

Restrictions on Credit: A Public Policy Analysis of Payday Lending

Petru S. Stoianovici
The Brattle Group
&
Michael T. Maloney
Clemson University

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ABSTRACT

Using state-level data between 1990 and 2006, we find no empirical evidence that payday lending leads to more bankruptcy filings; this finding casts doubt on the “cycle of debt” argument against payday lending. We capture the intensity of payday lending activity in a state by the number of payday lending stores. We control for restrictions on payday lenders by including in the analysis six variables that we construct that rank legislative provisions across states and across time. We use two different estimation procedures: difference-in-difference and Granger causality. Using either method, we find no empirical evidence that payday lending leads to more bankruptcy filings.

I. INTRODUCTION

From a normative point of view, the interference of the government in voluntary transactions 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 in 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, we analyze the market for a form of short-term credit known as payday lending. We address the question of whether the *presence* of payday lending stores has negative welfare implications in the communities where the stores are present.

Using state-level data between 1990 and 2006, and two different empirical approaches, we find no evidence to support that payday lending leads to more bankruptcy filings — a measure of consumers’ welfare. This finding suggests that there is no evidence to support the “cycle of debt” argument against payday lending.

We 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, we 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. We find that the main restriction that influences the presence of payday lenders is price restrictions.

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

II. BACKGROUND ON PAYDAY LOANS AND WELFARE EFFECTS OF CREDIT RESTRICTIONS

II.1 Background on payday loans

A payday loan (also known as deferred deposit advance or loan, cash advance, payday advance) is a form of single-payment, unsecured short-term credit of a small amount. The lender keeps the personal check issued by the debtor until the maturity of the loan; the face amount of the check includes both principal and interest. In some jurisdictions, the transaction could also be based on an agreement authorizing the lender to make an electronic withdrawal from the borrower's checking account on the maturity date.

Underwriting standards vary across lenders, but the lender generally requires proof of the borrower's income (recent pay stubs usually suffice) and that the borrower have 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 redeem the check. 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 further charges.

A typical two-week \$300 cash advance will usually have a \$45¹ charge, which corresponds to a 391% 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 present.² 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. 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, Community Financial Services Association of America (CFSA), comprised of more than 150 members representing over half of the payday lending stores.⁴ As the number of

¹ This information comes from industry sources, 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 (e.g., WA state DFI [2006]).

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

³ Snarr (2002).

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

payday lending stores increased in the last 10-15 years, both in absolute numbers and per capita, reaching today around 23,000⁵ store locations nationwide, 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 significant restrictions, for example, in Delaware, to a complete, explicit 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 \$25,000 per transaction — and it was subject to class-action lawsuits. In addition, 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 loan seems to be the price ceiling imposed.

There were several arguments invoked in passing legislative restrictions against this type of loan, among which were:⁷

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

The typical two-week period could be mostly demand- (and not supply-) driven; for example, consumers might want a self-imposed constraint. Since in most cases the charges are the same per transaction, the more a borrower 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 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

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

⁶ See Ga. 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.”

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 a payday lender might not retrieve the applicant's credit score from a credit reporting agency⁸, it does check the past behavior of the applicant with that lender and its 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 these 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.

II.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 applicable laws 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 they 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 make a principled argument that the consumer is deceived in a payday lending 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 repay 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 received when the contract was signed.

The reason the borrower might not repay the loan at maturity is another story. If his intention were to cheat on his contract, and not pay the agreed upon interest and principal, it is not clear that we should take his side merely because he is presumably disadvantaged.

In this context, the "cycle of debt" argument works only if the costs that the default borrower has to bear are hidden and disproportionately high, and if the lenders

⁸ However, some payday lenders use other external services to verify the applicant, for example, Teletrack; 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%.

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 from using 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 income and financial sophistication, he is more likely to be unable to repay at maturity. He will either default or try to refinance. If he "rolls over" or renews the debt, the result will be an 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 on welfare as a result of extra borrowing. The restrictions, in this way, would reduce the excess credit. The potential for welfare opportunistic behavior would be mitigated, and the socially 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. These authors conclude 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 their credit histories with their customers to credit reporting agencies, they prevent "good" borrowers from moving into to the low-cost credit market. He suggests, however, that requiring or encouraging fringe creditors to report their credit histories 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, such as 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 were 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, as 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 (i.e., 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 and what is the net welfare effect. The answer to both of these questions is an empirical matter.

