Vanderbilt University Law School

Law & Economics

Working Paper Number 11-5

Information Asymmetries in Consumer Credit Markets: Evidence from Payday Lending

Paige Marta Skiba, Vanderbilt University

Will Dobbie, Harvard University

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Will Dobbie†
Harvard University

Paige Marta Skiba
Vanderbilt University

September 2011

Abstract

Information asymmetries are prominent in theory but difficult to estimate. This paper presents a new empirical test for information asymmetries that exploits sharp discontinuities in the eligibility for payday loans. For each additional $100 of available credit, individuals take out payday loans that are $49.20 to $54.30 larger. The causal impact of a $100 larger loan is a 3.2 to 4.0 percentage point decline in the probability of default, a 22 to 35 percent decrease from the mean default rate. However, borrowers who *choose* $100 larger loans are 6.0 to 7.8 percentage points more likely to default than borrowers who choose smaller loans.

*We are extraordinarily grateful to Bo Becker, John Friedman, Roland Fryer, Lawrence Katz, Brigitte Madrian, Justin Sydnor, David Toniatti, and Crystal Yang for their comments and feedback. Jake Byl, Susan Carter and Kathryn Fritzdixon provided excellent research assistance. Funding was provided by the Multidisciplinary Program on Inequality and Social Policy at Harvard. We thank audiences at CU Boulder, Harvard University, University of Michigan, and the European Finance Assocation Meetings in Stockholm for valuable comments. The authors can be reached via email at dobbie@fas.harvard.edu or paige.skiba@vanderbilt.edu.

†Corresponding author: dobbie@fas.harvard.edu.

1 Introduction

Theory has long emphasized the importance of asymmetric information in explaining credit market failures. Information asymmetries and the resulting credit market failures have been used to explain anomalous behavior in consumption, borrowing, and labor supply. Motivated in part by this research, policymakers and lenders have experimented with various interventions to circumvent such problems. Yet, the success of these strategies depends on which information asymmetries are empirically relevant. Credit scoring and information coordination can help mitigate selection problems, while incentive problems are better addressed by improved collection or repayment schemes.

Distinguishing between different types of asymmetries is difficult even if loan terms are randomly assigned. Loan size and the probability of default may be correlated because borrowers with larger loans have a greater ex-post incentive to default, or because borrowers with a higher ex-ante risk of default select larger loans. As a result, there is little evidence on which information asymmetries are important in credit markets.[1]

This paper provides new evidence on the empirical relevance of asymmetric information in subprime consumer credit markets using unique data from two payday lenders. Our empirical strategy exploits the fact that payday loan amounts are a discontinuous function of net pay. Firms in our sample offer loans in $50 increments, up to but not exceeding half of an individual's net pay. As a result of this rule, there exist loan eligibility cutoffs around which very similar borrowers are offered different size loans. The crux of our identification strategy is to compare the average level of default for individuals earning just

[1]Ausubel (1999) discusses the challenges to empirically identifying specific information asymmetries in credit markets. Chiappori and Salanie (2000) and Finkelstein and McGarry (2006) do the same for insurance markets.

above and below these cutoffs. Intuitively, we attribute any discontinuous relationship between average default and net pay at the eligibility cutoffs to the causal impact of loan size.

We use a simple model of borrower behavior to show that, under plausible assumptions, our regression discontinuity design identifies the within-borrower incentive effect of loan size (e.g. moral hazard). A cross-sectional regression of default on loan size combines the selection and incentive effects of loan size. By subtracting our regression discontinuity estimate from the cross-sectional coefficient on loan size, we obtain an estimate of selection.

Our empirical analysis begins by documenting significant credit constraints among payday borrowers. For each additional $100 of available credit, individuals take out loans that are $49.20 to $54.30 larger. Surprisingly, relaxing these credit constraints leads to lower rates of default. Our regression discontinuity estimates suggest that a $100 increase in loan size decreases the probability that a borrower defaults by 2.8 to 3.8 percentage points. This is a 22 to 35 percent decrease from the mean default rate. The effect of a larger loan is larger for borrowers with higher baseline credit scores and borrowers who are over 40.

The positive within-borrower effect of loan size is, however, more than offset by adverse selection into larger loans in the cross-section. We estimate that borrowers who choose $100 larger loans are 6.0 to 7.8 percentage points more likely to default than observationally equivalent borrowers who choose smaller loans. Taken together, our results are therefore consistent with the view that adverse selection alone can lead to credit constraints in equilibrium.

The key threat to our interpretation of the results is that individuals may opt out of borrowing if they are not eligible for a large enough loan. Such selective borrowing could invalidate our regression discontinuity design by creating discontinuous differences in borrower characteristics around the eligibility cutoffs. We evaluate this possibility by testing whether the density of borrowers is a continuous function of the loan-eligibility cutoffs, and by examining the continuity of observable borrower characteristics at the cutoffs. Neither of these tests points to evidence of selective borrowing that would invalidate our empirical design.

Our analysis is conceptually similar to Adams, Einav and Levin (2009), who exploit exogenous variation in price and minimum down payments to identify moral hazard and adverse selection in an automobile loan market. Adams et al. (2009) estimate that for a given auto loan borrower, a $1,000 increase in loan size increases the rate of default by 16 percent. Individuals who borrows an extra $1,000 for unobservable reasons having an 18 percent higher rate of default than one those does not. More generally, our work fits into an important empirical literature identifying moral hazard and adverse selection in credit markets in the United States (Ausubel, 1999; Ausubel, 1991; Edelberg, 2003; Edelberg, 2004) and abroad (Klonner and Rai, 2006; Karlan and Zinman, 2009).

