Heterogeneous Effects of Household Credit: The Payday Lending Case

Christine L. Dobridge[†]

The Wharton School

November 2014

Abstract

I provide empirical evidence that access to credit has heterogeneous, state-dependent effects on household material well-being, even within the market for one particular credit product—in my case, payday lending. Using unique, detailed data on household location and consumption patterns, I show that access to payday credit lowers material well-being in "normal" states of the world. Payday loan access results in substantial declines in nondurable goods spending overall and in housing-related spending particularly. Following temporary negative shocks, however—extreme weather events like hurricanes and blizzards—I show that payday loan access helps households smooth consumption and improves material well-being. After extreme weather events, payday loan access mitigates declines in spending on food, mortgage payments, and home repairs.

†The Wharton School, University of Pennsylvania, 3620 Locust Walk, Suite 2400, Philadelphia, PA 19104. Phone: (240) 994-4716. E-mail: dobridge@wharton.upenn.edu.

I. Introduction

U.S. households are heavy users of credit. There was $13.2 trillion in household debt outstanding in 2010—about equal to total U.S. gross domestic product in that year. Seventy-seven percent of households held some form of debt, with the largest share of families holding mortgage debt (48.7 percent), followed by outstanding installment debt (46.9 percent) and credit card balances (46.1 percent). Debt payments represent a considerable fraction of household income as well. The median ratio of debt payments to family income was 18 percent for households holding debt in 2010.[1] Such high levels of household debt have tended to attract negative attention from the public and the media. But is credit access truly harmful to households and the economy?

Economic theory suggests the effects on well-being are instead likely to be heterogeneous. On one hand, canonical economic models of consumer credit show that credit access improves household utility by allowing users to smooth consumption over income fluctuations or other negative shocks. On the other hand, when individuals have an unusually strong preference for current consumption—problems of "self control" when it comes to consumption—credit access can lower household utility because household borrow to excess (Laibson, 1997; O'Donoghue and Rabin, 1999; Heidhues and Koszegi, 2010). In addition, credit access may lower well-being for some borrowers due to asymmetric information between lenders and borrowers, either because lenders are better able to forecast financial outcomes due to experiences with many borrowers (Bond, Musto and Yilmaz, 2009), or because of borrowers' poor financial literacy (Lusardi and Tufano, 2009). In these cases, individuals will borrow even if it makes them worse off in the end.

In this paper, I ask the question "Does credit access improve household well-being?" I study the effect of access to one specific form of credit: payday lending, the market for small-value, short-term loans taken at an annual percentage rate of around 400 percent. Payday lending's effect on household well-being has been particularly controversial. Proponents of

---

[1] Data are from 1) the Federal Reserve website, Flow of Funds Accounts, Table B.100, line 32 and 2) Bricker, Jesse, Arthur B. Kennickell, Kevin B. Moore, and John Sabelhaus "Changes in U.S. Family Finances from 2007 to 2010: Evidence from the Survey of Consumer Finances." *Federal Reserve Bulletin*, vol. 98, no 2, (February 2012), pp. 1-80.

payday lending maintain that it is an important backstop for families facing emergencies that lack access to other credit options (Andersen, 2011). Opponents of payday lending, however, charge that lenders trap poorly informed individuals in a cycle of repeated borrowing at usurious interest rates and exacerbate financial distress (Parrish and King, 2009).

I study the effects of payday lending on material well-being specifically, using data on household spending from the Consumer Expenditure Survey (CE). Consumption is a natural outcome to study with respect to credit access because in most theoretical models, households derive utility from spending and credit access affects utility through a spending channel. In addition, household spending is a better proxy of material well-being than household income from a theoretical perspective and is a common measure of material well-being in the economics literature (Meyer and Sullivan, 2004).

The payday lending market is a particularly suitable laboratory in which to evaluate the effects of credit on well-being for two reasons. First, the arguments for and against payday lending tend to mirror the theoretical arguments regarding effects of consumer credit more broadly. And empirical work to date has far from resolved the argument. Authors have found highly mixed results of payday lending on household financial conditions and other measures of well-being. On the negative side, authors have found that payday borrowing results in households reporting difficulty paying their rent, mortgage and other bills (Melzer, 2011), that it increases personal bankruptcy filing rates (Skiba and Tobacman, 2011), and that it leads to declining job performance and eligibility to re-enlist in the Air Force (Carrell and Zinman, 2008). On the positive side, authors have found that access to payday loans mitigates foreclosures following natural disasters (Morse, 2011), that banning payday lending results in more bounced checks and complaints against debt collectors (Morgan, Strain and Seblani, 2012), and that capping payday loan interest rates leads to households reporting a decline in overall financial conditions (Zinman, 2010). Bhutta (2014) finds little evidence that payday lending has any effect on household financial conditions on average. He finds no effect of payday access on credit scores, credit delinquencies, or the likelihood of overdrawing credit lines.

The second reason payday lending is a suitable laboratory is that variation in access to payday lending by geography and over time lends itself to identifying an effect of payday credit particularly well. In general, it is difficult to isolate the effect of credit access on household

outcomes. Household credit and spending choices are determined simultaneously and are both likely correlated with unobserved household characteristics, leading to issues of simultaneity bias and omitted variable bias in regression analysis. In addition, access to credit is not randomly assigned. Regulators and credit providers both play a role in determining household access to credit. State regulatory actions may be confounded with other economic factors that can influence household spending. And in the payday market particularly, lenders likely make location decisions based on the characteristics of potential borrowers with the goal of maximizing profitability.

I address these challenges by following Melzer's (2011) novel identification strategy, which compares the spending patterns of two types of households that live in states banning payday lending: 1) households who live close to the border of payday-allowing bordering state and hence have access to payday loans, and 2) households that live far from the border of a payday-allowing state and hence do not have access to payday loans. This strategy ameliorates the endogeneity concerns associated with studies that use state-level changes in payday loan availability to identify the effects of lending.

I conduct two main tests. First, I analyze how payday lending affects household spending overall, in the normal state of the world. I use confidential data on the census tract of each household in the CE survey to calculate the distance of households in states prohibiting payday lending to states allowing payday lending. I look for effects on nondurable and durable goods spending broadly as well as spending on specific items such as housing, food, and entertainment.

It is not a given that I should see any spending effects of the payday loan market overall since these loans have to be repaid and theory suggests that credit access helps households smooth consumption, not change consumption patterns. However, there are several reasons I may see an effect overall. First, if payday lending itself increases economic hardship as opponents claim and some work finds (Melzer, 2011; Skiba and Tobacman, 2011), I would expect to see that payday loan access results in overall spending declines reflecting such financial distress. Second, if the typical payday loan borrower has present-biased preferences that cause severe self-control problems, I would expect that easy access to extra cash may exacerbate

over-consumption.[2] In this case, I may observe households spending more on luxury goods and services than they would otherwise. While studying the spending effects of payday lending is not a direct test of preferences by any means, observing increases in luxury good spending for households may be indicative of self-control problems.

