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Payday Lending: New Research and the Big Question

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Abstract and Keywords

Payday lending is controversial. In the states that allow it, payday lenders make cash loans that are typically for $500 or less, and the borrower must repay or renew the loan 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. This article describes the payday-lending business and explains why it presents challenging public-policy issues. It surveys recent research that attempts to answer the “big question,” one that is fundamental to the public-policy dispute: Do payday lenders, on net, exacerbate or relieve customers' financial difficulties? The article argues that despite research efforts of a talented group of economists, we still don't know the answer to the big question.

Keywords: payday lenders, money lenders, cash loans, public-policy issues

1. Introduction

Payday lending is controversial. In the states that allow it, payday lenders make cash loans that are typically for $500 or less, and the borrower must repay or renew the loan 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 (APR) is about 400 percent.

In this chapter, I briefly describe the payday-lending business and explain why it presents challenging public-policy issues. The heart of this chapter, 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 payday loan are straightforward, 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 nonsufficient funds (NSF) fee for each check that overdraws my account and a $15 returned check fee for each (p. 682) 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 fees to reconnect the services. These fees 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. People could focus on the immediate benefits the cash 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 (Caskey 1994), about the time this type of business was developing. I have always avoided the big question, because I did not know how to answer it. In recent years, however, a number of talented economists have tackled it. I argue in this chapter 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 implications, so it is important to sort out the reasons why people choose these loans. I conclude with a brief discussion about possible (and 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.

2. 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. Stegman (2007) wrote a prominent article surveying research on payday lending. As he noted, in 2007, estimates indicated that there were over 20,000 payday-loan stores nationally (p. 683) that were 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 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 what had been 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, each customer is allowed to have only one payday loan at a time. To enforce this regulation, the states maintain central databases in 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 state's adult population. In Oklahoma, 113,576 individuals borrowed from payday lenders from April 2008 through March 2009, or about 4.2 percent of the state's 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 paychecks, the loans would not be highly controversial. Critics of the industry emphasize not only that the loans are costly but also 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 and Management 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, “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” (75). In (p. 684) 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 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” (5). Data from Florida (Veritec 2009b) indicate that the average number of transactions per consumer from June 2008 through May 2009 was 8.4, but 30 percent of the customers in that year had 12 loans or more, and these 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 and accounted for 63.5 percent of the 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 gathered 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.[1] In addition, the information gained through household surveys is less reliable, because 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 per year, with somewhat higher household incomes. Many have at least a high school education. The customers are, relative to the US 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 and Management 2007: 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.

(p. 685) One explanation for payday loans that we can rule out is that people borrow from payday lenders because they don't know the overall 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 and Management 2007: 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.

3. 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 one million loans in 2006. Payday lending was also widespread in the neighboring state of Washington, where lenders made 3.5 million loans in 2006. Through 2009 they remained active in Washington, since the state's fee ceiling ($15 per $100 advanced with no minimum loan term) permitted them to operate profitably.[2]

Zinman (2010) used the change in the law in Oregon to study how cutting access to payday lending affected potential loan clients. As the Oregon Department of Consumer and Business Services 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 2010: 548).

Because 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 (p. 686) 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 before and after 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 21.1, after the Oregon reform there was a larger decline in the percentage of prereform customers who reported using payday or other short-term loans in Oregon than in 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.

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 recent 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 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" (553).

Morgan and Strain (2008) note that Georgia effectively banned payday lending in May 2004 and North Carolina did so in year-end 2005.[5] The authors study the effect that these bans had on (1) quarterly changes in the percentage of returned ("bounced") checks per 100 checks processed, (2) monthly changes in complaints filed with the Federal Trade Commission (FTC) against lenders and third-party debt collectors per 100,000 state residents, and (3) quarterly changes in Chapters 7 and 13 bankruptcy filings per 1,000 state residents. A Chapter 7 bankruptcy filing discharges all nonexempt debts while a Chapter 13 filing is a plan to repay the debts, or a portion of the debts, over time. The researchers believe that these outcome measures are good indicators of personal financial stress.