Our discontinuity approach complements this literature in three ways. First, the institutional features of the payday loan market allow for a particularly sharp research design. Adams et al. (2009), whose work is most closely related to ours, use price and down payment variation across time, credit categories, and region to identify the impact of moral hazard. The identification relies on the fact that they have controlled for all other sources of endogenous variation. In contrast, we focus on a single, well identified source of variation in loan size to identify moral hazard. Second, the institutional features of the payday loan market make it an ideal setting to test for credit market failures. Payday borrowers tend to have low incomes and poor credit histories, making them particularly vulnerable to market failures. Default comes with few penalties outside of calls from the payday lender and restricted access to future payday loans. Most notably, payday loan defaults are not typically reported to traditional credit rating agencies. Asymmetric information problems are exacerbated by precisely the kinds of commitment problems typical in the payday loan market (Athreya, Tam and Young, 2009; Chatterjee, Corbae, Nakajima and Rios-Rull, 2007; Livshits, MacGee and Tertilt, 2010; White, 2007; White, 2009). Perhaps as a result of these market features, two-thirds of payday borrowers report not having applied for credit at least once in the past five years due to the anticipation of rejection, and nearly three quarters report having been turned down by a lender or not given as much credit as applied for in the last five years (Elliehausen and Lawrence, 2001; IoData, 2002). Third, we are the first to explore the role of information frictions in the payday loan market, one of the largest and fastest growing sources of subprime credit in the United States. Since the emergence of payday lending in the mid-1990s, annual loan volume has grown from approximately $8 billion in 2000 to between $44 billion by 2008 (IHS Global Insights, 2009). Nearly 19 million households received a payday loan in 2010. In comparison, the subprime automobile loan market totaled approximately $50 billion in 2006 (Power and Associates, 2007), while the value of new subprime mortgages rose from around $100 billion in 2000 to a peak of $600 billion in 2006 (GAO-09-848R, 2009).

Our paper also adds to a large literature documenting consumer credit constraints. The majority of this literature has inferred credit constraints from the excess sensitivity of consumption to expected changes in labor income (e.g. Hall and Mishkin, 1982; Altonji and Siow, 1987; Zeldes, 1989; Runkle, 1991; Stephens, 2003; Stephens, 2006; Stephens, 2008) or tax rebates (e.g.

Souleles, 1999; Parker, 1999; Johnson, Parker and Souleles, 2006). Card, Chetty and Weber (2007) and Chetty (2008) also find excess sensitivity of job search behavior to available liquidity, which they interpret as evidence of liquidity constraints. Further evidence of consumer liquidity constraints comes from Gross and Souleles (2002), who use detailed data from a credit card company to show that increases in credit generate an immediate and significant rise in debt.

Finally, our paper is related to a rapidly expanding literature examining the impact of payday credit. There is evidence that loan access may help borrowers smooth negative shocks (Morse, n.d.) and avoid financial distress (Morgan and Strain, 2008). On the other hand, there is also evidence that loan access may erode job performance (Carrell and Zinman, 2008), increase bankruptcy (Skiba and Tobacman, 2009), and lead to increased difficulty paying mortgage, rent and utility bills (Melzer, forthcoming).

The remainder of the paper is structured as follows. Section 2 provides background on our institutional setting and describes our data. Section 3 presents a simple model of borrower behavior that motivates our empirical analysis. Section 4 describes our empirical strategy. Section 5 presents our results. Section 6 concludes.

2 Data and Institutional Setting

Our data come from two payday lenders that operate 1,236 stores in 20 states. In a typical payday loan transaction, individuals fill out loan applications and present their most recent pay stubs, checking-account statements, utility or phone bills, and a government-issued photo ID. Lenders use applicants' pay stubs to infer their next payday and assign loan due dates on that day. The customer writes a check for the amount of the loan plus a finance charge that is typically $15-$18 per $100 borrowed. The lender agrees to hold the check until the next payday, typically about two weeks, at which time the customer redeems the check with cash or the lender deposits the check. A loan is in default if the check does not clear.

The maximum amount an individual can borrow is a discontinuous function of net pay. Both firms in our sample restrict borrowers to loans that are no larger than half of their net pay. Because stores in our sample offer loans in $50 increments, the maximum loan size increases discontinuously at $100 pay intervals. The credit available to borrowers is depicted in Figure 1. Note that Tennessee only offers loans up to $200, while all other states in our sample offer loans up to $500.[2]

Our specific data consist of all approved loans from January 2000 through July 2004 in Ohio and Tennessee for the first firm in our data (hereafter Firm *A*) and from January 2008 through April 2010 in Kansas and Missouri for the second firm in our data (hereafter Firm *B*).[3] We combine these data with records of repayment and default for both firms. This gives us information on borrower characteristics, loan terms, and the subsequent loan outcomes. Our data from Firm *A* include information on each borrower's income, home address, gender, race, age, checking account balance, and subprime credit score (hereafter credit score).[4] Our data from Firm *B* is more sparse, including only

[2]In August 2011, Tennessee increased the limit to $500 but during our sample period the cap remained at $200.

[3]Firm *A* offers loans in continuous amounts in the other 14 states in which it operates. We drop these states from our analysis as we have no way of separately identifying the impact of incentives when available credit is determined continuously. Ohio and Tennessee offer loans in discontinuous due to a legacy policy. Firm *B* operates in eight other states where complete data are not yet available.

[4]A third party called Teletrack computes credit scores distinct from FICO scores for payday loan applicants. For more information on this subprime credit scoring process see Agarwal, Skiba and Tobacman (2009).

information on each borrower's income, home address, and age.

As default precludes subsequent borrowing, we restrict our sample to the first loan made to individuals. We also restrict our sample to borrowers paid biweekly with valid income data. Within each pay frequency, we employ a regression discontinuity design to identify the effect of loan size on default. Focusing on one group of borrowers, in this case those who are paid biweekly as they make up nearly 50 percent of the sample, allows a more straightforward presentation of the results. Results are identical if we include all borrowers. Finally, we drop individuals with incomes in the top or bottom 1 percent of the sample, restricting our analysis to borrowers with biweekly earnings between \$200 and \$1,800. This leaves us with 4,621 observations for Firm A and 8,624 observations for Firm B.

Summary statistics for our core sample are displayed in Table 1. The typical borrower at Firm A is more likely to be female and black, is 37.26 years old, has a biweekly income of \$715.83, and has a checking account balance of \$227.06. The typical first loan is for \$190.24. The typical borrower at Firm B is 36.74 years old with a biweekly income of \$822.78. Borrowers at Firm B take out somewhat larger first loans - \$257.69 - than those at Firm A, likely because both Kansas and Missouri cap loans at \$500.

Default rates at both firms are high. 10 percent of borrowers default on their first loan at Firm A, and 39 percent default during the sample period. At Firm B, 21 percent of borrowers default on their first loan, and 61 percent default during the sample period.