The second test I carry out is to directly study whether payday loan access helps families smooth consumption during periods of temporary financial distress in a "bad" state of the world. I use extreme weather events such as hurricanes and blizzards as an exogenous, negative shock to households. I test whether households with payday loan access have higher spending after the event than those without payday loan access. Severe weather events are strictly exogenous with respect to spending and payday loan access and they also plausibly represent periods of temporary financial distress. Severe storms can cause damage to one's home or car, for example, requiring unexpected outlays for repairs. Or bad weather can close one's workplace, causing a temporary drop in income for hourly workers. This analysis is similar to Morse (2011), but I use a broader set of extreme weather events occurring over a wider geographic area and time horizon. In addition, Morse's work studies the effect of payday lending on foreclosures while my work studies household consumption, allowing for a direct test of consumption smoothing.

My findings show that the effects of payday credit on household spending are heterogeneous and state dependent. First, I show that granting households access to payday lending *reduces* household material well-being on average, in a normal state of the world. Payday loan access reduces aggregate reported household spending, with the majority of the spending reductions occurring in shelter and food expenditures. I find that households with access to payday lending report lower total expenditures, and that this effect is distributed in both nondurable and durable spending. These results are concentrated in households with a greater propensity to be payday borrowers—those with income between $15,000 and $50,000. In terms of the concentration of spending reductions, I find that the spending reduction is concentrated in spending on shelter (including rental payments as well as mortgage payments) and food (food at

---

[2] Payday borrowers are often associated with having present-biased preferences in the literature. The frequent rollover of payday loans despite the high interest rates is consistent with non-standard preferences (Melzer, 2011). Estimating a dynamic programming model of consumption, saving, borrowing and default, Skiba and Tobacman (2008) find default patterns among payday loan users to be most the consistent with partially-naive quasi-hyperbolic discounting specifically. And Parsons and Van Wesep (2012) examine the welfare effects of payday credit using a model where agents are paid at regular intervals and are present-biased sophisticates.[2]

home and food away from home) particularly. These results are consistent with loan access causing households overall financial distress as critics contend. They are particularly consistent with Melzer's (2011) result that households with payday loan access report having difficulty paying their rent, mortgage and other bills. I find only weak evidence that payday loan access results in an increase in spending on luxury or so-called temptation goods; I see some evidence that households in the $15,000 to $50,000 income range increase the level of spending on alcohol and tobacco products but I see no change in spending on entertainment and I see a reduction in spending on apparel.

My second main finding shows that in a bad state of the world—following a temporary period of financial distress—access to payday lending *increases* material well-being for the average household. For households *without* payday loan access, an extreme weather event lowers spending on nondurables (defined broadly) by $22 on average in the month of the event. For those *with* payday loan access, however, spending is $35 higher after the shock than for those without access. In particular, I find that payday loan access mitigates declines on food at home consumption, shelter spending, mortgage payments, and home repairs. Households *without* payday loan access spend $31 and $18 less on shelter and home repairs in the month of an extreme weather event than in a non-event month. Households *with* payday loan access spend $30 and $36 more than households without access after the weather event. These results provide a direct test showing following periods of financial distress, payday loan access smooths consumption.

My work contributes to the empirical literature on payday lending by 1) highlighting the heterogeneous, state-dependent nature of the effects of this market on household well-being and by 2) reconciling some of the conflicting evidence to date on the welfare effects of payday lending. As noted above, authors have found highly mixed results on the effects of payday loan access on household well-being. To date, it has been difficult to reconcile these mixed results in the literature, in large part due to the apples-and-oranges nature of the datasets and methodologies used in the various analyses; the analyses were often simply not comparable. Most studies find evidence of either positive or negative effects of payday lending on well-being. As Melzer (2011) writes, for example: "I find no evidence that payday loans alleviate economic hardship." It is difficult to know if the conflicting findings are due to bias resulting form methodological issues or if access to the payday loan market did have such heterogeneous

effects. My work shows that indeed, the effects of payday loans on household well-being are heterogeneous and depend on whether the household is currently undergoing a period of temporary distress or not. In bad states of the world, I find that payday lending helps smooth consumption and improves material well-being. In normal states of the world, however, it worsens material well-being for households.

My work should also be of interest to policymakers considering actions targeted at payday lenders. The payday market remains the subject of much public policy attention in the United States. Since 1999, 19 states have changed the legality of payday lending, with 11 allowing the practice and 8 prohibiting it; a total of 14 states ban payday lending at present (Morgan, Strain and Seblani, 2012). In 2007, Congress responded to criticism that payday lenders target service members by passing legislation that caps interest rates on loans to military personnel, effectively banning payday lending to these individuals. In 2012, the Consumer Financial Protection Bureau (CFPB) held hearings on payday lending to help gauge the potential role for additional federal supervision of the market (CFBP, 2012). The CFPB has since included payday lenders as institutions under their supervision and has taken several enforcement actions against payday lenders for deceptive practices (CFPB, 2014). My results suggest that regulators' concerns about payday lending worsening household financial conditions overall are valid. However, my results showing that payday lending does help households smooth consumption after temporary periods of financial distress points to the need for continued access to emergency credit for credit-constrained households. Eliminating access to the payday loan market entirely could worsen well-being for households in distress.

The remainder of the paper proceeds as follows. Section II gives an overview of the payday loan market. Section III presents the empirical methodology used for the analyses of the overall effect of payday loan access and the effect of payday loan access after temporary periods of financial distress. Section IV describes the data used. Section V discusses the results and I conclude in Section VI.

II. Overview of the Payday Loan Market

Payday lending is the practice of using a post-dated check or electronic checking account information as collateral for a short-term, low-value, high interest rate loan. To qualify,

borrowers need personal identification, a valid checking account, and proof of steady income from a job or government benefits, such as Social Security or disability payments.

The typical loan size ranges from $100 to $500 over a term of two weeks, the usual time span between paydays, and the majority of loans are for $300 or less (Elliehausen 2009). Payday lenders usually charge an average of $10 to $20 per $100 borrowed, which implies an interest rate of about 260% to 520% APR. Of new payday loans, 36% are repaid at the end of the initial loan term and about another 20% are renewed once or twice. A considerable fraction of new loans are renewed numerous times, however. Twenty-two percent are renewed six or more times and over 10% of new loans are renewed ten or more times. Most borrowers take out just one series of loans in a year (48%), but 26% of borrowers take out two series of loans, 15% take out three series of loans, and 11% take out four or more series a year (CFPB, 2014).