III. LITERATURE REVIEW¹¹

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 for payday loans and the supply (mainly the location of payday lenders); 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),

¹¹ For a more detailed literature review see Petru S. Stoianovici, "Restrictions on credit: A public policy analysis of payday lending" (Ph.D. dissertation, Clemson University, 2008), ("Stoianovici, 2008").

Morgan (2007), Morgan and Strain (2007), Wilson et al. (2008), Carrell and Zinman (2008).

Using store locations in 20 states at the end of 2003 / beginning of 2004, Graves and Peterson (2005) found support for the hypothesis that payday lenders locate in the proximity of the military personnel. They identify four main characteristics that make military personnel more likely to use payday loan.

Graves (2003) shows that in metropolitan Louisiana and in Cook County (Illinois), banks and payday lenders tend to be located in different areas. He finds that payday lenders enter poor and minority neighborhoods at a lower rate than banks exit them. This is contrary to Burkey and Simkins (2004), who find that payday lenders and banks tend to cluster in the same areas in North Carolina, which they interpret as evidence that banks and payday stores are complements, not substitutes.

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 credit products similar to 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 located do not have a significant effect on store profitability in Flannery and Samolyk (2005), after controlling for loan volume.¹²

Stegman and Faris (2003) estimate that a measure of rollovers — the percentage of chronic borrowers —, after the number of clients, is the second most important variable that explains payday lenders' revenues, and a proxy for payday lenders' profits. 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 prevalence of rollovers in that store).

Huckstep (2006) argues that the reason for the large number of stores and high operating costs (that translate into high fees) relative to other lenders is that payday loans are a product chosen mostly for convenience. Because of this, payday lenders compete in a local area and have a "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 latter was chosen because of the similarity in terms of business model. He finds that the average 7.63% profit margin of payday lenders is lower than the profit margins of both 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

¹² 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's customers.

engage in predatory lending, the lenders would benefit from pushing borrowers into debt traps. If payday lenders preyed 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.”

Morgan and Strain (2007) test the welfare implications of payday lending by analyzing the change in a number of welfare proxies 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 borrowers by forcing them into more expensive alternatives, which contradicts one of the arguments 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 supports the idea that the payday lending market is competitive.

Morse (2007) uses propensity score matching to identify communities that are likely to be cash constrained based on different socioeconomic variables. The communities that have payday stores and those that do not have them are similar in terms of welfare characteristics considered (foreclosures, deaths, drug treatment, and births). Then she uses the propensity scores to compare the zip-code-level communities that faced a disaster (wildfire, flooding, storm / hail, earthquake, landslide, and tornado) to similar cash constraint ones that did not face such disasters. She finds evidence that the welfare variables considered are improved after disaster shocks in the presence of payday lending. Furthermore, she 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-loan 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 expenditure shocks (p. 25-6). They also find 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 (measured by the outcome of the *first* payday loan application, accepted or rejected) 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 to this paper's results.

Carrell and Zinman (2008) find that allowing payday lending in a state leads to a decrease in performance and retention outcomes of Air Force personnel.

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

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

In this paper we 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. We try to capture the intensity of the presence of payday lending activity through the number of payday stores. We also have longer panel data, which capture the outset of the industry, and in addition we measure the effects not only of the presence of payday lenders, but also of the change in the relevant legislation.

IV. DATA AND THE EMPIRICAL APPROACH

Ideally, the welfare effects of payday lending should be analyzed by tracking welfare measures of payday borrowers over time. Data limitations, however, do not allow us that. Instead, we use the variation in payday lending legislation and the number of payday lending stores across time and states to assess the aggregate effects at the state level.

Given the prevalence of repeated borrowing,¹³ since payday loans users accept borrowing with a relatively high cost, it is likely that they do not have other, less expensive, alternatives. Elliehausen and Lawrence (2001) show that more than 60% of payday-loan 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 interpretation 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.¹⁴

¹³ For example Stegman (2007), Chessin (2005).

