WORKING PAPERS

RESEARCH DEPARTMENT

**WORKING PAPER NO. 10-32
PAYDAY LENDING: NEW RESEARCH AND
THE BIG QUESTION**

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October 2010

RESEARCH DEPARTMENT, FEDERAL RESERVE BANK OF PHILADELPHIA

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Payday Lending: New Research and the Big Question

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I. Introduction

Payday lending is controversial. In the states that allow it, payday lenders make cash loans that are typically for $500 or less that the borrower must repay or renew on his or her next payday. The finance charge for the loan is usually 15 to 20 percent of the amount advanced, so for a typical two-week loan the annual percentage interest rate is about 400 percent.

In this article, I briefly describe the payday lending business and explain why it presents challenging public policy issues. The heart of this article, however, surveys recent research that attempts to answer what I call the "big question," one that is fundamental to the public policy dispute: Do payday lenders, on net, exacerbate or relieve customers' financial difficulties?

It is easy to make the case that payday lending should be beneficial. The terms of a pay-day loan are easy to understand and no one is forced to take a loan. If people choose to do so, it must be because they believe it to be their best alternative. To make this concrete, consider one example. Suppose that I am one week away from my payday, my bank account is nearly empty, I don't have a credit card or I've already borrowed to the limit on my card, and I have some bills to pay. I could write checks to pay the bills, knowing that I will pay a $30 non-sufficient funds

(NSF) fee for each check that overdraws my account and a $15 returned check fee for each check my bank refuses to pay. If I must pay two $100 utility bills, such fees could easily exceed the fee on a $200 payday loan. Alternatively, I might simply delay paying the utility bills, but then I will incur late payment charges and perhaps have my utility service disconnected. These fees and the cost of the disruption could easily exceed the finance charge on a payday loan.

The argument that payday loans could make people worse off is based on a different view of human behavior. According to this view, some people are tempted by easy access to cash. They focus on the immediate benefits it brings them and either they don't think about the financial and personal cost of repaying the loan or they underestimate this cost. Such myopic individuals might be better off if they did not have access to payday loans. Suppose, for example, I am in the same situation described above, but I have the option of working overtime to earn money to pay my bills. As a myopic individual, I might choose the $200 payday loan, leaving me to repay $230 in two weeks. When the next pay period arrives, rather than repay the $230, I only pay the $30 finance charge and renew the loan to delay having to work overtime or having to cut my expenses. If such behavior continues for several pay periods, soon I will pay more in finance charges than I originally borrowed.

I have been interested in payday lending and have periodically written about it since I first learned of it in the early 1990s, about the time the business was developing. But I have always avoided the big question, since I did not know how to answer it. In recent years, however, a number of talented economists have tackled it. I argue in this paper that none of their efforts have yet provided a convincing answer, but these efforts are worth examining because they include excellent examples of quasi-experimental research techniques as well as simulation studies. Moreover, the researchers reach different conclusions on a topic with important public policy

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implications, so it is important to sort out why. This survey concludes with a brief discussion of possible fruitful new paths for research on payday lending.

I should state at the outset that payday lenders do not direct their services to the very poor, but rather to moderate-income households who have little financial savings and who lack access to lower-cost credit. In many cases, their customers have severely impaired credit histories or they have reached their limit on lower-cost sources of credit, such as credit cards.

II. Payday Lending and Public Policy

Payday lending is important because it is a big, controversial business that states and the federal government are struggling to decide how to regulate. In 2007, Michael Stegman (2007) wrote a prominent article surveying research on payday lending. As he noted, at that time estimates indicated that there were over 20,000 payday loan stores nationally making over $40 billion in loans annually. Stegman also noted that payday lending was effectively banned in about 12 states, including some populous ones such as New York and Pennsylvania, because state laws did not permit the lenders to charge high enough interest rates to be profitable. Moreover, in October 2007, the federal government effectively banned payday lending to military personnel when it set a usury ceiling of 36 percent annual percentage rate (APR) on loans to servicemen and women. Since Stegman's article, a small number of states have joined those effectively banning payday lending, but the picture nationally is not substantively different from that described by Stegman.

Data from Florida and Oklahoma suggest just how big payday lending might become if the industry could operate in all states. In those two states, customers are allowed to have only one payday loan at a time. To enforce this regulation, the states maintain central databases in

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which payday lenders must register customers. A private company, Veritec, manages the data systems for the two states. Veritec (2009a) data from Florida indicate that about 738,000 individuals borrowed from payday lenders in that state from June 2008 through May 2009, or about 5.1 percent of the adult population. In Oklahoma, 113,576 individuals borrowed from payday lenders from April 2008 through March 2009, or about 4.2 percent of the adult population (Veritec, 2009b). This suggests that if all states were to liberalize their regulations governing payday lenders to the same degree that these two states have, in one year almost 11 million Americans would borrow from payday lenders and, as discussed below, many would do so repeatedly. In other words, payday lending is a big business that would be even bigger if restrictive states were to liberalize their regulations.

Despite the high cost of payday loans, if most customers borrowed to meet very infrequent emergency expenses and then repaid the loans out of their next paycheck, the loans would not be highly controversial. Critics of the industry emphasize not only that the loans are costly but that they allegedly lead to a "debt trap." The idea is that someone originally borrows, say, $300, to pay pressing bills, but by the next pay period, she is in a worse situation because she faces a new round of bills and, in addition, has to repay the lender about $350. In this situation, she may take out a new payday loan to repay part, or all, of the principal of the previous one. With interest rates of 15 to 20 percent per two weeks, a customer who borrows frequently will soon pay more in finance charges than her average cash advance.

Stegman's 2007 article made this same point and provided data indicating that many payday loan customers borrow repeatedly. More recent data reinforce this finding. A study for the California Department of Corporations (Applied Management and Planning Group, 2007) found, for example, that 19 percent of loan customers took out 15 or more loans over an 18-

month period. Only 16 percent took out just one. The study also included focus groups with a small number of customers. Based on the focus groups, the study reported (p.75), "When asked if they would recommend payday loans to others, most indicated that they would provide the information about payday lending, but would also provide cautions to the 'addictive', 'repetitive', and 'vicious' cycle that can be a part of the payday lending experience." In Colorado (Administrator of the Colorado UCCC, 2008), during 2007, payday loan customers with 12 or more loans accounted for 67 percent of all loans; 65 percent of loans were made on the same day that a customer repaid a previous loan. As the Colorado report (p. 5) stated, "During 2007 the 'average' consumer paid about $573.06 in total finance charges to have borrowed $353.88 for a period of little more than five and one-half months at each ... location with which that consumer did business." Data from Florida (Veritec, May 2009b) indicate that the average number of transactions per consumer from June 2008 through May 2009 was 8.4, and 30 percent of the customers in that year had 12 loans or more. These 30 percent of customers accounted for 61 percent of all payday loans made in that year. In Oklahoma, the average number of transactions per customer was 9.3 from April 2008 through March 2009, 32.5 percent of the customers in that year took out 12 or more loans, accounting for 63.5 percent of loan volume (Veritec, 2009b).