Morgan and Strain's research strategy is to compare the pre- and postban differences in the outcome variables in Georgia and North Carolina to the same differences in other states. 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. (p. 687)

Table 21.1 Summary of Zinman's (2010) Key Results

	Oregon Baseline	Oregon Follow-up	Washington Baseline	Washington Follow-up	Difference-in-Difference
Any payday loan in last three months	1	0.505	1	0.789	–0.284***
Used any short-term loan in last three months	1	0.570	1	0.830	–0.260***
Harder to get short-term loan in last three 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 six 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**

(*) $p < 0.10$

(**) $p < 0.05$

(***) $p < 0.01$

For each of the dependent variables, Morgan and Strain run a regression of the following form by using monthly or quarterly data from 1997 through early 2007:

$$DV_{st} = a + a_s + a_t + bUR_{st} + cGA + dNC + e(\text{Post-Ban}_{GA}) + f(\text{Post-Ban}_{NC})$$
$$+ g(GA x \text{Post-Ban}_{GA}) + n(NC x \text{Post-Ban}_{NC}) + e_{st}$$

where DV_{st} is the dependent variable for state *s* at time *t*, *a* is a constant, a_s and a_t are estimates of fixed differences across states and periods of time, and UR_{st} is the state unemployment rate at time *t*. The other variables are indicator variables, that is, $GA = 1$ if the data are from Georgia and zero otherwise. With this specification, *c* and *d* are estimates of the difference in the means for the dependent variable for GA and NC relative to the mean for all other states; *e* and *f* measure the difference in the means of the dependent variable before and after the GA ban and before and after the NC ban. The DD coefficients, *g* and *n*, are the key coefficients of interest. They measure the difference in the means of the dependent variable between the pre- and postban period for GA and NC relative to the other states controlling for the other factors on the right-hand side.

As shown in Table 21.2, the pattern with respect to changes in complaints against lenders relative to other states is mixed across the two states. Complaints against debt collectors increased in Georgia and North Carolina relative to changes (p. 688) in other states following the payday-loan bans. The number of returned checks per 100 checks processed increased relative to the change in other states. The number of Chapter 7 bankruptcy filings increased in the two states relative to the changes in other states but the number of Chapter 13 filings per capita fell. In all cases except one, the key coefficients are statistically significant at a 5 percent or higher confidence level.

Table 21.2 Summary of Morgan and Strain's (2008) Key Results

Dependent Variable	**Estimated DD Coefficient for Georgia**	**Estimated DD Coefficient for North Carolina**
Complaints against lenders (per 100,000 population)	0.02**	–0.03**
Complaints against debt collectors (per 100,000 population)	0.74***	0.23***
Returned checks (per 100 checks processed)	0.18**	0.14
Chapter 7 bankruptcy filings (per 1,000 population)	0.44**	4.03***

Chapter 13 bankruptcy filings (per 1,000 population)	–3.00***	–1.25***

(**) Significant at 5% level

(***) Significant at 1% level.

The authors interpret their results as supporting the conclusion that access to payday loans helps people to avoid bouncing checks and incurring the associated fees, helps people to avoid filing Chapter 7 bankruptcy, and helps people to avoid falling behind on other bills and having run-ins with aggressive debt collectors.

In a meticulous empirical project, Morse (2011) uses zip-code-level data from California to estimate the welfare effects of access to payday loans following a natural disaster. In her study, she measures welfare by changes in mortgage foreclosures and changes in property crimes within a zip code. She contrasts these changes for socioeconomically matched zip codes in which payday lenders exist and zip codes in which they do not.

Morse 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 across communities with and without payday lenders could be misleading. Her solution is to study the difference in the difference-in-differences. Specifically, she estimates the percentage of people in each California zip code who are likely to be credit-constrained by 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. 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 (p. 689) time of the disaster and for some time subsequent to the disaster. For the communities hit by disasters, she finds a matching nondisaster community based on its estimated propensity to be credit-constrained and whether it has a payday lender. She estimates the welfare impact of access to payday lenders in the face of a natural disaster by examining differences in changes in rates of 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.

