Tornado in Credit Desert:
Role of Consumer Credit Access in Disaster
Recovery

by

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Abstract

I study the effect of credit access restrictions on post-disaster financial outcomes of subprime consumers in Arkansas, a state with the lowest usury cap of 17 percent. Due to the restrictive cap, neither payday nor consumer finance companies operate in Arkansas, while they do in all six neighboring states. Using the difference-in-difference approach, I find that borrowers in border zip codes are less likely to be delinquent on mortgage debt, and have a lower drop in credit score in the post-disaster period in comparison with borrowers in center zip codes. The result is consistent with the adverse effects of credit rationing followed by consumer protection law.

Thesis Supervisor: Christopher Palmer
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Acknowledgments

Contents

List of Figures

List of Tables

Chapter 1

1.1 Introduction

Does the access to alternative credit help under-banked households smooth income shocks and lead to better credit performance or make them more financially vulnerable? According to the rational framework, in the absence of monopoly power and operational inefficiencies, any restriction on credit access will always make the borrowers worse-off. When the assumption on rationality is relaxed, however, and borrowers are assumed to have behavioral biases, there is room for consumer protection laws to improve the welfare, although they lead to credit rationing. The overall effect of these laws is still ongoing debate and depends on the market under question and the margins of adjustments from the borrower's and lenders' sides.

In this paper, I study the effect of consumer credit access restrictions on subprime borrowers in Arkansas. The state is a convenient place to learn the consequences of the protection laws for two reasons. Firstly, the borrowers of the same state have different access to alternative credit: only borrowers who live on the border of Arkansas can obtain a cash installment loan or a payday loan with low travel costs. Secondly, the state is subject to frequent natural disasters: tornadoes, floods, and wind storms, state-wide or local. Consequences of adverse weather conditions include vehicle or housing damage, crop damage, energy shut down. Such shocks lead to urgent repair spending and can serve as a proxy for income shocks for households without rainy

days funds. Thus, the state provides a quasi-experimental environment for testing the effect of negative shocks on borrowers with differential access to credit resulting from the usury law.

Arkansas is historically known for its most restrictive usury law in the United States. Over the period 2005 - 2017, the interest rate cap on consumer credit changed from 10% to 17% in 2011, and the explicit payday lending ban were introduced[1]. The usury law does not apply to national banks since they can charge the highest interest rate allowed in the home state. Neither does it apply to online marketplace lenders since the cap works by the place of origination, and lenders intentionally originate loans in Utah, the state not subject to the restrictive usury law. Besides the payday market, which has been extensively studied in the literature, another category of lenders affected by the law is the consumer finance companies that provide short-term installment credit. The average interest rate on installment loans is significantly lower than on payday loans, but it is high enough to consider the operation under 17% usury law unprofitable. [14] show that no consumer companies operate in Arkansas. While I cannot confidently identify the restricted access to what market, payday, or cash installment, drives the effect on borrowers, the credit bureau data allows me to confirm the rationing in the cash-installment market and test its effect indirectly.

I combine the information from several data sets to implement the study. I get the borrowers' financial outcomes from the Equifax Credit Bureau data merged with McDash Analytics loan-level mortgage performance[2]. The sample of the borrowers is therefore restricted to mortgage-holders, which can bias my estimates downwards since homeowners are less likely to be under-banked and affected by the absence of consumer finance companies in the state. At the same time, it provides me with conservative estimates and allows me to illustrate the complementarity between secured and unsecured debt: the restriction on the unsecured consumer finance loans affects the performance of the mortgage obligations. Secondly, I construct the database of

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[1]Before 2011, payday lending was illegal as well, since the usual payday rates of over 300% by far exceed 10%

[2]I am grateful to Christopher Palmer and Antoinette Schoar for providing me the access to the Equifax-McDash.

disasters in Arkansas by combining the information from publicly available reports of the coordinates of the disaster and its approximate damage with FEMA disaster summaries and aid requests. These sources allow me to assign disaster to the particular zip-code in the credit bureau data. Finally, I am using zip-code and county-level data from the American Community Survey to control for pre-treatment demographic characteristics of the areas.

Using the event study approach and difference-and-difference regression design, I find that disasters push people to originate more installment debt, and the effect is more pronounced for subprime borrowers. The effect is quantitatively small, which can be due to the measurement error in the location of the disaster, or due to the fact, that all borrowers in my sample are mortgage holders, which introduces a downward selection bias. Subprime borrowers living on the border who experienced disaster are less likely to be 60 and 90 days delinquent overall and on their mortgage credit, and have a lower drop in credit score than borrowers living in the center of the state. The results survive controlling for the pre-treatment zip code level income, unemployment rate, demographic characteristics, and the restriction of the sample to the recent period 2011-2017.

The estimates I obtain are generalized difference-in-difference treatment effects since treatment is spread across time and units, zip codes, can be treated and controls several times (non-absorbing treatment). The identifying assumptions of this empirical approach are the exogeneity and unpredictability of the disaster shock to the economic conditions of the region, the homogeneous treatment effect of disaster across time, and the stable structure of the control and treatment groups. To allow for the estimation of pre- and post-treatment outcomes, I set the event window in the way that treated and control zip-codes are not exposed to the disaster within a year since the previous disaster.

1.1.1 Literature Review

My study belongs to the intersection of the consumer finance literature on the welfare effects of alternative credit access restrictions and usury laws in particular and the

environmental economics literature on the consequences of natural disasters.

The trade-off between consumer protection and credit access has long been studied in household finance with application to different markets and countries. The recent studies for the US market focus on the effects of the CARD Act of 2009 ([34], [1], [25]), overdraft regulations ([11], [12]), payday lending bans, and usury restrictions ([14], [16], [29], [3], [2] [30], [33], [38]).

Some authors aim to establish whether the price restrictions affect credit access ([34], [1], [25], [11]). [34] finds that the CARD Act restrictions on lenders' ability to adjust the interest rate in response to the new information led to the credit rationing. [1], [25], in the opposite, did not find evidence of restricted credit access after the CARD Act fee limits. [11] shows that the banks decrease the supply of overdraft fees in response to the pricing strategy restrictions, and [12] find that the removal of cap for overdraft fees leads to the higher price of credit and increases credit access for low-income borrowers. Other authors examine different margins of adjustments by borrowers and lenders followed by regulation. For example, [29] show that while the change in the binding interest rate cap does not lead to the restricted access of credit in the car loans market, retail lenders increase the sales price of the vehicles. Consumers, in their turn, can adjust by substituting the restricted credit with other types of credit: [32], and [11] provide evidence that consumers tend to substitute the high-cost overdraft credit with payday loans. Other examples of adjustments to interest rate caps include the tacit collusion among local lenders and an increase in servicing fees if they are not stated in the usury law, which is a common practice in developing countries ([17]).

From theoretical point of view, in the absence of monopoly power, restrictive usury laws are welfare destroying. If the borrowers are assumed to have behavioral biases, such as temptation, overoptimism, or time-inconsistency of preferences, however, price restrictions can help them to avoid over-borrowing and debt trap situation ([36], [32]). The empirical evidence based on the variation of the payday bans and interest rate caps changes provides mixed results.

The case of Arkansas has been studied in the literature of usury laws ([14] provide

a review), and the restriction of credit access followed by the restrictive policy was documented. [14] and [31] revisit the evidence using the Equifax Credit Consumer Panel and the survey of cash installment lenders respectively. The authors document that 90% of cash installment loans in Arkansas belong to the consumers residing in the counties bordering other states, which makes Arkansas the "consumer credit desert". I complement their findings and contribute to the literature on Arkansas and usury laws by illustrating one of the adverse effects of restricted credit access - the inability of obtaining cash to smooth negative shocks.