In 2010, about 12 million individuals were estimated to have taken out a payday loan (PEW, 2012). Loan volume for store-front locations was estimated at $29.3 billion that year, with revenue of $4.7 billion. Online payday loan volume, which has been growing rapidly, was estimated at $10.8 billion with $2.7 billion in fees (Stephen's Inc., 2012). Looking at demographics of borrowers, they are more likely to be female, single-parents, African American, and have a high-school degree or some college education than the general population (Bourke, Horowitz and Roche, 2012). Since one generally needs a valid bank account and pay stub as proof of employment to qualify for a loan, payday borrowers are not in the poorest population cohort; still, the typical borrower is part of a lower-than-average income household. Twenty-five percent of payday borrowers report income of less than $15,000, while 56%have income between $15,000 and $50,000 and 16% report income greater than $50,000 (Bourke, Horowitz and Roche 2012; note, the breakdown does not sum to 100% because some households do not report income).

Payday loan borrowers also tend to have limited liquid assets and be credit constrained. About 55% of borrowers reported not having savings or reserve funds in 2007. At the time of taking out their most recent payday loan, about 45% reported not having a credit card and 22% reported that they would have exceeded their credit limit if they had used a credit card. Twenty-eight percent said they could have borrowed from a friend or relative, and 17% said they could have used savings (Elliehausen, 2009).

In survey evidence for why households take out payday loans, 69% of borrowers reported using their first loan for "recurring expenses:" 53% for regular expenses like utilities, car payments or credit cards, 10%for rent or mortgage payments, and 5% for food (Bourke, Horowitz and Roche 2012; note, the breakdown does not add to 69% due to rounding). Sixteen percent of payday borrowers in the survey report using the loan for an "unexpected emergency/expense" while 8% report using the loan for "something special," and 7% report "other" or "don't know."

III. Empirical Methodology

III.I Overall Effect of Payday Loan Access

To test the overall effect of payday loan access on household spending, I follow Melzer (2011) and use a strategy that relies on variation in access to payday lending geographically and over time. Many studies rely on state-level variation in the legality of payday lending or variation in households' proximity to a payday lender to identify an effect of lending on household outcomes (Table 1 summarizes the state law changes).[3] These strategies raise concerns, however. Legislative decisions are likely to be correlated with household financial conditions or other state-level policies that may affect household welfare, which would result in the difference-in-difference analysis not identifying a causal effect of payday loan access. Lenders' location decisions are also likely correlated with household characteristics and financial conditions, which may limit a causal analysis.

To ameliorate these endogeneity concerns, Melzer's strategy takes advantage of variation that is independent of state-level legislative decisions or households' proximity to particular payday lending locations. The strategy compares two types of households that live in states that that ban payday lending: 1) households that live close to the border of a state that allows payday lending and hence, still have relatively easy access to the payday loan market and 2) households that live far from the border of a payday-allowing state and hence, have limited payday-loan access. Melzer provides suggestive evidence that borrowers travel across state borders to obtain

---

[3] In order to preserve the confidentiality of the Consumer Expenditure Survey sampling areas, I cannot report the payday-banning states included in the sample.

payday loans—payday lenders have a higher propensity to locate near the borders of states that prohibit payday loans after conditioning on local observable economic conditions.

The empirical model is as follows:

$$\text{Expenditure}_{ict} = \beta_1 \text{PaydayAccess}_{ct} + \beta_2 \text{Border}_c + \gamma W_{it} + \delta X_{st} + \delta Z_{st} + \alpha_s + \alpha_t + \varepsilon_{ist} \qquad (1)$$

In this model, i indexes households, c indexes census tracts and t indexes the month in which a particular quarter's spending ended. Expenditure is the dollar value or the log dollar value of spending over the quarter ending in month t. The regression sample is limited to households in states that ban payday lending. PaydayAccess is a dummy variable that equals 1 if a household in a state that bans payday lending lives in a census tract within 25 miles of a state that allows payday lending—Melzer's cutoff for living close to a payday-allowing state. PaydayAccess equals 0 if a household lives in a state that bans payday lending but the household's census tract is farther than 25 miles from the border of a state that allows payday lending. W is a vector of household-level controls: housing tenure, education level of the survey's reference person, race of the survey's reference person, age of the reference person, family size, income class, and a cubic in household income (as a proxy for permanent income). X is a vector of state-level controls: personal income growth, the log of personal income, and the log of house prices. Z is a vector of county-level controls: the unemployment rate and employment growth. I include fixed effects for state and month (final month of the quarterly survey) in the model and cluster the standard errors at the county level. I estimate the model using OLS for all households in the sample as well as for households with income between $15,000 and $50,000 (households with the greatest propensity to be payday borrowers, as in Melzer, 2011).

III.II: Effect of Payday Loan Access after a Temporary Negative Shock

In order to directly test whether payday lending helps households smooth consumption following periods of temporary financial distress, I analyze whether payday loan access affects household spending following an extreme weather event. Extreme weather events are exogenous with respect to household spending and represent plausible temporary, negative shocks to household finances. An extreme weather event could prevent an hourly employee from making it

to work for several days, for example, acting as an income shock. In addition, weather could cause damage to one's home or car, requiring an unexpected outlay for repairs. This is a similar strategy used by Morse (2011), except that Morse's analysis relies on interacting the weather event with the presence of a payday lender in a household's zip code. As discussed above, defining payday loan access as proximity to the border of a payday-allowing state has the advantage of being independent from store location decisions.

To perform this analysis, I examine the interaction of access to payday lending and weather shocks. I interact *PaydayAccess* with the dummy variable *WeatherEvent* that equals 1 if any weather event that caused monetary damages occurred in the county in which a particular census tract was located. The empirical model is as follows:

$$\text{Expenditure}_{ict} = \beta_1 \text{PaydayAccess}_{ct} + \beta_2 \text{WeatherEvent}_{nt} +$$

$$\beta_3 \text{PaydayAccessXWeatherEvent}_{cnt} + \beta_4 \text{Border}_c + \gamma W_{it} + \delta X_{st} + \delta Z_{nt} + \alpha_s + \alpha_t + \varepsilon_{icnst} \qquad (2)$$

The time indicator t now represents the month of household spending; I use monthly expenditures in this analysis to match the month of the income shock with the month of spending. *PaydayAccess* is defined as in the section above. The household-level, state-level, and county-level controls are the same as above and I also include state and month fixed effects and cluster standard errors at the county level.

In this model, the coefficient β_2 measures the spending effects of experiencing an extreme weather event in a given month when a household does not have access to payday lending. The coefficient β_3 measures the difference in spending after a weather event for households with payday loan access compared to households without payday loan access. This coefficient will be positive if payday credit access boosts household spending during temporary, negative shocks. The total spending effect of a weather shock when a household has payday loan access is then β_2 + β_3. The spending effect of allowing payday lending when no weather shock has occurred is measured by the coefficient β_1.