Table 21.3 Summary of Morse's (2011) Key Result

Explanatory Variables	**Estimated Coefficient**
Disaster present	1.104***
Payday lender present	0.110
Lender and disaster present	–0.450***

Observations	2306
R-squared	0.098

(***) Significant at 1% level.

For this identification strategy 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 nondisaster 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.

Morse runs a series of regressions, depending on whether the dependent variable is changes in rates of foreclosures or changes in property crimes and what variables she includes on the right-hand side. In Table 21.3, 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 with and without payday lenders one year before the natural disaster and four to seven quarters after the event relative to the change in the nondisaster zip codes. In the regression, Morse also includes control variables for the extent of property damage, changes in home prices, changes in payrolls, and changes in the number of business establishments as well as several interaction terms for these variables. The table does not include the coefficients for these controls.

Between 1996 and 2002 for the California zip codes in her sample, the mean number of foreclosures per 1,000 owner-occupied homes before a 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 **(p. 690)** seven quarters after the disaster. The coefficient on *payday lender present* is positive but it is not statistically significant. 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 foreclosures per 1,000 owner-occupied homes rises about 0.65 rather than the 1.1 in disaster communities without payday lenders.

As Morse notes, her results do not address the big question: do payday lenders exacerbate or relieve customers' financial difficulties? But they do support a narrower conclusion—the presence of payday lenders in a community helps some homeowners cope with unexpected financial distress, whether caused by a natural disaster or some other event.

Carrell and 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 this correlation may not reflect causation.

Carrell and Zinman try to avoid this problem by using the observation that postings across states of enlisted Air Force personnel are roughly random. In other words, the personnel, whom Carnell 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 performances 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 or her career. Such behaviors could include civilian or military court convictions, letters of reprimand, confirmed incidents of sexual harassment, or financial irresponsibility. Reenlistment eligibility depends on satisfactory job performance.

The airmen are in various occupations within the Air Force and are in for various enlistment terms. For each military base and in each year, the authors obtained data on who is eligible to reenlist, 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. **(p. 691)**

Table 21.4 Summary of Carrell and Zinman’s (2008) Key Results

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

Using these data, the authors estimate the following equation:

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

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 five members in the group and three 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, unemployment, 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 (B_1) are given in Table 21.4. 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 2 through 4 suggest that most of the results are driven by the effects on first-term enlistees, who tend to be the youngest. The authors examine similar results for several subgroups of data. I refer the reader to the original paper for details.

Melzer (2011) 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 various reasons. Melzer (p. 692) 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 residents 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 operating 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.[6]

The data that Melzer uses to assess people's financial well-being come from the Urban Institute's National Survey of America's Families (NSAF). The NSAF 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 treatment due to a lack of insurance or money, and zero otherwise.

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 linear probability regression model of the following form:

$$\mathrm{Y}_{\mathrm{icst}} = \alpha + \beta\, PaydayAccess_{\mathrm{ct}} + \gamma Border_{\mathrm{c}} + \rho \mathrm{X}_{\mathrm{it}} + \delta \mathrm{Z}_{\mathrm{ct}} + \eta_{\mathrm{st}} + \varepsilon_{\mathrm{icst}}$$

In this equation, $\mathrm{Y}_{\mathrm{icst}}$ is an indicator of financial hardship for family or person *i*, living in county *c* and state *s* at time *t*. *PaydayAccess* is a binary variable that equals one if households in county *c* have access to payday loans in year *t* and zero otherwise. *Border* is a binary variable that equals one if the county is within 25 miles of a state border. *X* and *Z* are vectors that contain a wide range of control variables for the characteristics of the households and the counties. η_{st} 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 because they could 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 (p. 693) 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. 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 21.5 Summary of Melzer's (2011) Key Results

	Unconditional Mean	Coefficient on Payday Access
Dependent variable: Any reported family financial hardship	0.292	0.053 (0.019)
# observations		24,641
R^2		0.08
Dependent variable: Any health-care postponed	0.179	0.045 (0.016)
# observations		17,581
R^2		0.08

Standard errors, in parentheses, were calculated with observations clustered by county.

