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RESTRICTIONS ON CREDIT: A PUBLIC POLICY ANALYSIS OF PAYDAY LENDING

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RESTRICTIONS ON CREDIT: A PUBLIC POLICY ANALYSIS OF PAYDAY
LENDING

A Dissertation
Presented to
the Graduate School of
Clemson University

In Partial Fulfillment
of the Requirements for the Degree
Doctor of Philosophy
Applied Economics

by
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Accepted by:
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ABSTRACT

Using state level data between 1990 and 2006, I find no empirical evidence that payday lending leads to more bankruptcy filings, which casts some doubt on the debt trap argument against payday lending. I capture the intensity of the payday lending activity in a state by the number of payday lending stores.

I control for restrictions on payday lenders by including into the analysis six variables that I construct that rank legislative provisions across states and across time.

DEDICATION

To all that stayed close to me through good and bad times.

To all who made and make free inquiry possible, to all who strove and strive to push the limits of understanding.

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CHAPTER ONE

INTRODUCTION

From a normative point of view, the interference of the government into voluntary transaction has to have one of two main goals: a Pareto improvement, or enhancing social equity. If the public policy is not intended to increase efficiency, then it can be justified as a means to transfer wealth, and one can engage into a political economy analysis of the factors that lead to that particular policy, and its costs. Since all policies have costs, a second concern is to what extent the government intervention in the form of enacted laws and regulations achieves the proposed goals.

In this paper, I analyze the market for a form of short-term credit, known as payday lending. I address the question of whether the *presence* of payday lending stores has negative welfare implications in the communities where they are present.

Using state level data between 1990 and 2006, and two different empirical approaches, I find that there is no empirical evidence that payday lending leads to more bankruptcy filings - a measure of consumers' welfare. This suggests that there is no statistical evidence to support the circle of debt argument against payday lending.

I use the variation in the state laws during the same period to identify important payday lending restrictions that influence payday lenders presence. Based on variation across states and across time in restriction provisions, I construct six variables that capture price restrictions, licensing related or entry restrictions, and other business practice restrictions. These variables rank the provisions across states and time. I find

that the main restriction that influences the presence of payday lenders is the price restrictions.

Chapter two gives a short background on payday lending, and some of the potential welfare effects of regulating the credit market. In the next chapter, I review the literature. I discuss the data and the empirical approach in chapter four. I investigate whether payday lending stores lead to higher bankruptcy incidence in chapter five. Chapter six concludes.

CHAPTER TWO

BACKGROUND ON PAYDAY LOANS AND WELFARE EFFECTS OF CREDIT RESTRICTIONS

Background on payday loans

A payday loan (also known as deferred deposit advance or loan, cash advance, payday advance) is a single payment unsecured short-term credit of small amounts.

The lender keeps until the maturity of the loan the personal check issued by the debtor that includes both the principal and the interest (the transaction could also be based on an agreement authorizing the lender to make an electronic withdrawal from the borrower checking account on the maturity date).

The lender requires some proof of income (recent pay stubs usually suffice), and a checking account. The maturity of the check usually coincides with the borrower's next paycheck or deposit of funds date. At maturity the check is either deposited by the lender or the borrower pays in cash to get it back.

The check is not collateral, but acts on the margin as a deterrent to default and reduces the cost of collection, because returned checks generate farther charges.

A typical two-week \$300 cash advance will usually have a \$45¹ charge, which corresponds to a 390% non-compounding APR.

There is evidence that in fact borrowers take several loans during a year, which would suggest that to a certain extent a rollover phenomenon is present².

¹ This information comes from industry insiders, and it is consistent with previous studies - for example, "80% of all payday loans across the country are reportedly less than \$300" in Stegman (2007), p. 169, Chessin (2005), or reports - for example, WA state DFI (2006).

Elliehausen and Lawrence (2001), Chessin (2005), Stegman (2007), Skiba and Tobacman (2007) summarize the reasons individuals take payday loans, alternatives available, and demographic characteristics of payday lending users.

At the beginning of the 1990's these lending services were provided mainly by small independent check cashing outlets and pawnshops. Now the industry expanded significantly, and the providers include large regional or national multi-service providers (some of them publicly traded)³.

This increase in the industry is consistent with the evolution of the store locations of the members of the national trade association, The Community Financial Services Association of America (CFSA), comprised of more than 150 members representing over half of the payday lending stores⁴.

As the number of payday lending stores increased in the last 10-15 years, both in absolute numbers and per capita (see appendix A), reaching today around 23,000⁵ store locations nation wide, many states that did not have already restrictions in place, started to respond in state assemblies.

The enacted payday lending specific restrictions vary greatly across states, from no significance restrictions, for example in Delaware, to a complete ban in Georgia. In 2004 in Georgia payday lending was declared public nuisance, it was included in the definition of racketeering - which could bring up to 20 years in prison and fines of

² See for example Stegman (2007), Chessin (2005).

³ Snarr (2002)

⁴ www.cfsa.net/about_cfsa.html, accessed on 3/27/2008

⁵ My own estimate, based on 2007 data from regulatory bodies.

\$25,000 per transaction -, and it was subject to class-action lawsuits. In addition, the creditors were barred from collecting the debt, civil penalties of up to triple the amount of money gained in interest and fees were established, and the proceeds of the loans were taxed at a rate of 50%⁶.

The most important restriction on this type of loans seems to be the price ceiling imposed.

There were several arguments invoked in passing legislative restrictions against this type of loans, among which⁷:

- outrageous / abusive charges;
- pushing people into a debt trap (the circle of debt argument);
- targeting military personnel;
- targeting the minorities and the poor;
- irresponsible lending.

The typical 2-week period could be mostly demand (and not supply) driven; for example, consumers might want a self-imposed constraint. Since in most of the cases the

⁶ See S.B. 157, Act 440, signed 4/14/2004, effective 5/1/2004.

⁷ For example, Georgia House of Representatives Daily Report Number 16, February 12, 2004: “Committee members attending hearings on the subject heard horror stories from people, many of whom served in the armed forces, who fell behind on their payments and were pressured into taking out another loan. These new loans carried additional fees and charges, eventually resulting in payoffs that were exponentially larger than their original loans. Additionally, members learned that payday lending institutions were using many loopholes, and association schemes to avoid Georgia’s usury laws, allowing them to charge exorbitant interest rates, some of which climbed as high as 500 percent. [...] in order to prevent payday lenders from preying upon the men and women who serve in our nation’s armed services, House members added special restrictions on payday loans to military personnel.”

OCC Advisory Letter 2000-10: “payday lending can pose a variety of safety and soundness, compliance, consumer protection, and other risks to banks. [...] The OCC will closely review the activities of national banks engaged or proposing to engage in payday lending, through direct examination of the bank, examination of any third party participating in the transaction under an arrangement described above, and, where applicable, review of any licensing proposals involving this activity. These examinations will focus not only on safety and soundness risks, but also on compliance with applicable consumer protection and fair lending laws.”

charges are the same per transaction, the more a person renews a loan (effectively increasing its maturity) the longer maturity they would want (perhaps realizing that the constraint they impose on themselves does not work). In other words, we should see that the maturity is a positive function of the number of rollovers.

It is likely however, that the main reason why payday loans are usually of small amount with a maturity no longer than two weeks - even in states where the loan and maturity limits are larger or no limits exist -, has to do with the risk of the loan. It seems that this is the optimal combination that allows a reasonable assessment of the risk of default for these subprime individuals, based on the potential source of repayment.

Payday lenders do screen the applicants. Even though the payday lenders might not pull up the applicant's credit score from a credit reporting agency (however, some payday lenders use other external services to verify the applicant - for example, Teletrack⁸), they do check the past behavior of the applicant with them and their affiliates (and in some states with all payday lenders in that state). The proportion of rejected payday lending applications in Skiba and Tobacman (2007) was about 20%⁹.

The very small default rate¹⁰ for this small unsecured loans, would suggest that borrowers value the option to come back.

Thus, the argument of irresponsible credit used as proof of lenders preying on innocent victims (without taking into account the ability to repay) loses its strength.

⁸ www.teletrack.com/successes/payday.html, accessed on 3/27/2008.

⁹ Rejection rate of 19.89% = 17.4%*99.6% + (100% - 17.4%)*(100% - 96.9%).

¹⁰ Between 1996 and 2004, in Colorado 3.34% of the total loan volume were charged-off Chessin (2005); industry insiders claim that the national average is around 2%.

Welfare effects of credit restrictions

Restrictions on credit diminish the ability of individuals to smooth their consumption in the presence of income or expenditure shocks (like loss of employment, medical emergency).

Unless they increase the efficiency or the competition, restrictions on a business activity are likely to lead to increases in costs. In the long-run, since there are no barriers to exit the industry, these will be passed onto consumers. Morse (2007) argues that the threat of legislative restrictions acts like a potential barrier, reducing the competitiveness of the industry below what it could be otherwise.

In addition to direct increases in costs caused by compliance with the rules and regulations, there is an indirect negative effect on consumers: the uncertainty with respect to future legislative provisions (based on high recent volatility across states) will make the expected required returns for the lenders higher than it would be otherwise.

However, there are several reasons why regulating the credit market might increase the efficiency of that market. For example,

- the presence of market power
- asymmetric information, and externalities
- time inconsistent preferences.

It is hard to convey that there is too much room for misleading in a payday contract because it is very simple in terms of the costs and structure: there are no hidden costs. If the borrower keeps his promise to return the loan at maturity, then the cost is

just the difference between the value of the check that the lender will deposit at maturity and the amount that the borrower got when the contract was signed.

Why the borrower does not return the loan at maturity is another story. If his intention was to cheat on his contract it is not clear that we should take his side just because he is presumably poorer.

In this context, the circle of debt argument works only if the costs that the default borrower has to bear are hidden and disproportionately high, and if the lenders would impose a high minimum amount loan that would make the borrower less likely to be able to repay it, which is not supported by the evidence.

Paternalism could be justified if the supplier has an information advantage over the consumer. Suppose the consumer is not "smart enough" to figure out that the loan on average has to be refinanced, i.e., he accepts a short term interest rate for essentially what is a medium term loan. To prevent the supplier to use this asymmetry in information to prey on the borrower, restrictions could be put into place to stop the consumer from entering contracts that are not in his own interest.

If the borrower does not understand the high costs of the loan, and if he is at the bottom distribution of the income and financial sophistication, he is more likely to be unable to repay it at maturity. He will either default or try to refinance. If he will get into a rollover or renewal of the debt, the result will be a rapid increase in his indebtedness. This is because payday loans are designed as short term loans, but become increasingly costly as medium or long term instruments.

Posner (1995) argues that “it is difficult to say that all or even most poor or not so poor people who accept risky credit do so because they have bad judgment” (p. 296). He points out that in a society that has welfare programs, restrictions on voluntary transactions can increase efficiency. He suggests that the survival of usury over time can be explained by our welfare system. Usury laws prevent some of the lending to high-risk borrowers who are or would be as a result of extra borrowing on welfare. The restrictions, in this way, would reduce the excess credit. The potential welfare opportunistic behavior would be mitigated, and the social desired standard of living could be achieved at a lower cost.

Borrowers are affected differently by restrictions on the ability of the lender to recover the loan, depending on their probability to default. Barth et al. (1986) point out that borrowers with relatively low probabilities to default are made worse off by restrictions on credit because of the resulting higher charges. Borrowers with relatively higher probabilities to default could be made better off, especially if they act opportunistically, but whether or not they gain is an empirical matter. Barth et al. (1986) argue that restrictions on creditor remedies leave the typical borrower worse off.

Brooks (2006) argues that there is a source of negative externality in the fringe market: since most creditors do not report to credit reporting agencies their credit history with their customers, they prevent “good” borrowers to move into to the low cost credit market. He suggests, however, that requiring or encouraging fringe creditors to report their credit history with their customers might be preferred to preventing fringe credit.

If the discounting factor of the future utility depends on the time span to that future, and not upon the period in which it occurs, the preferences will be time inconsistent, and the individuals will have self-controlling problems. Strotz (1956) shows that under these conditions, precommitment could enhance one's utility.

Individuals with hyperbolic discount functions – for which the instantaneous discount rate for future consumption falls as the time horizon increases -, have dynamically time-inconsistent preferences. Constraints on their future behavior - like limits on their ability to consume -, could improve their welfare.

Laibson (1997) shows that individuals with hyperbolic discount functions could be better off if they are able to impose liquidity constraints on themselves by investing into illiquid assets. For them, having access to finance can make them worse-off since it reduces their ability to commit. His efficiency improvement results hold in general if there are no unforeseen emergencies.

To summarize, the net welfare effects of restrictions on credit are ambiguous:

a) some individuals will use high cost payday loans until they overcome the bad times. Even if they borrow often from payday lenders during a period of time, they are more likely to be left worse off as a result of more restrictions, since credit access will be reduced; in addition, one of the effects of regulations is an increase in costs and thus in the price paid. Restrictions on payday lending could stop or discourage people in this group from engaging in voluntary trade, and could force them to choose a less preferred alternative (presumably more costly).

b) It is quite possible, on the other hand, like with any other market transaction, that some individuals will make mistakes and will find themselves ex-post in a worse position than anticipated. Some individuals might, but not necessarily, be left better off with more restrictions on payday lending (for example, individuals who have self-control problems). Even for the second group it's not clear that they would be better off. The relevant question is what their opportunity cost is, what they would do if more restrictions on payday lending will leave them out of this market.

From a public policy point of view, the question is not whether there are individuals who will be made better off by more restrictions, or by less restrictions (almost certainly the answer is yes to both), but rather which group dominates, what is the net welfare effect – which is an empirical matter.

CHAPTER THREE

LITERATURE REVIEW

Early period is dominated by consumer advocacy studies, which mainly have correlations and cross-tabulations, and studies which are predominantly descriptive and legislation focused.

Studies on payday lending often arrive at contradictory conclusions.

Many authors have tried to identify the factors that influence the demand and the supply for payday loans; for example, Graves (2003), Graves and Peterson (2005), Stegman and Faris (2003), Burkey and Simkins (2004).