Data on the demographic characteristics of payday loan customers come from the lenders and customer surveys. Both have their limitations. The loan files of payday lenders do not include information on other adults in the household in which the borrower lives, so they do not reveal household incomes. Surveys of payday loan customers, typically by telephone, can contain such information, but such surveys do not reach all customers and almost half of those they do reach deny that they took out a payday loan, despite evidence provided by lenders indicating

that they did.[2] In addition, the information gained through household surveys is less reliable, since it is not corroborated by documentary evidence. In any case, recent data do not change the basic description that Stegman provided. The vast majority of payday loan customers have jobs or another reliable income source, and all have bank accounts, since this is a precondition of underwriting. The majority of payday loan customers earn $15,000 to $40,000, with somewhat higher household incomes. A strong majority has a high-school education or more. The customers are, relative to the U.S. population, disproportionately black or Hispanic.

Customer surveys frequently ask why customers borrow. The standard explanation, well documented by Stegman, is that they do not have convenient access to a lower-cost source of credit and they want or need to make an expenditure for which they do not have sufficient cash on hand. In the 2007 California survey (Applied Management, p. 47), 50.2 percent of loan customers said that they took the loan primarily to pay bills, and 22.3 percent said that they mainly used it to buy groceries or other household goods. At a deeper level, however, such information is unsatisfactory. If, for example, an individual incurs an unexpected medical expense and then doesn't have enough money to buy groceries and takes out a loan, is the loan for the groceries or the medical expense? If someone can't pay her bills because earlier she spent her paycheck on a vacation, is the loan for the bills or the vacation? Surveys can't answer such questions. One would need a detailed history of the expenditure patterns and incomes of the individuals as well as the thought processes behind their budgeting, and no study related to payday lending has done this. As I discuss in the conclusion, longitudinal studies with ethnographic components might be valuable.

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[2] For example, see the report for the California Department of Corporations (Applied Management, p. 40).

One explanation for payday loans that we can rule out is that people borrow from payday lenders because they don't know the cost. It is true that many payday loan customers don't know the APR on their loans, despite the fact that the lenders are required to reveal it prominently. But people know the finance charge. In the California survey (Applied Management, p. 57), to cite one recent example, 92 percent of the respondents said that they were aware of the fees on their loans before taking them out.

III. Quasi-Experimental Studies Seeking to Answer the Big Question

In this section I review the work of a number of talented economists who have tried to determine whether payday lenders, on net, exacerbate or relieve their customers' financial difficulties. My survey of their work is not comprehensive. In most cases, I discuss just their most important results. Often the researchers try alternative specifications and employ robustness tests that space constraints do not allow me to discuss.

On July 1, 2007, Oregon imposed a fee cap on payday lending that led most, but not all, lenders in the state to close their operations. Prior to the new law, payday lenders were active in the state, making almost 1 million loans in 2006. Payday lending was also widespread in the neighboring state of Washington, where lenders made 3.5 million loans in 2006. They have remained active in Washington, since it has not lowered the fees it allows the lenders to charge ($15 per $100 advanced with no minimum loan term).

Jonathan Zinman (2010) used the change in the law in Oregon to study how cutting access to payday lending affected loan clients. As the Oregon Department of Consumer and Business Services (2006) reports, in 2006 the average payday loan in Oregon was $328 and the average finance charge was $54. Given the short maturity of most loans, the average APR was

486 percent. The new law capped finance charges at roughly $10 per $100 advanced and set a minimum loan term of 31 days for a maximum APR of 150 percent. Most payday lenders decided that their business would not be sufficiently profitable under this restriction. At year-end 2006, there were 346 payday loan storefronts in the state. By February 2008 there were 105 (Zinman, p. 548).

Since the industry was aware in advance that the Oregon fee cap was coming, it sponsored telephone surveys of loan customers in both states. The "baseline" survey was conducted in June and July of 2007, just prior to, or contemporaneous with, the new fee cap, and covered 1,040 people (520 in each state) who had borrowed from payday lenders in the prior three months. The "follow-up" survey was conducted in November and December of 2007, five months after the fee cap was imposed.[3] It covered 400 people, 200 in each state, who participated in the first survey and who agreed to participate in the follow-up survey.[4]

Zinman's research strategy is to compare the change in the customers' responses pre- and post-reform in Oregon to the change in responses in Washington. The key assumption is that, absent the change in Oregon's law, the changes in the responses would have been the same. In other words, the assumption is that any differences in the differences (DD) over time in the customers' characteristics are due to the restrictions on payday lending in Oregon.

As shown in Table 1, after the Oregon reform there was a larger decline in the percentage of pre-reform customers who reported using payday or other short-term loans in Oregon than in

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[3] I assume that respondents who denied that they had taken out a payday loan were excluded from the baseline survey, since that survey indicates 100 percent use of payday loans. As noted elsewhere, in other surveys of known loan customers, almost half deny having used a payday loan.

[4] The demographic characteristics of the respondents in the baseline surveys in Washington and Oregon differed moderately, suggesting that respondents in Washington might be in somewhat different labor markets than those in Oregon. In addition, the attrition rates between the baseline and follow-up surveys differed between the states. Zinman recognizes that this could bias his estimates, and he introduces a reasonable weighting procedure to try to correct for the problem. This does not qualitatively change his results.

Washington. There was also a larger increase in the percentage of Oregon customers compared to Washington customers who reported that it was more difficult to obtain a short-term loan. Zinman concludes that the Oregon cap clearly reduced access to payday loans and to short-term credit generally.

Table 1

	Oregon Baseline	**Oregon Follow-up**	**Washington Baseline**	**Washington Follow-up**	**Difference-in-difference**
Any payday loan in last 3 months	1	0.505	1	0.789	-0.284***
Used any short-term loan in last 3 months	1	0.570	1	0.830	-0.260***
Harder to get short-term loan last 3 months	0.158	0.388	0.045	0.090	0.185***
Unemployed	0.121	0.151	0.131	0.131	0.030
"…your financial situation in the last 6 months" getting worse	0.172	0.207	0.181	0.156	0.060
"Thinking about the future, do you expect your financial situation to" get worse	0.046	0.066	0.061	0.036	0.046*
Positive response to any of the above three questions	0.279	0.345	0.313	0.262	0.177**

Source: Zinman (2010)   * p < 0.10   ** p<0.05   *** p<0.01

Many backers of the Oregon fee cap would applaud the apparent reduced availability of short-term credit, since they believe that easy access to high-cost short-term credit is harmful. The survey tried to address this point by asking about the respondents' employment status and perceptions of their general financial situation. As shown in the table, unemployment among the surveyed group increased more in Oregon than in Washington as did pessimistic perceptions of their past, and expected future, financial situations. The DD in expectations about future financial situations is statistically significant as is the DD for a composite index based on the data on unemployment and perceptions of past and future financial situations. Zinman (p. 553) believes that these survey results "suggest that the Oregon Cap reduced the supply of credit for payday borrowers, and that the financial condition of borrowers (as measured by employment status and subjective assessments) suffered as a result."