3 Theoretical Framework

3.1 Overview

Models of asymmetric information predict that information frictions will produce a positive correlation between loan default and the size or price of that loan.[5] In the moral hazard (e.g. adverse incentives) version of the model, individual borrowers are more likely to default on larger or more expensive loans. This assumption can be motivated in at least two ways. In an entrepreneurial setting, borrowers may have less incentive to exert effort when net returns to a loan are lower. If returns are concave in the loan amount, this implies a negative relationship between effort and loan size. In a more general setting, borrowers may have less incentive to repay a larger or more expensive loan even when they have the funds to do so. This can happen if the penalties of default increase less quickly than the benefits of default. In this scenario, borrowers are more likely to voluntarily default as the loan amount increases.

In models of adverse selection, borrowers with a higher ex ante risk of default view the likelihood of repayment as lower and, as a result, choose larger loans. As lenders cannot observe a borrower's risk type, adverse selection may also lead to low-risk borrowers being denied credit.

Theory does not rule out either advantageous selection or advantageous incentives (e.g. Bisin and Guaitoli, 2004; Parlour and Rajan, 2001; de Meza and Webb, 2001). Under non-exclusive contracting, for example, individuals borrowing from multiple sources may choose to pay down the largest loan obli-

[5]Models of asymmetric information typically assume limited commitment by borrowers, the idea that borrowers have the opportunity for personal bankrtupcy. An emerging literature suggests that asymmetric information issues are no longer relevant when limited commitment can be resolved (Athreya et al., 2009; Chatterjee et al., 2007; Livshits et al., 2010; White, 2007; White, 2009).

gation first. Or, borrowers may wish to maintain access to higher credit lines and choose not to default on those loans. In order to lead to credit constraints in equilibrium, however, the net impact of the selection and incentive effects must create a positive correlation between loan default and the size or price of the loan.

It is impossible to identify the separate impact of each of these channels with our available data. Instead, the goal of this paper is to document the presence of liquidity constraints in payday lending, and to assess the net empirical magnitude of the selection and incentive effects. The resulting estimates will likely reflect a number of the mechanisms discussed in this section.

3.2 A Conceptual Model

This section presents a simple model of borrower behavior that motivates our empirical exercise and clarifies precisely what assumptions are needed to identify the impact of selection and incentive effects in our context.

We consider a two-period model of borrower behavior. In period 1, the lender offers individuals a loan at the exogenously set interest rate R in any dollar amount $L \in [0, \bar{L}]$. We assume that $\bar{L}$ varies exogenously between individuals. The borrower then decides how much to borrow given her expected income in the first and second period, Y_1 and Y_2, and her type θ. We introduce uncertainty into the model by assuming that in the second period there is a mean zero, identically and independently distributed shock to each borrower's income, ε.

Conditional on the realization of ε, the borrower decides whether or not to repay the loan or to default in the second period. If the borrower repays the loan, she consumes her second period income less the loan amount, $Y_2 - LR + \varepsilon$.

If the borrower defaults, she is able to consume all of her second period income $Y_2 + \varepsilon$, but receives disutility $D(L, \bar{L}, \theta)$. We assume that the disutility from defaulting on the loan is weakly increasing in loan amount L, as the firm may pursue debtors more aggressively when they owe more money. We also assume that default is more costly for borrowers with higher θ ($\frac{\partial D}{\partial \theta} > 0$).

Let utility in period one be $C_1(Y_1+L)$. Let utility in period two be $C_2(Y_2 - LR + \varepsilon)$ if the borrower repays and $C_2(Y_2 + \varepsilon) - D(L, \bar{L}, \theta)$ if the borrower defaults.

We solve the model by considering each step separately, working backwards from the second period to the first.

Period 2: Taking loan size as given, the borrower chooses whether or not to repay the loan given the realized shock to expected second period income. A borrower repays if the utility gained from repaying the loan is greater than the utility gained from consuming the loan amount. This implies that a borrower repays the loan if and only if:

$$C_2(Y_2 - LR + \varepsilon) \geq C_2(Y_2 + \varepsilon) - D(L, \bar{L}, \theta) \tag{1}$$

This in turn implies that for each borrower there is a $\varepsilon = \varepsilon^*(L, \bar{L}, \theta)$ where she is indifferent between repaying the loan and defaulting on the loan. For $\varepsilon \geq \varepsilon^*(L, \bar{L}, \theta)$, borrowers choose to repay the loan. For $\varepsilon < \varepsilon^*(L, \bar{L}, \theta)$, borrowers choose to default.

If the marginal cost of repayment with respect to loan amount is less than the marginal cost of default, we have the usual moral hazard result that the probability of repayment is decreasing in loan size ($-\frac{\partial C_2}{\partial L} R < -\frac{\partial D}{\partial L}$).

In our empirical setting, we estimate the incentive effect by isolating variation in loan amount L driven by changes in the available loan terms $\bar{L}$. In this scenario, borrowers with larger loans due to variation in $\bar{L}$ are more likely to default only if the marginal cost of repayment with respect to a change in $\bar{L}$ is less than the marginal cost of default with respect to $\bar{L}(-\frac{\partial C_2}{\partial L}\frac{\partial L}{\partial \bar{L}}R < -\frac{\partial D}{\partial L}\frac{\partial L}{\partial \bar{L}} - \frac{\partial D}{\partial \bar{L}})$.

As we will discuss in the next section, our instrumental variable estimates will identify the incentive effect of loan size only if raising an individual's credit limit impacts default solely through loan size (e.g., $\frac{\partial D}{\partial \bar{L}}=0$). This assumes, for example, that conditional on loan size L individuals are not more likely to repay lenders offering higher credit lines $\bar{L}$ in order to protect future access to credit. If this assumption is violated, our reduced-form estimates will represent the net impact of increasing an individual's credit limit more generally.

