For Better and for Worse? Effects of Access to High-Cost Consumer Credit

Christine L. Dobridge

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Abstract

I provide empirical evidence that the effect of high-cost credit access on household material well-being depends on if a household is experiencing temporary financial distress. Using detailed data on household consumption and location, as well as geographic variation in access to high-cost payday loans over time, I find that payday credit access improves well-being for households in distress by helping them smooth consumption. In periods of temporary financial distress—after extreme weather events like hurricanes and blizzards—I find that payday loan access mitigates declines in spending on food, mortgage payments, and home repairs. In an average period, however, I find that access to payday credit reduces well-being. Loan access reduces spending on nondurable goods overall and reduces housing- and food-related spending particularly. These results highlight the state-dependent nature of the effects of high-cost credit as well as the consumption-smoothing role that it plays for households with limited access to other forms of credit.

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1. Introduction

U.S. households are heavy users of credit. In 2013, 75 percent of households held some form of debt, with $13.8 trillion in total household debt outstanding in that year. Debt payments represent a considerable fraction of household income as well: a median of 16 percent of income for households with debt.[1] Such high levels of household debt have tended to attract negative attention from the public and the media, but economic theory predicts both positive and negative effects of credit access on household well-being.[2] On one hand, canonical economic models show that credit access boosts household utility by allowing users to smooth consumption over shocks (Friedman, 1956; Modigliani and Brumberg, 1954; Hall, 1978). On the other hand, credit access may worsen well-being for households with unusually strong preferences for current consumption (i.e., "self-control problems", Laibson, 1997; O'Donoghue and Rabin, 1999; Heidhues and Koszegi, 2010), those with poor financial literacy (Lusardi and Tufano, 2015), or when lenders are more informed than borrowers about likely outcomes (Bond, Musto and Yilmaz, 2009). In this paper, I investigate empirically how the effect of credit access on well-being differs across not just different types of borrowers, but across states of the world as well: "distress" states versus "average" states, particularly.

Studying the effects of consumer credit presents several empirical challenges. First, 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. Second, 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 well-being as well.

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[1] Data are from 1) the Federal Reserve Flow of Funds Accounts, Table B.100, line 32 and 2) Bricker, Jesse, Lisa Dettling, Alice Henriques, Joanne Hsu, Kevin Moore, John Sabelhaus, Jeffrey Thompson, and Richard Windle (2014). "Changes in U.S. Family Finances from 2010 to 2013: Evidence from the Survey of Consumer Finances," *Federal Reserve Bulletin*, vol. 100 (September), pp. 1-41.

[2] For example, the student loan market has attracted a large amount of media attention in recent years. Recent headlines include "Student Loans, the Next Big Threat to the U.S. Economy?" (Businessweek, 2014); "How the $1.2 Trillion College Debt Crisis Is Crippling Students, Parents And the Economy" (Forbes, 2013); "Student Debt and the Crushing of the American Dream" (New York Times, 2013).

I address these challenges by studying the effect of access to the payday loan market specifically and by using data on detailed household spending that is also matched with household location. The payday lending market is a particularly suitable laboratory to evaluate the effects of high-cost credit on household well-being for two reasons. First, there is a great deal of variation in access to payday lending across geographic locations and over time, which helps to better identify the direct effects of credit access and alleviate concerns about empirical validity. Second, the debate on payday lending's effects has been particularly polarized and the arguments for and against payday lending tend to mirror the theoretical arguments regarding effects of consumer credit more broadly. Proponents of payday lending maintain that it is an important backstop for families facing emergencies that lack access to other credit options (Andersen, 2011). Critics 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). The Consumer Financial Protection Bureau (CFPB), which regulates the payday loan industry, writes that "payday and similar loans lead to consumers trapped in debt" and in June 2016, the CFPB proposed a regulation to "protect consumers from payday debt traps" (CFPB, 2016a). As I discuss in depth below, empirical work to date has far from resolved the argument on the effects of payday lending on household well-being. New work studying the effects of payday loans remains relevant to this debate, therefore, as well as to the debate on credit access in general.

In this paper, I study how high-cost credit access affects material well-being, as measured by detailed data on household consumption 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 consumption and credit access affects utility through this channel. In addition, household spending is a common measure of material well-being in the literature and is a better proxy of material well-being than household income from a theoretical perspective (Meyer and Sullivan, 2004).