I also contribute to the intersection of finance with environmental economics literature by studying the effect of natural disasters on borrowers' credit performance. Most of the papers in the area focus on flooding and hurricanes effects on economy, contracts and products pricing, and households. For example, [10] studies the long-run effects of disasters on government spending and finds that regions exposed to the disasters increase social expenditure unrelated to disasters in the long run. [8], [35] study the effect of natural disasters on mortgage pricing. For example, [35] finds the flood risk leads to credit rationing when insurance limit is binding, which affects the composition of people who live in flood zones. [18] study the effect of Hurricane Katrina in 2005 on the debt balances of the households in the affected areas. They find a strong decrease in borrowers' mortgage debt, indicating that homeowners used the insurance payments to pay out their debt instead of repairing the house. [23] find doubling mortgage delinquencies rates, increase in auto-debt balances, decline in mortgage balances for consumers who were likely affected by Hurricane Harvey. Some papers study the heterogeneous effect of disasters across households. [26] find that loans in the area affected by Harvey are more likely to be 90 days delinquent, with long run effect depending on whether the borrowers were required to have insurance. [19] study the effect of post-disaster public assistance on the survival of establishments and employment. [4] show that differences in pre-disaster financial constraints of people living in affected areas explain the difference in their post-disaster bankruptcy rates. Relative to this literature, I add one more dimension of heterogeneity: the access to consumer credit, which would matter for more financially constrained individuals.

I also focus on the particular type of disaster common to Arkansas area, tornado, which is unpredictable in its location, and is associated with less price adjustment and selection of residence ([19]).

The closest to my study is the work by [33] who shows that natural disasters in California increase the number of foreclosures in the affected zip codes, whereas the proximity of zip code to the payday lender location significantly offsets this effect. I complement this evidence by studying the more restrictive environment introduced by usury laws in Arkansas. The setting allows me to circumvent the endogeneity of lenders' location problem [33] is solving in her work: location of lenders can be related to characteristics of the areas that affect the disaster outcome. In the case of Arkansas, no consumer finance lenders operate within the state, and only the proximity to the border matters for the access to credit.[3] Using individual-level credit performance data, I do not find the effect of the disaster on foreclosures in my sample.

In this work, I use a subset of financial outcomes as a measure of welfare, assuming financial health is correlated with individual well-being. Although it is a reasonable assumption, there are a lot of other dimensions that I do not consider in the project: health, bills repayments, subjective quality of life, investment in durable goods, consumption. The study of the effect of restricted credit access on these outcomes is subject to data limitations and left for future research.

The paper proceeds as follows. In section 2, I provide an overview of the consumer finance loan market in Arkansas and the history of usury laws in the state. In section III, I introduce the empirical strategy. In section IV, I describe the main data sources and the sample construction process. In section V, I discuss the empirical results. Section VI concludes.

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[3]Almost all border counties have lenders from the neighboring state located close to the border, accept for the counties neighboring Mississippi state, since the area around the river is not well populated (or there are not enough bridges across the states), which could be related to the disaster risk - I check if the results hold after I exclude this state.

1.2   Background

1.2.1   Market of Consumer-Finance Loans and Arkansas Usury Law

Arkansas has a fixed interest rate cap on consumer credit of 17 percent, the strictest usury limit in the United States, and the explicit ban on the payday lending industry. The usury rate in Arkansas has been historically low, it was set to 10% by the 1874 constitution, and remained at this level up to 2011. Over this period, there were attempts to relax the ceiling, all rejected by voters ([14]). In addition to consumer protection, there can be other reasons for the usury law to take place. For example, [22] suggest the social insurance motive as the main driver of historical usury ceilings: stricter usury laws help transfering the resources from lenders to borrowers hit by the negative income shock when the supply of loans is inelastic. Thus, the level of interest ceiling is related to the demographic structure of the state population, inequality, and income shocks in particular. While this theory explains the differences in a historical adaption of usury laws in Arkansas, it does not justify the presence of the law nowadays, when lenders have different investment alternatives.

Today the law mainly applies to consumer finance companies, that provide short-term high-interest rate cash installment loans to the subprime market. Using the survey of consumer finance companies, [31] show that subprime borrowers in Arkansas hold significantly less cash installment loans than borrowers in neighboring states, and [14] illustrate the presence of "consumer credit desert" using representative Consumer Credit Panel data. I strengthen their finding showing (Figures 1-2) that the credit desert persists when the sample is restricted to the borrowers with mortgages.

In Figure 1, I construct the ratio of the consumer finance outstanding balance to the non-mortgage balance for subprime borrowers[4] as of the first quarter of 2013 using merged Equifax-McDash extract. The figure illustrates that only people on the border zip codes have the share of consumer finance comparable to the neighboring states. In

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[4]Here and throughout the paper I define the subprime borrower as a person with credit score less than 620 more than 60% of time observed in the data

the center of Arkansas, the share does not exceed 9% while in the other states it can account for up to 29% of non-mortgage balance. Figure 2 provides the evidence on the average consumer finance debt amount held by the Arkansas residents in the first quarter of 2016, and is consistent with the previous finding of credit rationing. This relation holds throughout the sample period 2005-2017 independent of the change in the interest rate cap in 2011, since both of the caps levels are too low for consumer finance companies to run their businesses in the state.

The installment loan credit, a missing market in Arkansas, is an important source of credit for higher risk borrowers, rationed from the traditional unsecured credit markets. These people typically are in the early life-cycle stage, have little or moderate current income, little discretionary income, no liquid wealth (AFSA survey 2013).[5] People usually spend the installment loans on car purchase, acquisition of labor saving appliances, home or car repair, emergency health care expenditures.

In contrast with payday loans, the installment nature of credit allows for the lower interest rate on the short-term debt and the more flexible payment schedule. Why is it still unprofitable for installment lenders to operate under the 17% ceiling? [13] provides the analysis of the cost structure of consumer finance companies based on the National Commission on Consumer Finance (NCCF) in 1968, and [7] reexamine it using data from the Federal Reserve Board's 2015 Survey of Finance Companies. Despite technological advances and credit bureaus development that facilitate the application and verification processes, the operating cost of consumer finance companies that include monitoring, origination, income verification, repayment enforcement, and require labor and software investment are still high relative to the loan sizes. These fixed costs explain the higher price of the smaller loans. The authors' calculations reveal that the break-even APR for $1,187 loan, the average size of the installment loan provided by consumer finance companies, is 60,62 %.

Are borrowers in Arkansas who do not have access to consumer finance credit to substitute it with the other sources of unsecured debt? Using the extract from the

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[5]According to the Experian report 2012, the national average of subprime consumers is 20%. I find the same share of subprime borrowers in the Arkansas sample.

Consumer Credit Panel [14] do not find a significant difference in the use of credit cards between the borrowers in Arkansas and the neighboring states. The subprime borrowers on the border of the state if anything hold more credit card debt than the borrowers in the center of the state. At the same time, they find higher balances of auto-finance retail credit held by Arkansas borrowers relative to the borrowers in other states. Consistent with [29], the retail auto credit market that is subject to usury limit, do not restrict access to credit since the lenders have the sales price as a margin of adjustment.

In Table 1, I compare subprime non-mortgage debt balances by the type of debt in Arkansas with other six neighboring states: Louisiana, Missouri, Mississippi, Oklahoma, Tennessee, and Texas. The average amount of consumer finance debt, as well as the probability of holding a consumer finance loan, are significantly lower in Arkansas versus its neighbors (675 dollars versus the average of 1366 dollars, 20 % versus the average of 31%). At the same time, the average holdings of other sources of non-mortgage credit are comparable across all states. The first two rows of Table 3 compare the mortgage debt holdings across states. Notably, the average monthly mortgage payment amount is the lowest in Arkansas: 926 dollars versus 1147.9 overall average.

According to the credit bureau data, subprime consumers in Arkansas do not substitute consumer finance debt with other sources of credit. However, credit bureaus do not have information on the holdings of alternative unsecured credit, such as payday, rent-to-own, and pawnshops. I use the data of the National Financial Capability Study that surveys the financial attitudes of households every three years to study whether the Arkansas borrowers hold more alternative finance debt compared to the borrowers in neighboring states. The households are asked whether they took a payday/rent-to-own/pawnshop or auto title loan in the last 5 years, and how many credit cards they have. The survey also provides the income and age bin of the respondents. Table 2 summarises the results of the regressions where the use of one of the sources of credit is a dependent variable, and the fixed effects of the Arkansas state are omitted so that the coefficients of the effect of each state is the probability

of using the type of credit relative to Arkansas. Consistent with the payday lending ban in Arkansas, the results suggest that the borrowers within the state are less likely to use the payday credit in comparison with borrowers in neighboring states. There is no significant difference in the use of credit cards and rent-to-own loans. The borrowers in Arkansas are more likely to hold the debt from pawn shops and auto title loans than borrowers in some bordering states, although the effect is not driven by the low-income borrowers. The overall evidence suggests that there are no strong substitutes for consumer finance credit in Arkansas.