IV: Data

IV.I: Consumer Expenditure Data

The main outcome variables of interest in this analysis are categories of household spending including broad measures of spending (overall spending on durable goods and nondurable goods) as well as more narrow categories (e.g., food, shelter, utilities and health care). I use data from the Consumer Expenditure Survey (CE), Interview Survey, a nationally representative survey of spending that is published by the Bureau of Labor Statistics (BLS). In the CE survey, households are interviewed for five consecutive quarters on their spending over the previous three months.[4] In addition to including highly detailed data on household spending, the survey also includes detailed data on household demographics and data on household balance sheets. There are about 7,000 households surveyed a quarter, for a total of about 28,000 surveys collected a year and there are a total of 91 geographic sampling areas across the country.

The geographic information available in the public-use Consumer Expenditure (CE) survey data files are limited to state and MSA-level indicators, which are only available for a subset of households. In order to construct the measure of a CE household's distance to the closest state that allows payday lending, I use confidential data on each household's census tract location accessed at the BLS headquarters.

I study four aggregate measures of expenditures as well as a number of specific spending categories. The aggregate measures that I study are 1) total household expenditures, 2) a broad measure of nondurable expenditures, 3) a narrow measure of nondurable expenditure categories (following Lusardi, 1996), and total durable goods. The specific expenditure categories I use follow from the major breakdown of goods as in Kearney (2004). I deflate expenditures to constant 2010 dollars using the consumer price index for all urban consumers (CPI-U, not seasonally adjusted). For analysis of the overall effect of payday loan access on household spending, I use data at the quarterly spending level.

To construct the sample, I follow the literature in limiting the sample to exclude households living in student housing, those that report an age of less than 21 or greater than 85,

---

[4] Note, a "consumer unit", which is defined an independent financial entity within a household, is the unit of observation in the survey. I will use the term "household" interchangeably with consumer unit.

those that incompletely report income, those that report age changing by more than one between quarters, or those that report the number of children changing by more 3 between quarters. I provide a detailed description of how the spending variables, household credit variables, and data sample were constructed in Appendix A. I use a data sample from 1998 to 2010 as the payday lending market started developing in the 1990s and the first payday loan access law change was in 1999. I end the sample in 2010 in order to limit confounding effects of the online payday lending market, which has been growing over time (Bourke, Horowitz and Roche 2012). Since households in any state may access payday loans online, the growth of this market confounds the geographic variation used to identify the effects of payday loan access in this paper.

Table 2 presents summary statistics for the expenditure categories that I analyze in this study—quarterly average spending levels and standard deviations, indexed to 2010 dollars using the CPI-U. Column 1 shows households that do not have access to payday lending and column 2 shows households that have access to payday spending (about 70 percent of the qualified household). Average spending for both groups totals around $11,000 a quarter with spending on durable goods making up about two-thirds of total spending. Nondurable spending defined broadly totals about $3,750 a quarter while nondurables spending defined narrowly totals about $2,750. The largest individual categories of spending are shelter ($2,500), transportation ($2,000) and food at home ($1,100). Notably, while there is no statistical differences in the aggregate spending levels of each group, there are larger differences in the breakdown of spending by detailed category. Households without payday loan access spend more on housing, food, and apparel expenditures, while households with payday loan access spend more on health care and entertainment.

I present summary statistics for household demographics of households with and without payday loan access in Table 3. There is no statistical difference between these households in terms of income, marital status, or education levels. Households without payday access are more likely to be homeowners and the family size is slightly larger in households with payday loan access (2.54 versus 2.51). The share of Caucasian households does not differ between the two samples, but the rest of the racial composition does; households with access to payday lending are more likely to be Hispanic or Asian and less likely to be African American.

To test whether payday lending improves material well-being in the face of a negative shock to household financial conditions, I use data on extreme weather events from the University of South Carolina's Sheldus Hazard Database. This database compiles county-level information on dollar losses and fatalities from 18 types of events including hurricanes, thunder storms, floods, and blizzards. By using data on household location, I can more precisely match extreme weather events to the households most likely to have been affected by these weather events. As discussed above, in order to more precisely match the timing of weather events to the timing of household spending, I use monthly spending data in the CE files for this analysis.

I present summary statistics for the weather event dataset in Table 4. In order to preserve confidentiality of the CE sampling areas, the information I present is limited but shows that extreme weather events occur frequently for households in the CE sample studied here and that the economic magnitude of these events is economically meaningful. Of the total number of monthly household spending observations in the sample (192,000), weather caused property damage in a household's county in about a third of those months (67,000). These weather events affect a considerable number of households with payday loan access; among these households, there were 22,000 monthly household observations in which weather damage was recorded in a household's county. In any month with damage, the average property damage recorded for a county was about $1.4 million. The weather events with the greatest frequency of occurring in the total sample are storm events (25,782), wind events (23,094), wind-related winter weather (9,460) and flooding (8,518). Multiple weather events in a given month are a frequent occurrence.

V. Results

V.I Results: Overall Effect of Payday Loan Access

I first investigate the overall effect of payday loan access on aggregate household expenditures. Table 5 shows the estimated coefficient on *PaydayAccess* from the regression specification in equation (1); the table shows results for four measures of aggregate spending: total expenditures, nondurable expenditures defined broadly, nondurable expenditures defined

narrowly, and durable good expenditures. I present results for all households in the sample as well as for households with incomes between $15,000 and $50,000—the income range in which the majority of payday loan borrowers fall (following Melzer (2011)). I present results for specifications with household expenditures defined both in levels and the natural logarithm of expenditures. The coefficient in the levels specification can be interpreted as the dollar change in quarterly household spending resulting from access to the payday loan market. The coefficient in the log-linear specification can be interpreted as the percentage change in quarterly household spending resulting from access to the payday loan market. Utilizing a log-linear specification has the advantage of mitigating the effects of any outliers in the regression; for this reason, the log-linear specification may be preferred to the levels specification.

The results show households with payday loan access have lower household spending on average, across aggregate spending categories. The estimated coefficient on *PaydayAccess* is negative in each regression, indicating that payday access reduces household expenditures on aggregate expenditures—nondurable expenditures as well as durable expenditures. For all households on the sample, I find that payday access results in a 5.5 percent reduction in total household spending on average. The corresponding dollar value reduction is about $600 a quarter, although this estimate is not statistically significant. The results indicate that payday loan access reduces nondurables spending using both the narrow and broad definitions of nondurables spending. Nondurable spending defined narrowly falls by about $220 a quarter (6.3 percent), while nondurable spending defined broadly falls by about $310 a quarter (6.3 percent); the estimated effect of payday loan access is significant in both the levels and log-linear specification. As there are 1.7 adults per household, this corresponds to a monthly spending reduction of about $40 and $60 a month, respectively. I find a reduction in durables spending as well (5.3%), although again the reduction is only statistically significant for the log specification. I see similar results when limiting the data sample to households in the $15,000 to $50,000 income class. I see that households in this income range also report lower household expenditures across aggregate spending categories. In this set of regressions, however, the effect of payday loan access on household spending is statistically significant more often in the levels specification. The effect on overall expenditures is now significant when measured in levels as well as in logs; the coefficient can be interpreted as households with payday loan access reporting $575 lower total expenditures ($112 per adult, per month).