Other researchers try to analyze the industry and the financial performance of payday loan firms: for example, Stegman and Faris (2003), Flannery and Samolyk (2005), Morgan (2007), Huckstep (2006).

Others focus on how the consumers are affected by access to payday lending: Karlan and Zinman (2007), Skiba, and Tobacman (2007), Melzer (2007), Morse (2007), Morgan (2007), Morgan and Strain (2007), Wilson et al. (2008).

For more details about payday lending industry see Stegman (2007), Mann and Hawkins (2007).

Johnson (2002) and Chin (2004) recommend harsher federal legislation of payday lending. Johnson (2002) supports federal, as opposed to state laws, arguing that payday lenders are predatory because many systematically break the consumer protection laws.

However, no empirical work is included to support his conclusions or to show that his policy proposal increases efficiency or equity. Similarly, Chin (2004) argues that the state payday lending restrictions are not sufficient to protect the consumers against abusing practices, and that minimum regulation at federal level is necessary - including a price ceiling -, based on which states could have harsher rules. He also suggests that price ceilings are necessary to prevent a recession that could result from excess borrowing by the high risk group. No formal empirical analysis is provided to show his proposal as welfare improvement, other than descriptive tabulations.

For military targeting by payday lenders see Graves and Peterson (2005). Using store locations in 20 states at the end of 2003 / beginning of 2004 they found support for the hypothesis that payday lenders locate in the proximity of the military personnel. They argue that this is evidence of military personnel being targeted by payday lenders, because the distribution of banks around military bases is not similar, and thus, the distribution of payday lenders in counties or zip codes within three miles of military bases can not be explained by potential commercial development patterns and zoning ordinances. Furthermore, they argue that the large area covered makes it less likely (but not impossible) that demographic characteristics are driving the results.

But this is what we should expect to get. Graves (2003) shows that in metropolitan Louisiana and in Cook County (Illinois) banks and payday lenders tend to be located in different areas. This is contrary to Burkey and Simkins (2004), who find that payday lenders and banks tend to cluster in the same area in North Carolina, which they interpret as evidence that banks and payday stores are complements, not substitutes.

If payday lenders substitute the banks in this market segment, the result that payday lenders target military personnel is equivalent to stating that banks discriminate against military personnel by refusing to serve them.

However, the fact that payday lenders are located in the proximity of military bases is not necessarily something evil, and it does not prove that payday lenders prey on the military. They could just respond to the demand. The correlation suggests that military personnel have some characteristics that make them more likely to demand payday loans. Actually, Graves and Peterson (2005) identify four main characteristics that make military personnel more likely to use payday loan.

Graves (2003) addresses the question of minority and the poor targeting by the payday lenders, by analyzing the difference between the distribution of income and ethnicity between residents in payday lending neighborhoods and neighborhoods without payday lenders. He finds that payday lenders enter poor and minority neighborhoods at a lower rate than banks get out of them.

Albeit the econometric analysis is probably too simple (he tests whether there is a significant difference between the mean of these characteristics), his results weakly support payday lending industry claims that their success is caused by the under provision of this market segment by the traditional credit institutions.

However, Stegman and Faris (2003) find that in low and moderate income communities in Charlotte, North Carolina, payday lenders enter the market before the banks leave it. But this could be consistent with a scenario in which the banks stopped

offering similar products with payday loans before they left the area, and before payday lending stores entered it.

Stegman and Faris (2003) find race to be a significant factor in explaining the demand for payday loans. Burkey and Simkins (2004) also find that race is an important factor in explaining the location of both payday lending stores and banks in North Carolina.

Even though they have a small effect on default rates, economic and demographic factors in the neighborhood where the store is located¹¹ do not have a significant effect on store profitability in Flannery and Samolyk (2005), after the loan volume is controlled for.

Stegman and Faris (2003) estimate that a measure of rollovers – the percentage of chronic borrowers – is the second most important variable to the revenues and a proxy for profits, after the number of clients.

This is in contradiction with the newer study by Flannery and Samolyk (2005), who use proprietary individual store data from two large payday lenders and show that store profitability is cost driven. They find that profitability depends on the volume of loans, but not on the proportion of frequent borrowers (a measure of the degree of rollover in that store).

Huckstep (2006) argues that the reason for the large number of stores and high operating costs (that translate in high fees) relative to other lenders is that payday loans are a product chosen mostly for convenience. Because of this payday lenders compete in

¹¹ And based on Graves and Peterson (2005) - which find that the store managers want to be within three miles from their target demographic – the characteristics of that store customers.

local area and have “high density of stores, and keep those stores open beyond normal business hours” (p. 210-1)

He compares the profitability of seven publicly traded payday lenders against that of six commercial lenders and Starbucks. The later was chosen because of the similarity in terms of business model. He finds that the average 7.63% payday lenders profit margin is lower than both the profit margins of commercial lenders (13.04%) and Starbucks (9%).

Morgan (2007) constructs a model to test if payday lenders engage in predatory lending. Assuming the lenders have an information advantage over the borrower and engage in predatory lending, the lenders would benefit from pushing borrowers into debt traps. If payday lenders would prey on borrowers, we would expect to find higher debts in states with higher loan limits, and also higher defaults.

Based on the 1995 and 2001 Survey of Consumer Finance household level data and on differences in the maximum loan size across states, Morgan (2007) does not find evidence that the presence of payday lenders lowers the welfare of less educated and more uncertain income households: even though the debt level is higher, the defaults are not. This would suggest that payday lenders do not engage in predatory lending.

Melzer (2008) using a 1997, 1999, and 2002 survey of economic hardship measures, and within state variation in loan access argues that having access to payday loans leads to “increased incidence of difficulty paying mortgage, rent and utilities bills; moving out of one’s home due to financial troubles; and delaying needed medical care, dental care and prescription drug purchases”.

He constructs the variable of interest based on the fact that some counties in the three states that ban payday lending that he considers are close enough to the border of a state that allows it, so that there is a border effect. In other words, individuals that live in the border counties of banning states have access to payday lending and those who live farther from the border within the banning states do not.

One of the problems with his analysis is that in all three states he bases the analysis on, payday lenders were not explicitly banned. They were absent because of the small interest rate caps, which affect all small loan creditors¹², not only payday lenders. So, from this point of view, his analysis holds and will have to be interpreted in the same way if one replaces “payday lenders” with any other small loan lender (like credit unions etc.).

He tests for the presence of a border effect based on payday lending stores location information collected in July 2007 from the state regulatory agency.

There are two reasons why he is likely to overstate this cross border effect.

He uses a 25 miles distance to the border of payday store in the allowing state, and a 25 miles distance to the border in the banning state as cutting points. Graves and Peterson (2005) show that payday lending store managers want to be within three miles from their target demographic.

This is consistent with what I have seen in my data: store location patterns seem to support a relatively small radius of patronizing consumers for each store: for example,

¹² Other than banks after the 1978 Supreme Court decision, *Marquette vs. First Omaha Service Corp.*

Ace Cash Express opened a store in Augusta, Georgia less than 1.8 miles away from its own store.

Second, there is likely an upward bias in the number of payday lending stores used to test for the border effects. He overstates the presence of payday lenders, at least in three states. For example, in Kentucky, Rhode Island payday lenders are licensed as check cashers, but there is no distinction between check casher only and check cashers that are lenders. Similarly in New Hampshire, where the licensing data are kept at “small lenders” level, which include payday loans, title loans, and other small loans.

In addition, the intensity of the presence of payday lenders seems to be ignored. For example, in New Hampshire the number of total small lenders was about 100 (payday lenders are a subset of that).

There is likely sample selection bias that is not addressed: it is not clear that he chose the three states randomly.

There were other relevant states to be included in the analysis for his chosen time frame: in total 10 in which payday lending was effectively banned, 11 that had a regime change.

One might argue that the analysis should be carried out for all the states to see if his results are spurious.

Morgan and Strain (2007) test the debt trap hypothesis of payday lending by analyzing the change in the welfare of consumers in states that banned payday lending in 2004 and 2005 (Georgia and North Carolina) relative to states that did not.

They find that households in Georgia are worse off as a result of the ban: they bounce checks more, have more complaints against lenders and debt collectors, and go bankrupt more often. This suggests that payday lending gives consumers more options, and banning it reduced the welfare of debtors, by forcing them into more expensive alternatives, which contradicts the debt circle argument against payday lending. They find similar results for North Carolina.

Morgan (2007) also finds that cities with a higher density of payday stores and pawnshops tend to have lower charges on loans, which support the idea that the payday lending market is competitive.

Morse (2007) uses propensity score matching to identify the communities that are likely to be cash constrained based on different socioeconomic variables. The communities that have payday stores and those that don't have them are similar in terms of welfare characteristics considered (foreclosures, deaths, drug treatment, and births). Then he uses the propensity scores to compare the zip level communities that faced a disaster (wildfire, flooding, storm/hail, earthquake, landslide, and tornado) with similar cash constrained ones that did not.

He finds evidence that the welfare variables considered are improved after disaster shocks in the presence of payday lending.

Furthermore, he finds that high bank density (measured relative to the mean) does not have the same positive welfare effects as payday lending.

Wilson et al. (2008) found similar results in a laboratory experiment that sought to test how individuals who face similar constraints to those faced by payday loans

customers are affected by access to payday loans: “78.1% of the subjects with access to payday loans benefited from both the existence of and their subsequent use of payday loans” when they were faced with expenditures shocks (p. 25-6). He also finds that if the number of loans is above a certain threshold (ten), individuals are less able to absorb negative shocks than similar individuals that do not have access to payday loans.

Skiba and Tobacman (2007) look at the effect of access to payday loans on borrowing activity, bankruptcy, and crime. They do not find any effect on crime, but they find that higher access to payday loans leads to higher frequency of borrowing.

They also find suggestive but *inconclusive* evidence that access to payday loan credit (measured by the applicants’ payday loans approval) increases chapter 13 bankruptcy filing rates, which is in contradiction with this paper’s results.

However, there are several reasons why their results might be problematic.

What they are actually addressing is *the impact of access to payday loan credit* (measured by the outcome of the first payday loan application, accepted or rejected) on:

- subsequent payday-loan applications;
- pawn loan borrowing;
- personal bankruptcy petitions.

This is different in important ways than access / denial of credit induced by regulations. What they capture is the behavior of two groups of individuals that have different relevant characteristics: some are considered too risky by the payday lenders, and the others are not.

If payday lending has any welfare effects (positive or negative) - at least in terms of policy implication -, the relevant population for analysis is not all applicants, but rather approved applicants.

In other words, if, for example, we are interested to see if payday lending leads borrowers into a cycle of debt (and eventually to bankruptcy), those who apply but are rejected should not be included in the analysis (even if they are cash strapped).

The access to payday credit is estimated based only on the success of the application to the payday lender that they base their analysis on. In most instances, an individual could borrow from several payday lenders. If payday borrowing has any effects on bankruptcy etc., then what's relevant is the overall payday history, not only the history with one payday lender.

Furthermore, they use only the outcome of the first application as a measure of access to payday credit. It is likely that conditional on being approved once, the subsequent history is more important.

When they analyze the effects on bankruptcy, they looked at bankruptcy filings in three Texas Districts, based on which they matched the individuals in their data set. However, we can not tell a priori if they misrepresented or they overrepresented the proportion of those who filed for bankruptcy and had borrowed from a payday lender. They use the data from a payday lender which also offers pawn loans, and we do not know how representative that lender is for the whole payday lending industry. There are a few major players at the national level, and we know that their data do not come from

the biggest one (Advance America), which does not offer pawn loans, and has more than twice the payday lending stores than the second biggest chain¹³.

Another source of problem in their study is that their data set is truncated. They have payday transaction data for 4 years, from 9/2000 to 8/2004. The pawnshop data spans from 1/1997 to 11/2004 and is for the same company. The bankruptcy data are from 1/2001 through 6/2005, the arrests data are from 2000 to 2004. They do not seem to address this problem (the history of a lot of individuals does not start with their data set, so a lot of the relevant information is missing, and is not missing consistently across individuals).

This paper contributes to the literature on payday lending in several ways.

Most of the papers study the effects of payday lending based on *access* to payday lending, without a measure of the intensity of the industry.

As far as I am aware, only Graves (2003), Graves and Peterson (2005), Burkey and Simkins (2004) and Stegman and Faris (2003) incorporate the number of payday lending stores into the analysis.

Graves and Peterson (2005) use store locations in 20 states at the end of 2003 / beginning of 2004 to see if payday lenders tend to be located in the proximity of the military personnel.

Graves (2003) analyzes the spatial distribution of banks and payday lenders in metropolitan Louisiana, and in Cook County, Illinois.

¹³ My own estimate, based on 2007 data from payday lenders and regulatory bodies.

Burkey and Simkins (2004) try to explain the location of both payday lending stores and banks in North Carolina in 1999 and 2000 based on demographic characteristics.

Based on Charlotte, North Carolina data in 1999 and 2000, Stegman and Faris (2003) use the number of payday lending stores to model payday lenders revenues. They also use these data to see how payday lending stores are located relative to the banks.

This paper is similar with Skiba and Tobacman (2007) who have payday transaction data for 4 years, but they measure the effects of being granted a first payday loan.

In this paper I use the number of payday lending stores for each state between 1990 and 2006 to capture the effect of the *intensity* of the industry on the welfare of consumers, measured by the bankruptcy filings. I try to capture the intensity of the presence of payday lending activity through the number of payday stores.

I also have a longer panel data, which capture the outset of the industry, and in addition I measure the effects not only of the presence of payday lenders, but also of the change in the relevant legislation.

CHAPTER FOUR

DATA AND THE EMPIRICAL APPROACH

Ideally, the welfare effects of payday lending should be analyzed by tracking over time the individuals who are using this type of loans with respect to different measures of welfare.

Data limitations however do not allow me that.

Instead, I use the variation in payday lending legislation and the number of payday lending stores across time and states to assess the aggregate effects at state level.

Given the prevalence of repeated borrowing¹⁴, since payday loans users accept to borrow with such a relatively high cost, it is likely that they don't have other less expensive alternatives. Elliehausen and Lawrence (2001) show that more than 60% of payday loans users did not use their bank cards because they did not have enough available credit. This means that individuals who fit the profile of a payday loan borrower are likely to be more exposed to negative shocks. This is consistent with Skiba and Tobacman (2007) who find that the bankruptcy rate of payday loan users is ten times higher than the national average.