1.2.2 Disasters in Arkansas

Arkansas is a state subject to frequent natural disasters. The main types of disasters include tornadoes, floods, storms, and ice storms. In Figure 7, I plot the collected disaster events by the three-digit zip code area over time. The size of dots illustrates the relative strength of disaster expressed as damage per zip code population. One can notice that disasters' occurrence clusters by location, and there are dates when most of the zip codes are exposed. At the same time, there are several local shocks when only one or two zip codes are affected. The largest disaster in the sample is the ice storm in 2009 that touched the northern part of Arkansas. It led to a massive power outage across several areas, and 70% of businesses were affected.

1.3 Empirical Strategy

First of all, I restrict the sample of disasters to large disasters only (the damage larger than 80$ per person), which leads to 45 affected zip codes-dates. For each zip code affected at a certain date, I assign a control group of unaffected zip codes that did not experience a disaster within a year around the event. Since disasters can hit the same area several times, the treatment in my setting is non-absorbing, and I assume that the effect of the local disasters does not last longer than half of a year.[6] I consider the following regression design: $Y_{izt} = \alpha_z + \delta_t + \sum_\tau \beta_D^\tau D_{zt}^\tau + \beta_B Border_z +$

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[6]I check that results hold after considering larger window and severe disasters only.

$\sum_\tau \beta^\tau D_{zt}^\tau \times Border_z + \gamma X_{izt} + \epsilon_{izt}$ where Y_{izt} is the outcome of interest, α_z and δ_t are two-way zip code and time fixed effects, $Border_z$ is an indicator for whether most of the population within zip code live in the border of the state, X_{izt} are the control variables. τ: $-3 <= \tau <= 4$ indicates the time before and after the disaster with $\tau = 0$ meaning the disaster event. In the main specification, I collapse the monthly data to quarterly data. Standard errors are clustered at a zip code level.

The coefficient β_D measures the difference in outcomes between borrowers living in zip codes affected by disaster and borrowers living in non-disaster zip codes over the periods τ. The identifying assumption for the causal interpretation of this coefficient is that without a disaster financial outcomes of borrowers in affected and non-affected zip-codes would have similar deviations from their zip-code and time averages.

The coefficient β measures the difference in the effect of the disaster between the border and non-border zip-codes. The identifying assumption is that without a disaster, the difference in outcomes of borrowers in treated border and central zip-codes would not be different from this difference in control zip-codes. To check whether this assumption holds, I am using the event-study design to see if the significance of the coefficients is sensitive to the inclusion of the pre-trends.

The coefficient β is interpreted as a credit access effect of the disaster consequences under the assumption that the effect of the disaster is the same for border and central zip-codes. Why the effect of disaster for border zip-codes can be different from the center ones? The first concern that might arise is that these areas are systematically exposed to the different types of disasters so that the nature of the damage and income shock is different. 37% of disasters in my sample correspond to tornadoes, the other 24% are floods, and the rest are the winter storms. While tornadoes are local, floods can be more likely to happen in the east of the state closer to the Mississippi River, and winter storms usually hit the northern areas. Helpful for my identification, Arkansas is large enough to travel by car from the center to the border of the state (the one-way trip would take 3.5 hours) but small enough for one disaster to touch central and border areas at the time. In the main specification, I consider tornadoes only (Figure 7). The second concern is that the capital of Arkansas, Little Rock, located in the

center of the state, has more infrastructural advances and the proximity to the capital can alleviate the consequences of the disaster. However, this will result in the central zip codes being less affected by disaster, and it would bias my estimates downwards in estimating the effect of the credit access. Nevertheless, I consider several control variables to account for the difference in economic and demographic characteristics of zip-codes in the pre-treatment periods. Table 5 provides a summary of them. I divide zip codes into three groups based on average income and unemployment rate in the year before the treatment. Individual-level controls include the credit score bin and the age of the oldest account bin.

Additional identification challenge arises from the level of data aggregation: I can identify the location for the borrowers only up to the three-digit level zip-code area, which subsumes 4-5 counties on average (Figure 4). It leaves me with 14 zip-codes for the whole state, where only 4 of them are not bordering the neighboring state. To address this challenge, I exploit the population density within these zip codes (Figure 5). For each border zip code area, I compute the percent of the population living in the border counties and replace the geographical definition of the "border" zip code with the population-based measure. Conveniently for my empirical design, most of the bordering zip codes have over 80 % of the population living either at the border or in the center of the state. My final definition of the border zip codes includes 7 three-digit areas (717, 718, 726, 727, 723, 724, 729).

I use several alternative specifications to isolate the effect of credit access. First, I study the effect of the disaster on new debt origination, focusing on consumer finance and other installment credit. If credit access affects disaster recovery, I should see a higher increase of debt origination for border zip codes in the post-disaster period. Second, I compare the effects for the whole sample of borrowers and subprime borrowers since the credit access channel should matter more for under-banked people who do not have access to the traditional source of unsecured debt.

I study the effect of the disaster on the number of credit performance outcomes: delinquency and mortgage delinquency occurrence, delinquency amounts, foreclosure, bankruptcy, number of inquiries, credit score. Some of my outcomes are binary

24

variables, and I additionally estimate the logit specification of the regression.

To exclude the concerns about the recession period and the different sensitivity of the zip codes to the time trends over this time, I estimate the regressions for the period 2010-2017.

1.4   Data Description

The main data for this study, which I describe in this section, are Equifax Consumer Credit Bureau data merged with McDash mortgage data, and disaster information from FEMA and open sources. I validate the construction of the sample using representative Consumer Credit Panel (CCP) state-level data from the FED website and compare my summary statics to the ones provided by [14], who use the CCP data. Information on population, zip code level income, number of housing units, demographic characteristics comes from the American Community Survey. To compare the investment behavior and risk attitudes of Arkansas consumers to consumers in the neighboring states, I exploit the National Capability Survey, as well as CPS Supplement, and CCP Consumer Expectation Survey. Finally, I use 5-digit zip code-county 2010 relationship files from Census to aggregate the statistics at the county level to the 3-digit zip code level.

1.4.1   Equifax Credit Bureau Data

To study the financial outcomes of Arkansas borrowers I use Equifax credit bureau extract merged with McDash information on mortgage performance. The origination, balances, and monthly payment amounts of each individual are split by the type of debt: mortgage, auto finance loans, bank credit cards, retail cards, consumer finance loans, student loans. The performance measures (number and amount of accounts 30, 60, 90, 120 days delinquent, bankruptcy status, foreclosures) are observed at the aggregate level, and at the mortgage level. The riskiness of borrowers is measured by the vantage credit score. The level of geographical location is represented by a three-digit zip-code level.

I merge all available periods to obtain panel data that runs monthly from 2005 to 2017. Table 1 provides the summary statistics for holdings and performance of mortgage and non-mortgage debt of subprime borrowers from Arkansas and neighboring states.

The natural question is how representative the extract of homeowners is, compared to the total population. I use the aggregated by states data of the Equifax Consumer Credit Panel to validate the levels and the dynamics of debt balances and delinquencies with the sample of people with mortgages. Figure 8 plots CCP the average mortgage debt in Arkansas and the neighboring states as well as across the United States.[7]. The average mortgage debt level in my sample for Arkansas is much higher, of the magnitude of 100000, the level consistent with the average debt of mortgage holders. Figure 6 compares the credit card and auto-finance debt balances, and one can see that homeowners have a 3000 higher non-mortgage debt level on average. The dynamics of the debt holdings in my extract follow the dynamics of the representative CCP sample. The challenge in finding the effect of disaster is that homeowners in Arkansas are not credit-constrained compared to the average Arkansas borrower. The maps on Figures 1 and 2, however, show that Arkansas subprime borrowers in my sample hold considerably less consumer finance debt than subprime borrowers in other states, even conditional on having a house. In the main specification, I restrict the sample to subprime borrowers to account for the sample selection.

1.4.2 Disaster Data

I obtain the disasters narratives from the US Storm Events database [8], collected by the National Center for Environmental Information. It contains the information about the date, coordinates of disaster, type of the disaster, counties affected, approximate damage, as well as the narrative and the source of report. My level of aggregation forces me to consider large enough disasters, that affected several counties in the

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[7]Interestingly, the figure captures the sharp decline of mortgage balances in Louisiana in 2005 (consequences of the Hurricane Katrina).