Donald Morgan and Michael Strain (2009) note that changes in state laws and changes in the Federal Deposit Insurance Corporation's (FDIC) policies affected the availability of payday lending in Washington D.C. and eight states (Maryland, Georgia, New Hampshire, North Carolina, West Virginia, Pennsylvania, Oregon, and Rhode Island) between 1998 and 2008. In New Hampshire and Rhode Island, legal changes made payday lending economically feasible and the lenders entered these states. In the other cases, the legal and policy changes led all or most payday lenders to exit the markets.[5]

Morgan and Strain study the effect that these changes in access to payday loans had on: (1) quarterly changes in the percentage of bounced checks per 100 checks processed, (2) monthly

[5] I worry that Morgan and Strain's classifications may not be precise. Some payday lenders continued to operate in Maryland after its June 2002 restrictions using "credit service organizations" such as is done in Texas, and some continued to operate in Oregon after it reduced the fees they could charge. Since payday lenders operate in Virginia, it is unclear whether the restriction in D.C. significantly limited D.C. residents' access to the loans. Finally, the FDIC ruling that restricted access to payday loans in Pennsylvania also affected access in some other states, which could cause problems with Morgan and Strain's comparison group.

changes in complaints filed with the Federal Trade Commission (FTC) against lenders and debt collectors per 100,000 state residents, and (3) quarterly changes in Chapters 7 and 13 bankruptcy filings per 10,000 state residents. A Chapter 7 bankruptcy filing discharges all non-exempt debts, while a Chapter 13 filing is a plan to repay the debts, or a portion of them, over time. The researchers believe these outcome measures are good indicators of personal financial stress.

Morgan and Strain use data from 50 states and the District of Columbia as well as data from 43 Federal Reserve regional check-processing centers to estimate the effects of changes in access to payday loans on the dependent variables listed above. The key assumptions are that access to payday loans did not change in other states in the relevant time period and that the authors are able to control for other factors that could account for differences in the trends in the dependent variables across the states.

For each of the dependent variables, Morgan and Strain run a regression of the following form:

$$Y_{st} = a + a_s + a_t + bX_{st} + \gamma \textit{Payday Permitted}_{st} + e_{st}$$

where Y_{st} is the dependent variable for state s at time t, a is a constant, a_s and a_t are estimates of the fixed differences across states and periods of time, and X_{st} is a vector of state economic conditions (log income per capita, annual per capita income growth, the unemployment rate, and in some models, the lagged value of the dependent variable). *Payday Permitted* is a dummy variable that equals one if state s allows payday lending at time t, and zero if not.

Table 2

Dependent Variable	Estimated Coefficient on *Payday Permitted*
Chapter 7 bankruptcy	-0.066 (0.28)
Chapter 13 bankruptcy	0.190 (0.093)
Complaints against lenders	-0.0484 (0.028)
Complaints against debt collectors	-0.139 (0.069)
Percentage of checks returned	-0.101 (0.052)

These results are from regressions with the lagged value of the dependent
variable on the right-hand side. Standard errors are in parentheses.

Using this approach, the authors find that access to payday loans is not statistically signif-

icantly associated with Chapter 7 bankruptcy filings, but it does have a positive and statistically

significant association with Chapter 13 filings when the right-hand side includes the lagged value

of the dependent variable (Table 2). They find that access to payday lending has a negative and

statistically significant association with FTC-filed complaints against lenders and debt collectors.

Finally, access to payday lenders has a negative and statistically significant association with the

percentage of checks that are returned for insufficient funds. In this last case, the regressions ex-

clude the payday loan changes in Rhode Island, New Hampshire, and Washington, D.C., since

these regions are not closely tied to one of the Fed's regional check processing centers.[6]

Morgan and Strain conclude that households may be more likely to file for Chapter 13

bankruptcy if they have access to payday loans than if they don't, they may be less likely to com-

plain about lenders and debt collectors, and they may be less likely to write bounced checks.

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[6] As the authors note, several of the Fed's check processing centers process checks from more than one state, including some of the states that changed access to payday loans. This dilutes the authors' ability to detect the effect of the change on returned checks.

The authors hypothesize that this could mean that access to payday lending helps households switch from informal bankruptcy, where people simply do not pay bills and are subject to collectors' pressures, to Chapter 13 bankruptcy, where payment plans are established and collection calls are supposed to stop. They also propose that access to payday lending may enable people to substitute away from high-cost bank overdrafts when facing cash shortfalls to lower-cost payday loans.

In an ambitious empirical project, Adair Morse (forthcoming) uses zip-code-level data from California to estimate the welfare effects of access to payday loans subsequent to a natural disaster. In her study she measures welfare by the change in mortgage foreclosures rates and changes in incidences of property crimes within a zip code. She compares these changes for zip codes in which payday lenders exist and zip codes in which they do not. But she recognizes that payday lenders decide where they will locate partly based on the socio-dynamics of a community, so a simple DD measure of foreclosures, for example, across communities with and without payday lenders could be misleading. Her solution is to study the difference in the difference-in-differences. That is, she first matches communities that are similar in key aspects. In each match, one of the communities has at least one payday lender and the other does not. In a second step, among these matched communities, she identifies a subset that experienced a natural disaster of a specified minimum magnitude between 1996 and 2002 and a subset that did not. She estimates the welfare impact of access to payday lenders in the face of a natural disaster by examining the difference in the change in home mortgage foreclosures or crimes between communities with and without payday lenders that were not hit by a disaster and communities with and without payday lenders that were hit by a disaster.

For this to work, two assumptions must hold. First, absent the disasters, the change in welfare between the disaster-hit communities with and without payday lenders would have been the same on average as the change in welfare between the matched non-disaster communities with and without payday lenders. Second, one must assume that payday lenders do not intentionally or unintentionally tend to locate in communities that are more resilient to natural disasters.

In applying this concept, Morse begins by estimating the percentage of people in each California zip code who are likely to be credit constrained using demographic data from the zip codes and a statistical model that links such data to credit constraints based on the Survey of Consumer Finances. Then Morse identifies which zip code districts were subject to natural disasters (floods, landslides, wildfires, or storms) that inflicted significant property damage between 1996 and 2002 and which ones had payday lenders at the time of the disaster and for some time subsequent to the disaster. For the communities hit by disasters, she finds a matching non-disaster community based on its estimated propensity to be credit constrained and whether it has a payday lender.

Using these data, she runs a series of regressions, depending on whether the dependent variable is the change in rates of foreclosures or changes in property crimes and what variables she includes on the right-hand side. In Table 3, I present just one set of her results that she emphasizes. In this regression, the dependent variable is the change in quarterly foreclosures per 1,000 owner-occupied homes one year before the natural disaster and four to seven quarters after the event. The change is measured for the zip code experiencing the disaster and for its matched zip code district that did not experience the disaster. In the regression, Morse also includes control variables for the extent of property damage, the change in home prices, the change in pay-

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rolls, and the change in number of business establishments as well as several interaction terms

for these variables. The table does not include the coefficients for these controls.

Table 3

Explanatory Variables	Estimated Coefficient
Disaster present	1.088**
Payday lender present	0.136
Lender & disaster present	-0.458***
Observations	2,292
R-Squared	0.110

*significant at 10% level   ** significant at 5% level   *** significant at 1% level

Between 1996 and 2002 for the California zip codes in her sample, the mean number of

foreclosures per 1,000 owner-occupied homes pre-disaster was 2.4 per quarter. The coefficient

on *disaster present* is 1.1, implying that a disaster in a zip code without payday lenders raises

foreclosures in a community from about 2.4 per 1,000 households prior to the disaster to about

3.5 per quarter in the four to seven quarters after the disaster. The coefficient on *payday lender*

present is positive but not statistically significant. The positive coefficient, as noted above, may

simply signal that lenders tend to locate in communities where people are financially stressed.