The relatively large magnitude of the regression coefficient estimates raises the question of whether these magnitudes are plausible. It is likely that loan fees for payday loans are underreported in the CE and that the reduction in aggregate expenditures reflects a reduction in expenditures excluding loan charges. While banking fees and finance fees are reported in the quarterly CE survey, households are known to underreport expenditures for so-called "sin" goods and services (gambling, alcohol and tobacco for example), of which payday loan fees may be included. The average payday loan has a $20 fee per $100 of loans spent and since the typical loan is around $300 or less, that implies a fee of about $60 per loan. A $125 spending reduction per adult, per month would suggest that two loans are being taken out per person in the survey on average each month. While a large fraction of payday loans are rolled over for at least one additional period and payday borrowers report taking a number of loans through the month, this is likely an implausibly large magnitude. Below I investigate other explanations for the spending reduction than the reduction solely being due to a payday loan charges not being included in reported household spending.

Next I examine how the spending reductions are split between the detailed expenditure categories. Table 6 shows the coefficient on *PaydayAccess* from empirical specification (1), with each row representing a separate regression coefficient on the listed expenditure category as the dependent variable. Columns 1 and 2 in the table show estimates from a log-linear and linear regression specification, respectively, for all households in the sample. Columns 3 and 4 show corresponding estimates for households in the $15,000 to $50,000 income category. I find that households with payday loan access report the largest reductions in spending on shelter and on food. I see that households with payday loan access on average spend $570 less a quarter on shelter (a category that includes broad expenditures on both owned dwellings and rented dwellings). Shelter expenditures only include spending on mortgage interest, not mortgage principle. The mortgage category reported in the table shows total mortgage payment spending (principle and interest) and the results show that households with payday loan access spend about $250 less a quarter on mortgage expenditures. Households with payday loan access spend about $150 less in rent payments per quarter.

The reductions in spending on food resulting from payday loan access are also substantial. These households spend $87 and $88 less a quarter on food at home and food away from home, respectively, than households without payday loan access. The coefficient estimates

15

are significant for these expenditure categories in both the level and the log-linear regression specifications, for all households and for households in the $15,000 and $50,000 income category. The other notable category of spending declines is in apparel; households spend $72 less on apparel a quarter and the reductions in apparel spending are significant across all 4 specifications reported in the table. I see some small reduction in health care spending for households with payday loan access, although only in the log-linear specification for all households.

These results are in line with Melzer's (2011) findings that access to payday loan credit overall causes households to report having more difficulty paying the rent, the mortgage, and medical bills. They also accord with his conclusions that for low-income households, payday loan fees result in households having fewer funds to spend on other bills.

One channel for payday loan access affecting other categories of household spending is if loan fees result in households having fewer funds available for other expenditures. Another reason that payday loan access could affect household spending, however, is if the typical payday loan borrower has present-biased preferences that cause severe self-control problems. In this case, easy access to extra cash may exacerbate over-consumption, causing households to spend more on luxury goods and services than they would otherwise. I investigate this hypothesis by looking at whether payday loan access causes any change in spending on in so-called temptation goods (as in Bertrand and Morse, 2009), particularly spending on alcohol, tobacco, and entertainment. I only find weak evidence to support this hypothesis. I find that households with payday access in the $15,000 to $50,000 income category report a $45 increase a month in spending on alcohol and tobacco products, and this increase is significant at the 1 percent level. It is not significant in the other specifications, however. I also see no significant increase in entertainment in spending overall.

Finally, I also find a sizeable increase in transportation spending for households with payday loan access ($194 or about an 8 percent increase). This result raises the question of whether *PaydayAccess* is correlated with other commuting-related expenses that may be affecting the other spending results as well (perhaps explaining why the magnitude of the effects is so large). I have further work to do to investigate this possibility.

V.II. Results: Effect of Payday Loan Access following a Temporary Negative Shock

Next, I investigate the whether access to the payday loan market affects spending following periods of temporary financial distress, represented by an extreme weather event occurring in the month. Using the extreme weather events as a natural experiment, this analysis provides a direct test of whether credit access helps household smooth spending around negative shocks. First I study the effects on aggregate household spending, using the four measures studied above. Table 7 shows results from empirical specification (2), which interacts the effects of payday loan access and a weather event occurring in a given month. Each column represents one regression of the dependent variable named at the top of the column on the explanatory variables as well as the control variables described above. Panel A of Table 7 shows results for the specification with the dependent variables in levels and Panel B shows results for the natural logarithm of the dependent variable.

I find evidence that payday lending does play a valuable consumption smoothing role for households facing temporary periods of financial distress; households with payday loan access spend more on nondurables after temporary, negative financial shocks than those without payday loan access. For households without payday loan access, an extreme weather event lowers monthly spending on nondurables defined broadly by $22 on average and on nondurables defined narrowly by $15 on average. For those with payday loan access, however, monthly spending is $35 higher and $30 higher on broad and narrow nondurables, respectively, than for those without access after the weather shock. I see a similar result in the log-linear specification. An extreme weather event reduces reported household spending on both broad and narrow nondurables by 1.4 percent and 1.5 percent, respectively for households without payday access. Household with payday loan access, however, report 2.8 percent and 2.6 percent higher spending than households without payday loan access following the weather event. I do not see an effect on total expenditures in either specification, however, because there is no statistically significant effect on durable good spending.

Looking at the effect of payday loan access on specific spending categories following a weather event (Table 8), I find a similar pattern as above for expenditures on several specific categories—food at home, shelter spending, mortgage payments, and home repairs. As in Table 7, Panel A shows a specification with the expenditure measured in levels and Panel B shows a

specification with the expenditure measured as a natural logarithm. I show results for the categories with statistically significant results. The results for food expenditures at home are the most robust across specifications. Extreme weather events result in a reduction of $7 (1.5 percent) in monthly spending on food at home for households without payday loan access. For those with payday access, spending on food at home is $12 (2.9 percent) higher after the weather event than for those without payday loan access.