[8]https://www.ncdc.noaa.gov/stormevents/

zip code area. Based on narratives and the coordinates of the event, I assign the disasters to the three-digit zip code area of the credit bureau sample. I validate the collected data using FEMA post-disaster assistance applications available at zip-code and county-level data. Since the disasters are large enough, FEMA reports most of them. I merge the disasters data with the ACS population data to estimate the damage per person and housing unit within a zip-code area. The damage ranges from 30 to 2000 dollars per person, but in the main specifications, I focus on large disasters with damage higher than eighty dollars per person.

1.5 Estimation Results

1.5.1 Financial performance in the border and the center regions

I start with summarising the impact of disaster on borrowers of border and center zip codes.

Tables 6-10 report the estimation results of the regression (1) described in section 3, and Figures 8-11 plot the corresponding event study coefficients. All regressions include zipcode and time fixed effect, standard errors are clustered at a zipcode level, I use age, credit score, last year zipcode level income, and unemployment rate bins as controls.

Table 6 (Figure 8) illustrates the effect of the disaster event on the 60- and 90-days delinquency, measured as an indicator variable. I find that borrowers exposed to the disaster and living in the center have 0.8 percent higher 60-days and 0.2 percent higher 90-days delinquency rates in the post-disaster period. The effect is likely to be driven by mortgage delinquency: 60-days delinquency rate increases by 0.5 percent and 90-days delinquency rate increases by 0.2 percent for quarters after the disaster. Border zip-codes, however, do not experience significant increase in delinquencies.

Table 7 shows the results for other indicators of financial performance. Center zip codes experience a 24-points drop in credit scores, while border zip codes offset this

decline by more than a half. The probability of being bankrupt, however, increases by half for both of the areas. It reflects the absence of the long-run effect post-disaster differences between borrowers in central zip codes and border zipcodes. I do not find significant impact of disaster on forecloses, which could be explained by the lack of power.

Finally, Figure 11 summarises the effect of disaster on new debt origination of subprime borrowers. I find the increase in auto-loan debt, and consumer finance debt for both center and border zip codes. For borrowers without credit cards, however, the directions are the opposite: center zip codes experience a decline in the consumer finance origination, while border zip codes do not have such an effect.

1.5.2 Is the difference explained by the restricted credit access?

The results of the previous subsection show that center zip codes have worse post-financial performance than the border zip codes. There can be several explanations apart from the credit access for this effect to take place: center zip-codes could be subject to stronger disasters, weaker insurance policies or government support.

To isolate the credit access channel I consider the effect of disaster for prime borrowers. Tables 8-10 present the results. Overall, prime borrowers from center and border regions do not experience any effect of disaster on financial performance. Table 10 and Figure 10 provide a potential explanation for this effect to take place: prime borrowers from both center and border zip codes have 700 dollars higher credit card debt origination (250 dollars higher current balances) int he post-disaster period.This effect does not take place for subprime borrowers, that are more likely to be financially constrained.

While the placebo test for prime borrowers provides a support for the alternative credit access hypothesis, additional work can be done to isolate the effect. I plan to collect the disaster data for the neighboring states to check if borrowers there originate more consumer finance debt in the post disaster period, and compare their

28

performance to the performance of Arkansas borrowers. The absence of the difference in consumer loans origination between border and center, however, does not necessarily speak to the fact that border zip codes use the same amount of credit - they also have access to payday lenders who do not report to credit bureau, and their financial performance can be explained by the use of this credit.

1.5.3 Discussion

How large are the adverse post-disaster outcomes for the borrowers in the center zip codes? I find 15 percent increase in 90-day delinquency rate and 4 percent drop (24 points) in credit score.

Closest to my work, the paper by [19] studies the effect of tornado on households' financial performance. In contrast to my results, the authors do not find significant long-run effects of disaster on households' balances and delinquencies outcomes. They find a 1 percent decline in bill delinquencies for subprime borrowers when disaster help was available. These estimates are the same as for border zip codes borrowers in my sample (Table 6, Column 2).

[33] finds the foreclosure rate of 4.5 per 1000 homes higher for homes with more than 10 miles distance from the payday lender, while the proximity to the lender offsets this increase by a half. My point estimates for foreclosure are comparable to her (Table 7), although not significant. The effect of the distance from the lender is much higher in all my estimates - I find that people in border counties do not have any effect on delinquency. It can be explained by more restricted environment I consider: no payday lenders operate within the the state, and no evidence of substitution for the high cost credit is found.

1.6 Conclusion

I find the suggestive evidence for negative effect of restricted credit access on the financial outcomes of subprime borrowers using a disaster as a proxy for income shock. Borrowers of Arkansas living in the areas where the consumer finance credit

is easily available do not worse their financial performance in the disaster and post-disaster periods, while this is the case for borrowers who live in the center of the state. While the result illustrates one of the adverse effects of credit rationing, a consequence of the usury law, it does not speak about the overall effect of credit restrictions on consumers' well-being. First, I observe only a subset of Arkansas borrowers, who were able to obtain the mortgage credit. It could be the case, that borrowers have behavioral biases, such as present bias or self-control, and tend to borrow more credit than they think they can repay in good times, which could lead to their default in the future, and the loss of access to traditional sources of credit. I would not see these borrowers in my data, and future research is needed to see whether there is any evidence of the debt trap as a result of the access to the high-cost credit. Second, while my findings speak to the positive effect of access to credit in the disaster periods, there can be additional advantages for subprime borrowers. Since consumer finance companies submit the reports to the credit bureau, they can improve the credit scores of subprime borrowers who repay their obligations on time, which would help them to get access to the traditional unsecured credit, such as credit cards. Finally, other outcomes unobserved in the data could be affected by the restricted access to consumer credit: medical bills and health outcomes, durable purchases, investment in education, vacation spending, crime rates - these are the directions for future research.

Appendix A

Tables

Table A.1: Non-mortgage debt balances of subprime borrowers in Arkansas and neighboring states