Now to Period 1: Given the distribution of ε and the available loan terms $\bar{L}$, individuals choose loan amount L to maximize expected utility:

$$\max_{L\in[0,\bar{L}]} C_1(Y_1+L)+\int_{\varepsilon^*(L,\bar{L},\theta)}^{\bar{\varepsilon}}(C_2(Y_2-LR+\varepsilon))dF(\varepsilon)+\int_{\underline{\varepsilon}}^{\varepsilon^*(L,\bar{L},\theta)}(C_2(Y_2+\varepsilon)-D(L,\bar{L},\theta))dF(\varepsilon) \tag{2}$$

Noting that $C_2(Y_2 - LR + \varepsilon^*(L,\bar{L},\theta)) = C_2(Y_2 + \varepsilon^*(L,\bar{L},\theta)) - D(L,\bar{L},\theta)$, the F.O.C. is

$$\frac{\partial C_1}{\partial L} \geq \int_{\varepsilon^*(L,\bar{L},\theta)}^{\bar{\varepsilon}} \frac{\partial C_2}{\partial L} R dF(\varepsilon) + \int_{\underline{\varepsilon}}^{\varepsilon^*(L,\bar{L},\theta)} \frac{\partial D}{\partial L} dF(\varepsilon) \tag{3}$$

where we equate the marginal benefit of the loan in period one with the expected marginal cost in period two. Note that the F.O.C. holds with equality only when the desired loan amount is obtainable $L \leq \bar{L}$. When borrowers desire $L \geq \bar{L}$, borrowers are liquidity constrained.

If C is concave in L and the cost of default increases less quickly with respect to L than the cost of repayment ($\frac{\partial D}{\partial L} < \frac{\partial C_2}{\partial L}R$), we have the normal adverse selection result where borrowers with a higher ex ante risk of default choose larger loans. This is because riskier borrowers are more likely to default in the second period ($\frac{\partial \varepsilon^*}{\partial \theta} < 0$), and therefore face a lower expected marginal cost of credit. The empirical difficulty we face is separating the correlation between default and loan choice generated by θ (e.g. the selection effect) from the causal impact of loan size on default holding θ constant (e.g. the incentive effect). The next section describes our empirical strategy to separate the twox.

4 Empirical Strategy

Our strategy to identify the causal impact of loan size exploits the fact that loan size is a discontinuous function of net pay. Consider the following model of the causal relationship between default (D_i) and loan size (L_i):

$$D_i = \alpha + \gamma L_i + \varepsilon_i \tag{4}$$

The parameter of interest is γ, which measures the causal effect of loan size on default (e.g. the moral hazard, or incentive effect). The problem for inference is that if individuals select a loan size because of important unobserved determinants of later outcomes, such estimates may be biased. In particular, it is plausible that people who select larger loans have a different probability of default holding loan size constant: $E[\varepsilon_i | L_i] \neq 0$. Since L_i may be a function of default risk, this can lead to a bias in the direct estimation of γ using OLS. The key intuition of our approach is that this bias can be overcome if the distribution of unobserved characteristics of individuals who just barely qualified for a larger loan are the same as the distribution among those who just barely disqualified:

$$E[\varepsilon_i|pay_i = c_l + \Delta]_{\Delta\to 0^+} = E[\varepsilon_i|pay_i = c_l - \Delta]_{\Delta\to 0^+} \tag{5}$$

where pay_i is an individual's net pay and c_l is the eligibility cutoff for loan size l. Equation (5) implies that the distribution of individuals to either side of the cutoff is as good as random with respect to unobserved determinants of default (ε_i). In this scenario, we can control for selection into loans using an indicator variable equal to one if an individual's net pay is above a cutoff as an instrumental variable. Since loan size is a discontinuous function of pay, whereas the distribution of unobservable determinants of default ε_i is by assumption continuous at the cutoffs, the coefficient γ is identified. Intuitively, any discontinuous relation between default and net pay at the cutoffs can be attributed to the causal impact of loan size under the identification assumption in equation (5).

Formally, let loan size L_i be a smooth function of an individual's pay with a discontinuous jump at each loan-eligibility cutoff c_l:

$$L_i = f(pay_i) + \lambda_l \sum_{l=100-500} (pay_i \geq c_l) + \eta_i \tag{6}$$

where λ_l measures the contemporaneous increase in debt in response to a line increase at l, per dollar of line increase. λ_l can be interpreted as the marginal propensity to borrow estimated by Gross and Souleles (2002).

In practice, the functional form of $f(pay_i)$ is unknown. We follow Angrist and Lavy (1999) and approximate $f(pay_i)$ as a second-order polynomial in pay. Using a higher-order polynomial or a linear spline in net pay yields similar results. We also control for state and month by year effects in all specifications.

Adding controls for age, gender, ethnicity, baseline credit score, and baseline checking account balance leaves the results unchanged. To address potential concerns about discreteness in pay, we cluster our standard errors at the net pay level (Lee and Card, 2008).

The key threat to a causal interpretation of our instrumental variable estimates is that individuals may opt out of borrowing if they are not eligible for a large enough loan. Such selective borrowing could invalidate our empirical design by creating discontinuous differences in borrower characteristics around the eligibility cutoffs. In Section 5.4 we evaluate this possibility in two ways: by testing whether the density of borrowers is a continuous function of loan-eligibility cutoffs, and by examining the continuity of observable borrower characteristics around the cutoffs. Neither of these tests points to the kind of selective borrowing that invalidates our empirical design.

A second necessary assumption is that raising an individual's credit limit impacts default only through loan size (e.g., $\frac{\partial D}{\partial \bar{L}}=0$). This assumes, for example, that individuals do not strategically repay lenders who offer higher credit lines in order to protect future access to credit. If this assumption is violated, our reduced-form estimates represent the net impact of increasing an individual's credit limit more generally. This is the same assumption used by Adams et al. (2009) to identify the impact of moral hazard in the subprime auto loan market.

Under the above assumptions, we can use equation (6) as the first stage to estimate the average causal effect for individuals induced into a larger loan by earning just above a cutoff. A simple extension of our approach, first pioneered by Adams et al. (2009), also allows us to also estimate the magnitude of selection in our sample. A cross-sectional regression of default on loan size combines both selection and incentive effects. By subtracting our estimate of the incentive effect from the cross-sectional coefficient on loan size, we obtain an estimate of selection. This approach assumes that the estimated incentive effect is the relevant estimate for the full population. This assumption would be violated if borrowers just around the nine eligibility cutoffs have a different marginal return to credit than other borrowers.

An alternative approach to estimating the extent of selection in our sample is to try to explicitly control for all other sources of variation in loans, so that selection is the only remaining source of variation. In our context, this means regressing loan size on default within loan-eligibility groups (as defined earlier), where all borrowers should be offered the same loans and all differences in loan size should be due to selection. This approach relies on the assumption that the eligibility groups control for all variation in available loans. We report estimates from this strategy along with our primary OLS results.