For a review of the bankruptcy procedure see, for example, Luckett (1988) or Fay et al. (2002).

There are two main ways in which a debtor could get some relief under the bankruptcy laws: file for liquidation of his non-exempt assets under chapter 7, or file

¹⁴ For example Stegman (2007), Chessin P. (2005).

under chapter 13. Under chapter 7 debtors' non-exempt assets are sold and any unsecured debts that can not be covered from the proceeds are discharged, with no other obligation for the filer. Under chapter 13 the debtor keeps his current assets, but promises to repay his debt (or a fraction of it) out of his future income.

Borrowers who are pushed into repeat borrowing are more likely to file for chapter 7, especially if they don't have many assets (if you feel that you got somehow cheated, you are more likely to want to have the debt discarded).

Gross and Souleles (2002) argue that the default costs have decreased; in addition the filer could ask in the petition to have the court filing fees waived.

The main question addressed in this paper is whether payday lending directly or indirectly contributes to an increase in bankruptcy.

Bankruptcy is a reasonable measure of welfare.

Payday lending could lead on the margin to bankruptcy especially if the debt trap is significant (Morgan, 2007).

Intuitively, the circle of debt argument might be flawed because the industry can not built itself in the long run seeking to bankrupt its customers (if that would be the case, its customer base market will shrink).

Bankruptcy is associated with real welfare problems for consumers.

In Himmelstein et al. (2005), "during the two years before filing for bankruptcy 40.3% had lost telephone service; 19.4% had gone without food; 53.6% went without needed doctor or dentist visits because of the cost; and 43.0% had failed to fill a prescription, also because of the cost." After filing for bankruptcy, "about one-third of

debtors continued to have problems paying their bills. [...] Because of the bankruptcy on their credit reports, after about 7 months from the filing, they had already been turned down for jobs (3.1% of debtors), mortgages (5.8%), apartment rentals (4.9%) or car loans (9.3%).”

In states with higher bankruptcy exemptions levels, low-income households tend to have less credit, and pay higher interest rates on automobile loans than similar borrowers in states with low exemptions Gropp et al. (1997).

To capture the relationship between payday lending and bankruptcy, two estimation procedures are employed: a difference in difference estimation and a Granger causality test between payday lending stores and personal bankruptcy filings.

Using the variation in legislation across states and across time, and assuming that there are no significant and systematic cross-border effects, two groups of states have been identified:

- the control group: states with no change (where payday lenders were not *present* at all, or were present during all periods), and
- the treatment group: states that have seen a change in the *presence* of payday lender (they entered or exit during the period).

More on the exact specifications in chapter five.

The factors that influence bankruptcy filings have not been unambiguously determined in the literature. In addition, factors that explain the variation across time and

space based on bankruptcy petitioners characteristics and interviews are not consistently significant in studies that use aggregate filings.

Legal environment is important in Fay et al. (2002), Domowitz and Sartain (1999), but not in Domowitz and Eovaldi (1993), Buckley and Brinig (1998).

Social and economic shocks are significant in Himmelstein et al. (2005) (medical), Domowitz and Sartain (1999) (medical), Domowitz and Eovaldi (1993), Agarwal and Liu (2003) (unemployment, medical, divorce), but are not in Fay et al. (2002) (medical, unemployment, divorce).

Credit card or consumption related debt play a role in Domowitz and Sartain (1999), Lockett (1988), and social norms in Gross and Souleles (2002), and Buckley and Brinig (1998).

Himmelstein et al. (2005), based on their own 2001 survey of debtors in bankruptcy courts, find that about half of the individuals surveyed claimed that the main reason for their personal bankruptcy was medical.

In addition, it did not take too much: for those that claimed medical reasons as the main cause of bankruptcy, the out-of-pocket costs since the start of illness were on average close to twelve thousands dollars, and the average out-of-pocket medical costs (excluding insurance premium) were close to thirty seven hundred. Furthermore, 75.7% had insurance at the onset of illness, 60.1% had private coverage, but one third of them lost coverage during their illness. Gaps in coverage were present over the two years previous to the filing for 38.4% of them, and 32.6% of debtors claiming a medical

problem as the main cause for bankruptcy were uninsured at the time of filing for bankruptcy.

The median income in the year prior to bankruptcy filing was \$25,000.

Domowitz and Sartain (1999) based on a sample of two 1980 quarters of bankruptcy filings in five court districts found that medical and credit card debt are the most important contributors to bankruptcy at household level.

Home ownership and exemption levels influence the choice of bankruptcy chapter. Specifically, home ownership, as expected, increases the probability of choosing chapter 13 over 7, medical debt has a negative impact on this choice, being married or employed a positive effect. The less the total debt relative to all claimed exemptions is, the more likely the filer will chose chapter 7, but this effect is marginally insignificant (t-value is 1.8).

One of the potential problems of their analysis is that they normalized the household assets to household income, in spite of the fact that bankruptcy exemptions are not set relative to income (with the exception of unpaid wages in some states). In addition, unused exemption levels can not be used across asset categories.

The net benefits from filing under chapter 7 are given by the unsecured debt discharged in bankruptcy minus the value of non-exempt assets that the households would have to give up, the legal costs, the negative effect on future access and cost of credit, stigma costs, the cost of acquiring information about the bankruptcy process and so on.

Fay et al. (2002) analyze the factors that influence the bankruptcy at household level using Panel Study of Income Dynamics (PSID) data.

Based on variation in exemption levels across states, they find that the financial benefits from bankruptcy positively influence the probability of filing. Also, that discharging of debt is the driving factor, and that giving up assets in the process is insignificant.

Lagged bankruptcy filing in the household's district has a positive and significant effect on the probability of filing (less stigma, and lower information costs).

They also find that both the lagged household's income and the reduction in income between year t-2 and t-1 are significant and negatively related to the probability of filing. But they argue that the mis-measurement of the wealth variable (since wealth data are collected only in five-year increments, they used current income as a proxy for change in income since the last collection time) makes these results unreliable. Instead they look to see if adverse events that happened in the previous year have any effect on filings. They find that having health problems and having spells of unemployment in the previous year (household head or spouse) have positive but insignificant effects on the probability of filing. The only adverse factor that was significant and positive at more than 7.7% level was whether the household head was being divorced in the previous year.

They also control for demographic characteristics: age of the household head, the head's education level, family size, home ownership, whether the household owns a business. These are also assumed to proxy the cost of lost future access to credit as a

result of bankruptcy. In addition, they included state or county level variables: state income growth, state income standard deviation, county unemployment rate.

The authors acknowledge potential measurement errors induced by availability of unsecured debt and non-housing assets only in five-year increments, but find that this measurement error does not significantly affect the results.

However, it is possible that the way the financial benefits were constructed had induced another measurement error in the key variable of their study.

The assets at risk of liquidation are included in many exempt categories (including unpaid wages). Not including all exemptions might induce a downsize bias in the financial benefits variable.

They constructed for each year for each household:

$$\text{FinBen} = \max[D - \max(W - E, 0), 0]$$

where

FinBen = Financial Benefits from filing

D = unsecured debt

W = household wealth net of secured debt

E = bankruptcy exemption, and it's the sum of equity in owner-occupied homes exemption, equity in vehicles exemption, and personal property applicable to financial assets exemption, plus the wild card exemption (which allows the exemption of an unused amount of the homestead exemption - in order not to discriminate against the non-homeowner – and which can be applied to any asset).

This aggregation overstates the size of the assets at risk, since they seem to be based on the difference between total household wealth net of secured debt and the sum of different category exemptions. More accurately, the financial benefits should have been calculated as:

$$\text{FinBen} = \max \{ D - \sum_j [\max (W^j - E^j, 0)], 0 \}$$

where j denotes an exemption category.

This problem is mitigated (but not eliminated since it comes at a cost) if the filer acts opportunistically before the filing by moving assets between different exempt categories.

Moreover, even though the benefits for filers under chapter 13 have to be at least as large as those under chapter 7 (since the filer revealed chapter 13 to be preferred over chapter 7), the financial benefits are not the unsecured debt discharged, because debt has to be paid. The benefits are rather in terms of costs saved from an involuntary liquidation, net of the legal cost and other costs associated with bankruptcy.

The variable FinBen is likely censored in the case of chapter 13 filings.

Another potential problem is that even though the sample size is larger than 55 thousands observations, in their sample there are only 254 bankruptcy filings. In addition, the PSID filing rate is only about half of the national level.

Agarwal and Liu (2003) argue that the small number of bankruptcy filings in Fay et al. (2002) likely is associated with too small variation in adverse macroeconomic variables, and as a result it could explain why they are not able to capture the effects of the macroeconomic variables.

Using quarterly data on personal bankruptcies per capita, from the fourth quarter of 1960 to the first quarter of 1985, Domowitz and Eovaldi (1993), after adjusting for the joint-filing provisions of The Bankruptcy Reform Act of 1978, argue that the main cause of the increase in personal bankruptcy filings was not the change in the bankruptcy law, but rather “business-cycle conditions, expectations phenomena, and demographic trends” (p. 805).

Using random samples of chapter 7 and 13 individual cases filed in five bankruptcy court districts from October 1978 through March 1979 and in April-September 1980, and a different empirical approach, they also find that the change in the law induced by the Bankruptcy Reform Act of 1978 did not have significant effects on filings, once business-cycle factors are considered.

White (1998) argues that bankruptcy filings are much lower than what they could be based solely on the benefits from filing. This could be explained in part by two factors. Asymmetric information between creditors and debtors results in lower collection efforts by creditors (creditors do not try to collect on default loans in all instances), and as a result fewer filings take place. In addition, the availability of the bankruptcy in the future is valuable to the debtors.

Using proprietary 1995-2001 monthly credit card data at individual level from a national issuer, and estimating duration models of delinquency, Agarwal and Liu (2003) find county unemployment rate to be an important factor in explaining delinquency.

They also find state average income statistically significant and negative, the number of individuals with no medical coverage in the state to be positive and significant, divorce rates marginally significant.

Even though they control for state specific legal variables, including homestead, property and garnishment exemption levels, they do not report the significance and sign of the coefficients on these variables.

Based on different specifications that include different lags of unemployment rate and change in unemployment rate, they argue that the level of unemployment rate is more important than the change in unemployment rates. However, this misinterprets their specifications, because they actually estimate the effects of different lags.

For example,
 their (12)
$$\beta_1 * U(t-6) + \beta_2 * \Delta U[(t-3)-(t-6)] = \beta_2 * U(t-3) + (\beta_1 - \beta_2) * U(t-6) =$$

$$= 0.0032 U(t-3) + 0.01461 U(t-6)$$

Luckett (1988) analyzes the bankruptcy legislative changes and presents a summary of the most important literature on bankruptcy before the late 1980s. He argues that consumption related debt is likely a key factor in explaining the rising trend in the personal bankruptcy filings after WWII.

Buckley and Brinig (1998) attribute most of the variation in district filings to changes in social norms. Their data cover chapter 7 and chapter 13 annual filings between 1980 and 1991. The legal variables used (an estimate of the maximum real value of assets exempt from seizure in Chapter 7 by unsecured creditors, and a dummy that captures the bankruptcy court district enforcement of safeguards against debtor

misbehavior under Chapter 13) do not appear to have a significant effect on filings. They suggest that econometric analysis of bankruptcy filings should include social variables predictors.

They also argue that one period lag is the reasonable lag of the independent variables.

Gross and Souleles (2002) identify two explanations for the trends in personal bankruptcy filings and delinquency rates on credit cards: the deterioration in the risk composition of borrowers, and the increase in borrowers' willingness to default as a result of a decrease in the default costs, including social, information, and legal costs. They use a proprietary individual credit card accounts data covering 1995-97 from several different card issuers, which have many variables that control for risk.

They argue that even though risk variables are highly significant in predicting bankruptcy, the changes in the risk composition and other standard economic variables alone are not enough to explain the dynamics of defaults and bankruptcies. The decline in default costs (like social stigma or information costs) seems to be empirically important.

Personal bankruptcy filings graphs are presented in appendix B. All demographic and shock variables included in the bankruptcy analysis are listed in chapter five, and summarized in appendix C.

Data sources and Legislative variables

Population, percent of individuals not covered by private or government health insurance, poverty rates data come from U.S. Census Bureau.

Poverty rates were available only for 1994-2006. In the regressions where poverty rates are used on the right hand side, this is the implicit time span of the analysis.

Income data comes from the Bureau of Economic Analysis (within the U.S. Department of Commerce).

Unemployment and CPI data are from the Bureau of Labor Statistics (within the U.S. Department of Labor).

Divorce data are from the National Center for Health Statistics. No data for 1992 were available; also some states and years are missing (total missing observations is 170 out of 867).

Consumer Credit Outstanding data source is the Fed.

The number of payday lending stores comes from The Community Financial Services Association of America (CFSA), the industry trade association. More details on this variable are provided in chapter five.

Personal bankruptcy filings data are from Administrative Office of the U.S. courts.

There are three relevant groups of legal payday lending restrictions identified in this paper:

- A) Price restrictions
- B) Licensing / entry restrictions

C) Business practice restrictions

Price restrictions are mainly fee limits.

Licensing / entry restrictions are mainly bond and net assets requirements, which are likely to have at least two opposing effects on consumers: act as potential entry barriers into the market, hence potentially less competition, and secondly making reputation more important.

Reputation might play a role if there are informational asymmetries between the consumers and the loan providers.

Suppose, as I mentioned before, the supplier has an information advantage over the consumer (the consumer is not "smart enough" to figure out that the loan on average has to be refinanced, i.e., he accepts a short term interest rate for essentially what is a medium term loan), and uses this asymmetry in information to prey on him. A bond might reduce the problem of supplier adverse selection in the market.¹⁵

In addition, for a given expected rate of return for this business, by directly increasing the costs of operation through licensing like fees, and through immobilizing some capital with lower return (the bond), they contribute to higher consumer prices.

Business practice restrictions lead to increases in the production costs.