Table 21.5 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 21.4 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.[7]

Skiba and Tobacman (2011) studied the effects of the use of payday loans on personal bankruptcy by using individual loan records supplied by a large payday-loan company. Amazingly, the company provided the researchers with complete data from over one 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.

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 (p. 694) 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 by 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 scores could differ in a number of ways from those the company accepted. The rejected applicants might, for example, have had 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 whose scores 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 payday-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 a loan adds to the borrower's 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. Because they emphasize the results for the two-year lag, I discuss those results here. In the two years following an applicant's first loan application with the company, approximately 4,232 applicants (or 2.9 percent) filed for bankruptcy. As indicated in Table 21.6, the only striking difference 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. (p. 695)

Table 21.6 Summary of Skiba and Tobacman (2011) Basic Bankruptcy Data

	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 Chapter 7 bankruptcy within two years of application	≈0.80*	0.53	0.73	0.51	0.52
Percentage filing for Chapter 13 bankruptcy within two years of application	≈2.1*	1.63	1.88	2.03	3.62

(*) Author's estimate, based on data in Table II of Skiba and Tobacman (2011).

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:

$$Bkcy_i^{\tau} = B_0 + B_1 AboveThr_i + f(CreditScore_i) + \gamma X_i' + \delta M_i' + \varepsilon_i$$

where $Bkcy^{\tau}_i$ is the number of bankruptcy filings (Chapter 7, Chapter 13, or both) for person i in the τ years following his or her first loan application, $AboveThr_i$ is a dummy variable to indicate whether the applicant's credit score was above the approval threshold, $CreditScore_i$ 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 demographic 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 tenure, and months at current residence), $M'ti$ is 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 systematically 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 having a credit score above the threshold (and therefore almost certainly 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 (p. 696) estimate for the coefficient on *AboveThr* suggests that a loan approval increases the chances of a Chapter 13 bankruptcy filing by about 1.6 percentage points in the subsequent year. This is a big effect, given an average annual bankruptcy rate of 1.4 percent among all applicants in the sample.[8] The effect is not statistically significant for the smaller set of individuals within 0.1 standard deviations of the threshold and it is not statistically significant for Chapter 7 bankruptcies.

4. 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. For one, some of the studies find access to payday lending is beneficial and some find it harmful. More important, the results of each of the studies are simply suggestive; that is, they are based on at least one of two strong assumptions that could well be wrong, casting doubt on the reliability of the results. First, as 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 preban and postban 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 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" (549). 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, however. 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 accounts for the changes in payday customers' attitudes in the two states. It is also possible that Vancouver's preparations for the 2010 Winter Olympics affected people's attitudes in Seattle but not in Portland.

(p. 697) A similar point applies to the Morgan and Strain (2008) study. As noted above, they find that, relative to changes in other states, Chapter 7 bankruptcy rates increased, per capita complaints against debt collectors increased, and the number of returned checks per 100 checks processed increased when people lost access to payday loans in Georgia and North Carolina. This is after controlling for variation in state unemployment rates. Without an empirical model of what drives changes in bankruptcy rates, check-return rates, or debt collection complaints, however, it is hard to know what to control for in the regressions. It could be, for example, that check returns are largely unrelated to unemployment levels but are correlated with bank market shares because checks written to payees using the same bank as the payer clear more quickly. If so, Morgan and Strain should control for changes in bank concentrations across the states. One can easily imagine similar issues for bankruptcy rates and debt collection complaints.[9] In addition, Morgan and Strain find that the ban on payday loans in Georgia and North Carolina were associated with increases in Chapter 13 bankruptcy filings relative to the changes in other states. This is particularly striking since Skiba and Tobacman (2011) find that, in Texas, Chapter 13 filings among payday-loan customers are almost twice as common as Chapter 7 filings. Finally, one has to wonder whether the political changes in the two states that led them to ban payday lending also led to other regulatory changes that affected the dependent variables in the study.