5 Results

5.1 Impact of Credit Line Increase on Loan Amount

The effect of a credit line increase on loan amount is presented graphically in Figure 2, which plots average loan amounts in \$25 income bins. The fitted values come from a regression of loan size on nine loan eligibility indicators and a quadratic in net pay. That is,

$$L_i = \alpha_1 + \alpha_2 pay_i + \alpha_3 pay_i^2 + \sum_{l=100-500} \alpha_l (pay_i \geq c_l) + \varepsilon_i \tag{7}$$

where α_l is the effect of having an income above the cutoff for loan size l. The eligibility cutoffs are highly predictive of average loan size. While average loan amount is approximately constant between the cutoffs (and after the $200 loan cutoffs in Tennessee), loan size increases sharply at each cutoff.

Table 2 provides the formal estimates for the figure just described. We regress loan amount on the maximum loan an individual is eligible for, a quadratic in net pay, and month by year and state effects. Column 1 presents our baseline results using data from both firms in our sample. Column 2 limits the sample to borrowers at Firm A where we have control variables. Column 3 adds controls for gender, race, a quadratic in credit score, and a quadratic in checking account balance. The results are remarkably consistent across these specifications. In our baseline specification with both firms, individuals take out loans that are 49.2 cents larger for each additional dollar of credit. Borrowers at Firm A take out loans that are 54.3 cents larger for each additional dollar of credit, and 54.0 cents larger after adding demographic controls.

Our last set of results in column 4 allows the effect of each cutoff to vary. Cutoffs that are multiples of $100 appear to have somewhat larger coefficients, with the $100 cutoff having the largest impact on loan size (0.886, se=0.148). This suggests that borrowers with very low incomes may be more credit constrained than wealthier borrowers. Otherwise, there are no obvious trends across the nine cutoffs.

Our results from Table 2 provide new evidence that individuals in subprime credit markets are credit constrained. Perhaps not surprisingly, the point estimates from Table 2 suggest that payday loan borrowers are significantly more constrained than other individuals. While payday borrowers consume approximately 50 cents out of every additional dollar of available credit, Gross and Souleles (2002) find that a $1 increase in a credit cardholder's limit raises card spending by 10 to 14 cents, and Johnson et al. (2006) find that households immediately consumed 20 to 40 cents for every $1 increase in their 2001 tax rebate.

5.2 Causal Impact of Loan Amount on Default

Two stage least squares estimates of the causal impact of a larger payday loan on default are presented in Table 3. The dependent variable is an indicator variable equal to one if a loan ends in default. We instrument for loan amount using the maximum loan size an individual is eligible for. The second stage regression is therefore:

$$D_i = \alpha_1 + \alpha_2 pay_i + \alpha_3 pay_i^2 + \alpha_4 X_i + \alpha_5 L_i + \varepsilon_i \tag{8}$$

where α_5 represents the causal effect of loan size on default.

Following our first stage results, column 1 presents our baseline results using data from both firms in our sample, controlling only for a quadratic in net pay, and month by year and state effects. Column 2 limits the sample to borrowers at Firm A where we have control variables. Column 3 adds controls for gender, race, a quadratic in credit score, and a quadratic in checking account balance. Column 4 uses a set of nine eligibility indicators as an instrument for loan size. We report standard errors clustered at the net pay level in parentheses, and multiply all coefficients and standard errors by 100 so that our results can be interpreted as the percentage point change in default associated with a $1 increase in loan size.

Surprisingly, there is a negative impact of loan amount on the probability of default. The estimated effect size is both statistically and economically significant across all four specifications. A $100 increase in loan size is associated with a 3.8 percentage point decrease in the probability of a loan ending in default in our pooled sample. This is more than a 22 percent decrease from the mean default rate of 17 percent. The effect of a \$100 increase in loan size is 3.5 percentage points for loans made by Firm A, and 3.2 percentage points after adding demographic controls. This is just over a 35 percent decrease from the mean default rate of 10 percent at Firm A. In our most flexible specification where we instrument for loan size with a set of nine eligibility indicators, the effect is 2.8 percentage points. The estimated effect size is approximately equivalent to the impact of increasing a borrower's biweekly income by \$200.[6]

Table 4 presents results separately by baseline credit score, age, and gender. The sample is restricted to borrowers at Firm A as we lack demographic information for borrowers at Firm B. We control for group status, a quadratic in net pay, and month by year and state effects. The effect of loan size on default is significantly larger for borrowers with above median credit scores, and for borrowers who are over 40. For every additional \$100 lent, borrowers with above median credit scores are 5.1 percentage points less likely to default than borrowers with lower credit scores. Borrowers over 40 are 2.3 percentage points less likely to default for every additional \$100 lent compared to borrowers under 40. The effect of loan size does not appear to differ by gender.

5.3 Selection Results

OLS results relating loan size to default are presented in Table 5. These cross-sectional estimates combine the causal impact of loan size with the selection of borrowers into different size loans. Under our identifying assumptions, the impact of adverse selection alone is the coefficient from our OLS regressions minus the coefficient from our two stage least squares results in Table 3.

[6]Appendix Table 1 presents results for default on any loan within the first 6 months of a borrower's first loan. Three of the four point estimates are negative, though none are statistically different from zero. Results are similar using default within 3, 9, or 12 months of a borrower's first loan.

As in Tables 3 and 4, the dependent variable is an indicator variable equal to one if a loan ends in default. We report robust standard errors in parentheses and multiply all coefficients and standard errors by 100 so that our coefficients can be interpreted as the percentage point change in default associated with a \$1 larger loan. Column 1 presents our baseline results using data from both firms in our sample, controlling only for a quadratic in net pay, and month by year and state effects. Column 2 limits the sample to borrowers at Firm A where we have control variables. Column 3 adds controls for gender, race, a quadratic in credit score, and a quadratic in checking account balance.

Consistent with the view that information frictions lead to credit constraints in equilibrium, there is a positive association between loan size and the probability of default. A \$100 increase in loan size is associated with a 4.0 percentage point increase in the probability of default in our baseline results, and a 3.4 percentage point increase controlling for demographic characteristics. This suggests that borrowers who *select* a \$100 larger loan are 6.0 to 7.8 percentage points more likely to default, more than a 40 percent increase from the mean rate of default.