Shelter and home repairs are a second category in which I see statistically significant effects of payday loan access following an extreme weather event. For households without access to payday lending, monthly expenditures on shelter overall and on mortgage payments are $31 and $11 lower, respectively. For those with payday access, spending is $30 and $19 higher after the weather event than for those without. Home repair expenditures are $18 lower following a weather event for households without payday loan access. Payday loan access more than mitigates that decline in home repairs; households with access spend $36 more on home repairs after the weather event than those without access. These results provide a clean test that following periods of financial distress, payday loan access helps households smooth consumption. My result for mortgage payments are in line with Morse's (2011) results that show payday lending mitigates the increase in foreclosures that occurs following natural disasters in California. I build on Morse's work by showing a direct consumption smoothing mechanism that mitigating financial distress. In addition, I show that the consumption effect is broader than in mortgage payments alone.

VI. Conclusion

In this paper, I investigate whether households benefit from increased access to payday credit—a market that has grown rapidly since the late 1990s and that has come under regulatory scrutiny for the high fees charged per loan transaction. I study the effects of payday loan access on household material well-being for households in two states of the world: 1) the average effect in a "normal" state of the world and 2) the effect of access in a "bad" state of the world (households that have recently experienced a temporary, negative shock to household finances). I show that the effect on material well-being is state dependent. Under normal conditions, payday loan access reduces average household spending on non-loan expenditures substantially,

particularly expenditures on rent, mortgage payments and food. After temporary periods of financial distress (an extreme weather event), however, payday loan access mitigates the spending declines that occur for households that experience the shock but don't have access to payday credit; loan access helps households smooth consumption over the shock.

These results provide empirical evidence on the heterogeneous nature consumer credit's on household well-being; the effects vary even *within* the market for one specific credit product. The finding that payday loan access results in household spending declines overall is consistent with evidence in the literature to date that payday lending is indeed associated with increased economic hardship for households overall. In distressed conditions, however, payday lending does appear to aid households facing emergencies, helping households keep food on the table and pay the mortgage.

References

Community Financial Services Association of America. "What Is a Payday Advance?" 2012.

Consumer Financial Protection Bureau. "CFPB Data Point: Payday Lending," 2014.

Andersen, Darrin. "Response to Federal Reserve Bank of Kansas City Study: "Could Restrictions on Payday Lending Hurt Consumers?" " QC Holdings, 2011.

Bee, Adam, Bruce D. Meyer, and James X. Sullivan. "The Validity of Consumption Data: Are the Consumer Expenditure Interview and Diary Surveys Informative?" *NBER Working Paper*, August 2012.s

Bertrand, Marianne, and Adair Morse. "What do High-Interest Borrowers Do with Their Tax Rebate?" *American Economic Review, Papers and Proceedings* 99, no. 2 (May 2009): 418-423.

Bhutta, Neil. "Payday Loans and Consumer Financial Health," J*ournal of Banking and Finance*. (Forthcoming)

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

Bourke, Nick, Alex Horowitz, and Tara Roche. *Payday Lending in America: Who Borrows, Where They Borrow, and Why.* The Pew Charitable Trusts, 2012.

Consumer Financial Protection Bureau. "Consumer Financial Protection Bureau Examines Payday Lending." *Press Release*, January 2012.

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

Elliehausen, Gregory. "An Analysis of Consumers' Use of Payday Loans." *Monograph No. 31, The George Washington University School of Business Financial Services Research Program*, January 2009.

Greene, William H. "Fixed Effects and Bias Due to the Incidental Parameters Problem in the Tobit Model." *Econometric Reviews* 23, no. 2 (2004): 125-147.

Heidhues, Paul, and Boton Koszegi. "Exploiting Naivete about Self-Control in the Credit Market." *American Economic Review* 100, no. 5 (December 2010): 2278-2303.

Laibson, David. "Golden Eggs and Hyperbolic Discounting." *Quarterly Journal of Economics* 112, no. 2 (1997): 443-478.

Lusardi, Annamaria, and Peter Tufano. "Debt Literacy, Financila Experiences, and Overindebtedness." *Working Paper*, 2009.

Melzer, Brian T. "The Real Costs of Credit Access: Evidence from the Payday Lending Market." *Quarterly Journal of Economics* 126, no. 1 (2011): 517-555.

Melzer, Brian T., and Donald P. Morgan. "Competition and Adverse Selection in a Consumer Loan Market." *Working Paper*, 2012.

Morgan, Donald P. "Defining and Detecting Predatory Lending." *Federal Reserve Bank of New York, Staff Reports No. 273*, 2007.

Morgan, Donald, Michael Strain, and Ihab Seblani. "How Payday Credit Access Affects Overdrafts and Other Outcomes." *Journal of Money, Credit and Banking* 44, no. 2-3 (March-April 2012): 519-531.

Morse, Adair. "Payday Lenders: Heroes or Villians." *Journal of Financial Economics* 102, no. 1 (2011): 28-44.

O'Donoghue, Ted, and Matthew Rabin. "Doing It Now or Later." *American Economic Review* 89, no. 1 (March 1999): 103-124.

Parrish, Leslie, and Uriah King. *Phantom Demand: Short-term Due Date Generates Need for Repeat.* Center for Responsible Lending, 2009.

Parsons, Christopher A., and Edward D. Van Wesep. "The Timing of Pay." *Working Paper*, July 2012.

Sawtelle, Barbara A. "Income Elasticities of Household Expenditures: A U.S. Cross-Section Perspective." *Applied Economics*, May 1993: 635-44.

Skiba, Paige Marta, and Jeremy Tobacman. "Do Payday Loans Cause Bankruptcy?" *Working Paper*, February 2011.

Skiba, Paige Marta, and Jeremy Tobacman. "Payday Loans, Uncertainty, and Discounting: Explaining Patterns of Borrowing, Repayment, and Default." *Working Paper*, August 2008.

Zinman, Jonathan. "Restricting Consumer Credit Access: Household Survey Evidence on Effects Around the Oregon Rate Cap." *Journal of Banking and Finance* 34, no. 3 (2010): 546-556.