	AR	LA	MO	MS	OK	TN	TX	All
Consumer finance								
Holds consumer loan	0.204	0.353	0.249	0.374	0.319	0.343	0.324	0.317
	(0.403)	(0.478)	(0.433)	(0.484)	(0.466)	(0.475)	(0.468)	(0.465)
Amount Current, Consumer Finance	675.4	1815.0	1077.1	2031.6	1355.0	1726.8	1272.8	1366.0
	(2252.2)	(4139.0)	(3034.2)	(4260.8)	(3474.6)	(4275.9)	(3133.5)	(3440.3)
Original Balance/Credit Limit, Consumer Finance	5136.3	7395.5	5938.4	7531.6	5950.3	7108.9	6028.6	6321.9
	(5419.8)	(7860.5)	(7783.0)	(7580.7)	(6522.3)	(7892.2)	(7058.9)	(7284.2)
Monthly Payment Amount, Consumer Finance	182.5	271.1	236.2	287.1	232.5	242.7	301.3	276.1
	(542.2)	(440.1)	(589.6)	(530.1)	(490.6)	(570.7)	(1988.0)	(1510.9)
Current Balance, Consumer Finance	3233.8	5208.7	4366.6	5531.4	4222.3	5034.2	4114.5	4401.5
	(4365.3)	(6853.2)	(8139.6)	(6753.1)	(5364.5)	(6789.0)	(5945.1)	(6351.6)
Bank Credit Cards								
No. of Accts Current, Bank Card	1.451	1.255	1.418	1.164	1.346	1.338	1.489	1.416
	(2.140)	(1.891)	(2.088)	(1.871)	(2.075)	(2.039)	(2.189)	(2.113)
Amount Current, Bank Card	3694.4	3357.4	4154.2	2905.6	3959.9	3866.2	4340.4	4053.9
	(8530.3)	(8128.9)	(9608.6)	(7677.1)	(9480.0)	(9465.0)	(10315.0)	(9722.8)
Original Balance/Credit Limit, Bank Card	9819.1	9544.0	11485.4	8558.1	10976.8	10599.3	11803.0	11144.8
	(14194.1)	(14829.2)	(16537.5)	(13449.8)	(16474.3)	(16123.5)	(17877.2)	(16877.0)
Monthly Payment Amount, Bank Card	764.6	774.3	883.4	723.1	885.3	789.5	1114.4	971.2
	(3198.6)	(3828.0)	(3632.1)	(3385.5)	(3845.8)	(3248.4)	(4813.5)	(4256.6)
Current Balance, Bank Card	8483.7	8098.7	9833.9	7287.7	9513.8	9004.0	10174.7	9573.1
	(12199.0)	(12456.7)	(14258.8)	(11576.6)	(14049.3)	(13626.4)	(15291.2)	(14417.1)
Auto loans								
Holds auto bank loan	0.197	0.181	0.207	0.152	0.281	0.181	0.211	0.206
	(0.398)	(0.385)	(0.405)	(0.359)	(0.449)	(0.385)	(0.408)	(0.404)
Amount Current, Auto Bank	3893.8	3697.5	3256.1	2611.4	5916.1	3087.1	4874.7	4287.2
	(10450.1)	(10551.5)	(8822.9)	(8360.0)	(13266.8)	(9646.6)	(12601.5)	(11529.3)
Current Balance, Auto Bank	19589.6	20253.8	15643.5	16964.2	20939.6	16929.4	22747.6	20632.4
	(15840.5)	(17068.6)	(13645.5)	(14502.2)	(18038.6)	(17334.5)	(18570.2)	(17741.8)
Holds auto finance loan	0.348	0.328	0.303	0.335	0.271	0.315	0.369	0.342
	(0.476)	(0.469)	(0.460)	(0.472)	(0.445)	(0.465)	(0.483)	(0.474)
Current Balance, Auto Finance	22095.2	21585.9	17480.0	20349.0	20051.7	18366.7	22860.8	21417.6
	(16045.9)	(16103.8)	(13490.6)	(15139.1)	(15709.5)	(14114.9)	(17302.3)	(16403.9)
Student loans								
Holds student loan	0.234	0.211	0.242	0.236	0.222	0.219	0.210	0.218
	(0.424)	(0.408)	(0.429)	(0.425)	(0.415)	(0.414)	(0.407)	(0.413)
Current Balance, Student Loan	39456.8	41005.1	40314.3	45727.7	36841.8	43469.4	39250.1	40233.2
	(43720.0)	(47323.2)	(46718.5)	(52377.7)	(44888.8)	(51557.6)	(46634.2)	(47439.8)
Retail Credit Cards								
Holds retail card	0.354	0.306	0.338	0.317	0.307	0.324	0.343	0.334
	(0.478)	(0.461)	(0.473)	(0.465)	(0.461)	(0.468)	(0.475)	(0.472)
Current Balance, Retail Card	2602.9	2256.1	2359.7	2397.1	2359.2	2351.0	2452.8	2413.2
	(3426.1)	(3128.4)	(3186.1)	(3316.6)	(3234.7)	(3194.7)	(3469.8)	(3356.3)
Other loans								
Holds other loan	0.0996	0.148	0.0909	0.169	0.154	0.117	0.123	0.124
	(0.299)	(0.355)	(0.288)	(0.375)	(0.361)	(0.321)	(0.328)	(0.329)
Current Balance, Other	12895.5	10937.8	12216.6	12052.7	11930.3	12148.4	9931.4	10872.7
	(32826.5)	(36624.6)	(73360.2)	(42157.7)	(39208.9)	(73743.2)	(19896.7)	(40963.9)

Source: Merged Equifax-McDash extract, 2013

Table A.2: Types of alternative credit used by Arkansas borrowers in the past 5 years relative to the neighboring states

	Dependent variable:				
	payday	credit cards	rent-to-own	pawn shop	auto loan
	(1)	(2)	(3)	(4)	(5)
stateLA	0.039**	0.013	−0.022	−0.028*	0.008
	(0.015)	(0.015)	(0.014)	(0.017)	(0.017)
stateMO	0.036**	0.009	−0.003	−0.030*	−0.035***
	(0.015)	(0.015)	(0.014)	(0.016)	(0.017)
stateMS	0.034**	0.004	−0.0002	−0.006	−0.017
	(0.015)	(0.015)	(0.014)	(0.017)	(0.017)
stateOK	0.037**	−0.023	−0.006	−0.011	0.018
	(0.015)	(0.015)	(0.014)	(0.017)	(0.017)
stateTE	0.039***	0.004	−0.021	−0.025	−0.041**
	(0.015)	(0.015)	(0.014)	(0.016)	(0.017)
stateTX	0.041***	0.009	0.003	0.032**	−0.030*
	(0.014)	(0.014)	(0.014)	(0.016)	(0.016)
I(income_less15k + income_bn1525)	0.009	−0.329***	0.063***	0.147***	−0.189***
	(0.023)	(0.022)	(0.021)	(0.025)	(0.024)
income_less15k	−0.019	−0.073***	−0.027**	−0.001	−0.078***
	(0.013)	(0.013)	(0.012)	(0.014)	(0.014)
income_bn2535	0.097***	−0.241***	0.067***	0.114***	−0.125***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)
income_bn3550	0.069***	−0.161***	0.037***	0.083***	−0.048***
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)
stateLA:I(income_less15k + income_bn1525)	0.062**	0.009	0.045	−0.010	−0.019
	(0.029)	(0.028)	(0.027)	(0.031)	(0.031)
stateMO:I(income_less15k + income_bn1525)	0.077***	−0.013	0.004	−0.019	0.055*
	(0.029)	(0.028)	(0.027)	(0.031)	(0.031)
stateMS:I(income_less15k + income_bn1525)	0.063**	−0.043	−0.016	−0.050	0.027
	(0.029)	(0.027)	(0.027)	(0.031)	(0.031)
stateOK:I(income_less15k + income_bn1525)	0.099***	−0.044	−0.001	0.011	0.009
	(0.029)	(0.028)	(0.027)	(0.031)	(0.031)
stateTE:I(income_less15k + income_bn1525)	0.094***	−0.018	0.005	−0.005	0.032
	(0.029)	(0.028)	(0.027)	(0.031)	(0.031)
stateTX:I(income_less15k + income_bn1525)	0.035	0.020	−0.009	0.003	0.019
	(0.028)	(0.027)	(0.026)	(0.030)	(0.030)
Constant	0.131***	0.849***	0.183***	0.329***	0.338***
	(0.023)	(0.023)	(0.022)	(0.025)	(0.026)
Observations	10,824	14,663	10,842	10,840	14,744
R^2	0.087	0.206	0.084	0.138	0.132
Adjusted R^2	0.084	0.204	0.081	0.136	0.129
Residual Std. Error	0.351 (df = 10789)	0.399 (df = 14627)	0.331 (df = 10807)	0.381 (df = 10805)	0.448 (df = 14708)
F Statistic	30.112*** (df = 34; 10789)	108.591*** (df = 35; 14627)	29.121*** (df = 34; 10807)	50.973*** (df = 34; 10805)	63.654*** (df = 35; 14708)

Note: National Financial Capability Study, 2009-2018

*p<0.1; **p<0.05; ***p<0.01

Table A.3: Non-mortgage debt balances of subprime borrowers in Arkansas

	AR Interior	AR Border	All
Consumer finance			
Holds consumer loan	0.160	0.174	0.166
	(0.367)	(0.379)	(0.372)
Current Balance, Consumer Finance	687.0	704.4	694.8
	(2953.1)	(2483.6)	(2752.1)
Bank Credit Cards			
No. of Accts Current, Bank Card	1.236	1.094	1.173
	(1.999)	(1.899)	(1.956)
Current Balance, Bank Card	5418.7	4827.9	5153.2
	(11039.0)	(10748.7)	(10913.4)
Auto loans			
Holds auto finance loan	0.272	0.229	0.252
	(0.445)	(0.420)	(0.434)
Current Balance, Auto Finance	6916.4	5332.3	6204.6
	(13164.0)	(11226.1)	(12356.1)
Retail Credit Cards			
Holds retail card	0.286	0.275	0.281
	(0.452)	(0.446)	(0.449)
Current Balance, Retail Card	916.9	870.8	896.2
	(2274.8)	(2140.4)	(2215.5)
Other loans			
Holds other loan	0.0820	0.0969	0.0887
	(0.274)	(0.296)	(0.284)
Current Balance, Other	1355.6	1512.9	1426.3
	(10526.8)	(17174.9)	(13912.8)