Column 4 adds controls for the maximum loan a borrower is eligible for. If the eligibility categories control for all variation in what loans are available, this provides a direct estimate of the selection effect. Controlling for available credit, a \$100 increase in loan size is associated with a 4.6 percentage point increase in the probability of default. While more modest than our results from columns 1 through 3, our results from column 4 are consistent with the idea that adverse selection drives the negative relationship between loan size and default.[7]

[7] Appendix Table 2 presents results for default on any loan within 6 months of a borrower's first loan. The results are nearly identical to those presented in Table 5.

5.4 Tests for Quasi-Random Assignment

This section presents results from a series of specification checks. Our empirical strategy assumes that individuals do not selectively borrow based on the eligibility cutoffs. One specific concern is that individuals eligible for larger loans will be more likely to borrow. Such selective borrowing could invalidate our empirical design by creating discontinuous differences in borrower characteristics around the eligibility cutoffs. Although the continuity assumption cannot be fully tested, its validity can be evaluated by testing whether the observable characteristics of borrowers trends smoothly through the cutoffs, and by testing the density of borrowers around the cutoffs. Throughout this section we only discuss results from Firm A, where the richness of our data allows for more convincing checks of our identifying assumptions.

Table 6 tests whether observable baseline characteristics trend smoothly through the loan eligibility cutoffs. If there is a discontinuous change at the cutoffs, that would indicate that borrowers who are eligible for larger loans differ from borrowers who are not eligible for larger loans in a way that would invalidate our research design. We regress each baseline characteristic on the maximum loan a borrower is eligible for, a quadratic in net pay, state effects, and month by year effects. We multiply coefficients and standard errors by 100 to make the coefficients easier to interpret. Borrowers eligible for larger loans are somewhat more likely to be older and less likely to be male compared to borrowers not eligible for larger loans, though both results are only statistically significant at the ten percent level. There are no differences in ethnicity, credit score, or checking account balance around the eligibility cutoffs. Results are identical if we allow the effect to vary by cutoff. Given the mixed signs and general lack of statistical significance, we interpret Table 6 as showing no clear evidence that our identifying assumption is violated.

A second robustness test is to check whether the frequency of borrowers changes at the loan eligibility cutoffs. Our approach is similar to McCrary (2008), who suggests a simple extension of the local linear density estimator to test the unconditional density of observations on either side of a regression discontinuity. Specifically, we collapse data from each of the states where Firm A operates, including those who offer continuous loans, into equal-sized bins for each state. The key variables in our data are the fraction of observations in each bin, and the net pay amount that the bins are centered around. We then regress the fraction of observations in each bin on the maximum loan a borrower is eligible for, and a quadratic in net pay. We control for the \$100 bin each observation falls into to control for wage setting effects unrelated to the type of selective borrowing we are testing for. The \$100 bins are identified using variation in the states where loans are offered in continuous amounts and, as a result, there is no incentive for borrowers to select in or out of the sample around the loan eligibility cutoffs. We present results using bins of width \$10 to \$50 to ensure that our results are robust to this choice.

Table 7 presents results for whether the frequency of borrowers changes at the eligibility cutoffs. There are no unexpected jumps in the fraction of borrowers around the loan eligibility cutoffs. The coefficient on the credit line variable is small and not statistically significant across all of the considered bin widths. In unreported results, we allow the estimated effect of each cutoff to vary. The coefficients on the eligibility indicators are small, inconsistent in sign, and we cannot reject the null hypothesis that the indicator variables are jointly equal to zero at any bin width.

6 Conclusion

This paper has documented severe credit constraints among payday borrowers. Surprisingly, relaxing these credit constraints leads to lower rates of default. Our regression discontinuity estimates suggest that a $100 increase in loan size decreases the probability that a borrower defaults by 2.8 to 3.8 percentage points. This positive within-borrower impact of additional credit is more than offset by adverse selection into larger loans. Borrowers who *choose* $100 larger loans are 6.0 to 7.8 percentage points more likely to default than borrowers who choose smaller loans. Together, our results are therefore consistent with the idea that adverse selection alone can lead to credit constraints in equilibrium.

Our finding that borrowers are less likely to default when offered larger loans is notable given the emphasis on moral hazard by policymakers and within the theoretical literature. Our results should spur the development of new dynamic incentive schemes to improve repayment rates, while helping guide future theoretical and empirical work on credit market failures. Our results also highlight the significant adverse selection problems faced by firms in the subprime credit market. Improved screening strategies or information sharing may play an important role in alleviating these frictions.

With that said, the welfare effects of resolving information frictions in credit markets are still unknown. A better understanding of which behavioral model characterizes the behavior of borrowers in our data would go a long way towards addressing this issue. We view the parsing out of these various mechanisms, both theoretically and empirically, as an important area for future research.

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Table 1
Summary Statistics

	Firm *A*		Firm B	
	Mean	N	Mean	N
Age	37.26	4,621	36.74	8,623
Loan Amount	190.24	4,621	257.69	8,624
Net Biweekly Pay	715.83	4,621	822.78	8,624
Default on First Loan	0.10	4,621	0.21	8,624
Default in First 6 months	0.30	4,621	0.52	8,624
Default in First 12 months	0.35	4,621	0.58	8,624
Default on Any Loan	0.39	4,621	0.61	8,624
Male	0.30	2,766		
White	0.18	2,598		
Black	0.82	2,598		
Credit Score	550.05	4,035		
Checking Balance	227.06	4,532		

This table reports summary statistics for two payday lending firms. Columns 1 and 2 are based on first time borrowers at Firm *A* who are paid biweekly. Columns 3 and 4 are based on first time borrowers at Firm B who are paid biweekly. We drop borrowers with incomes in the top or bottom 1 percent of the sample.