Any increase in the costs of providing the services will be shared (depending on the relative elasticities of demand and supply) between the consumers and producers.

There are no compelling reasons to believe that the supply side is not elastic enough in

¹⁵ If that would be the case we would expect the public payday lenders to be relatively more supportive of pro-bond restrictions than non public payday lenders.

the long run. Thus, ultimately the increase in the costs induced by the regulation will fall mainly on the consumers.

The legislation considered is just at state level. It does not include any national or local restrictions on the business considered. However, national level restrictions that affect payday lenders in the same way regardless of the state are captured in the time fixed effects.

Restrictions at the local level, like city zoning laws, shape both the size and location of the stores.

Changes in payday lending regulation at national level affect businesses in different states differently, depending on the state and local rules in place at that time.¹⁶

The relevant explicit or equivalent payday lending legislation was identified based on information from:

- state legislature web sites
- the state agency that regulates this industry web sites
- LexisNexis Academic (main source for annotated statutes)

The information was cross referenced with government agencies reports, National Conference of State Legislatures data, industry reports, newspaper articles at different times from various sources.

¹⁶ During the period analyzed, there is only one significant change at the national level that affected states differently: effective 7/1/2005, FDIC changed its payday loans guidelines, in an attempt to discourage long term debt cycles: “banks should ensure that payday loans are not provided to customers who have had payday loans outstanding from any lender for a total of three months in the previous 12-month period”.

In 2/2006 FDIC advised FDIC insured banks that they could no longer offer payday loans through marketing and servicing agents.

The relevant payday lending provisions were extracted from the current legislation for all U.S. states. This allows me to obtain a ranking of payday lending state restrictions across states.

To capture the dynamics of payday lending state legislature activity, the enacted state laws were tracked back to 1990.

When the text of the laws that modified the provisions that affect payday lending was not available on-line from the state legislature or the state agency that regulates the industry, the legislative changes were inferred in two ways.

a) If annotated statutes for that state were available, the information was obtained by comparing the provisions in the statutes in two adjacent years, before and after a known enacted bill back to the 1991 statutes (the oldest LexisNexis on-line statutes).

b) If annotated statutes were not available, the information was inferred by comparing the statutes provisions that affect payday lending from the last available law, in each year back to the 1991 statutes.

Each legislative variable was constructed to reflect a ranking across states and time of state legislative restrictions on payday lending.

This was accomplished by extracting first, and then tabulating the relevant provisions on each key issue, for all states and for all years. Then the coding rules were subjectively decided so that they create a ranking of payday lending restrictions consistent across states and across time. The exact coding criteria used are described in appendix D.

The legislative variables are constructed such that a larger number means more restrictions.

When a relevant provision in the law became effective during the year, the index value for that variable for that year changes proportionally with time of the year in which the new change had effects.

For example, in 2004, the maximum charges for a three hundred dollars 14-day payday loan in Kansas were about \$23. Based on the rules described in appendix D, the variable “*Max charge restrictions*” was coded in 2004 for Kansas as 4.

In 2005 there was enacted a law that changed the maximum charges for a three hundred dollars 14-day payday loan in Kansas to about \$45. This act was effective 7/1/2005. Using the same algorithm, after this change, “*Max charge restrictions*” was coded as 3.

For 2005, “*Max charge restrictions*“ was coded as

$$3.5 \approx [4 * (12/31/2005 - 7/1/2005) + 3*(7/1/2005 - 12/31/2004)] / 365 = \\ = (4* 182+ 3*183) / 365.$$

In rare instances when the exact effective date was not known, the law was considered effective 7/1.

Georgia, with its S.B. 157 effective 5/1/2004, is the only state that during the period analyzed explicitly and purposely banned payday lending. As a result, for Georgia all legislative restriction variables after the effective date of the bill (for 2004 I weighted for the proportion of the year in which it was effective, as described above), were attributed maximum restriction values.

When the legislative changes or specific provisions for loans similar to payday loans were not identified, the variables were left missing (70 out of 867 observations).

When there was no payday lending specific legislation, even if payday lenders did not operate in the state at that time, the provisions that govern a short-term unsecured loan that have the most favorable for the lender interest restrictions were used.

CHAPTER FIVE

DO PAYDAY STORES LEAD TO HIGHER BANKRUPTCY INCIDENCE?

In this paper I try to see if there is any relationship between payday lending and a measure of consumer welfare, personal bankruptcy filings.

The number of payday stores might be an appropriate measure of the industry size because the loans are of small amount, with very short maturity. Flannery and Samolyk (2005) find that the high charge is justified by the fixed operating costs and loan loss rates, Chessin (2005) finds that the default rate is small (65% of the default rate on credit cards).

For payday loans more volume requires more workers, i.e., there are relatively smaller economies of scale than, for example, a bank. This could also explain why the payday lenders usually increase in size by increasing their number of their locations.

This seems to be consistent with data: as I have mentioned before, Ace Cash Express opened a store in Augusta, GA less than 1.8 miles away from its own store.

In 2003, 85% of the firms listed in Yellow Pages under SIC "6141-01 Loans" and "6099-03 Check cashing services" (where payday lending were typically included) had 1-4 employees, 96% 1-9 employees. These SIC's do not include credit unions, pawnshops, tax return preparation services, savings and loans associations, trust companies, other depository institutions.

The number of payday lending stores was not available for years prior to 2006. Instead, I use as a proxy the number of payday lending stores member of CFSA for each

year and state, which was constructed based on CFSA stores that were in business in March 2006 and the year in which each store was established.

This variable has two potential measurement error sources:

- survivorship bias (it does not include CFSA stores closed); but since the industry experience mainly expansion, this is likely not that much of a problem;
- non-completeness bias (it does not include non-CFSA members).

The second is a problem only if CFSA membership is systematically different across time and across states. I do not have any reasons to believe that that might have happened.

Furthermore, I try to assess the potential bias empirically: the correlation coefficient between CFSA stores in 2006, and the actual number of payday stores (based on my own estimate from regulatory agencies) was 0.9046. If I exclude the states with no payday lending stores, the correlation coefficient becomes 0.8912. This would suggest that using CFSA data might be a reasonable approach.

To capture the relationship between payday lending and bankruptcy, I use two estimation procedures: a difference in difference estimation, and to reinforce the results, a Granger causality test between payday lending stores and personal bankruptcy filings.

Difference in difference estimation

Using the variation in legislation across states and across time, and assuming that there are no significant and systematic cross-border effects, three groups of states have been identified:

- states with no payday stores during the entire time span (7 states),
- states with payday stores during the entire time span (19 states), and
- states which did not have payday stores in some periods, but had payday stores in other periods (24 states).

To estimate the effect of presence of payday lending store a difference in difference estimation is used. For estimation purposes the states are grouped into two categories:

- the control group: states with no change (where payday lenders were not present at all, or were present during all periods), and
- the treatment group: states that have seen a change in the *presence* of payday lender (they enter or exit during the period).

A dummy variable, *Change*, is created to separate the control and treatment group. For the control group *Change* takes value 0 for all years. For the treatment group, change is equal to one. To summarize, for each state and year, *Change* takes the values:

0: payday lenders were not present in all years in that state

0: payday lenders were present in all years in that state

1: payday lenders were not present in that year in that state, but they were present in that state in other years

1: payday lenders present in that year in that state, but they were not present in that state in other years

The model can be summarized as:

$$Y_{i,t} = \alpha + \beta^1 X^1_{it} + \beta^2 X^2_{it} + \gamma_1 D05 + \gamma_2 D06 + \gamma_3 Stores_{it} + \gamma_4 (Stores * Change)_{it} + \gamma_5 Change_{it} + \varepsilon_{it} \quad (1)$$

where

i is a state subscript

t is a time subscript, $t = 1990, \dots, 2006$

$Y_{i,t}$ is personal bankruptcy filings per one thousand persons

X^1_{it} is a vector of demographic characteristics:

- percentage of population age 65 and over
- the natural logarithm of per capita disposable personal real income (2006 thousands \$)
- consumer credit outstanding - disposable personal income ratio (this is available only at nation level)
- the natural logarithm of the percent population below poverty level
- personal bankruptcy filings per one thousand persons in the previous period – a proxy for “stigma costs”

X^2_{it} is a vector of shock variables:

- unemployment rate
- not covered by private or by government health insurance (% of population)

➤ divorce rate

D05 & *D06* are dummy variables for 2005 and 2006 to capture the effect of the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 - most of its provisions were effective 10/17/2005 -, which reduced significantly the debtor protection under chapter 7 liquidation.

Stores is the number of payday lending stores that are members of CFSA (per 1 million persons).

α , γ_1 , γ_2 , γ_3 , γ_4 , γ_5 - coefficients to be estimated.

β^1 , β^2 - vector of coefficients to be estimated.

Notice that the effect on bankruptcy filings of having payday lending stores is given by:

$$(Y_{Change=1}^{Stores>0} - Y_{Change=1}^{Stores=0}) - (Y_{Change=0}^{Stores>0} - Y_{Change=0}^{Stores=0}) = \gamma_4 * Stores$$

The interaction term in equation (1), (*Stores*Change*), captures the DID; it is the variable of interest in the estimation. It will show if the *presence* of payday stores is associated with more, less or about the same bankruptcy filings, for states in which the presence of the payday lending industry changed. For instance, Georgia had payday lending stores which were driven out by legislation¹⁷. The same happened to some states as a result of the FDIC ruling¹⁸.

This model was estimated separately for total personal bankruptcy filings, chapter 7 personal bankruptcy filings, and chapter 13 personal bankruptcy filings. The

¹⁷ See footnote 6 above.

¹⁸ See footnote 16 above.

results are presented in appendices E and F. I have tested for the presence of fixed¹⁹ and random²⁰ effects; based on Hausman tests, FE models seem to be preferred.

Specifications (1) through (3) include state and time effects, with robust standard errors. To avoid the problems created by the assumptions about the data generation process (the assumptions about the error distribution), I also estimate in (4) through (6) bootstrapped standard errors. This accounts for the specific characteristics of the data.

There is autocorrelation²¹ and heteroskedasticity²² present in the data. I correct²³ for both in specifications (7) through (9). Because divorce rate is missing (total missing observations is 170 out of 867), to increase precision, I re-run these models without divorce rate. The results are presented in appendix F. I have also run models that include poverty rates (which showed up insignificant, and because poverty rates are missing for 4 out of 17 years in my data, I did not include here those estimates²⁴). Different other lag structures for the independent variables have been tried, and also other logarithm transformations to allow for nonlinearities; the results (not shown here) were similar. The different bankruptcy exemptions across states are captured into the state and time fixed effects.

The estimates are by and large as expected. Consistent with previous literature, more filings in the past seem to lead to more filings. This can be explained by a

¹⁹ F-test with pooled regression model as the baseline for the comparison.

²⁰ Breusch and Pagan LM test for RE with pooled regression model as the baseline for the comparison.

²¹ Wald test for first order autocorrelation and Arellano-Bond test for higher order autocorrelation.

²² LR test.

²³ Using feasible GLS, panel-specific AR(1) autocorrelation structure, and heteroskedastic error structure.

²⁴ Available on request.

reduction in information costs, in the stigma and guilt associated with bankruptcy. In addition, the more people file for bankruptcy the less severe the reduction in credit access is. I capture these effects by the coefficient on the lag of personal total filings per one thousand persons which is significant and of the expected sign.

The lifecycle patterns of borrowing and savings would suggest that as people age, they would tend to rely more on their savings than on credit to finance consumption. This would suggest that we expect that, everything else the same, the higher the fraction of population 65 and over the lower the bankruptcy rate. I find that the percentage of population 65 or over is indeed inversely related to chapter 13 filings, but positively related to chapter 7 filings. The later result might happen if individuals in retirement without significant asset or savings face consumption shocks (for example, unexpected medical expenditures).

Unemployment rate has the expected sign: an increase in unemployment is associated with more bankruptcy filings.

Health insurance coverage did not show up to be a significant factor in bankruptcy.

Real per capita disposable income has the expected sign, but it is insignificant.

Buckley and Brinig (1998) point out that “divorce rates might be positively correlated with bankruptcy rates in two ways. If the social stigma of promise-breaking is weakened across the board, then divorce and bankruptcy rates might both increase. Second, an increase in the divorce rate might directly affect bankruptcy rates because divorce results in financial distress“ (p. 201-2). In addition, they argue that the causation

could go as well in opposite direction: “some people go bankrupt because they divorce, and some people divorce because they go bankrupt” (p. 202).

Fay et al. (2002) suggest that “divorce lawyers often counsel their clients to file for bankruptcy” (p. 714).

Divorce rates are found to be consistent with the suggestions in the literature: positively and significantly correlated with chapter 7 filings. Lagged divorce rates are found to be positively and significantly related to chapter 13 filings.

The coefficient on "*Change*" – the variable that separates the control and treatment group - captures the effect on both groups. The coefficient on "*Stores*" measures the effect that is not due to the *presence* of payday lenders.

The coefficient of interest from a public policy²⁵ point of view is "*Stores*Change*".

As mentioned above, this is the DID estimator (the effect of the change in *presence* of stores). It measures the effect of the *presence* of payday stores on bankruptcy filings.

Notice that since we use the number of payday lending stores per one million persons and not just "laws allow payday lending" or "payday lending stores present in state" (like in other papers), I also controlling for the intensity of the industry – which was not done before.

The DID coefficient will show if the *presence* of payday stores is associated with more, less or about the same bankruptcy filings.

²⁵ This is the most relevant effect to look at for allowing / banning payday lending.

Controlling for the intensity of the payday lending stores, I find that if anything the presence of payday stores in a state is associated with smaller chapter 7 bankruptcy filings. The effect is economically very small: chapter 7 filings go down by about 1 filing per 1 million persons.

The presence of payday stores does not seem to have any significant effect on chapter 13 and total personal bankruptcy filings.

Granger causality analysis

To reinforce these results I test for Granger causality between bankruptcy and payday lending stores.

Granger (1969) shows that a variable X is causing another variable Y if, given all available information about Y and other relevant variables, we are able to predict Y better using X than without it. If adding X to all other available information does not increase the forecast power for Y , then X does not cause Y .