To identify the effects of access to payday lending empirically, I follow the strategy used in Melzer (2011) and compare the spending patterns of two types of households living in states that ban payday lending: 1) households that 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. 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. As numerous states changed the legal status of payday loan operators during the sample period, this strategy takes advantage in both time variation and geographic variation in access to payday lending. It also ameliorates concerns associated with studies that use state-level changes in payday loan availability to identify the effects of payday lending because this strategy compares outcomes for households that are all located within states that prohibit payday lending.

I conduct two main tests. First, I study the effect of payday loan access on household material well-being in "distress" states of the world by using extreme weather events such as hurricanes and blizzards as exogenous, negative shocks to households. I test whether payday loan access helps households smooth consumption following these weather events by comparing the spending of households with payday loan access to those without access after the weather events. Severe weather events 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 extreme weather can close one's workplace, causing a temporary drop in income for hourly workers. Weather events are also strictly exogenous with respect to spending and payday loan access. I study effects on nondurable and durable goods spending broadly as well as spending on specific items such as housing, food, and entertainment.

Second, I analyze how payday loan access affects household spending in an "average" state of the world. As discussed above, I compare outcomes for households close to the border of payday-allowing states and far from the boarder of payday-allowing states to study the effect of overall access to payday lending. 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. If payday lending increases economic hardship as opponents claim and some work finds (Melzer, 2011; Skiba and Tobacman, 2015), I would expect to see that payday loan access results in overall spending declines reflecting such financial distress. Or, 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.[3]

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[3] Payday borrowers are often associated with 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

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.

My findings show that the effects of payday credit on household well-being are state-dependent, i.e., the effects depend on whether a household is experiencing temporary financial distress. First, I show that in a "distress" state, access to payday lending *increases* material well-being for the average household by helping households smooth consumption around the shock. Following an extreme weather event, households without payday loan access reduce spending on nondurables defined narrowly by $15 on average in the month of the event and reduce spending on nondurables defined broadly by $22 on average. For those *with* payday loan access, however, nondurables spending is $30 and $35 higher (defined narrowly and broadly, respectively) than for those without access after the shock. Payday loan access more than offsets the spending declines caused by the weather shocks.

Studying the breakdown of spending effects on specific goods, I find that payday loan access mitigates declines on consumption of food at home, mortgage payments, and home repairs particularly. Households *without* payday loan access spend $11 and $18 less on mortgage payments and home repairs, respectively, in the month of an extreme weather event than in a non-event month, for example. Households *with* payday loan access spend $19 and $36 more than households without access after the weather event on these two items, respectively. These results provide a direct test showing that following periods of financial distress, payday loan access smooths consumption.

Second, I show that payday loan access has the opposite effect on material well-being for households in an "average" state of the world; in average times, granting households access to payday lending *reduces* household material well-being by reducing household consumption. Payday loan access reduces reported household spending on nondurable goods and services. I find

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patterns among payday loan users to be most the consistent with partially naive quasi-hyperbolic discounting specifically. And Parsons and Van Wesep (2013) examine the welfare effects of payday credit using a model where agents are paid at regular intervals and are present-biased sophisticates.[3] Empirical work also suggests at least some subset of payday borrowers may be cognitively impaired; Bertrand and Morse (2011) show that providing more information to payday borrowers about the longer-term costs substantially reduces future loan take-up.

that households with access to payday lending report $219 less spending per quarter on nondurables defined narrowly and $314 on nondurables defined broadly. These results are concentrated in households with a greater propensity to be payday borrowers—those with income between $15,000 and $50,000. I find that the spending reduction is concentrated in spending on mortgage payments ($248 less spending a quarter), rental payments ($149 less spending a quarter), food at home ($87 less spending), and food away from home ($88 less spending). These results are consistent with payday loan access causing households financial distress overall, as critics contend. I find only weak evidence that payday loan access results in more spending on luxury or so-called temptation goods; I see some weak evidence that households in the $15,000 to $50,000 income range with payday loan access increase 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 work contributes to the empirical literature on payday lending by 1) highlighting the state-dependent nature of the effects of this market on household well-being and 2) reconciling some of the conflicting evidence to date on the welfare effects of payday lending. Most work studies the effect of consumer credit access in one setting only and as noted above, authors have found highly mixed results on the effects of payday loan access on household 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), increases personal bankruptcy filing rates (Skiba and Tobacman, 2015), increases use of food assistance benefits (Melzer, 2014), and leads to declining job performance (Carrell and Zinman, 2014). Baugh (2015) shows that following online payday loan restrictions, households reduced bounced checks and increased consumption by 3 percent. On the positive side, authors have found that access to payday loans mitigates foreclosures following natural disasters (Morse, 2011), helps households smooth spending between paychecks (Zaki, 2015), and that banning payday lending results in more bounced checks and complaints against debt collectors (Morgan, Strain and Seblani, 2012). Zinman (2010) finds that capping payday loan interest rates leads to households reporting a decline in overall financial conditions. Bhutta (2014) and Bhutta, Skiba and Tobacman (2015) find little evidence that payday lending has an effect on broad household financial conditions (e.g., credit sores or delinquencies) on average.