Table 1: Payday Loan Laws by State

Always Banned	Always Legal			Banned	Legalized
CT	CA	KY	OH	AR (Dec. 07)	AL (Jun. 03)
ME	CO	LA	SC	DC (Nov. 07)	AK (Jun. 04)
MA	DE	MN	SD	GA (May 04)	AZ (Apr. 00)
NJ	FL	MS	TN	MD (Jun. 00)	AR (Apr. 99)
NY	ID	MO	TX	NC (Dec. 05)	HI (Jul. 99)
VT	IL	MT	UT	OR (Jul. 07)	MI (Nov. 05)
	IN	NE	WA	PA (Nov. 07)	NH (Jan. 00)
	IA	NV	WI	WV (Jun.06)	ND (Apr. 01)
	KS	NM	WY		OK (Sep. 03)
					RI (Jul. 01)
					VA (Apr. 02)

Source: Morgan, Strain, and Seblani, 2012

Table 2: Summary Statistics, Expenditure Categories

	Payday Access = 0		Payday Access = 1		
	Mean	**SD**	**Mean**	**SD**	**(P-value difference)**
Total Expenditures	11,069	10,527	10,959	9,738	0.20
Nondurables: Narrow	2,758	3,262	2,733	2,320	0.27
Nondurables: Broad	3,750	3,854	3,739	3,076	0.73
Durable Goods	7,320	7,820	7,220	7,794	0.14
Food at home	1,149	759	1,132	742	0.01
Food away from home	471	900	454	933	0.03
Shelter	2,579	2,944	2,317	2,519	0.00
Rent Payments	723	1,290	543	1,105	0.00
Mortgage Payments	1,062	2,085	1,187	2,146	0.00
Utilities	844	607	869	541	0.00
Household Operations	529	1,667	517	1,519	0.37
Health Care	596	934	653	918	0.00
Education	254	1,718	255	1,752	0.96
Alcohol and tobacco	172	325	176	326	0.15
Apparel	360	666	318	972	0.00
Entertainment	526	1,151	551	1,814	0.07
Transportation	1,796	3,861	1,827	3,950	0.35
Sample size:	44,332		19,276		

Table 3: Summary Statistics, Demographic Variables

	Payday Access = 0		Payday Access = 1		(P-Value of Difference)
	Mean	**Std. Dev.**	**Mean**	**Std. Dev**	
Income	51.10	61.91	51.09	59.44	0.99
Married	0.54	0.50	0.54	0.50	0.31
Homeowner	0.65	0.48	0.71	0.46	0.00
Family Size	2.56	1.47	2.51	1.41	0.00
Age	50.39	15.84	50.25	15.63	0.32
Race					
White	0.79	0.41	0.79	0.40	0.83
Black	0.16	0.36	0.16	0.37	0.11
Asian	0.04	0.19	0.03	0.16	0.00
Hispanic	0.10	0.30	0.05	0.22	0.00
Other	0.01	0.10	0.02	0.13	0.00
Education					
Below High School	0.15	0.36	0.15	0.36	0.20
High School	0.28	0.45	0.29	0.45	0.27
Some College	0.26	0.44	0.26	0.44	0.81
Bachelors or higher	0.31	0.46	0.30	0.46	0.77
Sample size:	44,332		19,276		

Table 4: Summary Information: Weather Events

Obs. in data sample:	192,329
Obs. with a weather event in the county:	
Any	66,748
Flooding	8,518
All Storm Events	25,782
Wind	23,094
Wind/Winter weather	9,460
Obs. with payday loan access and any weather event in the county:	22,178
Mean county property damage in a month with a weather event:	$1,366,424

Table 5: Effect of Payday Loan Access on Quarterly Household Expenditures: Coefficient on *PaydayAccess*

Dependent Variable:	All Income		Income 15-50K	
	Ln	Level	Ln	Level
	(1)	(2)	(3)	(4)
Total Expenditures	-0.0556**	-599.6	-0.0484*	-575.3*
	[0.0257]	[366.4]	[0.0263]	[294.3]
Nondurables: Narrow	-0.0626**	-218.9**	-0.0512	-162.2*
	[0.0301]	[103.1]	[0.0359]	[90.50]
Nondurables: Broad	-0.0629**	-313.5**	-0.0441	-260.1**
	[0.0276]	[129.3]	[0.0312]	[114.3]
Durable Goods	-0.0530*	-286.1	-0.0531*	-315.2
	[0.0273]	[252.3]	[0.0278]	[204.6]
Obs.	63,605	63,605	21,028	21,028

This table presents results from empirical specification (1), regressions of quarterly expenditure categories on *PaydayAccess,* household-level controls (housing tenure, education level, race, age, family size, income class, and a cubic in household income), state-level controls (personal income growth, the log of personal income, and the log of house prices), county-level controls (the unemployment rate and employment growth) and state and year fixed effects. Each cell reports estimates for a separate regression using the dependant variables listed by row. Standard errors are presented in brackets below the coefficient estimates and are clustered at the county level. The sample period is 1998 to 2010. ***, **, and * indicate 1 percent, 5 percent and 10 percent significance, respectively.

Table 6: Effect of Payday Loan Access on Quarterly Household Expenditures: Coefficient on *PaydayAccess*

Dependent Variables:	All Income		Income 15-50K	
	Ln	Level	Ln	Level
	(1)	(2)	(3)	(4)
Shelter	-0.188***	-571.4***	-0.202***	-454.4***
	[0.0559]	[170.5]	[0.0624]	[138.9]
Rent Payments	-0.140**	-149.2**	-0.164**	-194.5***
	[0.0651]	[59.98]	[0.0758]	[71.39]
Mortgage Payments	-0.202***	-257.6***	-0.287***	-156.2**
	[0.0595]	[87.99]	[0.0814]	[60.58]
Food At Home	-0.0698**	-86.92**	-0.0844**	-115.4**
	[0.0316]	[38.71]	[0.0428]	[46.88]
Food Away From Home	-0.161***	-87.52***	-0.169**	-71.96**
	[0.0510]	[30.11]	[0.0661]	[31.23]
Alcohol and Tobacco	-0.036	15.01	0.0721	43.96***
	[0.0395]	[10.75]	[0.0625]	[14.64]
Utilities	0.0285	-9.389	0.0598**	25.46
	[0.0275]	[22.06]	[0.0284]	[21.30]
Health Care	-0.0606**	-29.65	-0.0452	-39.45
	[0.0294]	[24.78]	[0.0444]	[33.16]
Transportation	0.0818*	194.3**	0.0926*	144.6
	[0.0424]	[88.32]	[0.0539]	[111.6]
Education	-0.182	-2.62	0.0913	26.72
	[0.115]	[37.90]	[0.156]	[31.91]
Apparel	-0.144***	-72.46***	-0.115**	-67.52***
	[0.0445]	[22.79]	[0.0572]	[21.70]
Entertainment	0.0153	28.16	0.0133	0.449
	[0.0285]	[28.60]	[0.0341]	[20.82]
No. Households	63,605	63,605	21,028	21,028

This table presents results from empirical specification (1), regressions of quarterly expenditure categories on *PaydayAccess,* household-level controls (housing tenure, education level, race, age, family size, income class, and a cubic in household income), state-level controls (personal income growth, the log of personal income, and the log of house prices), county-level controls (the unemployment rate and employment growth) and state and year fixed effects. Each cell reports estimates for a separate regression using the dependant variables listed by row. Standard errors are presented in brackets below the coefficient estimates and are clustered at the county level. The sample period is 1998 to 2010. ***, **, and * indicate 1 percent, 5 percent and 10 percent significance, respectively.