This implies that if X causes Y , then $\Delta X(t) \Rightarrow \Delta Y(t+j)$, but $\Delta Y(t)$ does not lead to $\Delta X(t+j)$, for a lag j .

Panel data Granger analysis have to take into account the possible state heterogeneity in the relationship.

I start first with a simple bankruptcy filings – payday lending stores Granger causality test (no other variables included) *at state level*, and then I analyze the consistency of the results across states. If payday lending causes bankruptcy, then this relationship should consistently hold also at state level.

This simple Granger test at state level could be extended by controlling for other factors. However, the relatively short period of time of my data (17 periods maximum) does not allow me this.

By restricting the Granger analysis at state level I will avoid any heterogeneity problems, but at the same time not all the available information is used (since payday stores are not present in all the states, and other important variables that could affect the relationship between bankruptcy and payday lending are not controlled for).

I then repeat the Granger analysis with the pooled data, including economic and social variables that proved to be important in the DID analysis, and also controlling for variation in payday lending legislative restrictions across states.

The simple Granger causality test is based on the model:

$$\begin{aligned} \text{Bankruptcy filings}_{i,t} = & \sum_{j=1}^p \alpha_j * \text{Bankruptcy filings}_{i,t-j} + \\ & + \sum_{j=1}^p \beta_j * \text{Stores}_{i,t} + u_{i,t} \end{aligned} \quad (2)$$

$$\begin{aligned} \text{Stores}_{i,t} = & \sum_{j=1}^r \gamma_j * \text{Bankruptcy filings}_{i,t-j} + \\ & + \sum_{j=1}^r \delta_j * \text{Stores}_{i,t} + v_{i,t} \end{aligned} \quad (3)$$

The appropriate number of lags p , and r were determined using the Akaike Information Criterion (AIC).

The optimal number of lags for each state level is not always the same with the one that results from the pooled data (see appendix G). However, for total personal bankruptcy filings, I have done the analysis using both the optimal lag structure determined at the state level and the optimal lag structure determined with all states pooled. The conclusions of the Granger causality test did not change.

Under this specification, if payday stores cause bankruptcy, then β_j in equation (2) are different than zero, and γ_j in equation (3) are zero.

I have run these models by state, and then I have tested for these Granger causality requirements.

The results are summarized in table 1 below:

Table 1.

Simple Granger causality test between personal bankruptcy filings and the number of payday stores

	(1)	(2)	(3)	(4)
	total	total	ch. 7	ch. 13
<i>Stores "causes" Bankruptcy, and Bankruptcy does not "cause" Stores</i>	6	5	6	8
<i>Bankruptcy "causes" Stores, and Stores does not "cause" Bankruptcy</i>	3	7	2	4
<i>Bankruptcy "causes" Stores, and Stores "cause" Bankruptcy</i>	2	1	3	0
<i>Bankruptcy does not "cause" Stores, and Stores do not "cause" Bankruptcy</i>	21	19	21	20

The numbers are number of states

In (2) I used the number of lags that results from AIC at state level (instead from AIC with all data pooled) - see appendix G

The number 6 in column (1) means that in 6 states I failed to reject the hypothesis that γ_j were jointly significantly different than zero, and I rejected the hypothesis that β_j were jointly significantly different than zero.

This table suggests that payday lending stores do not cause bankruptcy.

I then improve this simple Granger testing by pooling the data and using other variables that might be important.

The Granger analysis with panel data is based on the model:

$$\begin{aligned} \text{Bankruptcy filings}_{i,t} = & \alpha_i + \sum_{j=1}^p \beta_j * \text{Bankruptcy filings}_{i,t-j} + \\ & + \sum_{j=1}^p \gamma_j * \text{Stores}_{i,t} + \sum_{j=1}^p \delta_j * Z_{i,t} + u_{i,t} \end{aligned} \quad (4)$$

$$\begin{aligned} \text{Stores}_{i,t} = & \eta_i + \sum_{j=1}^r \theta_j * \text{Bankruptcy filings}_{i,t-j} + \\ & + \sum_{j=1}^r \lambda_j * \text{Stores}_{i,t} + \sum_{j=1}^r \phi_j * W_{i,t} + v_{i,t} \end{aligned} \quad (5)$$

where $Z_{i,t}$ is a vector of social and economic variables that might influence bankruptcy filings, and $W_{i,t}$ is a vector of variables that might influence payday lending stores, including legislative variables.

The causality has the same interpretation: if payday stores cause bankruptcy, then γ_j in equation (4) are jointly different than zero, and θ_j in equation (5) are jointly not significantly different than zero.

Appendices H, I and J summarize the results of the joint tests of significance of payday lending stores in the bankruptcy equation, and of bankruptcy filings in the payday stores equation (total, chapter 7, and chapter 13 bankruptcy filings).

Again, there is no clear picture of causality of payday lending stores on bankruptcy.

It seems that if anything, there is a weak support of causality in the opposite direction. This could be explained by the reduced access to alternative credit induced by the bankruptcy, i.e. payday stores respond to demand factors.

Bankruptcy might lead to more payday stores through this channel: bankruptcy significantly reduces the credit scores in the short run, which makes access to credit both more expensive and more difficult. This would increase the demand for non-standard credit, including for payday lending.

I have run different estimation procedures:

- OLS with state and time fixed effects, and robust standard errors
- OLS with state and time fixed effects, and bootstrapped standard errors, and
- FGLS with panel-specific AR1 autocorrelation structure and heteroskedastic error structure.

I present in appendix I the FGLS estimation (corrected for autocorrelation and heteroskedasticity) with the optimal lag structure based on Akaike Information Criterion.

However, using 4 lags structure induces strong multicollinearity (with very high VIFs for the payday lending stores and bankruptcy filings variables).

I have also estimated the Granger causality with a more parsimonious one lag structure. The conclusion did not change. The results of the FGLS estimation are presented in appendix J.

Multicollinearity increases standard errors, reduces the t-values, and makes the coefficients insignificant even if they are not. However, even if collinearity would have induced some of the rejections of the joint hypothesis of payday stores insignificance in

the bankruptcy equation, reducing collinearity would not make joint significance of bankruptcy in the store equation less significant (if anything, it might make it more significant). This would suggest that even if multicollinearity is a problem in the estimation, it does not drive the results.

The Granger causality seems to confirm the DID approach: no empirical support was found to support the causality between payday lending and bankruptcy filings.

CHAPTER SIX

CONCLUSIONS

In this paper, I investigate the relationship between payday lending and personal bankruptcy filings – a measure of consumer's welfare.

Using state level 1990-2006 data, I control for the intensity of the presence of the payday lending industry, and for the explicit or equivalent payday lending legislation.

Based on variation across states and across time in restriction provisions, I construct six variables that capture price restrictions, licensing related or entry restrictions, and other business practice restrictions. These variables rank the provisions across states and time. I find that the main restriction that influences the presence of payday lenders is the price restrictions.

I test the hypothesized causality between payday lending and personal bankruptcy filings using two different empirical approaches: a difference in difference estimation, and a Granger causality analysis.

I find that there is no empirical evidence that payday lending leads to more bankruptcy filings.

This would suggest that there is no statistical evidence to support the circle of debt argument which is often used in passing legislation against payday lending.

These results do not imply that there might not be individuals who make mistakes and borrow beyond their means, paying significantly large charges relative to the

principal, but rather that they are likely to be in the tail of the distribution of payday clients, not a symptom of the industry.

The likely outcome of the state restrictions placed on payday lenders is an increase in the costs of doing business which will lead to higher prices than otherwise, leaving the consumers worse off. In addition, especially if the restrictions are as extreme as in Georgia where payday lending is explicitly banned, they are likely to reduce the access to credit.

APPENDICES

Appendix A

Payday lending stores members of The Community Financial Services Association of America

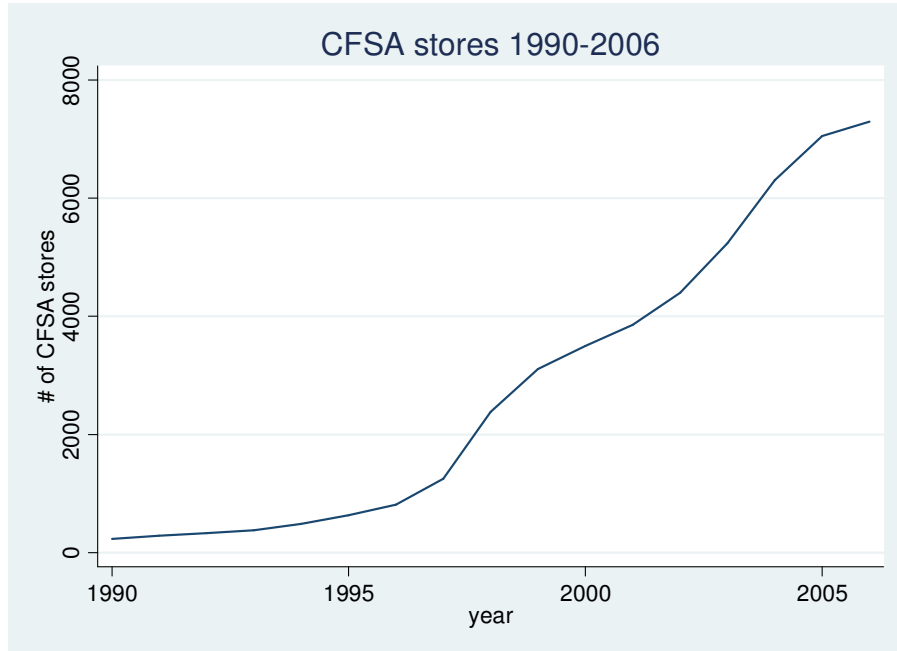


Figure A-1: The number of CFSA stores (level)

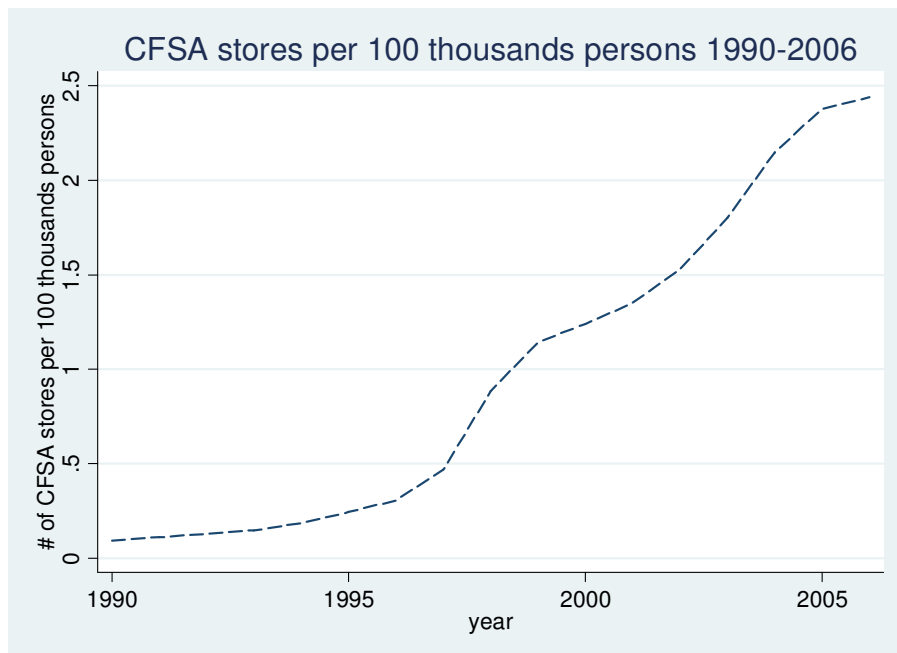


Figure A-2: The number of CFSA stores (per a hundred thousands persons)

Appendix B

Personal bankruptcy filings

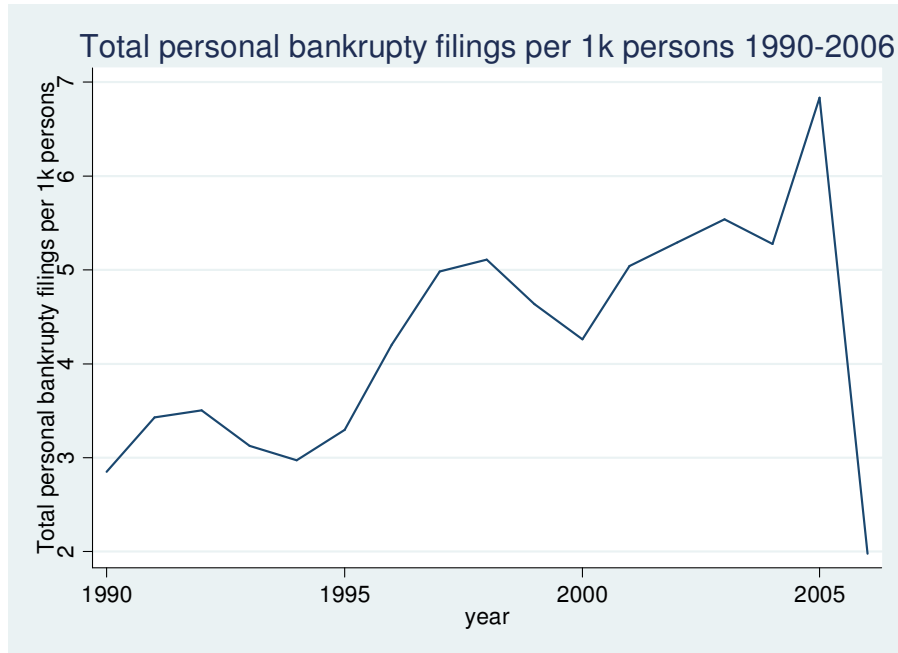


Figure B-1: Total personal bankruptcy filings per 1k persons 1990-2006

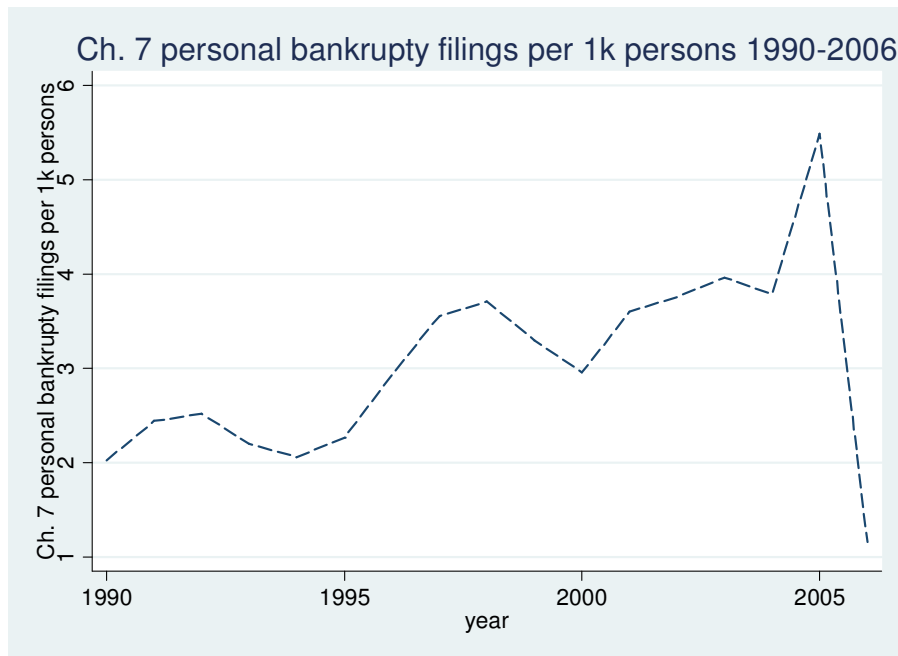


Figure B-2: Chapter 7 personal bankruptcy filings per 1k persons 1990-2006

Appendix B (Continued)