Table 2
Effect of Credit Line on Loan Size

	No Controls (1)	Control Sample (2)	Full Controls (3)	Separate Cutoffs (4)
Credit Line	0.492***	0.543***	0.540***	
	(0.018)	(0.023)	(0.023)	
\$100 Cutoff				0.886***
				(0.148)
\$150 Cutoff				0.595***
				(0.085)
\$200 Cutoff				0.641***
				(0.064)
\$250 Cutoff				0.498***
				(0.097)
\$300 Cutoff				0.578***
				(0.112)
\$350 Cutoff				0.258*
				(0.139)
\$400 Cutoff				0.706***
				(0.192)
\$450 Cutoff				0.486*
				(0.253)
\$500 Cutoff				0.745***
				(0.257)
F-Statistic	764.93	555.62	547.09	91.95
Observations	13245	4621	4621	4621

This table reports reduced-form estimates for the impact of a credit line increase on payday loan size. Coefficients are scaled so that they can be interpreted as the increase in loan size for each additional dollar of credit offered. All specifications include a quadratic in net pay, state effects, and month by year effects. Specification 1 includes borrowers from Firm *A* and Firm B. Specification 2 includes borrowers from Firm *A* only. Specifications 3 and 4 add controls for age, gender, ethnicity, a quadratic in credit score, and a quadratic in checking account balance to the Firm *A* sample. Standard errors are clustered at the net pay level. The F-statistic for the null hypothesis that the loan eligibility indicators are jointly equal to zero is reported for each specification. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3
TSLS Results of Loan Amount on Default

	No Controls (1)	Control Sample (2)	Full Controls (3)	Separate Cutoffs (4)
Loan Amount	-0.038^{***}	-0.035^{**}	-0.032^{**}	-0.028^{**}
	(0.014)	(0.016)	(0.015)	(0.015)
Observations	13245	4621	4621	4621

This table reports two stage least squares estimates for the impact of payday first loan size on default. Coefficients are scaled so that they can be interpreted as the percentage point change in default for each additional dollar lent. All specifications include a quadratic in net pay, state effects, and month by year effects. Specifications 1 through 3 instrument for loan size using the maximum loan amount an individual is eligible for. Specification 4 instruments for loan size using a set of nine loan eligibility indicators. Specification 1 includes borrowers from Firm *A* and Firm B. Specification 2 includes borrowers from Firm *A* only. Specifications 3 and 4 add controls for age, gender, ethnicity, a quadratic in credit score, and a quadratic in checking account balance to the Firm *A* sample. Standard errors are clustered at the net pay level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 4
TSLS Results by Subgroup

Good Credit	Poor Credit	p-value	Older	Younger	p-value	Male	Female	p-value
−0.059***	−0.008	0.000	−0.047***	−0.024	0.000	−0.029	−0.025	0.541
(0.016)	(0.016)		(0.016)	(0.016)		(0.020)	(0.020)	
2032	2019		1913	2843		864	2024	

This table reports two stage least squares estimates for the impact of payday first loan size on default by baseline credit score, age, and gender. Coefficients are scaled so that they can be interpreted as the percentage point change in default for each additional dollar lent. Good credit is defined as having a baseline credit score above the median credit score among first time borrowers. Older is defined as being 40 years old or older. All specifications include a quadratic in net pay, state effects, and month by year effects, and instrument for loan size using the maximum loan amount an individual is eligible for. The sample is limited to borrowers at Firm *A*. Standard errors are clustered at the net pay level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 5
OLS Results

	No Controls (1)	Control Sample (2)	Full Controls (3)	With Cutoffs (4)
Loan Amount	0.040***	0.032***	0.034***	0.046***
	(0.003)	(0.006)	(0.006)	(0.006)
Net Pay	−0.031***	−0.027***	−0.023***	−0.013
	(0.004)	(0.006)	(0.006)	(0.009)
Net Pay Sq	0.000***	0.000***	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Age			−0.212***	−0.205***
			(0.037)	(0.037)
Male			0.660	0.578
			(1.141)	(1.135)
Black			1.873	1.983
			(1.309)	(1.307)
Credit Score			−0.035***	−0.036***
			(0.011)	(0.011)
Credit Score Sq			0.000	0.000
			(0.000)	(0.000)
Checking Balance			−0.010***	−0.010***
			(0.003)	(0.003)
Checking Balance Sq			0.000*	0.000*
			(0.000)	(0.000)
R^2	0.053	0.040	0.090	0.096
Observations	13245	4621	4621	4621

This table reports OLS estimates for the association between payday first loan size and default. Coefficients are scaled so that they can be interpreted as the percentage point change in default for each additional dollar lent. All specifications include a quadratic in net pay, state effects, and month by year effects. Specification 1 includes borrowers from Firm A and Firm B. Specification 2 includes borrowers from Firm A only. Specification 3 adds controls for age, gender, ethnicity, a quadratic in credit score, and a quadratic in checking account balance to the Firm A sample. Specification 4 adds controls for loan eligibility using a set of nine loan eligibility indicators. Robust standard errors are reported. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 6
Test of Quasi-Random Assignment

	Age	Male	Black	Credit	Savings
Credit Line	0.577*	−0.030*	0.001	−0.833	6.113
	(0.294)	(0.017)	(0.016)	(4.351)	(7.285)
Observations	4621	2766	2598	4035	3878

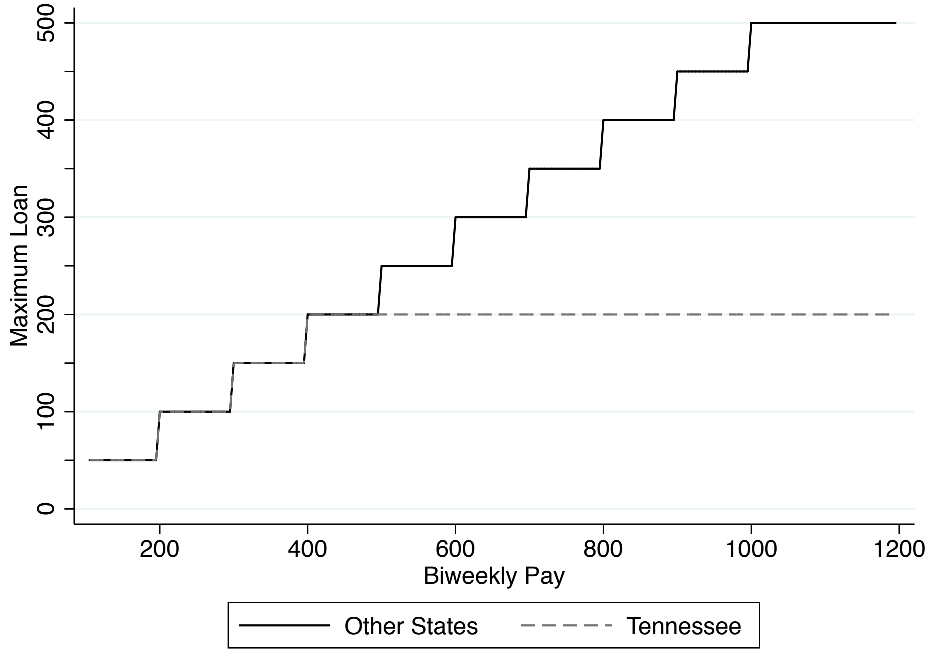
This table reports reduced-form estimates for available baseline characteristics. Coefficients are multiplied by 100. All specifications include a quadratic in net pay, state effects, and month by year effects, and include borrowers from Firm *A* only. Standard errors are clustered at the net pay level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 7
Density Test of
Quasi-Random Assignment