Morse (2011) finds 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 centers, which happens to be correlated with resilience in the face of disasters, perhaps because in older communities more homes have very modest mortgages. Morse is well aware of such concerns and runs a number of robustness tests. In one, for example, she controls for the presence of McDonalds restaurants as a proxy for the development of the service sector in a zip code. The hypothesis is that payday lenders may tend to locate in service sector clusters and that the service sector may be more resilient to disaster than other industrial sectors. The problem is that one can come up with many such credible hypotheses, such as the one I propose, and Morse's robustness checks cannot rule out all of them. Moreover, 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. She speculates that temporary financial distress could lead some people to larceny (largely shoplifting) but not auto theft or burglary. This is possible, but it is also possible that the link to larceny is a chance correlation.

Carrell and Zinman (2008) find that that airmen's access to payday loans increased reenlistment ineligibility and increased the likelihood of a UIF. Melzer finds that, among households with incomes between $15,000 and $50,000, access to payday loans will increase the likelihood of a household reporting a family **(p. 698)** financial hardship and increase 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 (2011) case, one might worry that his approach assumes that people in New Jersey who live near Pennsylvania or people in Massachusetts who live near New Hampshire or Rhode Island are subject to the same general economic forces as people in the counties in the rest of these states. That may not be true. Boston and New York City, for example, could thrive without that prosperity spreading to the counties farther removed from these cities. Melzer shares this concern, which is why he includes data on unemployment 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 households earning less than $15,000 and more than $50,000, few of whom would presumably use payday loans. In this case, the estimated coefficient on *Payday Access* is statistically insignificant. This supports Melzer's view but does not rule out other possibilities. Suppose, for example, that Camden, New Jersey, increased property taxes around the same time as payday lenders opened in Pennsylvania. This could lead to financial hardships for families earning between $15,000 and $50,000, but it would have little effect on those with incomes outside of this range because the lowest-income families tend to rent and the higher-income families can afford the taxes. To rule out such concerns, Melzer should indicate that he carefully studied the histories of the border regions for the relevant time periods and could not find any local events, other than the opening of payday-loan stores in nearby states, that would adversely affect families with incomes between $15,000 and $50,000. Such information would make Melzer's interpretation of his results more persuasive.[10]

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 or financially unstable households might, for example, tend to live in Camden or Lawrence, Massachusetts, or other deindustrialized cities. Melzer can't control for this possibility because his data do not include observations on family wealth, job stability, and so on. 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.

(p. 699) A similar criticism applies to Skiba and Tobacman's (2011) regression results, which suggest that being approved for a payday loan in Texas increased the likelihood of a Chapter 13 bankruptcy filing. The problem is that applicants with higher 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. This leaves Skiba and Tobacman with a conundrum, however. 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, then 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, then the differences in Chapter 13 bankruptcies are statistically significant. As they include more applicants farther away from the approval threshold, however, 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 assets to protect. If so, when they face financial difficulties they may be more likely to file for Chapter 13 bankruptcy, while applicants with lower credit scores and fewer assets simply ignore their creditors, especially in Texas given its severe restrictions on wage garnishments for most debts.

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. If someone finds that payday lending has a statistically significant positive correlation with average adult heights, for example, then I suspect a chance correlation rather than concluding 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.

As noted above, Morse had 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, I find it difficult to believe that easy access to a high cost $250 two-week cash advance significantly offsets disaster-related foreclosures.[11]

Although Carrell and Zinman (2008) had found that airmen's access to payday loans modestly increased reenlistment ineligibility and the likelihood of a UIF, 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 one is feeling carefree? 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.

(p. 700) 5. Other Approaches

In a paper that I much admire, Skiba and Tobacman (2008) 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 $1.22 in two weeks (or $1 today than almost $175 one year from now!). They also estimate the parameters of a model where customers are "sophisticated" quasi-hyperbolic discounters (meaning that 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 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 overpredict borrowing a few pay periods after the first loan and underpredict 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 underpredict the size of the initial loan by about one-third. In the year following the initial loan, the three models continue to underpredict 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 (p. 701) of customers. Perhaps the default rate on first-time loans is 12 percent because a subset of customers feels no default remorse, can't be reached by collection calls, and other reasons. 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, 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.

Wilson and his four coauthors (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 find that access to payday loans benefited more players of the game than it harmed, although the benefit was small. 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 according to the rules of the game, 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.