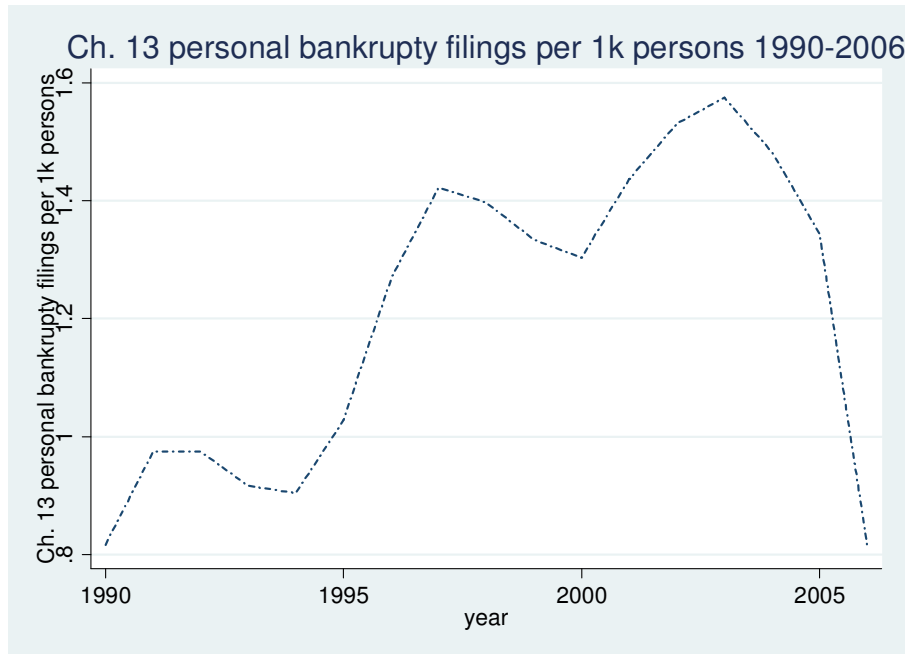


Figure B-2: Chapter 13 personal bankruptcy filings per 1k persons 1990-2006

Appendix C

Summary of variables

Variable		Mean	Std. Dev.	Min	Max	Observations
Population 65 or over (%)	overall	0.127	0.019	0.041	0.185	N = 797
	between		0.019	0.054	0.176	n = 50
	within		0.003	0.109	0.142	T-bar = 15.94
Unemployment rate	overall	5.118	1.351	2.260	11.290	N = 797
	between		0.940	3.131	7.175	n = 50
	within		0.999	2.538	9.233	T-bar = 15.94
Not covered by private or government health insurance (% of population)	overall	0.138	0.038	0.012	0.275	N = 797
	between		0.034	0.087	0.226	n = 50
	within		0.018	0.011	0.210	T-bar = 15.94
Percent of population below poverty level	overall	12.023	3.394	5.200	25.700	N = 624
	between		3.043	6.600	19.715	n = 50
	within		1.556	6.430	19.215	T-bar = 12.48
Divorce rate (per 1,000 total population residing in area) (%)	overall	4.342	1.231	1.900	11.400	N = 654
	between		1.155	2.419	8.295	n = 49
	within		0.513	2.347	7.447	T-bar = 13.35
Consumer credit outstanding - Disposable personal income ratio	overall	0.216	0.028	0.169	0.250	N = 797
	between		0.007	0.214	0.246	n = 50
	within		0.027	0.161	0.252	T-bar = 15.94
Natural logarithm of per capita disposable personal income (2006 thousands \$)	overall	3.320	0.156	2.912	3.898	N = 797
	between		0.145	3.069	3.787	n = 50
	within		0.075	3.162	3.559	T-bar = 15.94
# of CFSA stores (per 1 mil. persons)	overall	9.060	13.044	0.000	68.451	N = 797
	between		7.715	0.000	32.820	n = 50
	within		10.853	-15.180	59.891	T-bar = 15.94
Total non-business filings per 1k persons personal bankruptcy filings	overall	4.235	2.068	0.726	12.480	N = 797
	between		1.436	1.861	8.666	n = 50
	within		1.477	-0.325	10.843	T-bar = 15.94
Personal ch. 7 filings per 1k persons personal bankruptcy filings	overall	3.127	1.506	0.375	10.446	N = 797
	between		0.869	1.258	5.092	n = 50
	within		1.235	-0.522	9.013	T-bar = 15.94
Personal ch. 13 filings per 1k persons personal bankruptcy filings	overall	1.104	1.101	0.024	5.848	N = 797
	between		1.029	0.102	4.974	n = 50
	within		0.368	-0.608	2.419	T-bar = 15.94
Max charge restrictions	overall	2.779	2.020	0.000	5.000	N = 797
	between		1.781	0.000	5.000	n = 50
	within		0.900	-0.310	6.265	T-bar = 15.94
Bond requirements	overall	0.806	1.200	0.000	5.000	N = 797
	between		0.980	0.000	3.459	n = 50
	within		0.725	-0.890	4.972	T-bar = 15.94
Asset requirements	overall	1.131	1.264	0.000	5.000	N = 797
	between		1.126	0.000	4.750	n = 50
	within		0.566	-2.163	5.297	T-bar = 15.94

Appendix C (Continued)

Variable		Mean	Std. Dev.	Min	Max	Observations
License related fees	overall	1.573	0.909	0.000	5.000	N = 797
	between		0.804	0.186	4.000	n = 50
	within		0.414	-0.109	4.288	T-bar = 15.94
Rollovers restrictions	overall	1.805	2.173	0.000	5.000	N = 797
	between		1.657	0.000	5.000	n = 50
	within		1.445	-1.695	6.491	T-bar = 15.94
Loan / check maximum amount	overall	1.402	1.023	0.000	5.000	N = 797
	between		0.786	0.000	3.250	n = 50
	within		0.686	-0.179	4.736	T-bar = 15.94

Appendix D

Coding Criteria for the State Payday Lending Legislation Restriction Variables

Unless noted otherwise, each variable is coded between 0 (no restriction) and 5 (maximum restrictions); a smaller number means less restrictions.

- the scale for each variable was constructed based on the actual distribution of outcomes, time and across state consistency being the decisive factor
 - if there was some change during the year, but not enough to move it another index category, I adjusted the previous figure by a small number (+/-0.1, depending on whether the change is less or more restrictive)
 - if there is a change during the year, the value for that year was a time weighted average of the new value and the previous year value
- The procedure is described in more details in chapter IV.

A) Price restrictions - mainly fees limits

- The cost of a first \$300 (principal) 14-day loan if paid at maturity is
 - no cap [= 0]
 - >45, but some cap [= 1.5]
 - (30-45) [= 2.5]
 - (15-30) [= 3.5]
 - <15 [= 5]
- no explicit limit, but proposed max rates have to be approved [= 2.5]
- after maturity, no more than usury charges or other limit [+ 0.5]
- the calculated cost includes a non-recurring fee, like a set up initial customer account fee, that, if ignored, would move the cost of a first \$300 loan to another category [+ 0.5]; [+ 1.0] if the non-recurring fee dominates
- graduated rate /decreasing fee schedule in amount lent, if considering the whole range of the fee schedule, it will move it in another category [\pm 0.5]

B) Licensing / entry restriction - mainly bond and net assets restrictions

Bond requirements:

- no bond [= 0.0]
- bond instead of asset requirement for the same amount [= 0.1]
- \$1k - \$5k [= 0.5]
- \$10k - \$20k [= 1.5]
- \$25k [= 2.5]
- not specified [= 2.5]
- \$50k [= 3.5]
- \$100k+ [= 4.5]
- reduction in bond requirements for additional locations:
 - additional bond 10% of the first license [- 0.50]
 - additional bond 20% of the first license [- 0.25]
 - additional bond 25% of the first license [- 0.20]
- capped for multiple licenses:
 - \$ 25k [- 0.50]
 - \$ 50k [- 0.25]
 - \$150k [- 0.08]
 - \$200k [- 0.06]
 - \$300k [- 0.04]
 - \$500k [- 0.03]
- the bond limit amount for multiple licenses has a maximum number of allowed locations / licenses:
 - up to 10 places of business under each multiple office license [+ 0.25]

- accepts letter of credit or interest-bearing assets instead of the bond [- 0.25]
- must continue after the lender ceases operation, the renewal application is denied or the license is revoked:
 - for 2 years [+ 0.2]
 - for 3 years [+ 0.3]
 - for 5 years [+ 0.5]
- the bond can be increased based on the volume of the applicant's business, on the financial condition of the licensee or the applicant for a license, or for violations. [+ 0.1]
- continuous in nature [+ 0.1]

Asset requirements:

			liquid / readily available assets	liquid assets can be letter of credit	liquid assets have to be cash	accepts line of credit instead of the assets	requirements are for assets or capital	some reduction for smaller city / county size
(a)	no restrictions	[= 0.00]	-	-	-	-	-	-
(b)	≤\$5k	[= 0.00]	[+ 0.05]	[- 0.02]	[+ 0.02]	[- 0.03]	[- 0.05]	[- 0.05]
(c)	(\$5k - \$10k]	[= 0.00]	[+ 0.05]	[- 0.02]	[+ 0.05]	[- 0.05]	[- 0.10]	[- 0.05]
(d)	(\$10k - \$20k]	[= 0.50]	[+ 0.10]	[- 0.05]	[+ 0.10]	[- 0.10]	[- 0.20]	[- 0.10]
(e)	\$25k	[= 1.00]	[+ 0.25]	[- 0.15]	[+ 0.15]	[- 0.25]	[- 0.20]	[- 0.20]
(f)	\$30k	[= 1.25]	[+ 0.25]	[- 0.15]	[+ 0.15]	[- 0.25]	[- 0.20]	[- 0.20]
(g)	\$50k	[= 2.00]	[+ 0.50]	[- 0.25]	[+ 0.25]	[- 0.50]	[- 0.25]	[- 0.25]
(h)	\$75k	[= 2.50]	[+ 0.50]	[- 0.25]	[+ 0.25]	[- 0.50]	[- 0.25]	[- 0.25]
(i)	\$100k+	[= 3.00]	[+ 0.50]	[- 0.25]	[+ 0.30]	[- 0.50]	[- 0.25]	[- 0.25]

		additional location requirements % of the first location				continuous / maintained through the licensing year rather than limited to an annual basis	continuity requirement refer to liquid assets	continuity requirement does not refer to liquid nor to net assets
		17%	20%	25%	33%			
(a)	no restrictions	-	-	-	-	-	-	-
(b)	≤\$5k	[- 0.05]	[- 0.04]	[- 0.03]	[- 0.02]	[+ 0.05]	[+ 0.05]	[- 0.02]
(c)	(\$5k - \$10k]	[- 0.05]	[- 0.04]	[- 0.03]	[- 0.02]	[+ 0.10]	[+ 0.05]	[- 0.02]
(d)	(\$10k - \$20k]	[- 0.10]	[- 0.09]	[- 0.07]	[- 0.05]	[+ 0.15]	[+ 0.10]	[- 0.04]
(e)	\$25k	[- 0.25]	[- 0.23]	[- 0.18]	[- 0.13]	[+ 0.25]	[+ 0.15]	[- 0.06]
(f)	\$30k	[- 0.25]	[- 0.23]	[- 0.18]	[- 0.13]	[+ 0.30]	[+ 0.15]	[- 0.06]
(g)	\$50k	[- 0.50]	[- 0.45]	[- 0.35]	[- 0.25]	[+ 0.50]	[+ 0.25]	[- 0.10]
(h)	\$75k	[- 0.50]	[- 0.45]	[- 0.35]	[- 0.25]	[+ 0.75]	[+ 0.25]	[- 0.10]
(i)	\$100k+	[- 0.50]	[- 0.45]	[- 0.35]	[- 0.25]	[+ 1.00]	[+ 0.25]	[- 0.10]

- the asset requirement can be increased by the regulatory agency [+ 0.1]

- capped for multiple licenses:
 - \$ 25k [- 0.50]
 - \$ 50k [- 0.25]
 - \$ 75k [- 0.17]
 - \$ 220k [- 0.06]
 - \$250k [- 0.05]

Initial / recurring **license related fees:**

Licensing costs = application + license fees

Let A denote the annual licensing costs, after the first year.

To adjust for the first year licensing costs, which could be different than the renewal annual licensing costs, let the “full” first two-year licensing costs (application + license fees) be denoted by F2y.