Bin Size	Estimate
10	0.004
	(0.006)
	1943
20	0.009
	(0.013)
	990
25	0.011
	(0.017)
	795
33	0.012
	(0.024)
	600
50	0.019
	(0.040)
	405

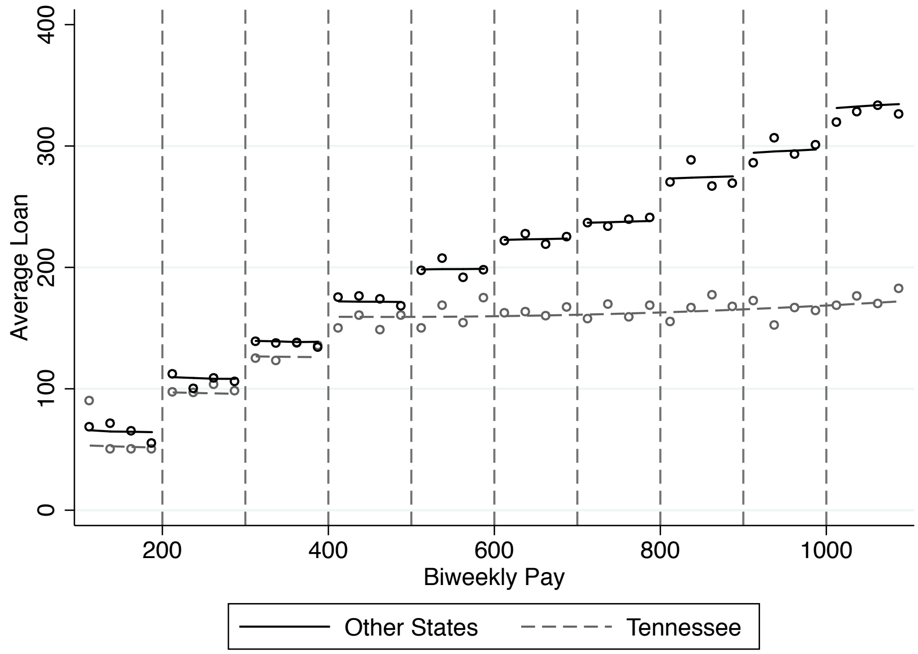
This table reports reduced-form estimates for the change in density at various bin sizes. The dependent variable is the fraction of observations in each bin. Coefficients are multiplied by 10,000. All specifications include a quadratic in net pay, and state effects. The sample includes borrowers from 13 states served by Firm *A*. Additional details are in text. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Figure 1
Maximum Loan Rule



This figure illustrates the loan eligibility rule used by firms in our sample. Individuals are eligible for loans up to but not exceeding half of netpay.

Figure 2
Effect of Credit Line on Loan Size



This figure plots average loan size for first time borrowers. The smoothed line comes from a specification including a quadratic in net pay, and a set of loan eligibility indicator variables. Bin size is $25.

6.1 Appendix A: Additional Results

Appendix Table 1

TSLS Results for Loan Amount on Default within 6 Months

	No Controls	Control Sample	Full Controls	Separate Cutoffs
Loan Amount	0.014	−0.009	−0.003	−0.004
	(0.021)	(0.027)	(0.026)	(0.026)
Observations	11746	3916	3916	3916

This table reports two stage least squares estimates for the impact of payday first loan size on default within the first 6 months of a borrower's first loan. Coefficients are scaled so that they can be interpreted as the percentage point change in default for each additional dollar lent. All specifications include a quadratic in net pay, state effects, and month by year effects. Specifications 1 through 3 instrument for loan size using the maximum loan amount an individual is eligible for. Specification 4 instruments for loan size using a set of nine loan eligibility indicators. Specification 1 includes borrowers from Firm A and Firm B. Specification 2 includes borrowers from Firm A only. Specifications 3 and 4 add controls for age, gender, ethnicity, a quadratic in credit score, and a quadratic in checking account balance to the Firm A sample. Standard errors are clustered at the net pay level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 2
OLS Results for Default within 6 Months

	No Controls	Control Sample	Full Controls	With Cutoffs
Loan Amount	0.050***	0.032***	0.035***	0.042***
	(0.005)	(0.011)	(0.010)	(0.011)
Net Pay	−0.014**	−0.022**	−0.013	−0.009
	(0.006)	(0.010)	(0.010)	(0.015)
Net Pay Sq	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Age			−0.492***	−0.490***
			(0.064)	(0.064)
Male			1.343	1.399
			(2.002)	(2.005)
Black			10.085***	10.086***
			(2.321)	(2.328)
Credit Score			−0.133***	−0.133***
			(0.022)	(0.023)
Credit Score Sq			0.000*	0.000*
			(0.000)	(0.000)
Checking Balance			−0.013**	−0.013**
			(0.006)	(0.006)
Checking Balance Sq			0.000	0.000
			(0.000)	(0.000)
R^2	0.064	0.032	0.153	0.155
Observations	11746	3916	3916	3916

This table reports OLS estimates for the association between payday first loan size and default within the first 6 months of a borrower's first loan. Coefficients are scaled so that they can be interpreted as the percentage point change in default for each additional dollar lent. All specifications include a quadratic in net pay, state effects, and month by year effects. Specification 1 includes borrowers from Firm *A* and Firm B. Specification 2 includes borrowers from Firm *A* only. Specification 3 adds controls for age, gender, ethnicity, a quadratic in credit score, and a quadratic in checking account balance to the Firm *A* sample. Specification 4 adds controls for loan eligibility using a set of nine loan eligibility indicators. Robust standard errors are reported. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.