Source: Merged Equifax-McDash extract, 2013

Table A.4: Credit performance of subprime borrowers in Arkansas

	AR Interior	AR Border	All
90 DPD rate	0.0628	0.0547	0.0592
	(0.243)	(0.227)	(0.236)
Amount 90 DPD, All Accounts	33725.7	35403.3	34422.6
	(54444.9)	(57639.1)	(55791.5)
60 DPD rate	0.104	0.0964	0.100
	(0.305)	(0.295)	(0.301)
Amount 60 DPD, All Accounts	50426.3	45416.9	48265.1
	(70754.8)	(71204.4)	(70986.1)
90 DPD Mortgage, rate	0.0160	0.0152	0.0157
	(0.126)	(0.122)	(0.124)
Amount 90 DPD, First Mortgage	104079.9	101433.9	102924.9
	(55699.0)	(59899.6)	(57551.3)
60 DPD Mortgage, rate	0.0419	0.0371	0.0398
	(0.200)	(0.189)	(0.195)
Amount 60 DPD, First Mortgage	109707.0	104967.4	107718.7
	(74260.4)	(83057.8)	(78088.4)
Bankruptcy Status	0.0902	0.0570	0.0753
	(0.287)	(0.232)	(0.264)
Foreclosures, number	0.0982	0.121	0.109
	(0.336)	(0.362)	(0.348)
Credit Score	557.0	557.0	557.0
	(47.63)	(47.70)	(47.66)

Source: Merged Equifax-McDash extract, 2013

Table A.5: Zip-code level demographic characteristics of Arkansas residents

	AR Interior	AR Border	All
Average Income, All Households	56287.3	54134.1	55210.7
	(7730.1)	(8755.6)	(8013.1)
Unemployment Rate	0.0853	0.0816	0.0834
	(0.0110)	(0.0144)	(0.0125)
Total population, mean	214035.5	211331.1	212683.3
	(107382.6)	(131766.0)	(115487.4)
Total housing units, mean	98959.0	94402.8	96680.9
	(45271.8)	(49494.7)	(45630.8)
Share of population under 19	0.127	0.134	0.131
	(0.00756)	(0.00946)	(0.00908)
Share of population with age 20-34	0.181	0.182	0.182
	(0.0204)	(0.0193)	(0.0191)
Share of population with age 35-59	0.319	0.315	0.317
	(0.00617)	(0.00598)	(0.00614)
Share of population aged over 65	0.182	0.177	0.180
	(0.0304)	(0.0334)	(0.0308)

Source: American Community Survey and Bureau of Labor Statistics, 2017

Table A.6: Mortgage Delinquency, Subprime Borrowers

	(1) 60 DPD	(2) 90 DPD	(3) 60 DPD Mortgage	(4) 90 DPD Mortgage
Post=1	0.00818**	0.00284	0.00479***	0.00253***
	(0.00242)	(0.00187)	(0.000741)	(0.000504)
Post=1 × Border=1	-0.0164***	-0.0120***	-0.00567*	-0.00327*
	(0.00358)	(0.00229)	(0.00195)	(0.00132)
Dependent variable mean	0.0849	0.0547	0.0334	0.0162
R^2	0.0953	0.0641	0.0414	0.0207
N	330110	330110	330110	330110

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.7: Financial Performance, Subprime Borrowers

	(1) Credit Score	(2) Bankruptcy status	(3) Foreclosure
Post=1	-24.84***	0.0198***	0.00338
	(1.431)	(0.00214)	(0.00279)
Post=1 × Border=1	16.93***	-0.00552	-0.00491
	(3.301)	(0.00320)	(0.00363)
Dependent variable mean	548.5	0.0447	0.0609
R^2	0.0398	0.0148	0.0869
N	330110	330110	295729

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.8: Mortgage Delinquency, Prime Borrowers

	(1) 60 DPD	(2) 90 DPD	(3) 60 DPD mortgage	(4) 90 DPD mortgage
Post=1	0.000139	-0.00000181	0.0000629	0.0000218
	(0.000139)	(0.0000756)	(0.0000295)	(0.0000287)
Post=1 × Border=1	0.000223	0.0000607	-0.00000498	-0.0000698**
	(0.000133)	(0.000120)	(0.0000312)	(0.0000204)
Dependent variable mean	0.00146	0.000815	0.000254	0.000132
R^2	0.0477	0.0427	0.0126	0.00816
N	987717	987717	987717	987717

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.9: Financial Performance, Prime Borrowers

	(1) Credit Score	(2) Bankruptcy Status	(3) Foreclosure
Post=1	2.950**	0.000315	-0.0000684
	(0.925)	(0.000467)	(0.0000592)
Post=1 × Border=1	1.302*	-0.000408	-0.000150
	(0.436)	(0.000395)	(0.000121)
Dependent variable mean	742.8	0.00396	0.000414
R^2	0.0946	0.0127	0.0249
N	987717	987717	987717

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.10: Post Disaster Credit Card Balances

	(1)	(2)	(3)
	Credit Card Origination (Subprime)	Credit Card Origination (Prime)	Credit Card Balance (Prime)
Post=1	-7.021	779.8**	254.7**
	(153.0)	(222.5)	(60.57)
Post=1 × Border=1	-263.4	-36.40	58.02
	(204.9)	(240.0)	(38.49)
Dependent variable mean	5382.4	21715.1	4511.1
R^2	0.104	0.143	0.177
N	311133	987717	850718