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. Most studies find evidence of either positive or negative effects of payday lending on well-being but it is difficult to know if the conflicting findings are due to the particular settings used, due to bias resulting from methodological issues, or if access to the payday loan market does have such differential effects on borrowers. By studying the same spending variables from the same dataset in two different settings, my work provides clear evidence that indeed, payday lending has different effects on household well-being depending on whether a household is experiencing temporary distress. My work is most closely related to that of Melzer (2011) and Morse (2011), but reconciles the differing results between the two papers. My result that payday loan access reduces household consumption on average is consistent with Melzer's (2011) result that households with payday loan access report having difficulty paying their rent, mortgage, and other bills. My results point to spending reductions as the direct mechanism for Melzer's findings and also align with Baugh's (2015) result that restricting online payday lending increases household spending. The weather-event analysis is similar to Morse (2011) and my result that payday loan access mitigates reductions in mortgage payments after weather events is in line with her foreclosure mitigation results. I build on Morse's work, however, by showing a direct consumption smoothing mechanism for payday lending helping to mitigate financial distress. In addition, I show that the consumption effect is broader than mortgage payments alone.

Finally, this work informs policymakers considering actions targeted at payday lenders. The payday loan market, as well as the market for other high-cost credit products like title loans and deposit advances, remains the subject of much public policy attention in the United States. As noted above, the CFPB proposed new regulations for payday lenders in June 2016, following several enforcement actions against payday lenders for deceptive practices (CFBP, 2016a, 2016b, 2014a). Twenty-two states changed the legality of payday lending from 1998 to 2010 (the sample period), with 11 allowing the practice and 11 prohibiting it (Morgan, Strain and Seblani, 2012; Bhutta, 2014). 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.

This work suggests that regulators' and lawmakers' concerns about payday lending worsening household financial conditions and well-being are well founded. However, my results showing that payday lending improves well-being in distress periods also points to the potential benefits of continued access to emergency credit for otherwise credit-constrained households, as

payday borrowers tend to be. Bhutta, Skiba, and Tobacman (2015), for example, find that "consumers apply for payday loans when they have limited access to mainstream credit." They find that about 40 percent of payday loan applicants do not have a general purpose credit card and in total, almost 80 percent of payday loans applicants do not have any credit available on credit cards. Eliminating or severely restricting access to the payday loan market, therefore, could worsen well-being for households in distress.

The remainder of the paper proceeds as follows. Section 2 gives an overview of the payday loan market. Section 3 presents the empirical methodology used for the analyses of the effect of access after temporary periods of financial distress and the overall effect of payday loan access. Section 4 describes the data used and Section 5 discusses the results. I conclude in Section 6.

2. 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 percent to 520 percent APR. Of new payday loans, 36 percent are repaid at the end of the initial loan term and about another 20 percent are renewed once or twice (CFPB, 2014b). A considerable fraction of new loans are renewed numerous times, however. Twenty-two percent are renewed six or more times and over 10 percent of new loans are renewed ten or more times. Most borrowers take out just one series of loans in a year (48 percent), but 26 percent of borrowers take out two series of loans, 15 percent take out three series of loans, and 11 percent take out four or more series a year (CFPB, 2014b).

In 2010, about 12 million individuals were estimated to have taken out a payday loan (Bourke, Horowitz and Roche, 2012). Loan volume for store-front locations was estimated at about $30 billion that year, while online payday loan volume, which has been growing rapidly, was

estimated at $14.3 billion (Stephens Inc., 2011). 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 percent have income between $15,000 and $50,000 and 16 percent report income greater than $50,000 (Bourke, Horowitz and Roche, 2012; note, the breakdown does not sum to 100 percent because some households do not report income).

Payday loan borrowers also tend to have limited liquid assets, be credit constrained, and have a weak credit history. About 55 percent of borrowers reported not having savings or reserve funds in 2007. At the time of taking out their most recent payday loan, about 45 percent reported not having a credit card and 22 percent 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 percent said they could have used savings (Elliehausen, 2009). As compared to non-borrowers, payday borrowers are much more likely to have been at least 60 days delinquent on a payment at some point in the last year and to have been turned down for credit sometime in the last five years (Morgan and Pan, 2012).