¹⁴ For a review of the bankruptcy procedure, see, for example, Lockett (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 under chapter 13. Under chapter 7 debtors' non-exempt assets are sold, and any unsecured debts that cannot be repaid from the proceeds are discharged, with no other obligation for the filer. Under chapter 13 the debtor keeps his 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 do not have many assets (borrowers with few assets and large amounts of unsecured debt tend to benefit more from the discharge provided by chapter 7). Gross and Souleles (2002) argue that borrowers' willingness to default has increased over time due to declines in default costs, including social, information and legal costs. .

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). Even so, the cycle of debt argument is fundamentally flawed because the industry can not build itself in the long run seeking to bankrupt its customers.

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 lenders (they entered or exited during the period). We give more on the exact specifications in section 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. All demographic and shock variables

included in the bankruptcy analysis, based on the bankruptcy literature,¹⁵ are listed in section five, and summarized in appendix A.

IV.1 Data sources and Legislative variables

Population, percent of individuals not covered by private or government health insurance, and 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 come 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). Consumer Credit Outstanding data source is the Federal Reserve Board. Personal bankruptcy filings data are from Administrative Office of the U.S. Courts.

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

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

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 reducing competition, and secondly making reputation more important.

Reputation might play a role if there were informational asymmetries between the consumers and the loan providers. Suppose, as we 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 the consumer. A bond might reduce the problem of supplier adverse selection in the market.¹⁶

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

¹⁵ For example, Fay et al. (2002), Domowitz and Sartain (1999), Domowitz and Eovaldi (1993), Buckley and Brinig (1998), Himmelstein et al. (2005), Agarwal and Liu (2003), Luckett (1988), Gross and Souleles (2002), White (1998).

¹⁶ 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.

Business practice restrictions lead to increases in production costs. Any increase in the costs of providing the services will be shared (depending on the relative elasticities of demand and supply) between consumers and producers. There are no compelling reasons to believe that the supply side is not elastic in the long run. Thus, ultimately the increase in the costs induced by the regulation falls mainly on 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 the national level affect businesses in different states differently, depending on the state and local rules in place at that time.¹⁷

The relevant explicit or equivalent payday lending legislation was identified based on information from state legislature web sites, the web sites of the state agencies that regulate this industry, and LexisNexis Academic (main source for annotated statutes). The information was cross-referenced with government agency reports, National Conference of State Legislatures data, industry reports, newspaper articles at different times from various sources. The relevant payday lending provisions were extracted from the current legislation for all U.S. states. This allows us 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

¹⁷ During the period analyzed, there is only one significant change at the national level that affected states differently: effective July 1, 2005, the FDIC changed its payday loan 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 February 2006 the FDIC advised FDIC-insured banks that they could no longer offer payday loans through marketing and servicing agents. This effectively terminated payday lending in states where payday lenders were using out of states chartered banks to circumvent state restrictions.

create a ranking of payday lending restrictions consistent across states and across time.¹⁸ 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 coding rules, the variable “*Max charge restrictions*” was coded in 2004 for Kansas as 4.

In 2005 a law was enacted that changed the maximum charges for a three hundred dollars 14-day payday loan in Kansas to about \$45. This act was effective July 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 July 1.

Georgia 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 we 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 interest restrictions for the lender were used.

V. DO PAYDAY STORES LEAD TO HIGHER BANKRUPTCY INCIDENCE?

In this paper we 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 finance charge associated with payday loans 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 a bank. This could also explain why payday lenders usually increase in size by increasing their number of locations. This seems to be consistent with data: as we have mentioned before, Ace Cash Express opened a store in Augusta, GA less than 1.8 miles away from another of its stores.

¹⁸ The exact coding criteria used are presented in Stoianovici, 2008 (see footnote 11 above) and are also available on request.

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 or other depository institutions.

The number of payday lending stores was not available for years prior to 2006. Instead, we use as a proxy the number of payday lending stores of CFSA members 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 experiences mainly expansion, this is likely not that much of a problem) and 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. We do not have any reasons to believe that that might have happened.

Furthermore, we try to assess the potential bias empirically: the correlation coefficient between CFSA stores in 2006, and the actual number of payday stores (based on our own estimate from regulatory agencies), was 0.9046. If we 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, we 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.

