.85** (1.33)	8.25*** (1.91)	7.19*** (2.29)	13.53*** (4.17)
N	90,000	33,000	69,000	59,000
R^2	0.17	0.172	0.209	0.208
Household controls?	Y	Y	Y	Y
County controls?	Y	Y	Y	Y
State-year FEs?	Y	Y	Y	Y

Notes: This table reports estimation results for regressions of an indicator for food stamp usage on an indicator for proximity to payday lending (*Payday Access*) and control variables. The regression model is the same as the first model of Table 4. Each specification is estimated in a subset of the main sample. The sample restrictions are defined by race in the first two models, and by county population density and size in the second two models. Standard errors, which are reported in parentheses, are calculated with observations clustered by state-*Payday Border* combination. Due to Census disclosure constraints, the number of observations in each regression is rounded to the nearest thousand.

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

Table 6: Child Support Paid and Payday Loan Access

Dependent Variable:	Any Obligation	Any Child Support Paid		
	Main	Main	Border and Near- Border Counties (< 50 miles)	Diff-in-Diff
Time Period:	1997–2007	1997–2007	1997–2007	1997–2007
Mean (%):	4.2	79.1	78.4	79.1
Payday Access	-0.34 (0.24)	-9.75*** (3.30)	-8.97*** (2.80)	-17.22*** (6.30)
Payday Border				8.24 (6.09)
N	126,000	5,000	3,000	5,000
R^2	0.55	0.09	0.09	0.09
Control for marital status and child outside HH?	Y	N	N	N
Household controls?	Y	Y	Y	Y
County controls?	Y	Y	Y	Y
State-year FEs?	Y	Y	Y	Y

Notes: This analysis explores whether proximity to payday lending affects child support obligations and payments. The dependent variables in the first model is an indicator for whether anyone in the household has a child support obligation. The dependent variable in the remaining three models is an indicator for whether, conditional on having an obligation, anyone in the household made a child support payment. All specifications include the same control variables used in Table 2, and the analysis of child support obligations also controls for whether the respondent is married and whether the respondent has a child that lives outside the household. The baseline regression sample includes all households in the Survey of Income and Program Participation (SIPP) between 1997 and 2007 that have annual income between \$15,000 and \$50,000. In the analysis of child support payments, which begins in the second column, the sample is limited to households that have a child support obligation. The sample for the next specification is restricted further, to households that live within 50 miles of a state border. A falsification test using the sample period 1991–1996 is not feasible because the SIPP did not ask whether households had child support obligations during that time period. The fourth and final specification is a differences-in-differences model estimated within the sample period 1997–2007. It isolates time-series variation in loan access by estimating the coefficient on *Payday Access*, a measure of current proximity to payday lending, while controlling for *Payday Border*. Standard errors, which are reported in parentheses, are calculated with observations clustered by state-*Payday Border* combination. Due to Census disclosure constraints, the number of observations in each regression is rounded to the nearest thousand.

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

Table 7: Child Support Received and Payday Loan Access

	Dependent Variable: Child Support Received < Owed			
	Main	Border and Near- Border Counties (< 50 miles)	Falsification	Diff-in-Diff
Time Period:	1997–2007	1997–2007	1991–1996	1991–2007
Mean (%):	61.0	60.4	40.3	54.2
Payday Access	10.34* (5.21)	11.75** (4.44)		9.02** (4.39)
Payday Border			1.38 (4.66)	0.77 (3.24)
N	5,000	3,000	2,000	7,000
R ²	0.06	0.06	0.04	0.09
Household controls?	Y	Y	Y	Y
County controls?	Y	Y	Y	Y
State-year FEs?	Y	Y	Y	Y

Notes: This analysis explores whether proximity to payday lending affects child support delinquency, as reported by child support recipients. The dependent variable is an indicator for whether any child support recipient in the household received less support than he or she was owed. The baseline regression sample includes all households in the Survey of Income and Program Participation (SIPP) between 1997 and 2007 that are owed child support and that live in the same county as the payer. The latter restriction is necessary to measure the payer’s proximity to payday lending, which is not reported directly in the SIPP. By virtue of knowing the recipient’s location, however, we can measure variation in the payer’s access to loans as long as the payer lives in the same county. All specifications include the same control variables used in the regressions reported in Table 2. The baseline specification in the first column is estimated within the time period 1997–2007. The second specification is estimated in the same time period but within a restricted sample: households that live within 50 miles of a state border. The third specification provides a falsification test by estimating the coefficient on *Payday Border*—an indicator for future proximity to payday lending—in the time period, 1991–1996, when payday loans were not readily available. The final specification is a differences-in-differences model estimated within the full sample period 1991–2007. It isolates time-series variation in loan access by estimating the coefficient on *Payday Access*, a measure of current proximity to payday lending, while controlling for *Payday Border*. Standard errors, which are reported in parentheses, are calculated with observations clustered by state-*Payday Border* combination. Due to Census disclosure constraints, the number of observations in each regression is rounded to the nearest thousand.

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

Table 8: Child Support Received and Payday Loan Access, Using Variation in Payer Location

	Dependent Variable: Child Support Received < Owed		
	Main	Border and Near- Border Counties (< 50 miles)	Falsification
	Child Support Payer in Same State		
	Sample: Time Period: Mean (%):	1997–2007	1997–2007 1991–1996
Payday Access	0.63 (3.38)	-0.72 (3.39)	
Payday Access × Payer Same County	6.91* (3.92)	11.20*** (3.64)	
Payday Border			2.34 (3.74)
Payday Border × Payer Same County			2.55 (3.33)
Payer Same County	-10.55*** (3.75)	-13.70*** (3.34)	-8.60*** (2.80)
N	8,000	4,000	4,000
R ²	0.05	0.06	0.05
Household controls?	Y	Y	Y
County controls?	Y	Y	Y
State-year FEs?	Y	Y	Y

Notes: This analysis explores whether the relationship between payday loan access and child support delinquency varies with the child support payer’s location. The dependent variable is an indicator for whether any child support recipient in the household received less support than he or she was owed. The regression sample includes all households in the Survey of Income and Program Participation (SIPP) between 1997 and 2007 that are owed child support and that live in the same state as the payer. Each model includes an indicator for whether the payer lives in the same county (*Payer Same County*), as well as its interaction with the indicator for proximity to payday lending (*Payday Access* or *Payday Border*). All specifications include the same control variables used in the regressions reported in Table 2. The baseline specification in the first column is estimated within the time period 1997–2007. The second specification is estimated in the same time period but within a restricted sample: households that live within 50 miles of a state border. The third specification provides a falsification test by estimating the main and interaction coefficients on *Payday Border*—an indicator for future proximity to payday lending—in the time period, 1991–1996, when payday loans were not readily available. Standard errors, which are reported in parentheses, are calculated with observations clustered by state-*Payday Border* combination. Due to Census disclosure constraints, the number of observations in each regression is rounded to the nearest thousand.

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