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

3. Empirical Methodology

3.1: Effect of Payday Loan Access after a Temporary Negative Shock

In order to 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. Lenders' location decisions are likely correlated with household characteristics and financial conditions, however, which may limit a causal analysis. Instead of relying on payday lender location, therefore, I follow Melzer (2011) and use a strategy that takes advantage of variation that is independent of state-level legislative decisions or households' proximity to particular payday lending locations.

The strategy defines payday loan access by comparing 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 payday loans. As numerous states changed the legality of payday lending over the sample period, this strategy uses variation in access geographically and over time. (Table 1 summarizes the state law changes during the sample period.)

The empirical specification is as follows:

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

$$\beta_3 \text{PaydayAccessXWeatherEvent}_{cnt} + \beta_4 \text{Border}_c + \gamma W_{it} + \delta X_{st} + \delta Z_{nt} + \alpha_s + \alpha_t + \varepsilon_{icnst} \qquad (1)$$

In this specification, i indexes households, c indexes census tracts and t indexes the month of spending. *Expenditure* is the dollar value or the natural logarithm of the dollar value of spending in month t. I use monthly expenditures in this analysis to match the month of the income shock

with the month of spending. *PaydayAccess* is a dummy variable that equals 1 if a household is in a state that bans payday lending and also 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. *WeatherEvent* is a dummy variable that equals 1 if any weather event that caused monetary damages occurred in in the month, in the county of a particular census tract. *Border* is a dummy variable for if a household is within 25 miles of any state border. The regression sample is limited to households in states that ban payday lending. [4]

I include the following additional control variables. 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 and cluster the standard errors at the county level.

This model examines the interaction of access to payday lending and weather shocks. 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 $\beta_2 + \beta_3$. The spending effect of allowing payday lending when no weather shock has occurred is measured by the coefficient β_1.

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

3.2 Overall Effect of Payday Loan Access

Next, I study how payday loan access affects household spending in an average period by studying the average effects of payday loan access overall.

The empirical specification 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 specification, *Expenditure* is the dollar value or the natural logarithm of the dollar value of spending over the quarter ending in month t. I use quarterly spending for this set of tests to reduce noise in the data as compared to studying monthly spending estimates. *PaydayAccess* and *Border* are defined as in the section above. The household-level, state-level, and county-level controls are also the same as above and I also include state and month fixed effects (final month of the quarterly survey period) and cluster standard errors at the county level. The coefficient β_1 is interpreted as the effect of payday loan access on household spending on average, i.e., across states of the world.

4: Data

4.1: 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, rent, mortgage payments, 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.[5] 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

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[5] 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.

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 is limited to state and MSA-level indicators and is 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, which I access 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 and services, similar to Kearney (2004). I deflate expenditures to constant 2010 dollars using the consumer price index for all urban consumers (CPI-U, not seasonally adjusted).

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, 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. In Appendix 1, I provide a detailed description of the expenditure category definitions and how I compute consistent income classes across survey years. 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 mortgage spending and food at home. 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 with payday access are statistically more likely to be homeowners (71 percent versus 65 percent) and have a smaller average family size (2.51 versus 2.54), but the economic magnitude of these differences are not large. The shares of White and Black households do not differ statistically between the two samples, but households with access to payday lending are less likely to be Hispanic or Asian.

4.2. Weather Event Data

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 sample studied here and that the economic magnitude of these events is meaningful. Of the total number of monthly household spending observations in the sample (192,000), weather caused some amount of 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.

4.3: Other Data Sources

As additional state- and county-level control variables, I use personal income data from the Bureau of Economic Analysis, house price data from the Federal Housing Finance Agency, and data on the unemployment rate and employment growth from the Bureau of Labor Statistics (BLS).

5. Results

5.1 Results: Effects of Payday Loan Access on Material Well-Being Following Financial Distress

I first investigate 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. This analysis provides a direct test of whether credit access helps household smooth spending around negative shocks. Table 5 presents results from empirical specification (1) for the four aggregate measures of household spending: total expenditures, nondurables defined narrowly, nondurables defined broadly, and durable goods. In the table, each column presents results from one regression of the dependent variable named at the top of the column on the explanatory variables described above. Panel A of Table 5 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 plays a direct consumption smoothing role for households facing temporary financial distress; households with payday loan access spend more on nondurables after extreme weather events 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. Payday loan access more than mitigates these declines, however. For those with payday loan access, 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 similar results in the log-linear specification. An extreme weather event reduces reported monthly 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, as there is no statistically significant effect on durable good spending.