If the licensing fees are not explicitly stated, but dollar limits are provided, and the rule for determining the fee is “to cover the cost of administering the law”, the upper limit was used.

- no license fees [= 0]
- annual licensing costs > 0 [= 0.5 + 0.002* A]
- background check / fingerprint cards amount fee required, but not specified [+ 0.1]
- first year licensing costs are different than those in subsequent years: [+ $\frac{F2Y - 2 * A}{5} * 0.002$]

- licensing fees do not include the costs related to conducting the investigation into the application (for example, the costs of the publication of the notice of the filing of the application, the costs of the hearing), which are supposed to be billed separately upon the completion of the investigation, or pro rata share of all costs and expenses reasonably incurred in the administration of the law[+ 0.25]

- licensing fees not explicitly stated, and there are no limits other than “to cover the cost of administering the law” [= 2.5]

- renewal at more than 1 year intervals (2 years) [- 0.1]
- fees for change of address, name, in control, or for the required registration of loan officers [+ 0.1]
- annual volume fee [+ 0.25]
- licensing fees reductions for multiple licenses:

	annual licensing costs for the first location	capped at up to 10 additional locations	additional locations licensing fees requirements % of the first location					
			0%	2%	10%	20-25%	40-50%	>50%
(a)	≤\$100	[- 0.01]	[- 0.05]	[- 0.04]	[- 0.03]	[- 0.02]	[- 0.01]	[- 0.01]
(b)	(\$100 - \$250]	[- 0.01]	[- 0.15]	[- 0.14]	[- 0.12]	[- 0.06]	[- 0.02]	[- 0.01]
(c)	(\$250 - \$500]	[- 0.05]	[- 0.50]	[- 0.49]	[- 0.43]	[- 0.31]	[- 0.10]	[- 0.05]
(d)	(\$500 - \$1000]	[- 0.07]	[- 0.75]	[- 0.72]	[- 0.60]	[- 0.38]	[- 0.13]	[- 0.07]
(e)	(\$1000 - \$1500]	[- 0.15]	[- 1.25]	[- 1.20]	[- 1.00]	[- 0.63]	[- 0.30]	[- 0.15]
(f)	> \$1500	[- 0.25]	[- 1.50]	[- 1.44]	[- 1.20]	[- 0.75]	[- 0.35]	[- 0.25]

- business examination costs (not included in the application or renewal fees) [+ 0.25]

C) Business practice restrictions - lead to increases in the production costs

Loan limitations: **number of rollovers:**

- 0 [= 5]
- 1 [= 4]
- [2-3 times] [= 3]
- (3-8) [= 2]
- >8 (specified) [= 1]
- not specified [= 0]

- allows renewals with *no stated* limit but only with charge restrictions
 - no extra charges allowed [= 4.5]
 - some extra charges allowed, but the restrictions dominate the charges that can be collected [= 4.3]
- allows renewals beyond the *stated* limit if a distinct advantage to the borrower (for example, extension without any other charges)
 - only one extra renewal with no extra charges [- 0.25]
 - unlimited renewals with no extra charges [- 0.50]
- time restriction
 - no rollover limits specified, but there are time charge restrictions
 - interest can not be included into the principal [= 1]
 - maxim 12 weeks after the contract date [= 2]
 - can renew beyond the *stated* rollover limit if maturity is increased to at least 28 days [- 0.50]
 - allows renewals beyond the *stated* limit within a given time, if the debtor has undergone recent consumer credit counseling [- 0.50]
- dollar limits on renewals (have to renew less than the original principal)
 - no rollover limits specified, but the principal has to be reduced 25% for a renewal [= 2]
 - the rollover limit is *stated*, but the principal has to be decreased
 - by minimum 5% [+ 0.25]
 - by minimum 10% [+ 0.50]
 - allows renewals beyond the *stated* limit if the principal is decreased by at least 10% [- 0.50]
 - allows renewals beyond the *stated* limit if the principal is increased by at least 10% [- 0.25]
- not allowed for a member of the military service [+ 0.25]
- state wide common database [+ 0.25]

Loan limitations: **loan / check** maximum **amount**

- ≤\$100 [= 5.0]
- (\$100-\$300) [= 3.0]
- (\$300-\$500) [= 2.5]
- (\$500-\$1,500) [= 2.0]
- >\$1,500, but limit [= 1.0]
- no limit [= 0.0]
- no explicit limit is stated other than a reference to the borrower's income [= 2.5]
- if the stated limit amount includes the fees, and if considering the charges it would move it into a different category [+ 0.25]
- amount is linked to the borrower's income or ability to repay [+ 0.50]
- have to check into a state wide common database [+ 0.25]

Appendix E

DID estimation: bankruptcy filings vs. payday lending stores

Dependent variables: personal filings per one thousands persons

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	total	ch. 7	ch. 13	total	ch. 7	ch. 13	total	ch. 7	ch. 13
Lag of personal total filings per 1k persons	0.560 (7.51)**	0.404 (6.16)**	0.155 (7.37)**	0.560 (10.27)**	0.404 (7.09)**	0.155 (5.93)**	0.954 (60.44)**	0.320 (10.23)**	0.247 (14.86)**
Population 65 or over (%)	17.780 (1.80)	19.033 (2.15)*	-1.263 (0.32)	17.780 (1.46)	19.033 (1.29)	-1.263 (0.15)	0.969 (0.86)	7.119 (2.56)*	-1.400 (0.75)
Unemployment rate	0.167 (2.29)*	0.052 (0.93)	0.114 (3.24)**	0.167 (1.82)	0.052 (0.74)	0.114 (2.38)*	0.168 (4.60)**	0.063 (1.75)	0.064 (3.99)**
Lag of unemployment rate	0.089 (1.09)	0.104 (1.48)	-0.015 (0.44)	0.089 (1.14)	0.104 (1.75)	-0.015 (0.36)	-0.091 (2.42)*	0.069 (1.89)	-0.005 (0.30)
Not covered by health insurance (% of population)	-0.550 (0.25)	1.051 (0.54)	-1.599 (2.56)*	-0.550 (0.19)	1.051 (0.40)	-1.599 (2.57)*	-1.512 (1.93)	-1.360 (1.85)	-0.230 (0.79)
Lag of not covered by health insurance (% of population)	-0.624 (0.34)	-0.492 (0.30)	-0.131 (0.18)	-0.624 (0.35)	-0.492 (0.25)	-0.131 (0.15)	-0.569 (0.70)	-0.250 (0.33)	0.412 (1.06)
Consumer credit outstanding - Disposable personal income ratio				56.885 (.)	47.740 (.)	9.188 (.)			
Lag of Consumer credit outstanding - Disposable personal income ratio				-43.818 (1.58)	-38.672 (1.54)	-4.970 (0.34)			
Divorce rate (per 1,000 total population residing in area) (%)	0.107 (1.31)	0.059 (0.84)	0.049 (1.64)	0.107 (1.44)	0.059 (0.79)	0.049 (1.45)	0.037 (0.95)	0.090 (2.48)*	0.001 (0.04)
Lag of divorce rate (per 1,000 total population residing in area) (%)	-0.017 (0.20)	-0.119 (1.58)	0.102 (3.59)**	-0.017 (0.20)	-0.119 (1.55)	0.102 (3.55)**	0.018 (0.47)	0.022 (0.60)	0.045 (2.38)*
Ln of per capita real disposable personal income	-1.222 (0.79)	-0.401 (0.29)	-0.825 (1.36)	-1.222 (0.68)	-0.401 (0.23)	-0.825 (1.29)	-0.866 (1.17)	-0.154 (0.29)	-0.211 (0.66)

Appendix E (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	total	ch. 7	ch. 13	total	ch. 7	ch. 13	total	ch. 7	ch. 13
Lag of ln of per capita real disposable personal income	0.031 (0.02)	-1.036 (0.79)	1.048 (1.73)	0.031 (0.02)	-1.036 (0.61)	1.048 (1.56)	0.382 (0.52)	-0.604 (1.14)	-0.217 (0.68)
Stores	-0.005 (0.54)	-0.002 (0.22)	-0.003 (1.39)	-0.005 (0.50)	-0.002 (0.20)	-0.003 (0.80)	-0.005 (2.40)*	0.002 (0.74)	0.003 (1.72)
<i>Stores*Change</i>	0.003 (0.31)	-0.001 (0.10)	0.003 (1.23)	0.003 (0.28)	-0.001 (0.09)	0.003 (0.71)	0.002 (0.91)	-0.009 (2.10)*	0.003 (1.46)
<i>Change</i>	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	-0.062 (1.42)	-0.075 (0.77)	0.072 (1.05)
2005 dummy	2.949 (6.19)**	2.689 (6.19)**	0.274 (1.76)	1.703 (10.35)**	1.754 (11.95)**	-0.050 (0.81)	1.699 (13.32)**	2.878 (15.06)**	-0.129 (1.25)
2006 dummy	-2.882 (5.46)**	-2.469 (5.21)**	-0.397 (2.19)*	-3.836 (18.28)**	-3.157 (18.56)**	-0.676 (6.13)**	-4.445 (30.77)**	-1.990 (8.77)**	-0.935 (7.62)**
Constant	1.477 (0.31)	2.798 (0.68)	-1.253 (0.71)	-0.548 (0.08)	1.457 (0.26)	-1.977 (0.59)	1.469 (2.38)*	2.050 (1.52)	0.903 (0.89)
Observations	561	561	561	561	561	561	560	560	560
Number of states	48	48	48	48	48	48	47	47	47
R-squared	0.87	0.86	0.53	0.87	0.86	0.53			
State fixed effects	Y	Y	Y	Y	Y	Y	N	N	N
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y

* significant at 5%; ** significant at 1%

(1) - (3) Robust t statistics in parentheses

(4) - (6) bootstrapped SE - z statistics in parentheses

(7) - (9) FGLS to correct for autocorrelation & heteroskedasticity - z statistics in parentheses

Appendix F

DID estimation: bankruptcy filings vs. payday lending stores (divorce rate omitted)

Dependent variables: personal filings per one thousands persons									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	total	ch. 7	ch. 13	total	ch. 7	ch. 13	total	ch. 7	ch. 13
Lag of personal total filings per 1k persons	0.628 (9.45)**	0.441 (7.75)**	0.186 (9.60)**	0.628 (13.09)**	0.441 (9.14)**	0.186 (6.39)**	0.953 (80.31)**	0.383 (17.12)**	0.203 (15.08)**
Population 65 or over (%)	5.171 (0.72)	8.691 (1.40)	-3.596 (1.13)	5.171 (0.61)	8.691 (0.83)	-3.596 (0.51)	0.351 (0.38)	6.888 (4.41)**	-2.663 (1.76)
Unemployment rate	0.148 (3.47)**	0.068 (1.68)	0.078 (4.22)**	0.148 (2.29)*	0.068 (0.96)	0.078 (2.03)*	0.065 (4.72)**	0.070 (3.63)**	0.038 (3.51)**
Not covered by health insurance (% of population)	-0.442 (0.29)	1.217 (0.93)	-1.640 (2.92)**	-0.442 (0.22)	1.217 (0.70)	-1.640 (2.28)*	-1.026 (2.55)*	0.008 (0.02)	-0.294 (1.07)
Consumer credit outstanding – Disposable personal income ratio	0.000 (.)	0.000 (.)	0.000 (.)	9.689 (.)	10.236 (.)	-0.394 (.)			
Ln of per capita real disposable personal income	-0.529 (0.54)	-0.900 (1.03)	0.353 (0.80)	-0.529 (0.43)	-0.900 (0.82)	0.353 (0.40)	-0.156 (1.28)	-0.126 (0.52)	-0.304 (1.52)
<i>Stores</i>	-0.001 (0.13)	0.002 (0.28)	-0.003 (1.55)	-0.001 (0.18)	0.002 (0.33)	-0.003 (0.88)	-0.002 (0.87)	0.008 (2.60)**	0.003 (1.96)
<i>Stores*Change</i>	0.002 (0.32)	-0.003 (0.47)	0.005 (2.49)*	0.002 (0.50)	-0.003 (0.55)	0.005 (1.45)	0.002 (0.86)	-0.005 (1.63)	0.003 (1.70)
<i>Change</i>	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	-0.003 (0.07)	0.012 (0.20)	0.003 (0.06)
2005 dummy	2.447 (7.09)**	2.507 (7.97)**	-0.050 (0.37)	1.800 (15.49)**	1.823 (18.71)**	-0.023 (0.71)	1.416 (15.72)**	2.406 (19.33)**	-0.128 (1.60)
2006 dummy	-3.494 (8.39)**	-2.675 (7.24)**	-0.807 (4.43)**	-4.098 (20.21)**	-3.313 (18.34)**	-0.782 (7.70)**	-4.722 (46.08)**	-2.561 (16.99)**	-0.880 (9.24)**
Constant	1.658 (0.51)	2.328 (0.80)	-0.601 (0.39)	-0.094 (0.02)	0.478 (0.12)	-0.530 (0.18)	0.680 (1.44)	0.354 (0.39)	1.158 (1.58)

Appendix F (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	total	ch. 7	ch. 13	total	ch. 7	ch. 13	total	ch. 7	ch. 13
Observations	745	745	745	745	745	745	745	745	745
Number of states	50	50	50	50	50	50	50	50	50
R-squared	0.88	0.86	0.57	0.88	0.86	0.57			
State fixed effects	Y	Y	Y	Y	Y	Y	N	N	N
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y

* significant at 5%; ** significant at 1%

(1) - (3) Robust t statistics in parentheses

(4) - (6) bootstrapped SE – z statistics in parentheses

(7) - (9) FGLS to correct for autocorrelation & heteroskedasticity - z statistics in parentheses