V.1 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 groups. 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 stands for the state, t is a time subscript which covers the period 1990 through 2006, and $Y_{i,t}$ is personal bankruptcy filings per one thousand persons.

Furthermore, 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 the national level)
- the natural logarithm of the percent population below the 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 October 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). Finally, α , γ_1 , γ_2 , γ_3 , γ_4 , γ_5 , and the vectors β^1 and β^2 are the 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 legislation.¹⁹ The same happened to some states as a result of the FDIC ruling.²⁰

¹⁹ See footnote 6 above.

²⁰ See footnote 16 above.

This model was estimated separately for total personal bankruptcy filings, chapter 7 personal bankruptcy filings, and chapter 13 personal bankruptcy filings. The results are presented in appendix B. We have tested for the presence of fixed²¹ and random²² 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), we also estimate in (4) through (6) bootstrapped standard errors. This accounts for the specific characteristics of the data.

There is autocorrelation²³ and heteroskedasticity²⁴ present in the data. We correct for both in specifications (7) through (9).²⁵ Because divorce rate is missing (total missing observations is 170 out of 867), to increase precision, we re-run these models without the variable divorce rate. We have also run models that include poverty rates (which are missing for 4 out of 17 years in our data, and which showed up insignificant). The results²⁶ lead to the same conclusions. Different lag structures for the independent variables have been tried, and we have also tried other logarithm transformations to allow for nonlinearities; the results (not shown here) were similar. The different bankruptcy exemptions across states are captured in 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 reduction in information costs, and in the stigma and guilt associated with bankruptcy. In addition, as more people file for bankruptcy, the less severe the reduction in credit access is. We 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, and we expect that, everything else the same, the higher the fraction of population 65 and over, the lower the bankruptcy rate. We 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 latter result might happen if individuals in retirement without significant assets 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.

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. Buckley and Brinig (1998) point out that “divorce rates might be positively correlated with bankruptcy rates

²¹ F-test with pooled regression model as the baseline for the comparison.

²² Breusch and Pagan LM test for RE with pooled regression model as the baseline for the comparison.

²³ Wald test for first order autocorrelation and Arellano-Bond test for higher order autocorrelation.

²⁴ LR test.

²⁵ Using feasible GLS, panel-specific AR(1) autocorrelation structure, and heteroskedastic error structure.

²⁶ Available on request.

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” (pp. 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).

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 policy point of view is "*Stores*Change*".²⁷ 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), we are also controlling for the intensity of the industry – which was not done before. The DID coefficient shows whether the *presence* of payday stores is associated with more, less or about the same bankruptcy filings.

Controlling for the intensity of the payday lending stores, we find that, if anything, the presence of payday stores in a state is associated with a smaller number of 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.

V.2 Granger causality analysis

To reinforce these results, we test for Granger causality between payday lending stores and bankruptcies. 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 some lag j .

Panel-data Granger analyses have to account for potential state-level heterogeneity. We start first with a simple Granger causality test of bankruptcy filings and the presence of payday lending stores *at state level* (no other variables included), and then we analyze the consistency of the results across different states. If payday lending causes bankruptcy, then this relationship should hold consistently across states. This simple Granger test at the state level could be extended by controlling for other factors. However, the relatively short period of time of our data (17 periods maximum) does not allow us to perform this analysis.

By restricting the Granger analysis at the state level, we 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).

²⁷ This is the most relevant effect to look at for allowing / banning payday lending.

We 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.²⁸ However, for total personal bankruptcy filings, we 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 from zero, and γ_j in equation (3) are zero.

We have run these models by state, and then we 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</i> "causes" <i>Bankruptcy</i> , and <i>Bankruptcy</i> does not "cause" <i>Stores</i>	6	5	6	8
<i>Bankruptcy</i> "causes" <i>Stores</i> , and <i>Stores</i> does not "cause" <i>Bankruptcy</i>	3	7	2	4
<i>Bankruptcy</i> "causes" <i>Stores</i> , and <i>Stores</i> "cause" <i>Bankruptcy</i>	2	1	3	0
<i>Bankruptcy</i> does not "cause" <i>Stores</i> , and <i>Stores</i> do not "cause" <i>Bankruptcy</i>	21	19	21	20

The numbers are number of states

In (2) we used the number of lags that results from AIC at state level (instead from AIC with all data pooled)

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

²⁸ Available on request.

hypothesis that β_j were jointly significantly different than zero. This table implies that payday lending stores do not cause bankruptcy.