Looking at the effect of payday loan access on various spending categories following a weather event (Table 6), I find a similar pattern as above for expenditures on several specific categories—food at home, mortgage payments, and home repairs. As in Table 5, Panel A of Table 6 shows a specification with the expenditure measured in levels and Panel B shows a log-linear specification. 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.

Mortgage and home repairs are two other categories 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 mortgage payments are $11 lower but for those with payday access, spending is $19 higher after the weather event than for those without. This result in particular is in line with Morse's (2011) result that payday lending mitigates the increase in foreclosures that occurs following natural disasters in California. Home repair expenditures are $18 lower following a weather event for households without payday loan access, but loan access more than mitigates that decline. Households with access spend $36 more on home repairs after the weather event than those without access. Results for the log-linear specification look broadly similar, although for this specification, I find a statistically significant consumption-smoothing result for food away from home but not for mortgage payments. In untabulated results, I do not find significant effects of payday loan access on household spending measured at the quarterly

level after weather events, suggesting that the consumption-smoothing benefits of payday lending are very short term.

Results from Table 5 and 6 also show that when households are *not* experiencing financial distress, payday loan access *reduces* household spending—i.e., payday lending reduces material well-being in a non-distress state of the world. The coefficient on PaydayAccess (β_1 from specification (1) above) is interpreted as the effect of household payday loan access in the absence of a weather event. I find that in the absence of a weather shock, payday loan access reduces spending on nondurable goods defined broadly, on food away from home, on mortgage payments, on apparel, and on health care (statistical significance depends on the specification, in some cases). These results suggest the effect of high-cost credit access on material well-being is state dependent and I investigate this finding further in Section 5.2.

5.2 Results: Effect of Payday Loan Access on Material Well-Being for Households on Average

I next investigate the overall effect of payday loan access on aggregate household expenditures. Table 7 shows the estimated coefficient on *PaydayAccess* from the regression specification in equation (2). As above, I show results for four measures of aggregate spending and for specifications with household expenditures defined in levels and as then natural logarithm of 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)). 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 on average. 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.

I find that households with payday loan access have lower household spending on average, across aggregate spending categories. The estimated coefficient on *PaydayAccess* is negative and statistically significant in each regression in column (1), indicating that payday access reduces household expenditures on aggregate expenditures, nondurable expenditures, and durable expenditures. For all households in the sample, I find that payday access results in a 5.5 percent reduction in total household spending on average. The results are the most robust for nondurables

spending. Nondurable spending defined narrowly falls by about $220 a quarter (6.3 percent) and nondurable spending defined broadly also 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 for nondurable goods. As there are 1.7 adults per household on average, this corresponds to a monthly spending reduction of about $40 and $60 a month per adult, respectively.[6] I find a reduction in durables spending as well (5.3 percent), although again the reduction is only statistically significant for the log-linear specification. I see similar results when limiting the data sample to households in the $15,000 to $50,000 income class; for these households, the effect of payday loan access on household spending is statistically significant more often in the levels specification.

Next I examine how the spending reductions are split between the detailed expenditure categories. Table 8 shows the coefficient on *PaydayAccess* from empirical specification (2), 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 food, rent and mortgage payments. The results show that households with payday loan access spend about $250 less a quarter on mortgage payments. 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 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;

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[6] The magnitude of these coefficients appears plausible. While banking fees and finance fees are reported in the CE survey and would be included in the "nondurable" goods category above, households are known to underreport nondurable expenditures, particularly for so-called "sin" commodities like alcohol, tobacco, and gambling (Meyer and Sullivan, 2009)). Payday loan fee reporting in the CE has not been studied directly but it is reasonable to think the loan fees are underreported, which could be contributing to some of the overall reported spending reduction. The average payday loan has a $20 fee per $100 of loans spent and since the typical loan is around $300, that implies a fee of about $60 per loan. As noted above, a sizeable fraction of new loans are renewed numerous times (CFPB, 2014). The spending reductions are also consistent with households experiencing broader financial distress (which, as discussed above, a number of researchers have found) and financial distress may also spill-over to spending in other ways such as bounced check fees and other late fees.

households spend $72 less on apparel a quarter; 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 this results is not as robust across specifications. 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 household spending categories 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 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 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 spending overall. The only other spending category for which I observe a spending increase is in utilities spending; payday lending may increase utilities spending on average if payday funds are particularly used to keeping the lights on and the heat running; this result is not very robust, however, so I hesitate to draw a strong conclusion. On the whole, the evidence is highly suggestive that payday loan access on average reduces household spending and material well-being.