The coefficient on *lender and disaster present* is negative and is the focus of Morse's analysis. It

implies that if a disaster happens in a zip code with payday lenders, the quarterly number of fo-

reclosures per 1,000 owner-occupied homes rises about 0.65 rather than the 1.1 in disaster com-

munities without payday lenders.

As Morse notes, her results do not technically address the big question, i.e., do payday

lenders, on net, exacerbate or relieve customers' financial difficulties?   But they certainly do

support a narrower conclusion — the presence of payday lenders in a community helps some

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homeowners cope with unexpected financial distress, whether caused by a natural disaster or some other event.

Scott Carrell and Jonathan Zinman (2008) exploit a unique data set in an effort to answer the big question. As with other researchers, Carrell and Zinman recognize that a central problem in attempting to answer this question is that payday lenders and financially distressed people might choose to locate near one another. If so, one would find that people with convenient access to payday lenders tend to be financially stressed, but his correlation may not reflect causation.

Carrell and Zinman try to avoid this problem using the observation that postings across states of enlisted Air Force personnel are roughly random. In other words, the personnel, whom Carrell and Zinman refer to as "airmen" for brevity, even though they include women, do not get to choose where they work. In addition, Carrell and Zinman note that several states with Air Force bases made significant changes to their payday loan laws between 1995 and 2007, effectively driving out the lenders or inviting them in. Carrell and Zinman use the variation in access to payday loans caused by changes in state laws and by military postings to test whether access to payday lending affected the job performance of enlisted airmen.

The outcome variables that they focus on are three measures of military personnel performance and retention for all enlisted airmen stationed at 67 domestic Air Force bases in 35 states for the time periods 1996-2001 or 1996-2007. The time periods varied depending on data availability. The three outcome measures are forced enrollment into a weight management program (WMP), the presence of an unfavorable information file (UIF), and reenlistment eligibility. Prior to 2004, airmen who were judged to be too heavy to be fit for military duty were required

to enter a WMP. An airman with a UIF has been sanctioned for severe misbehavior at some

point in his/her career. Such behavior could include civilian or military court convictions, letters

of reprimand, confirmed incidents of sexual harassment, or financial irresponsibility. Reenlist-

ment eligibility depends on satisfactory job performance.

The airmen are in various occupations within the Air Force and are in for various enlist-

ment terms. For each military base and in each year, the authors obtained data on who is eligible

to re-enlist, who has a UIF, and who must enter a WMP. The data are not for individuals, but for

small groups of airmen, clustered by occupational code and enlistment term.

Using these data, the authors estimate the following equation:

$$\Pr\left(Outcome_{ijbte}\right) = \beta_0 + \beta_1 Payday_{st} + X_{jbt}\beta_2 + \gamma_b + \varphi_{jte} + \varepsilon_s$$

The dependent variable is the probability of a specific outcome (such as reenlistment eligibility)

for individual *i*, in occupation *j*, on base *b*, in year *t* and enlistment term *e*. The probability is

based on the reported outcome for the members of the group. That is, if there are 5 members in

the group and 3 are eligible to enlist, each member of the group has a 3/5 probability of being

eligible to reenlist. *Payday* is a dummy variable that indicates whether the laws in the state were

favorable for operations of traditional payday loan stores at that time. The *X* vector includes data

on the group's standardized test scores, incomes, and the characteristics (average rent, unem-

ployment, etc.) of the economy around the base; γ_b is a base fixed effect; and φ_{jte} is a fixed effect

for specific occupations, time periods, and enlistment terms.

The estimates for the key coefficient of interest (*B₁*) are given in Table 4:

Table 4

Outcome measure →	Reenlistment Ineligibility	Unfavorable Information File	Weight Management Program
All enlistment terms	0.0111**	0.0019**	0.0013
First term only	0.0189*	0.0034*	0.0023
Second term only	0.0079	0.0010	-0.0014
Third term or higher	0.0049	0.0005	0.0011
Years of data	1996-2001	1996-2007	1996-2007

* significant at the 10% level   ** significant at the 5% level

These results imply that access to payday loans increased reenlistment ineligibility by 1.1 percentage points. Among all airmen in the data set, 28 percent were categorized as ineligible to reenlist, so this estimate implies that access to payday lending increases reenlistment ineligibility to 29.1 percent. The second column indicates that access to payday lending increased the likelihood of a UIF by 0.19 percent. In the full data set, 3.6 percent of the airmen have UIFs. The effect of access to payday lending on referral to the WMP is statistically insignificant. The results in rows two through four suggest that most of the results are driven by the effects on first-term enlistees, who tend to be the youngest enlistees. The authors examine similar results for several subgroups of data. I refer the reader to the paper for details.

Brian Melzer (forthcoming) also uses differences in the availability of payday loans across the states to assess the impact on people's financial well-being. He recognizes that states that restrict payday lending could differ in a number of dimensions from states whose regulations permit the industry to operate and that such differences could affect the financial well-being of families for a variety of reasons. Melzer accounts for this problem by contrasting the well-being of people living in restrictive states, but near a permissive state, with the well-being of people in a restrictive state who do not live near a permissive state. Specifically, Melzer focuses on the res-

idents of three restrictive states: Massachusetts, New York, and New Jersey. Because of state laws, there were no payday loan stores in these states during the period that Melzer studies, 1996-2001. But each of these three states bordered states where payday loan stores were present, at least for a part of this period. Changes in New Hampshire's laws enabled payday lenders to begin to operate in that state in January 2000 and some payday lenders operated in Rhode Island in 2000 and 2001, although they were not technically legal until mid-2001. These states share borders with Massachusetts. In Pennsylvania, payday loan stores began to appear in 1997. This state shares borders with New York and New Jersey.[7]

The data that Melzer uses to assess people's financial well-being come from the Urban Institute's National Survey of America's Families (NASF). The NASF surveyed approximately 42,000 nationally representative households in 1997, 1999, and 2002. The data include only the names of the county in which the households reside for counties with populations over 250,000.

Melzer considers that a household in a loan-prohibiting state had access to payday loans if it resided near a loan-allowing state. Given this assumption, his basic approach is simple. He uses responses from the NSAF to create two composite indices. The first is a variable that equals one if the respondent reported any one of several indicators of family financial stress (could not pay rent or bills in past year, had to move for financial reasons, had to reduce meals for financial reasons, or had to do without telephone service) and zero otherwise. The second is a variable that equals one if the respondent reported that he or she had delayed any type of medical treat-ment due to a lack of insurance or money, and zero otherwise.

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[7] Delaware, which shares a border with New Jersey, also permitted payday lending, but Melzer's data did not identi-fy which respondents lived in that region of the state.