Table 7: Effect of Payday Loan Access on Aggregate Monthly Household Expenditures After Extreme Weather Events

Panel A: Level Specification

	Dependent Variable:			
	Total Expenditures	**Nondurables: Narrow**	**Nondurables: Broad**	**Durables**
WeatherEvent	-51.25	-15.37*	-22.04*	-29.21
	[31.62]	[7.931]	[11.68]	[23.48]
WeatherEventXPaydayAccess	84.96	30.15**	34.90*	50.06
	[53.15]	[14.34]	[20.26]	[39.56]
PaydayAccess	-88.39	-46.64	-67.02*	-21.37
	[100.5]	[28.68]	[35.75]	[71.08]
Obs.	192,148	191,955	192,012	192,100
R-squared	0.466	0.426	0.41	0.411

Panel B: Ln Specification

	Dependent Variable:			
	Total Expenditures	**Nondurables: Narrow**	**Nondurables: Broad**	**Durables**
WeatherEvent	-0.00992	-0.0140**	-0.0145**	-0.00449
	[0.00727]	[0.00709]	[0.00729]	[0.00789]
WeatherEventXPaydayAccess	0.0151	0.0281**	0.0255*	0.000426
	[0.0130]	[0.0122]	[0.0132]	[0.0153]
PaydayAccess	-0.03	-0.0376	-0.0415*	-0.019
	[0.0219]	[0.0249]	[0.0230]	[0.0244]
Obs.	192,148	191,955	192,012	192,100
R-squared	0.466	0.426	0.41	0.411

This table presents results from empirical specification (2). WeatherEvent is a dummy variables equal to 1 if a household lives in a county that experienced a weather event in a month. Regressions include household-level controls (housing tenure, education level, race, age, family size, income class, and a cubic in household income), state-level controls (personal income growth, the log of personal income, and the log of house prices), county-level controls (the unemployment rate and employment growth) and state and year fixed effects. Standard errors are presented in brackets below the coefficient estimates and are clustered at the county level. The sample period is 1998 to 2010. ***, **, and * indicate 1 percent, 5 percent and 10 percent significance, respectively.

Table 8: Effect of Payday Loan Access on Specific Categories of Monthly Household Expenditures After Extreme Weather Events

Panel A: Level Specification

	Food At Home	Food Away from Home	Shelter	Rent	Mortgage Payments	Utilities	Health Care	Apparel	Transportation	Home Repairs
					Dependent Variable:					
WeatherEvent	-7.183**	-0.626	-31.16***	-6.932**	-11.18*	3.800*	-0.656	-5.793**	-3.591	-18.10*
	[2.805]	[3.183]	[9.639]	[3.242]	[5.712]	[2.273]	[3.919]	[2.773]	[15.30]	[9.691]
WeatherEventXPaydayAccess	12.11**	5.546	30.16*	5.38	18.72**	-3.303	5.821	-1.626	17.97	35.69*
	[5.573]	[4.860]	[17.09]	[4.927]	[8.980]	[4.088]	[6.638]	[6.021]	[23.38]	[18.50]
PaydayAccess	-15.15	-21.15***	-115.4***	-22.07	-59.84**	2.681	-10.39	-12.88*	45.80*	3.566
	[10.74]	[7.312]	[43.94]	[14.65]	[25.58]	[5.765]	[7.620]	[7.533]	[26.24]	[23.66]
No. Obs	191,003	147,242	189,543	62,771	73,276	187,429	143,062	116,778	178,496	30,102
R-squared	0.373	0.244	0.484	0.381	0.247	0.323	0.164	0.141	0.256	0.084

Table 8 (Continued): Effect of Payday Loan Access on Specific Categories of Monthly Household Expenditures After Extreme Weather Events

Panel B: Ln Specification

	Food at home	Food away from home	Shelter	Rent	Mortgage Payments	Utilities	Health Care	Apparel	Transportation	Home Repairs
					Dependent Variable:					
WeatherEvent	-0.0145**	-0.0230**	-0.00493	0.00282	-0.0154	0.0144*	0.000673	-0.00184	0.0108	-0.0449
	[0.00659]	[0.0113]	[0.0106]	[0.0106]	[0.0114]	[0.00816]	[0.0127]	[0.0143]	[0.00862]	[0.0328]
WeatherEventXPaydayAccess	0.0286**	0.0419**	0.00966	0.0201	0.0301	-0.0202	-0.00161	0.0035	-0.00635	0.105*
	[0.0115]	[0.0203]	[0.0182]	[0.0210]	[0.0213]	[0.0128]	[0.0200]	[0.0217]	[0.0169]	[0.0548]
PaydayAccess	-0.035	-0.123***	-0.0775**	-0.0316	-0.135***	0.0420**	-0.0486*	-0.0511	0.0375	-0.0569
	[0.0257]	[0.0384]	[0.0381]	[0.0429]	[0.0485]	[0.0199]	[0.0265]	[0.0380]	[0.0308]	[0.0899]
No. Obs	191,003	147,242	189,543	62,771	73,276	187,429	143,062	116,778	178,496	30,102
R-squared	0.373	0.244	0.484	0.381	0.247	0.323	0.164	0.141	0.256	0.084

This table presents results from empirical specification (2). WeatherEvent is a dummy variables equal to 1 if a household lives in a county that experienced a weather event in a month. Regressions include household-level controls (housing tenure, education level, race, age, family size, income class, and a cubic in household income), state-level controls (personal income growth, the log of personal income, and the log of house prices), county-level controls (the unemployment rate and employment growth) and state and year fixed effects. Standard errors are presented in brackets below the coefficient estimates and are clustered at the county level. The sample period is 1998 to 2010. ***, **, and * indicate 1 percent, 5 percent and 10 percent significance, respectively.

Appendix A:

<u>Income Classes</u>

Prior to 2004, the Consumer Expenditure Survey included only household income as directly reported. Due to the large share of non-response to income questions, the CE currently uses income imputation to fill in income blanks. In 2004 and 2005, the CE only published imputed data, and starting in 2006, the CE started publishing both the imputed income data and the reported data.

For this study, in order to maintain consistency across the sample period, I only include observations for complete income reporters for the sample years 1998-2003 and 2006-2010. I define complete income reporters as households that report non-zero income in at least one of the following categories: wages and salaries; unemployment compensation; income from nonfarm business, partnership or professional practice; farm income; Social Security payments or Railroad Retirement income; Supplemental Security Income; welfare income; and pension income. Since BLS only reports imputed income for 2004 and 2005, in those years, I exclude households for which BLS reported that all of the income categories above had been imputed because the data had been invalid blanks (data flags 2 or 5). To separate households into income classes, I use total, before-tax income (code fincbefx for 1998-2003 and fincbefm for 2004 and 2005).