6. Conclusion

In this paper, I investigate whether households benefit from increased access to high-cost 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) a

"distress" state of the world (households that have recently experienced a temporary, negative shock to household finances following extreme weather events) and 2) an "average" state of the world. After temporary periods of financial distress, payday loan access helps households smooth consumption over the shock period, helping households keep food on the table and pay the mortgage. Under normal conditions, however, payday loan access reduces average household spending on nondurable expenditures substantially, particularly spending on rent, mortgage payments and food. These results provide empirical evidence on the state-dependent nature of consumer credit's effects on household well-being—the effects vary even *within* the market for one specific credit product—and help reconcile the conflicting evidence fueling the debate on payday lending's effects specifically.

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Table 1: Payday Loan Laws by State During Sample Period

Always Banned	Always Legal			Banned	Legalized
CT	CA	KY	OH	AR (Dec. 07)	AL (Jun. 03)
ME	DE	LA	SC	AZ (Jun. 10)	AK (Jun. 04)
MA	FL	MN	SD	CO (Jan. 10)	AZ (Apr. 00)
NJ	ID	MS	TN	DC (Nov. 07)	AR (Apr. 99)
NY	IL	MO	TX	GA (May 04)	HI (Jul. 99)
VT	IN	MT	UT	MD (Jun. 00)	MI (Nov. 05)
	IA	NE	WA	NC (Dec. 05)	NH (Jan. 00)
	KS	NV	WI	NH (Jan. 09)	ND (Apr. 01)
		NM	WY	OR (Jul. 07)	OK (Sep. 03)
				PA (Nov. 07)	RI (Jul. 01)
				WV (Jun.06)	VA (Apr. 02)

Source: Morgan, Strain, and Seblani (2012); Bhutta (2014)

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
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
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 Statistics: Monthly Weather Events by County

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 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 (1). 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 6: Effect of Payday Loan Access on Detailed Expenditures After Extreme Weather Events

Panel A: Level Specification

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

Panel B: Ln Specification

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

This table presents results from empirical specification (1). 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 7: Effect of Payday Loan Access on Household Expenditures

| | All Income | | Income 15-50K | |
	Ln	Level	Ln	Level
	(1)	(2)	(3)	(4)
Dependent Variable:				
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 (2), 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 dependent 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 8: Effect of Payday Loan Access on Household Expenditures

| | All Income | | Income 15-50K | |
	Ln	Level	Ln	Level
	(1)	(2)	(3)	(4)
Dependent Variables:				
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]
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 (2), 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 dependent 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.

Appendix 1: Income Classes and Variable Definitions

Income Classes

Prior to 2004, the Consumer Expenditure Survey included only directly reported household income. 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 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).

Expenditure Variable Descriptions:

Appendix Table 1: Variable Descriptions

Total Expenditures:	TOTEXP
Nondurable Goods (Narrow):	FDHOME+FDXMAP+ALCBEV+UTIL+GASMO+ PUBTRA+PERSCA+TOBACC+MISC
Nondurable Goods (Broad):	Nondurable Goods (Narrow) + HOUSEHOP+ TEXTIL+SMLAPP+MISCEQ+APPAR+ENTERT+READ
Durable Goods:	TOTEXP-Nondurable Goods (Broad)
Rent Payments:	UCC: 201110
Mortgage Payments:	UCC: 220311, 830201
Food At Home:	FDHOME
Food Away from Home:	FDAWAY
Alcohol and Tobacco:	ALCBEV+TOBACC
Utilities:	NTLGAS+ELCTRC+ALLFUL+TELEPH+WATRPS
Health Care:	HLTHIN+MEDSRV+PREDRG+MEDSUP
Apparel:	MENBOYS+WOMGRL+CHLDRN+FOOTWR+OTHAPL
Entertainment:	ENTERT

All variable definitions are as in the CE EXPN files, except for Rent and Mortgage Payments, which are aggregated from Universal Classification Codes (UCCs) in the CE MTAB files to include only mortgage interest and principal payments and rent payments. (The rental payment category in the EXPN files includes rental repair costs, for example.) UCC number 830201, "reduction of mortgage principal," is coded as a negative value by the BLS; I use the absolute value to consider it as a positive expenditure.