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
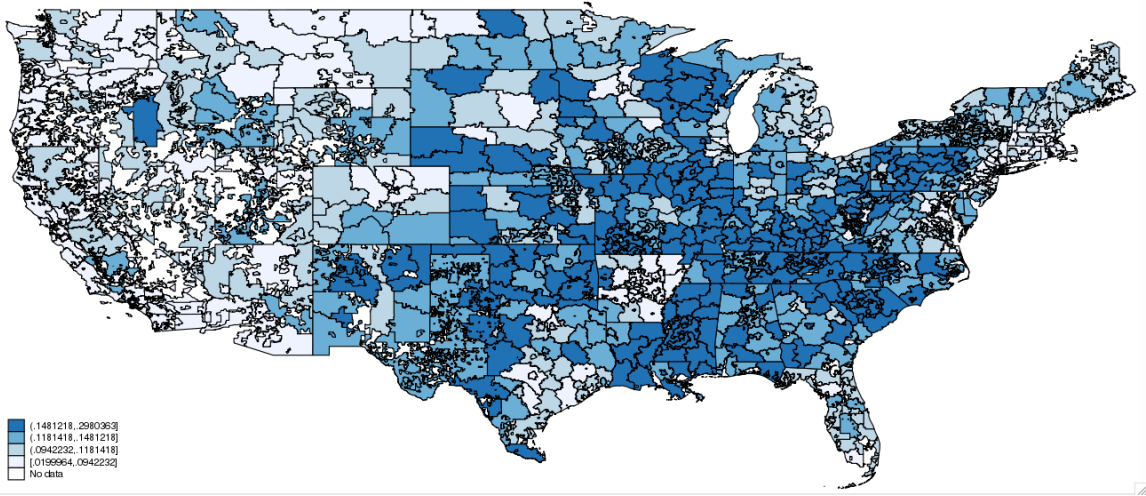
Figure B-1: Share of consumer finance balance in total non-mortgage outstanding balance. Subprime borrowers, 2013Q1
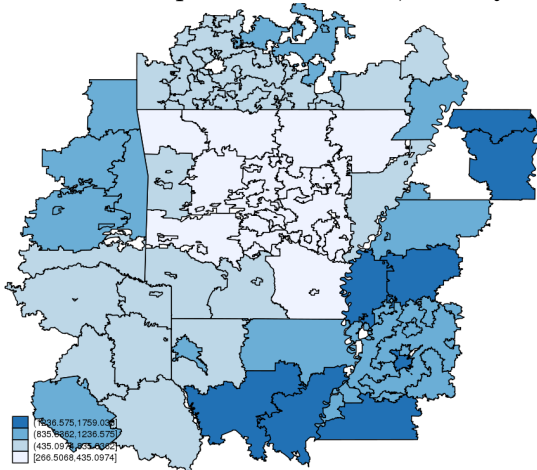
Figure B-2: Average amount of consumer finance balance held by subprime borrowers of Arkansas and neighboring states, 2016Q1
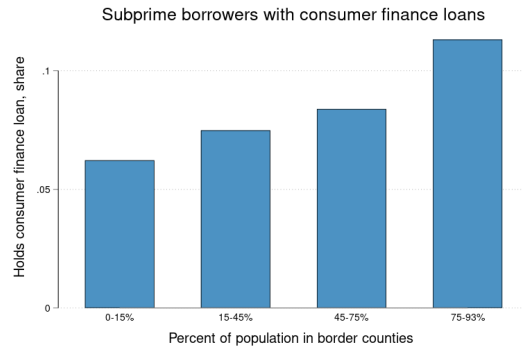
Figure B-3: Share of people with consumer finance balances across zip code groups, Arkansas, 2016Q1
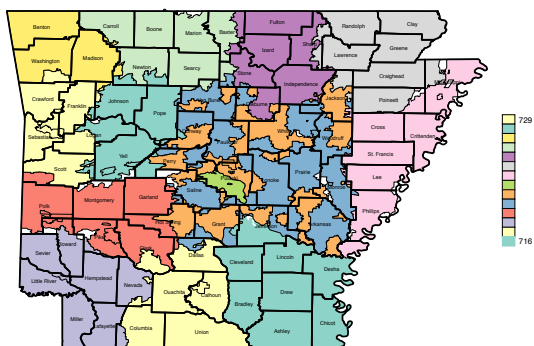
Figure B-4: Mapping between counties and three-digit zip codes in Arkansas
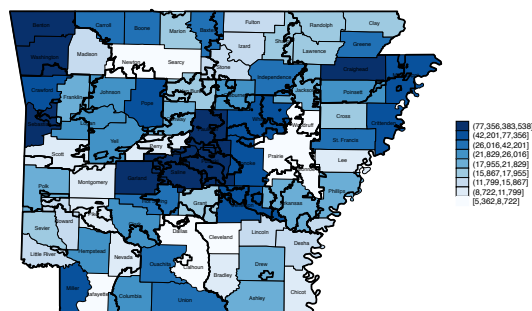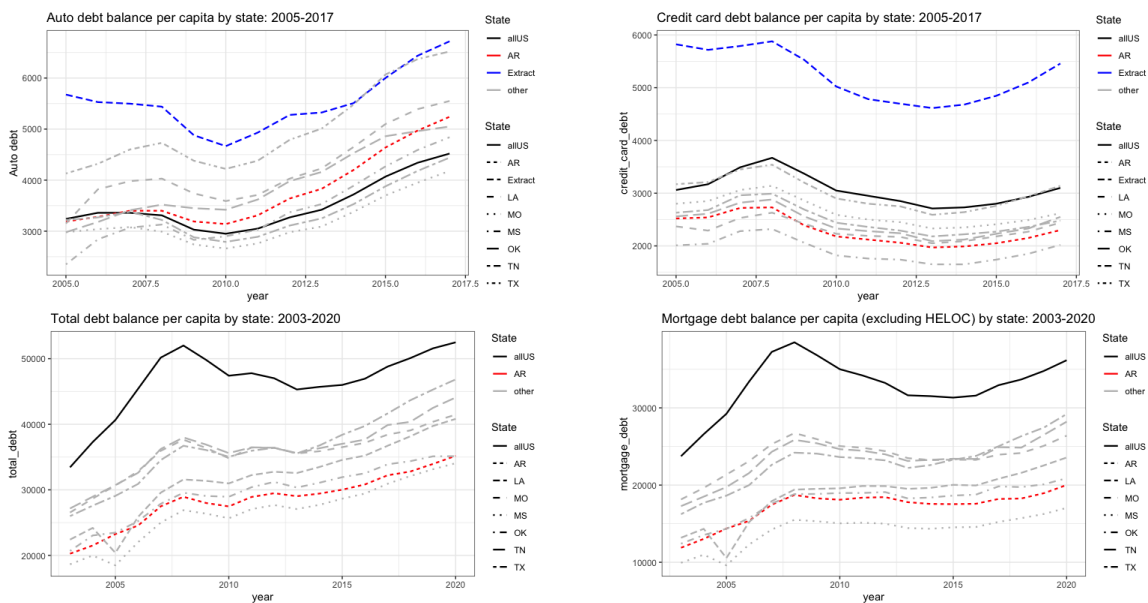

Figure B-5: Distribution of population across state of Arkansas

Figure B-6: Debt balances of borrowers in Consumer Credit Panel and merged Equifax-McDash Extract
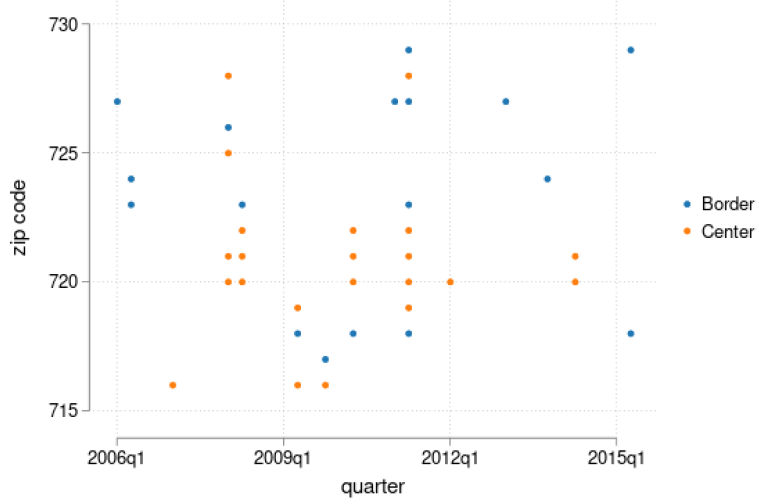
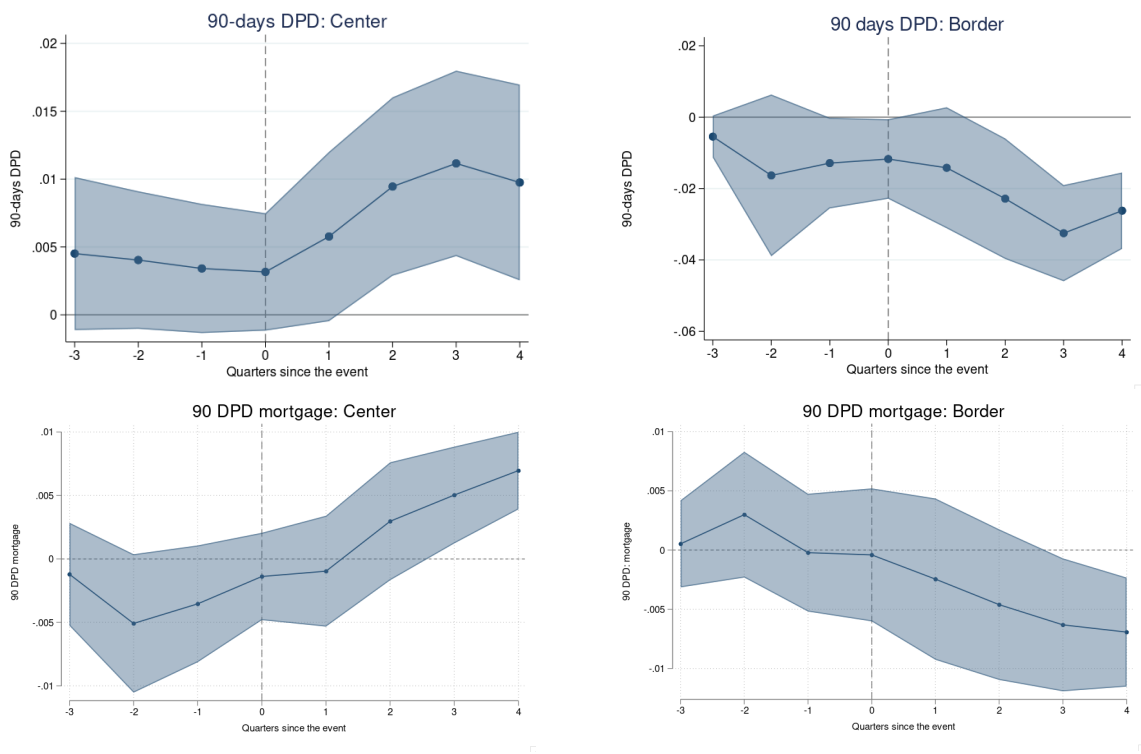Figure B-7: Tornadoes in Arkansas from 2006-2015 by three-digit zip code.