Next, we 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:

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

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

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.

Appendix C summarizes 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 bankruptcy filings).²⁹ Again, there is no clear picture of causality of payday lending stores on bankruptcy. It seems that if anything, there is weak support of causality in the opposite direction. This could be explained by the reduced access to alternative credit induced by the bankruptcy. In other words, payday lending stores respond to demand factors. Bankruptcy might lead to more payday stores through this channel: bankruptcy significantly reduces 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.

We 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. We run the FGLS estimation (corrected for autocorrelation and heteroskedasticity) with the optimal lag structure based on Akaike Information Criterion.³⁰ However, using 4 lags structure induces strong multicollinearity (with very high VIFs for the payday lending stores and bankruptcy filings variables). We have also estimated the Granger causality with a more parsimonious one lag structure. The conclusions did not change. The results of an FGLS estimation are presented for illustration purposes in appendix D.

Multicollinearity increases standard errors, reduces the t-values, and makes the coefficients insignificant even if they are not. However, even if collinearity induced

²⁹ The similar results for chapter 7 and chapter 13 are available on request.

³⁰ Available on request.

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 confirms the DID approach: no empirical support was found for causality between payday lending and bankruptcy filings.

VI. CONCLUSIONS

In this paper, we investigate the relationship between payday lending and personal bankruptcy filings — a measure of consumer's welfare. Using state level 1990-2006 data, we control for the intensity of the presence of the payday lending industry, and for explicit or equivalent payday lending legislation. Based on variation across states and across time in restriction provisions, we 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. We find that the main restriction that influences the presence of payday lenders is price restrictions.

We 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. We 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 "cycle of debt" argument 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 finance charges relative to 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 state restrictions placed on payday lenders is an increase in costs of doing business which will lead to higher prices than otherwise, leaving 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

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

Appendix A (Continued)

Variable		Mean	Std. Dev.	Min	Max	Observations
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
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 B

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*** (0.047)	0.404*** (0.046)	0.155*** (0.022)	0.560*** (0.057)	0.404*** (0.053)	0.155*** (0.024)	0.954*** (0.016)	0.320*** (0.031)	0.247*** (0.017)
Population 65 or over (%)	17.78** (7.913)	19.03* (10.72)	-1.263 (6.268)	17.78 (12.45)	19.03 (14.27)	-1.263 (7.458)	0.969 (1.122)	7.119** (2.779)	-1.400 (1.861)
Unemployment Rate	0.167** (0.081)	0.052 (0.060)	0.114** (0.047)	0.167* (0.094)	0.052 (0.069)	0.114** (0.051)	0.168*** (0.037)	0.063* (0.036)	0.064*** (0.016)
Lag of unemployment Rate	0.089 (0.068)	0.104* (0.053)	-0.015 (0.035)	0.0892 (0.081)	0.104* (0.058)	-0.015 (0.042)	-0.091** (0.038)	0.069* (0.037)	-0.005 (0.016)
Not covered by health insurance (% of population)	-0.550 (2.620)	1.051 (2.353)	-1.599** (0.636)	-0.550 (2.971)	1.051 (2.566)	-1.599*** (0.601)	-1.512* (0.784)	-1.360* (0.735)	-0.230 (0.293)
Lag of not covered by health insurance (% of population)	-0.624 (1.795)	-0.492 (1.808)	-0.131 (0.778)	-0.624 (1.885)	-0.492 (1.919)	-0.131 (0.855)	-0.569 (0.810)	-0.250 (0.754)	0.412 (0.388)
Consumer credit outstanding – Disposable personal income ratio				56.88	47.74	9.188			
Lag of Consumer credit outstanding – Disposable personal income ratio				-43.82 (28.87)	-38.67 (23.62)	-4.970 (13.93)			
Divorce rate (per 1,000 total population residing in area) (%)	0.107* (0.055)	0.060 (0.054)	0.049** (0.024)	0.107 (0.076)	0.060 (0.074)	0.049 (0.034)	0.037 (0.038)	0.090** (0.036)	0.001 (0.019)
Lag of divorce rate (per 1,000 total population residing in area) (%)	-0.017 (0.086)	-0.119 (0.082)	0.102*** (0.028)	-0.0171 (0.086)	-0.119 (0.076)	0.102*** (0.030)	0.018 (0.038)	0.022 (0.037)	0.045** (0.019)
Ln of per capita real disposable personal income	-1.222 (1.303)	-0.401 (1.167)	-0.825 (0.524)	-1.222 (1.904)	-0.401 (1.733)	-0.825 (0.626)	-0.866 (0.738)	-0.154 (0.527)	-0.211 (0.323)