The question Melzer asks is whether respondents are more or less likely to report these indicators of family or personal hardship based on their access to payday loans, controlling for a host of other factors. To do this, he specifies a probit regression model of the following form:

$$\Pr(Outcome_{ijt}) = \Phi(\alpha + \beta \, PaydayAccess_{jt} + \gamma X_{it} + \delta Z_j + \eta_t + \varepsilon_{ijt})$$

In this equation, $Outcome_{ijt}$ is an indicator of financial hardship for family or person i, living in county j at time t. $PaydayAccess$ is a binary variable that equals one if the households or persons in county j have access to payday loans in year t and zero otherwise. X and Z are vectors that contain a wide range of control variables for the characteristics of the households and the counties, including a dummy variable that equals one if the county is within 25 miles of a state border. η_t captures state-year fixed effects.

In the three loan-prohibiting states, Melzer assumes that if the geographic center of a county is within 25 miles of a state where payday lenders were active, the residents of that county had access to payday loans, since they can drive across the border. Residents in more distant counties are assumed not to have had access to payday loans. A key, and reasonable, assumption is that people do not choose where they live within a state based on whether they will be able to drive easily across a state border to get a payday loan. Similarly, they must not choose their location based on some other factor that happens to be correlated with payday loan access. In other words, for the residents of the prohibiting states, it is a random event (do they happen to live in a county near a state with payday lending) that determines whether they will have access to payday loans. Melzer notes that most payday loan customers come from households with annual incomes between $15,000 and $50,000, so he limits his analysis initially to survey respondents with household incomes in this range.

Table 5 presents Melzer's basic results. They suggest that households with incomes between $15,000 and $50,000 who had access to payday loans had a 34.3 percent likelihood of reporting a family financial hardship compared to 29.2 percent for similar households without access to payday loans. Similarly, individuals living in households within this income range who had access to payday loans had a 22.4 percent likelihood of reporting that they had delayed some medical treatment due to a lack of insurance or money compared to 17.9 percent for similar individuals who did not have access to payday loans. This suggests that access to payday loans markedly increases the financial stress of some households.[8]

Table 5

Dependent Variable	Unconditional Mean	Estimated Average Incremental Effect
Any reported family financial hardship	0.292	0.051***
# observations		24,998
Pseudo R^2		0.07
Any health care postponed	0.179	0.045***
# observations		17,581
Pseudo R^2		0.09

***Estimated coefficient on *Payday Access* in the probit model was statistically significant at 1 percent level.

Paige Skiba and Jeremy Tobacman (2008b) studied the effects of the use of payday loans on rates of personal bankruptcy using individual loan records supplied to these researchers by a large payday loan company. Amazingly, the company provided the researchers with complete data from over 1 million loan applications from 145,519 individuals who applied for the first time for a payday loan from this company at one of its outlets in Texas between September 2000 and August 2004. There are far more applications than individuals because many people applied for multiple loans over this time period.

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[8] To test the robustness of these estimates, Melzer reports the results from many specification variations that I cannot do justice to. All of the alternative specifications support his basic results.

As Skiba and Tobacman report, for first-time applicants, the company based its decision to approve or not approve the loan based almost exclusively on a credit score calculated by a third-party credit bureau, Teletrack. Among first-time loan applicants, 99.6 percent of those with scores below the threshold were rejected and 96.9 percent of those with scores above the threshold were approved. With names and Social Security numbers, Skiba and Tobacman matched the first-time loan applicants to records of personal bankruptcy filings in Texas using bankruptcy records from January 2001 through June 2005. Of the 145,519 first-time payday loan applicants, 2,705 filed for Chapter 7 (liquidation) bankruptcy and 5,626 filed for Chapter 13 during this time period.

The authors' research strategy is to contrast bankruptcy filings for payday loan applicants who are approved for a loan with those for applicants who are rejected.   The assumption is that if access to payday loans tends to create financial difficulties for people, then bankruptcy rates should be higher for approved applicants than for rejected applicants. As Skiba and Tobacman recognize, the fundamental problem with this exercise is that the people whom the company rejects for loans based on their credit score could differ in a number of ways from those the company accepted. The rejected applicants might, for example, have such bad previous credit records that almost no business would extend them credit, making it unlikely that they would need to file for bankruptcy. To address such issues, Skiba and Tobacman contrast the bankruptcy outcomes for applicants whose credit scores were somewhat above the rejection threshold with those that were somewhat below. Exactly how they define "somewhat" varies in different specifications, but the notion is that these applicants should have reasonably similar assets, incomes, and behavioral characteristics.

Another problem with this approach is that one does not know how long to wait after the initial loan approval or rejection to determine if the applicant filed for bankruptcy. A loan approval, even if it ultimately leads to financial difficulties, creates an instant cash inflow for the borrower, likely relieving immediate financial pressures. Moreover, borrowers can renew the loans, so it might take several weeks or months before the loan adds to the borrowers' financial stress. This argues for measuring bankruptcy outcomes a fairly long time after the initial loan. On the other hand, the longer the time period after the initial loan, the less convincing is the causal link between the loan and the bankruptcy outcome. Clearly, measuring bankruptcy outcomes very shortly after an initial loan or many years after the loan could be misleading, but there is no obvious guideline to indicate just how much time the researchers should allow to elapse between the initial loan and the bankruptcy measure.

Skiba and Tobacman confront these issues openly and present results for bankruptcy filings one year after the initial loan and two years after for applicants within various ranges of the approval threshold. Since they emphasize the results for the two-year lag, I discuss those results here. In the two years that follow applicants' first loan applications with the company, 2.7 percent (almost 4,000 applicants) filed for bankruptcy. As indicated in the table below, the only striking difference in bankruptcy filings between those below and above the loan approval threshold seems to be for Chapter 13 filings among households within 0.1 standard deviations of the loan approval threshold. Whether this, or any of the other differences, is statistically significant is left to the regression analysis discussed below.

Table 6

	All Applicants	Of applicants within 1 s.d. below approval threshold	Of applicants within 1 s.d. above approval threshold	Of applicants within 0.1 s.d. below approval threshold	Of applicants within 0.1 s.d. above approval threshold
Number of payday loan applicants	145,159	18,060	84,490	2,957	3,430
Percentage filing for Ch 7 bankruptcy within two years of application	≈0.72%*	0.47%	0.66%	0.51%	0.50%
Percentage filing for Ch 13 bankruptcy within two years of application	≈2.0%*	1.45%	1.77%	1.76%	3.18%

* Author's estimate, based on data in Table 3 of Skiba and Tobacman (2008b)

The authors recognize that factors other than being approved for a loan could affect bankruptcy filings, so they run a regression to control for the influence of other factors. Specifically, they estimate the following equation:

$$Bkcy_i^\tau = B_0 + B_1 Approved_i + f(CreditScore_i) + \gamma X'_i + \delta M'^t_i + \varepsilon_i$$

where $Bkcy^\tau_i$ is the cumulative number of bankruptcy filings (Chapter 7, Chapter 13, or both) for person i in τ amount of time, since his or her first loan application, $Approved_i$ is a dummy variable to indicate whether the loan was approved, $CreditScore_i$ is the applicant's Teletrack credit score, $f(CreditScore_i)$ indicates that the authors try various functional forms for how the credit score might affect subsequent bankruptcies, X'_i is a vector of the demographics and background characteristics of the loan applicant (gender, race, age, monthly income, number of bounced

checks, checking account balance, homeownership, use of direct deposit, pay frequency, job te-

nure, and months at current residence), M'^t_i is a set of dummies for month of first payday loan

application, and ε_i is the error term.