Appendix G

Akaike Information Criterion (AIC) optimal lag structure

(1)	(2)	(3)	(4)	(5)	(6)						
# of years with payday stores	st	total total filings per 1k persons	personal ch. 7 filings per 1k persons	personal ch. 13 filings per 1k persons	Stores (per 1 mil. persons)	(1)	(2)	(3)	(4)	(5)	(6)
13	AL	4	4	2	4	10	NE	0	0	3	3
2	AK	3	2	1		8	NV	0	0	4	1
17	AZ	0	0	2	2	7	NH	0	0	1	1
8	AR	2	2	1	1	0	NJ	0	0	2	
10	CA	4	4	4	2	17	NM	0	3	2	4
6	CO					0	NY	0	0	2	
0	CT	4	0	2		13	NC	4	4	2	1
8	DE	1	0	2	1	6	ND	2	2	1	4
8	DC	4	4	4	3	17	OH	4	4	4	3
12	FL	1	1	3	3	17	OK	4	4	2	1
14	GA	4	4	2	1	9	OR	4	4	1	3
10	HI	4	1	0	3	17	PA	4	4	2	1
9	ID	4	4	1	4	6	RI	0	0	1	
12	IL	4	4	2	1	13	SC	2	4	2	1
14	IN	4	4	1	1	5	SD	2	2	1	4
10	IA	0	0	0	1	17	TN	0	4	2	1
17	KS	4	4	1	1	17	UT	1	3	2	2
16	KY	4	4	4	1	0	VT	4	4	2	
17	LA	4	2	2	1	17	VA	0	0	2	2
3	ME	4	4	3		17	WA	0	0	2	1
17	MD	1	0	2	1	0	WV	4	4	1	4
0	MA	0	0	2		17	WI	4	4	2	1
9	MI	4	4	4	1	9	WY	0	2	2	2
0	MN	4	4	3			all states	4	4	2	4
11	MS	1	3	2	1		all states*	4	4	2	4
17	MO	4	4	2	1		* with other variables included				
8	MT	4	2	2	4						

Appendix H

Summary of Granger causality between personal *total* bankruptcy filings and payday lending stores

Specification	# of lags	Time FE	Other variables included in the bankruptcy eq.	Other variables included in the bankruptcy eq.	Ho: coef. on Stores are jointly = 0	Ho: coef. on bankruptcy are jointly = 0	Conclusion
OLS + FE + robust st. errors	4	y	note 1	note 2	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	4	y	note 1	note 2	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	4	y	note 1	note 2	reject	reject	b ≠ s
OLS + FE + robust st. errors	4	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	4	n	note 1	note 2	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	4	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + robust st. errors	1	y	note 1	note 2	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	1	y	note 1	note 2	fail to reject	reject	b => s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	y	note 1	note 2	fail to reject	reject	b => s
OLS + FE + robust st. errors	1	y	note 3	note 4	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	1	y	note 3	note 4	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	y	note 3	note 4	fail to reject	reject	b => s
OLS + FE + robust st. errors	1	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	1	n	note 1	note 2	fail to reject	reject	b => s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + robust st. errors	1	n	note 3	note 4	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	1	n	note 3	note 4	fail to reject	reject	b => s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	n	note 3	note 4	fail to reject	reject	b => s

Appendix H (Continued)

note 1: unemployment rate, population 65 or over (%), not covered by health insurance (% of population), natural logarithm of per capita disposable personal income (2006 thousands \$) (denoted in this table as income)

note 2: income + legislative variables (max charge restrictions, bond requirements, asset requirements, license related fees, rollovers restrictions, loan / check maximum amount)

note 3: unemployment rate

note 4: the legislative variables in note 2

b => s bankruptcy "causes" payday stores ;

b <=> s bi-directional causality ;

b ≠ s no causal relationship

Appendix I

Summary of Granger causality between personal *ch. 7* bankruptcy filings and payday lending stores

Specification	# of lags	Time FE	Other variables included in the bankruptcy eq.	Other variables included in the bankruptcy eq.	Ho: coef. on Stores are jointly = 0	Ho: coef. on bankruptcy are jointly = 0	Conclusion
OLS + FE + robust st. errors	4	y	note 1	note 2	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	4	y	note 1	note 2	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	4	y	note 1	note 2	fail to reject	reject	b => s
OLS + FE + robust st. errors	4	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	4	n	note 1	note 2	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	4	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + robust st. errors	1	y	note 1	note 2	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	1	y	note 1	note 2	fail to reject	reject	b => s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	y	note 1	note 2	fail to reject	reject	b => s
OLS + FE + robust st. errors	1	y	note 3	note 4	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	1	y	note 3	note 4	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	y	note 3	note 4	fail to reject	reject	b => s
OLS + FE + robust st. errors	1	n	note 1	note 2	fail to reject	fail to reject	b <=> s
OLS + FE + bootstrapped st. errors	1	n	note 1	note 2	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + robust st. errors	1	n	note 3	note 4	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	1	n	note 3	note 4	reject	reject	b ≠ s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	n	note 3	note 4	fail to reject	reject	b => s

Appendix I (Continued)

note 1: unemployment rate, population 65 or over (%), not covered by health insurance (% of population), natural logarithm of per capita disposable personal income (2006 thousands \$) (denoted in this table as income)

note 2: income + legislative variables (max charge restrictions, bond requirements, asset requirements, license related fees, rollovers restrictions, loan / check maximum amount)

note 3: unemployment rate

note 4: the legislative variables in note 2

b => s bankruptcy "causes" payday stores ;

b <=> s bi-directional causality ;

b ≠ s no causal relationship

Appendix J

Summary of Granger causality between personal *ch. 13* bankruptcy filings and payday lending stores

Specification	# of lags	Time FE	Other variables included in the bankruptcy eq.	Other variables included in the bankruptcy eq.	Ho: coef. on Stores are jointly = 0	Ho: coef. on bankruptcy are jointly = 0	Conclusion
OLS + FE + robust st. errors	2	y	note 1	note 2	reject	fail to reject	s => b
OLS + FE + bootstrapped st. errors	2	y	note 1	note 2	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	2	y	note 1	note 2	reject	fail to reject	s => b
OLS + FE + robust st. errors	2	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	2	n	note 1	note 2	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	2	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + robust st. errors	1	y	note 1	note 2	fail to reject	fail to reject	b <=> s
OLS + FE + bootstrapped st. errors	1	y	note 1	note 2	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	y	note 1	note 2	fail to reject	fail to reject	b <=> s
OLS + FE + robust st. errors	1	y	note 3	note 4	fail to reject	fail to reject	b <=> s
OLS + FE + bootstrapped st. errors	1	y	note 3	note 4	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	y	note 3	note 4	fail to reject	fail to reject	b <=> s
OLS + FE + robust st. errors	1	n	note 1	note 2	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	1	n	note 1	note 2	fail to reject	fail to reject	b <=> s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	n	note 1	note 2	fail to reject	fail to reject	b <=> s
OLS + FE + robust st. errors	1	n	note 3	note 4	fail to reject	reject	b => s
OLS + FE + bootstrapped st. errors	1	n	note 3	note 4	fail to reject	reject	b => s
FGLS + panel-specific AR1 autocorrelation structure + heteroskedastic error structure	1	n	note 3	note 4	reject	fail to reject	s => b

Appendix J (Continued)

note 1: unemployment rate, population 65 or over (%), not covered by health insurance (% of population), natural logarithm of per capita disposable personal income (2006 thousands \$) (denoted in this table as income)

note 3: unemployment rate

b => s bankruptcy "causes" payday stores ;

note 2: income + legislative variables (max charge restrictions, bond requirements, asset requirements, license related fees, rollovers restrictions, loan / check maximum amount)

note 4: the legislative variables in note 2

b <=> s bi-directional causality ;

b ≠ s no causal relationship

Appendix K

Granger causality test between payday lending stores and personal bankruptcy filings (AIC lag structure)

	(1) total	(2) Stores	(3) ch. 7	(4) Stores	(5) ch. 13	(6) Stores
L1. Personal total filings per 1k persons	0.747 (12.49)**	0.227 (0.99)				
L2. Personal total filings per 1k persons	0.378 (3.74)**	0.399 (0.91)				
L3. Personal total filings per 1k persons	-0.317 (3.00)**	0.262 (0.55)				
L4. Personal total filings per 1k persons	0.149 (2.17)*	-0.682 (2.32)*				
L1. Personal ch. 7 filings per 1k persons			0.455 (7.28)**	0.384 (1.46)		
L2. Personal ch. 7 filings per 1k persons			0.81 (7.75)**	-0.072 (0.15)		
L3. Personal ch. 7 filings per 1k persons			-0.429 (3.69)**	0.871 (1.57)		
L4. Personal ch. 7 filings per 1k persons			0.11 (1.42)	-0.918 (2.56)*		
L1. Personal ch. 13 filings per 1k persons					1.423 (35.81)**	-1.522 (2.05)*
L2. Personal ch. 13 filings per 1k persons					-0.481 (11.90)**	4.828 (3.53)**
L3. Personal ch. 13 filings per 1k persons						-3.36 (2.57)*
L4. Personal ch. 13 filings per 1k persons						0.222 (0.29)
L1. Stores	0.012 (2.40)*	1.301 (28.07)**	0.003 (0.65)	1.298 (27.87)**	0.004 (3.12)**	1.319 (31.04)**

Appendix K (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	total	Stores	ch. 7	Stores	ch. 13	Stores
L2.Stores	-0.012 (1.56)	-0.492 (6.43)**	0 (0.05)	-0.49 (6.40)**	-0.005 (3.59)**	-0.481 (6.78)**
L3.Stores	-0.015 (1.69)	0.23 (2.85)**	-0.013 (1.56)	0.236 (2.91)**		0.219 (2.84)**
L4.Stores	0.009 (1.28)	-0.029 (0.53)	0.006 (0.91)	-0.03 (0.52)		-0.03 (0.55)
L1. Unemployment rate	0.168 (4.15)**		0.157 (4.09)**		0.018 (1.89)	
L2. Unemployment rate	-0.116 (3.03)**		-0.083 (2.28)*		-0.021 (2.25)*	
L1. Population 65 or over (%)	0.876 (0.77)		1.599 (1.60)		-0.126 (0.43)	
L1. Not covered by private or government health insurance (% of population)	-1.344 (2.70)**		-1.129 (2.24)*		-0.157 (1.21)	
L1. Natural logarithm of per capita disposable personal income (2006 thousands \$)	-0.467 (2.94)**	-2.09 (2.76)**	-0.545 (3.87)**	-2.271 (2.93)**	0.029 (0.70)	-2.127 (2.73)**
2005 Dummy	1.52 (16.33)**		1.923 (21.83)**		0.041 (1.37)	
2006 Dummy	-4.225 (28.58)**		-2.93 (21.86)**		-0.293 (9.44)**	
L1. Max charge restrictions		-0.088 (1.54)		-0.108 (1.88)		-0.085 (1.45)
L1. Bond requirements		-0.095 (1.24)		-0.079 (0.96)		-0.053 (0.65)
L1. Asset requirements		-0.112 (1.51)		-0.051 (0.69)		-0.147 (1.76)
L1. License related fees		0.017 (0.15)		-0.076 (0.66)		0.092 (0.71)

Appendix K (Continued)

	(1) total	(2) Stores	(3) ch. 7	(4) Stores	(5) ch. 13	(6) Stores
L1. Rollovers restrictions		0.02 (0.43)		0.011 (0.24)		0.038 (0.81)
L1. Loan / check maximum amount		0.167 (1.48)		0.169 (1.44)		0.008 (0.07)
Constant	1.693 (2.69)**	7.832 (2.93)**	1.321 (2.41)*	7.127 (2.55)*	-0.067 (0.44)	8.375 (3.20)**
Observations	590	590	590	590	693	590
Number of states	50	50	50	50	50	50
Time effects	Y	Y	Y	Y	Y	Y

All equations estimated by FGLS to correct for autocorrelation & heteroskedasticity - z statistics in parentheses

* significant at 5%; ** significant at 1%

L1. in front of a variable X means the first lag of X, L2. in front of a variable X means the second lag of X, and so on

Specifications should be interpreted in pairs: (1) & (2), (3) & (4), and (5) & (6)

Appendix L

Granger causality test between payday lending stores and personal bankruptcy filings (*parsimonious lag structure*)

	(1) total	(2) Stores	(3) ch. 7	(4) Stores	(5) ch. 13	(6) Stores
L1. Personal total filings per 1k persons	0.955 (84.14)**	0.272 (3.60)**				
L1. Personal ch. 7 filings per 1k persons			0.916 (62.33)**	0.398 (3.79)**		
L1. Personal ch. 13 filings per 1k persons					0.944 (69.72)**	0.161 (1.37)
L1. Stores (per 1 mil. persons)	-0.002 (1.05)	1.032 (68.98)**	-0.001 (0.95)	1.035 (70.54)**	0 (0.35)	1.047 (72.55)**
L1. Unemployment rate	0.022 (1.81)		0.03 (3.01)**		-0.006 (0.92)	
2005 Dummy	1.293 (17.18)**		1.544 (23.96)**		-0.14 (4.82)**	
2006 Dummy	-4.877 (56.49)**		-4.283 (54.07)**		-0.504 (16.45)**	
L1. Max charge restrictions		-0.091 (1.82)		-0.103 (2.06)*		-0.107 (2.10)*
L1. Bond requirements		-0.073 (0.93)		-0.049 (0.61)		-0.104 (1.25)
L1. Asset requirements		-0.147 (1.96)		-0.104 (1.37)		-0.197 (2.56)*
L1. License related fees		0.083 (0.75)		0.004 (0.04)		0.089 (0.76)
L1. Rollovers restrictions		0.055 (1.14)		0.047 (0.97)		0.044 (0.90)
L1. Loan / check maximum amount		0.13 (1.17)		0.125 (1.13)		0.069 (0.59)

Appendix L (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	total	Stores	ch. 7	Stores	ch. 13	Stores
Constant	0.37 (4.41)**	-0.286 (0.67)	0.297 (4.37)**	-0.248 (0.59)	0.158 (4.31)**	0.421 (1.14)
Observations	745	745	745	745	745	745
Number of states	50	50	50	50	50	50
Time effects	Y	Y	Y	Y	Y	Y

All equations estimated by FGLS to correct for autocorrelation & heteroskedasticity - z statistics in parentheses

* significant at 5%; ** significant at 1%

L1. in front of a variable X means the first lag of X

Specifications should be interpreted in pairs: (1) & (2), (3) & (4), and (5) & (6)

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