Appendix B (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 (1.443)	-1.036 (1.373)	1.048* (0.590)	0.0312 (1.690)	-1.036 (1.752)	1.048 (0.666)	0.382 (0.738)	-0.604 (0.532)	-0.217 (0.319)
Stores	-0.005 (0.005)	-0.00181 (0.00592)	-0.00285 (0.00298)	-0.005 (0.009)	-0.002 (0.009)	-0.003 (0.003)	-0.005** (0.002)	0.002 (0.003)	0.003* (0.002)
<i>Stores*Change</i>	0.003 (0.006)	-0.001 (0.006)	0.003 (0.004)	0.003 (0.009)	-0.001 (0.009)	0.003 (0.004)	0.002 (0.002)	-0.009** (0.004)	0.003 (0.002)
<i>Change</i>							-0.062 (0.044)	-0.075 (0.098)	0.072 (0.069)
2005 dummy	2.949*** (0.442)	2.689*** (0.429)	0.274 (0.196)	1.703*** (0.167)	1.754*** (0.148)	-0.050 (0.061)	1.699*** (0.128)	2.878*** (0.191)	-0.129 (0.103)
2006 dummy	-2.882*** (0.346)	-2.469*** (0.341)	-0.397* (0.229)	-3.836*** (0.201)	-3.157*** (0.171)	-0.676*** (0.110)			
Constant	1.477 (4.772)	2.798 (3.996)	-1.253 (2.479)	-0.548 (6.623)	1.457 (5.704)	-1.977 (3.401)	1.469** (0.618)	2.050 (1.349)	0.903 (1.015)
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 standard errors in parentheses

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

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

Appendix C

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 C (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 D

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	0.904*** (0.0159)	0.0823*** (0.0318)				
L1. Personal ch. 7 filings per 1k persons			0.871*** (0.0181)	0.0825** (0.0370)		
L1. Personal ch. 13 filings per 1k persons					0.938*** (0.0149)	0.0492 (0.0694)
L1. Stores (per 1 mil. persons)	-0.000224 (0.00238)	1.064*** (0.0133)	0.00161 (0.00206)	1.067*** (0.0132)	-0.00101 (0.000688)	1.077*** (0.0121)
L1. Unemployment rate	-0.0656*** (0.0165)		-0.0560*** (0.0131)		-0.0238*** (0.00469)	
2005 Dummy	1.531*** (0.0767)		1.636*** (0.0644)		-0.0614*** (0.0148)	
2006 Dummy	-4.573*** (0.0897)		-4.113*** (0.0780)		-0.407*** (0.0198)	
L1. Max charge restrictions		-0.0963* (0.0496)		-0.108** (0.0505)		-0.0984** (0.0451)
L1. Bond requirements		0.0128 (0.0837)		0.00806 (0.0843)		0.0475 (0.0482)
L1. Asset requirements		-0.0441 (0.0497)		-0.0530 (0.0542)		-0.0649 (0.0410)
L1. License related fees		0.0987 (0.102)		0.0687 (0.101)		0.0537 (0.0715)
L1. Rollovers restrictions		0.0298 (0.0427)		0.0335 (0.0442)		0.0112 (0.0277)
L1. Loan / check maximum amount		-0.0295 (0.102)		-0.0367 (0.103)		-0.0855 (0.0852)

Appendix D (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	total	Stores	ch. 7	Stores	ch. 13	Stores
Constant	0.877*** (0.107)	0.765*** (0.281)	0.775*** (0.0894)	0.896*** (0.275)	0.226*** (0.0285)	1.081*** (0.244)
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 10%; ** 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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