To address the problem that people above the approval threshold could differ systemati-

cally from those below, Skiba and Tobacman limit the analysis to individuals whose credit

scores are within 0.5 or 0.1 standard deviations from the credit-score-approval threshold. Their

regression results suggest that being approved for a payday loan increases the likelihood of a

Chapter 13 bankruptcy filing for individuals with credit scores within 0.5 standard deviations of

the approval threshold. The point estimate suggests that the loan approval increases the chances

of a Chapter 13 bankruptcy filing by about 2.5 percentage points in the subsequent two years.

This is a huge effect, given that only about 2.0 percent of all applicants file for Chapter 13 bank-

ruptcy in the two years following their initial loan application.[9] The effect is not statistically sig-

nificant for the smaller set of individuals within 0.1 standard deviations of the threshold and it is

not statistically significant for Chapter 7 bankruptcies.

IV. Limitations of the Quasi-Experimental Studies

These quasi-experimental studies reflect substantial and careful empirical work, but, in

my view, they do not provide a reliable answer to the big question: Do payday lenders, on net,

exacerbate or relieve customers' financial difficulties? For one, some of the studies find access

to payday lending is beneficial and some find it harmful. But more important, the results of each

of the studies are simply suggestive; that is, they are based on at least one of two strong assump-

tions that may well not be true and therefore cast doubt on the reliability of the results. First, as

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[9] Moreover, some of the rejected loan applicants, especially those near the threshold, undoubtedly got loans from other payday lenders. This reduces the ability of the authors' methodology to detect a loan effect, since they have data from only the one lender.

noted earlier, the researchers must assume that the relevant changes in outcomes are driven by changes in access to payday lending and not something else. Second, they must assume that the people who have access to payday loans are, on average, similar to those who do not. The studies discussed above differ in the degree to which they are exposed to these two problems, but in my view, all are sufficiently exposed that we cannot have substantial confidence in the results of any of the studies.

Zinman (2010), for example, contrasted pre-ban and post-ban differences in unemployment and payday loan customers' optimism about their financial futures in Oregon, a state that sharply restricted payday lending, to the differences in Washington, a state that did not. The problem is that there are numerous good reasons why the trends in these states might diverge. Zinman (p. 549) addresses this issue briefly, writing, "Oregon and Washington are neighboring states that were on similar economic trajectories at the time of the surveys: both states had experienced four consecutive years of employment growth, and both states forecasted a flattening of employment rates for the latter half of 2007." But he would need to recount the histories of events in the two states in much greater detail than this to convince me that Oregon's payday loan restrictions accounted for the differences in differences. For example, the recession that began in late 2007 hit Oregon harder and earlier than it did Washington. In January 2007, Oregon's unemployment rate was 5.1 percent; by January 2009, it was 9.9 percent. In January 2007, Washington's unemployment rate was 4.6 percent and it had risen to 7.5 percent by January 2009. It is certainly possible that this difference accounts for the different trends that Zinman observed. It is also possible that Vancouver's preparations for the 2010 Winter Olympics affected people in Seattle but not Portland.

A similar point applies to the Morgan and Strain (2009) study. As noted above, they find that, relative to changes in other states, Chapter 13 bankruptcy filings increase, complaints against debt collectors and lenders per capita decrease, and the number of returned checks per 100 checks processed decrease when people have access to payday loans. The authors try to control for divergent economic paths across states by controlling for state unemployment rates and per capita income. But without an empirical model of what drives changes in bankruptcy rates, NSF rates, or FTC complaints, it is hard to know what to control for in the regressions. It could be, for example, that NSF rates are largely unrelated to per capita income levels but are correlated with the concentration of banking because checks drawn on the same bank clear more quickly. If so, Morgan and Strain should have controlled for changes in bank concentrations across the states. One can easily imagine similar issues for bankruptcy rates and FTC complaint patterns. Moreover, one has to worry that the change in the political climates in the states that altered their payday loan regulations also tended to be associated with other regulatory changes that could have affected the dependent variables in Morgan and Strain's regressions.

Morse (forthcoming) found that if a natural disaster happens in a zip code with payday lenders, the quarterly number of foreclosures per 1,000 owner-occupied homes rises about 0.65 rather than the 1.1 increase in disaster communities without payday lenders. I wonder, however, if payday lenders look for some type of business location, such as low-cost rentals in 1960s strip shopping malls, that happens to be correlated with resilience in the face of disasters, perhaps because in older communities more homes have very modest mortgages. In one of the robustness tests of her results, for example, Morse includes a dummy variable for whether a McDonald's restaurant was present in the zip code and interacts this dummy with a dummy for whether a disaster occurred. The coefficient on the interaction term is negative and statistically significant,

which one could interpret as meaning that the presence of McDonalds helps to mitigate home foreclosures following disasters. Morse interprets this result as indicating that service-oriented communities re-trench more quickly. I suspect that McDonalds may seek similar types of locations as do payday lenders and that in neither case do the interaction coefficients indicate that McDonalds or payday lenders mitigate foreclosures. It is also possible that Morse found a chance correlation between payday lender locations and changes in foreclosures that comes from trying a number of different welfare measures. For example, when Morse uses changes in vehicle thefts or burglaries as measures of community welfare, she does not find a statistically significant link to the presence of payday lenders in a disaster, but she does when the dependent variable is changes in larceny rates.

Carrell and Zinman (2008) found that that airmen's access to payday loans increased reenlistment ineligibility and increased the likelihood of an unfavorable information file. Melzer (forthcoming) found that among households with incomes between $15,000 and $50,000 access to payday loans increased the likelihood of reporting a family financial hardship and increased the likelihood of delaying some medical treatment due to a lack of insurance or money. In both cases, as in the cases above, I worry that the results may be shaped by other factors. Airmen may be randomly distributed around the country, but payday lending is not. The availability of payday lending is shaped by state laws, and states that permit payday lending likely differ in systematic ways from states that don't. Suppose, to cite one hypothetical example, states that permit payday lending also tend to permit gambling. In that case, Carrell and Zinman's results could pick up the effect of the availability of gambling, not payday lending. In Melzer's case, his results depend on the assumption that people in New Jersey who live near Philadelphia or people in Massachusetts who live near New Hampshire or Rhode Island are subject to the same general

economic forces as people in the populous counties in the rest of these states. That may not be true. Boston and New York City could prosper, for example, without that prosperity spreading to the counties farther removed from these cities. Melzer shares this concern, which is why he includes data on unemployment rates and household incomes for the counties, but these variables may not capture all of the factors that affect the well-being of families across the counties. To address this issue, Melzer tests whether his results hold for families with incomes below $15,000 and above $50,000, families who mostly do not use payday loans. In this case, the estimated coefficient on *Payday Access* is statistically insignificant. I suspect, however, that his sample size in the treatment counties for families with incomes below $15,000 must be quite small. If so, testing his results for these families is likely to yield a statistically insignificant result; that is, it is certainly possible that low- and moderate-income families in Camden, NJ, and Lowell, MA, for example, were subject to more financial distress than similar families in other regions of these states for reasons other than their access to payday loans, and that this might not be captured by county unemployment rates or household income levels.