6. Suggested Directions for New Research

Despite the research efforts of a talented group of economists, we still don't know the answer to this big question: Do payday lenders exacerbate or relieve customers' financial difficulties?

I'm not sure that we will ever get a definitive answer, but I suspect that we will get stronger indications from experimental approaches rather than from quasi-experimental simulations. In an ideal experiment, one would randomly grant payday loans to a group of applicants and randomly deny the loans to a similar (p. 702) 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, because, 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, because 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. One South African study suggests how such a study might be done, however. In a recent paper, Karlan and 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, because 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 the cases. Among similar applicants randomly flagged for denial, the managers denied loans to almost all. Because loan officers ultimately made the decision whether to approve an application, the researchers, in order to eliminate this element of discretion, then compared the outcomes for the marginal applicants that the computer assigned for approval to those that it rejected, regardless of the loan officers' decisions.

The researchers found that being flagged for approval had a positive and statistically significant effect on a consumption index, economic self-sufficiency index, and an index based on within-household influence, optimism, and perceived social status, where these indices were constructed by the researchers from the participants' answers to a series of postapplication survey questions conducted 6 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 United States since it was conducted in a different social and institutional context, but it does suggest how US researchers 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. Under the strong assumption that the rejected applicants would not find an alternative credit (p. 703) source that is a close substitute, comparing postapplication 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 in 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 approaches 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 the 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.

My last suggestion for future research is that we need to learn much more about payday lending over the Internet and other financial services that are structured to serve financially pressed households. It is my impression that Internet payday lending has been widespread for several years, even in some states that prevent storefront payday lending, yet no one has studied this phenomenon. In addition, there are very few good studies of bank overdraft-protection programs, the rent-to-own business, automobile title lending, and last-minute bill-paying 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, but this also means there is an important public-policy question for empirically oriented economists to tackle.

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Notes:

(1.) For example, see the report for the California Department of Corporations (Applied and Management 2007: 40).

(2.) In 2010, Washington established a limit of eight loans per customer per year, and many payday lenders exited the business.

(3.) I assume that respondents who denied that they had taken out a payday loan were excluded from the baseline survey, because that survey indicates 100 percent use of payday loans. As noted elsewhere, in other surveys of known loan customers, almost half of them deny ever having taken 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 the problem. This does not qualitatively change his results.

(5.) The report also examines the impact of Hawaii's decision in July 2003 to increase the maximum amount of a payday loan from $300 to $600. In this review, I do not discuss the results for Hawaii because Morgan and Strain give them less emphasis and these results are also the least convincing. Yellow page listings indicate that there were fewer than 30 payday-loan storefronts in that state at the time of the increase in the lending limit, and there are no data indicating that the increase in the loan limit had a profound effect on the number of storefronts or on the volume of payday lending. Thus, it is hard to believe that this regulatory change would have a detectable social impact. I strongly suspect, therefore, that the correlations that the authors find are spurious.

(6.) Delaware, which shares a border with New Jersey, also permitted payday lending, but Melzer's data did not identify any New Jersey respondents living near that state border.

(7.) To test the robustness of these estimates, Melzer reports the results from many specification variations that space constraints do not allow me to review. All of the alternative specifications support his basic results.

(8.) Moreover, some of the rejected loan applicants, especially those near the threshold, undoubtedly got loans from other payday lenders. This reduces Skiba and Tobacman's ability to detect a loan effect, because they have data from only the one lender.

(9.) In fact, a recent study (Lefgren and McIntyre 2009) of differences in personal bankruptcy rates across states found that wage garnishment laws and unwritten district court policies ("culture") had a statistically significant correlation with state bankruptcy rates, controlling for numerous other factors. The availability of payday loans did not.

(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. Because he has identifiable data only from counties with populations of more than 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 it would be 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. Payday lending first appeared in Pennsylvania in 1997, however, 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.

(11.) In California, the face amount of the check written to repay the loan cannot exceed $300. Because lenders are allowed to charge a $15 finance fee per each $85 advanced, most lenders will not advance more than $255.

(12.) In fairness to the authors, a UIF could mean that an airman has been sanctioned for financial irresponsibility, or it could mean that 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.

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