Figure B-8: Event study coefficients of the effect of the disaster shock. Dependent variable: Delinquency
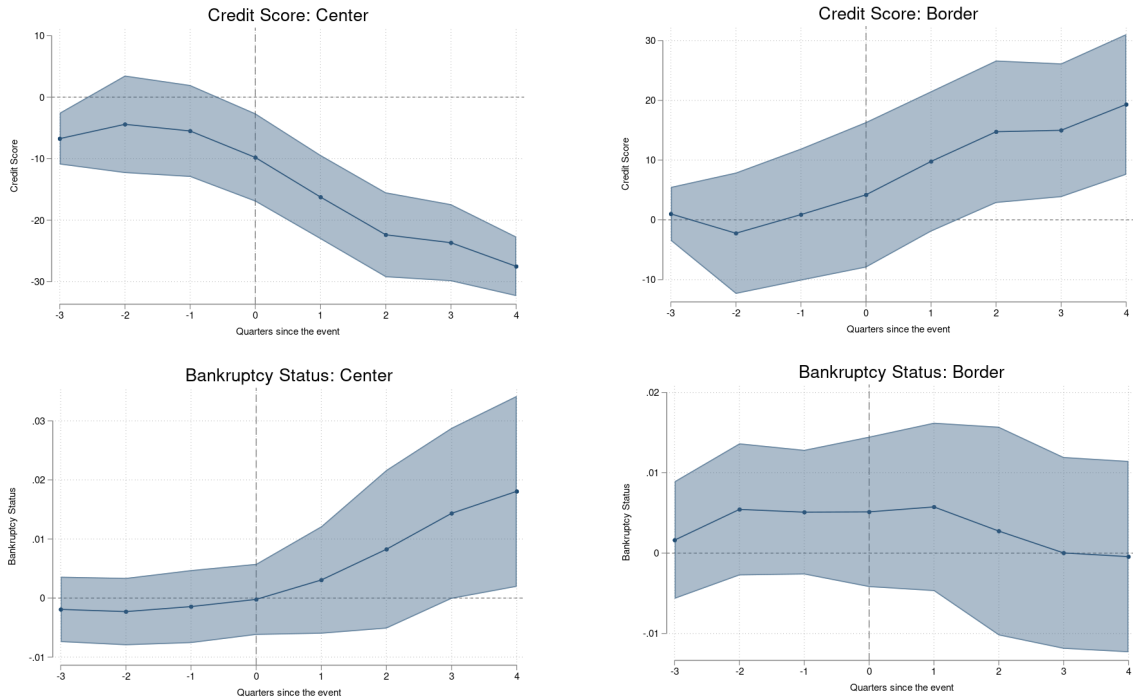
Figure B-9: Event study coefficients of the effect of the disaster shock. Dependent variables: credit score, bankruptcy
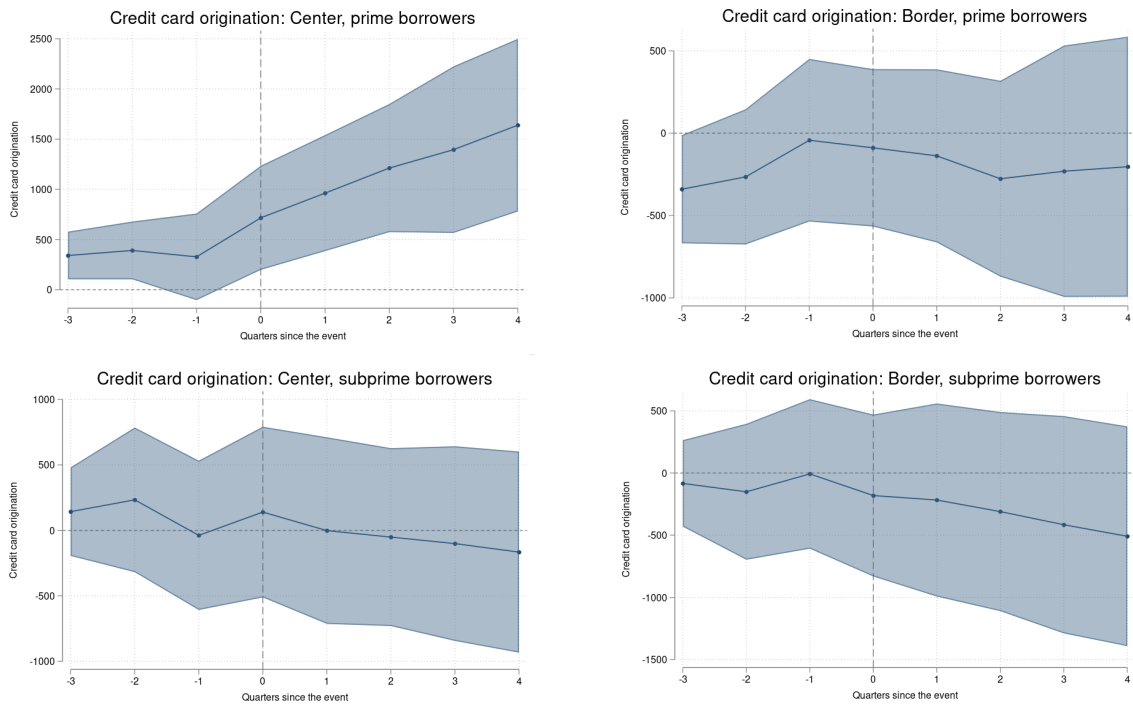

Figure B-10: Event study coefficients of the effect of the disaster shock. Dependent variables: credit card origination.
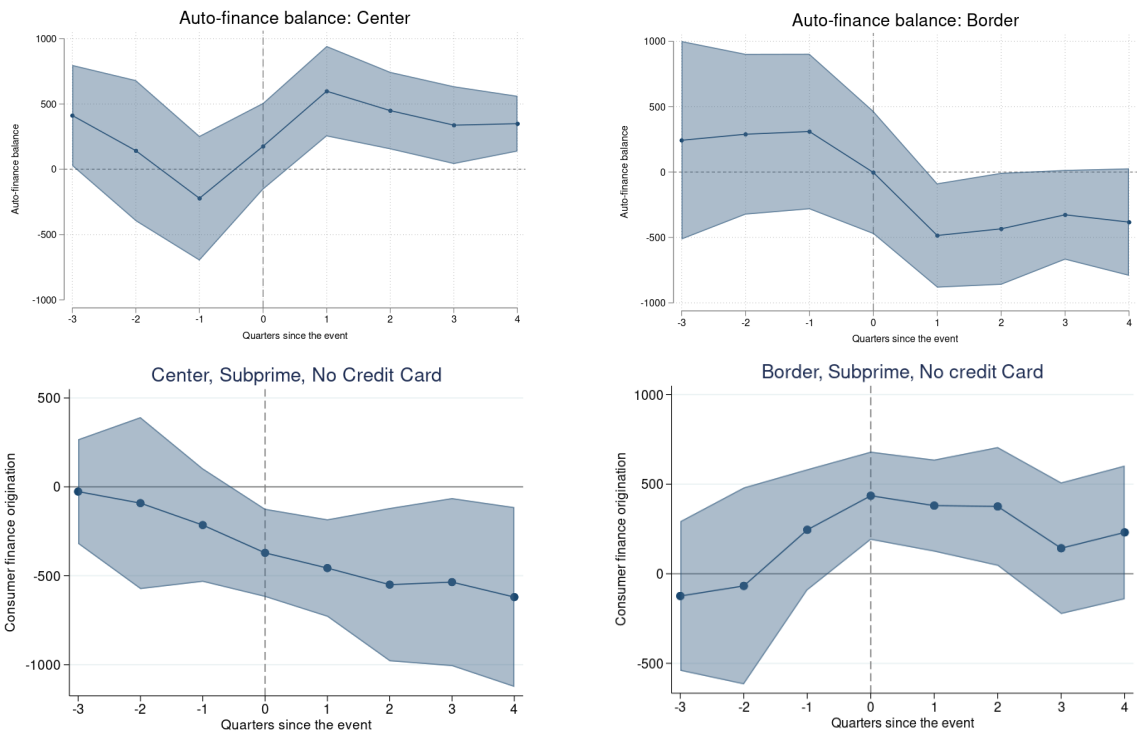
Figure B-11: Event study coefficients of the effect of the disaster shock. Dependent variables: auto-finance and credit card balances

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