The second danger that these quasi-experimental studies run is that the people who are subject to the treatment (gaining or losing access to payday loans) differ systematically from those who are in the comparison group and this could cause differences in their trajectories over time that are unrelated to the treatment. In Melzer's study, for example, the New Jersey households who have access to payday lending live near Philadelphia and the Massachusetts households live near New Hampshire or Rhode Island. Low-wealth and financially unstable households might, for example, tend to live in Camden, New Jersey, Lawrence, Massachusetts, or other deindustrialized cities with inexpensive housing and public transportation. If it is true that these border areas of the two states tend to attract low-wealth or financially unstable families,

this could explain Melzer's results. Such families might be subject to different economic trends than families in other parts of the states.[10]

A similar criticism applies to Skiba and Tobacman's (2008b) regression results, which suggest that being approved for a payday loan increases the likelihood of a subsequent Chapter 13 bankruptcy filing. The problem is that applicants with high credit scores are likely to be systematically different from applicants with low scores in both observable and unobservable ways. The closer the credit scores are to each other, the less significant is the problem. But this leaves Skiba and Tobacman with a problem. If they focus on the 6,387 applicants within a 0.1 standard deviation of the credit-score-approval threshold and control for observable differences, differences in bankruptcy outcomes are not statistically significant. If they focus on the larger group of applicants within a 0.5 standard deviation of the credit-score-approval threshold, the differences in Chapter 13 bankruptcies are statistically significant. But as they include more applicants farther away from the approval threshold, it becomes more likely that these applicants differ in systematic ways from each other. It could be, for example, that applicants with higher credit scores have more debts with traditional lenders or more assets to protect. If so, when they face financial difficulties they may be more likely to file for Chapter 13 bankruptcy, while appli-

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[10] I also worry about the accuracy of Melzer's classification of people's cross-border access to payday loans. Melzer classified the residents of 10 counties in Massachusetts, New Jersey, and New York, whose geographic centers are within 25 miles of a payday-loan-permitting state, as having access to payday loans, but he does not name the counties. Since he has identifiable data only from counties with populations over 250,000, all of Melzer's New York observations must come from Orange County, New York. A small part of this county is adjacent to Pennsylvania, but to a very rural part of Pennsylvania that would be unlikely to host payday lenders. Melzer classifies the residents of four western New Jersey counties as having access to Pennsylvania payday loan stores in 1998. But payday lending first appeared in that state in 1997, and it is unclear how many lenders were active in the Philadelphia area one year later. Melzer apparently classified the residents of three Massachusetts counties that border New Hampshire or Rhode Island as having access to payday loans in 2001. As Melzer notes, payday lending was not technically legal in Rhode Island until July 2001, but some check-cashers were offering the service in that state beginning in 2000. In addition, New Hampshire laws permitted payday lending beginning only in 2000. It is unclear how many lenders were active in these states by 2001 and Melzer provides no data.

cants with lower credit scores simply ignore their creditors, who also ignore them since there is almost nothing to gain from someone with few assets, little income, and a terrible credit history.

Holding constant the quality of the econometrics, I'm most suspicious that an author's results may be shaped by omitted variable problems or differences among the characteristics of the treatment and comparison groups when the logical links between the treatment and the measured outcomes are tenuous. If someone finds that payday lending has an impact on credit scores, this has a logical connection. But if someone finds that payday lending has a statistically significant positive correlation with average adult heights, for example, I suspect a chance correlation rather than conclude that payday lending causes people to be tall. Some of the studies discussed above involve outcome measures that, in my view, have weak logical connections to payday lending.

As noted above, Morse found that if a natural disaster happens in a zip code with payday lenders, the number of quarterly foreclosures per 1,000 owner-occupied homes rises about 0.65 rather than the 1.1 in disaster communities without payday lenders. While I can imagine that payday lending could be beneficial to some people in emergencies, it is just not plausible to me that easy access to a high cost $250 two-week cash advance significantly offsets disaster-related foreclosures.[11]

Carrell and Zinman (2008) found that airmen's access to payday loans modestly increased reenlistment ineligibility and the likelihood of an unfavorable information file (UIF), but it is unclear why access to payday loans might cause such changes. It is possible that payday loans cause financial stress, which leads to misbehavior and poor job performance, but this is not obvious. Does one misbehave more when one has financial worries or when feeling carefree, or

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[11] In California, the face amount of the check written to repay the loan cannot exceed $300. Since lenders are allowed to charge a $15 finance change per $85 advanced, most lenders will not advance more than $255.

are financial concerns unrelated to behavior?[12] Without data supporting a link between financial problems and UIFs or reenlistment ineligibility, I'm reluctant to conclude that the statistical correlation between state payday loan laws and these outcome measures reflects causation.

V. Other Approaches

In a paper that I much admire, Paige Skiba and Jeremy Tobacman (2008a) use the large data set that a payday lender provided them to fit three nested models of customer decision making. In all of the models, the customers have fluctuating incomes and high discount rates and are risk adverse, so they borrow from payday lenders (the only lenders in the model) to smooth consumption over time. For a given cost of default, Skiba and Tobacman estimate the parameters of the model whose simulations most closely match the lender's actual data on the percentage of customers borrowing in each pay period subsequent to their first loan, the average loan size conditional on borrowing, and the percentage of customers who default in a given pay period.

When they model the customers as exponential discounters who are paid biweekly, they find that a two-week discount rate of 0.82 best fits the data. This implies that a typical customer would rather have $1 today than $1.21 in two weeks or $1 today than nearly $175 one year from now. They also estimate the parameters of a model where customers are "sophisticated" quasi-hyperbolic discounters (meaning, the customers discount near-term two-week trade-offs more heavily than more distant two-week trade-offs, and they know this) and where the customers are naïve quasi-hyperbolic discounters (the customers discount near-term two-week trade-offs more heavily than more distant two-week trade-offs, but they believe that they will stop doing this and

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[12] In fairness to the authors, a UIF could mean that an airman has been sanctioned for financial irresponsibility, or he or she was sanctioned for a civilian or military court conviction, a letter of reprimand, or a confirmed incident of sexual harassment. The authors do not provide data indicating what percentage of UIFs involve financial irresponsibility.

become exponential discounters as soon as the current two-week period elapses). If customers use payday loans only because they are hyperbolic discounters, then their lifetime utility would be higher if the loans were banned.

The authors' graphs of the simulated values from the models indicate that all three models do a roughly similar job of fitting the data with respect to the percentage of customers who borrowed in each pay period in the year following their initial loan. Generally they fit the data well, but they over-predict borrowing a few pay periods after the first loan and under-predict borrowing a year after the first loan. The three models do not do a particularly good job fitting the average loan size pattern. All three under-predict the size of the initial loan by about one-third. In the year following the initial loan, the three models continue to under-predict the actual loan size, but the gap is smaller. The three models also fail to fit closely the observed default pattern. About 12 percent of first-time borrowers default. The models predict 3 to 5 percent. At the 15th pay cycle following the initial loan, the models predict that 14 to 17 percent of borrowers would default, whereas the data indicate that about 7 percent do.

This paper is a creative attempt to use a rich data set on loan customers to understand the thought process that leads to taking out and renewing payday loans, but no one model does a clearly superior job of fitting the data. In addition, reasonable minor variations on the models, some of which the authors discuss, might improve the models' ability to fit the behavior of the average customer, or of subsets of customers. Perhaps the default rate on first-time loans is 12 percent because a subset of customers feel no default remorse, can't be reached by collection calls, etc. To these individuals, the first-time loan is nearly a free one-time grant. Allowing for such heterogeneity in the population might improve the fit of the models. Of course, creating

multiple free parameters for different subsets of the population is bound to improve the fit of the model, but it makes the estimation of the free parameters more difficult and less precise.

My own view is that even the authors' rich data set won't distinguish clearly among the various models of human decision making or among multiple enriching variations on these models. It could be, to cite one possible variation, that many borrowers start out as perfectly naïve hyperbolic discounters, giving in to instant gratification but thinking that they will not do so in the future. As they repeatedly give in over time, however, they become sophisticated hyperbolic discounters. Using the authors' data to fit numerous reasonable theories of human behavior, especially for subpopulations, would be a daunting task.

Bart Wilson and his four co-authors (2010) study the welfare effects of payday loans in a laboratory simulation in which 318 undergraduates played a money management game with and without a payday loan option. Space constraints prevent me from discussing any of the details of the game, but the authors found that access to payday loans benefited more players of the game than it harmed, although the benefit was small and the authors did not test for statistical significance. This is an interesting approach to studying the welfare effects of payday loans, but I suspect it does not capture the factors emphasized by critics who argue that payday lending entraps people who heavily discount the future cost of repaying the loans. Participants in the game were trying to maximize their winnings from playing the game over a period of about an hour. Some participants might have overused the loans in the game and been penalized for it, but this type of strategic mistake in a one-hour game may not be equivalent to the myopia that critics argue entraps many payday loan customers.

VI. Suggested Directions for New Research

I have argued that despite the research efforts of a talented group of economists, we still don't know the answer to the big question: Do payday lenders, on net, exacerbate or relieve customers' financial difficulties?

I'm not sure that we will ever get a definitive answer to this question, but I suspect that we will get stronger indications from experimental approaches rather than quasi-experimental or simulations. In an ideal experiment, one would randomly grant payday loans to a group of applicants and randomly deny the loans as well as close substitutes to a similar group of applicants. One would then track indicators of financial stress over time across the two groups. The advantage of this approach is that one does not worry about other factors causing any observed differences in outcomes, since, presumably, the two groups have the same average exposure to intervening outside factors. One would also not worry that the average characteristics of the two groups differ at the outset of the experiment, since random assignment of a large enough set of applicants should ensure that they are similar.

No one has done such a study to date, since, I presume, it would be difficult to obtain the cooperation of a payday lender and it would be difficult to ensure that randomly rejected applicants do not simply borrow from another payday lender or use some close substitute, such as checking account overdrafts. But one South African study suggests how such a study might be done. In a recently published paper, Dean Karlan and Jonathan Zinman (2009) report the results from an experiment implemented in 2004 by a firm that makes small high-cost unsecured cash loans to low- and moderate-income individuals in that country. The lender's standard loan for a first-time borrower was a four-month installment loan with an APR of about 200 percent. In the

experiment, a computer randomly flagged for approval a set of applicants who would normally be denied loans, since they were modestly below the lender's approval threshold. Branch managers were told that a computer algorithm suggested these applicants should be approved. The managers could override the computer's approval decision and did so in about half of the cases. Among similar applicants randomly flagged for denial, the managers denied loans to almost all. Since loan officers ultimately made the decision whether to approve an application, to eliminate this element of discretion, the researchers compared the outcomes for the marginal applicants that the computer assigned for approval to those that it rejected, regardless of the loan officer's decision.

The researchers found that being flagged for approval had a positive and statistically significant effect on a consumption index, economic self-sufficiency index, and a control and outlook index, where these indices were constructed by the researchers based on the participants' answers to a series of post-application survey questions conducted six to 12 months after the loan application. Being flagged for approval had a negative and statistically significant effect on a mental health index. Karlan and Zinman also obtained credit scores from a credit bureau on all 787 marginal loan applicants in the study about 13-15 months and 25-27 months after the initial loan application. They found that being flagged for approval had no statistically significant effect on the marginal applicants' credit scores.

This study has no clear implications for payday lending in the U.S., since it was conducted in a different social and institutional context, but it does suggest how researchers in the U.S. might be able to obtain the cooperation of a payday lender to conduct an experiment. Rather than randomly rejecting applicants who would normally be approved, something most lenders would hesitate to do, the lender might randomly approve marginal applicants who would

normally be rejected. Assuming that the rejected applicants do not find an alternative credit source that is a close substitute, comparing post-application welfare measures of the rejected and approved marginal applicants could be informative.

A second fruitful approach that might help answer the big question would be ethnographic studies that carefully follow the budgeting decisions and thought processes of payday loan customers and their households over time. Such studies would necessarily have to be small scale and could be criticized for inevitable subjective data filtering by the ethnographers, but they could also offer rich insights to complement the traditional econometric and experimental approach of economists.

There is a third line of research related to the big question. It concerns the effects that consumer financial education can have on the demand for payday loans. Despite many calls for financial literacy education in the schools and elsewhere, we have very few random assignment studies of the effects of such education on people's behavior. If educational initiatives significantly reduced the demand for payday loans because they help people build savings and improve credit histories or because they make customers conscious of the high cost of payday loans and the risk of repeated renewals, this would suggest that financial education can raise people's welfare. In one recent experimental study (Bertrand and Morse, 2010) with a cooperating payday lender, researchers devised a very low-cost effort intended to highlight the cumulative dollar cost of repeated borrowing to the lender's customers. Members of the treatment group had a 0.48 probability of borrowing in one of the loan cycles in the four months following the educational treatment. Members of the control group had a 0.54 probability. Unfortunately, it is unknown whether the customers turned to other credit sources, whether the education had a long-lasting effect, or exactly why it influenced customers' behavior.

An interesting area for research around payday lending that does not concern the big question is the effect that the recession of 2007-2009 had on the industry. Journalists commonly assume that recessions must be good for lenders specializing in serving people with bad credit histories and few assets. But in earlier research (Caskey, 1994) I found a large decline in the number of pawnbrokers during the Great Depression. Pawnbrokers found that they could not sell the collateral people defaulted on, which led them to reduce the amount they lent on pledged items. With lower sales and lower loan balances, pawnbrokers' incomes declined and many went out of business. Payday lenders might have suffered from the 2007-2009 recession for similar reasons: customers' incomes were lower, many lost employment, and perhaps defaults were higher. At the time of this writing, however, available data from state regulatory agencies and publicly traded payday loan companies do not give a clear answer to this question.

My last suggested fruitful research topic is to focus on other financial services intended to serve low- and moderate-income households that have been relatively overlooked in recent years. These include bank overdraft protection programs, rent-to-own, title lending, and last-minute bill payment services.

My conclusion is both discouraging and encouraging. Despite major efforts by some talented economists, we still don't know the answer to the big question: Do payday lenders, on net, exacerbate or assuage customers' financial difficulties? But this also means that there is an important public policy question for empirically oriented economists to tackle.

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