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# Information Asymmetries in Consumer Credit Markets

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**Abstract**

This paper tests for incentive and selection effects in a subprime consumer credit market. We estimate the incentive effect of loan size on default using sharp discontinuities in loan eligibility rules. This allows us to estimate the magnitude of selection from the cross-sectional correlation between loan size and default. We find evidence of advantageous incentives and adverse selection. For a given borrower, we estimate that a \$100 increase in loan size decreases the probability of default by 3.7 to 4.2 percentage points, a 20 to 23 percent decrease from the mean default rate. The incentive effect is more than offset by adverse selection into larger loans. Borrowers who choose \$100 larger loans are 6.9 to 8.0 percentage points more likely to default than borrowers who choose smaller loans. Taken together, our results are consistent with the idea that information frictions lead to credit constraints in equilibrium.

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# 1 Introduction

Theoretical research has long emphasized the importance of asymmetric information in explaining credit market failures. Information frictions 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 depend on which 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 when 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 magnitude of incentive and selection effects in consumer credit markets. We use unique data from two firms who make payday loans, small uncollateralized consumer loans to individuals likely to face credit constraints. Our empirical strategy exploits the fact that payday loan amounts are a discontinuous function of net pay to identify the incentive effect of loan size. 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 a number of loan eligibility cutoffs around which very similar borrowers are offered different loans. The crux of our identification strategy is to compare the average level of default for individuals earning just above and below these cutoffs. Intuitively, we attribute any discontinuous relation between average default and net pay at the eligibility cutoffs to the causal impact of loan size.

A simple extension of our approach allows us to also estimate the magnitude of selection in our sample. A cross-sectional regression of default on loan size combines selection and the effect of incentives. By subtracting our estimate of the incentive effect from the cross-sectional coefficient

[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.

on loan size, we obtain an estimate of selection.

In our empirical analysis we find compelling evidence of advantageous incentives and adverse selection. We estimate that for a given borrower, a \$100 increase in loan size decreases the probability of default by 3.7 to 4.2 percentage points. This is a 20 to 23 percent decrease from the mean default rate. While not ruled out by theory, our finding of advantageous incentive is perhaps surprising given the emphasis on adverse incentives (e.g. moral hazard) by both policymakers and the theoretical literature. We explore the mechanisms behind this result by examining the heterogeneity of the incentive effect across borrowers. We find that the incentive effect is more advantageous for borrowers likely to have low discount rates. This is consistent with the idea that dynamic incentives play an important role in determining a borrower's response to loan terms.

The incentive effect is more than offset by adverse selection into larger loans. We estimate that borrowers who choose \$100 larger loans are 6.9 to 8.0 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 empirical design by creating discontinuous differences in borrower characteristics around the eligibility cutoffs. We evaluate this possibility in two ways: 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 work fits into an emerging empirical literature on the causes of credit constraints. There is evidence consistent with the presence of adverse selection in credit card markets (Ausubel, 1999; Ausubel, 1991), and both adverse selection and adverse incentives in mortgage markets (Edelberg, 2003; Edelberg, 2004). Outside of the U.S., there is evidence of adverse selection and, to a lesser extent, adverse incentives, in consumer loan markets (Klonner and Rai, 2006; Karlan and Zinman, 2009). Our approach is most similar to Adams, Einav and Levin (2009), who exploit exogenous variation in price and minimum down payments to identify the moral hazard and adverse selection effects of increased loan size on default in an automobile loan market.

Our discontinuity approach complements this literature in three ways. First, the institutional features of the payday loan market allow a particularly sharp research design. Eligibility for different size loans is based on a discontinuous rule: borrowers are eligible for loans up to but not exceeding half of their net pay. The resulting sharp discontinuities in loan size allow us to obtain more precise estimates of the causal impact of loan size on default. Second, the payday loan market is an ideal setting to test for credit market failures. 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-fourths 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). Given that payday borrowers are likely to have low incomes and poor credit histories, these market failures are perhaps not surprising. Finally, the rich demographic controls available in our data allow us to examine how selection and incentive effects vary across borrowers. This allows us to shed some light on the mechanisms through which these effects operate.

Our paper also contributes to a large literature documenting liquidity 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, 2008; Stephens, 2006; Stephens, 2003) 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. Our discontinuity approach allows more direct estimation of liquidity constraints from a borrower's response to an exogenous change in credit limit. Our approach is similar to 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.

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. 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.

Finance charges are typically $15-$18 per $100 of the loan amount, with loan size ranging from $50 to $500 in most states. As mentioned above, the maximum amount an individual can borrow is a discontinuous function of net pay.[2] 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 offer curve borrowers face 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.

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 resulting 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, only including 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

[2]Both firms in our sample claim that they implement this rule for simplicity and minimization of dollar denominations needed.

[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 the offer curve is continuous. 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).

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 allows a more straightforward presentation of the results. Our results are identical if we include borrowers paid weekly and monthly (see Appendix Tables 1 and 2). Finally, we drop individuals with incomes in the top or bottom 1 percent of the sample, restricting our analysis to borrower's making between \$200 and \$1,800 every two weeks. These restrictions leave us with 4,622 observations for Firm $A$ and 8,624 for Firm $B$.

Summary statistics for our core sample are displayed in Table 1. The typical borrower at Firm $A$ is 37.3 years old, has a biweekly net pay of \$715.97, and a checking account balance of \$227.90. The size of the typical first loan is \$191.40, somewhat smaller than average as Tennessee limits loans to \$200. Borrowers at Firm $A$ are also more likely to be female and black. The typical borrower at Firm $B$ is remarkably similar, with an average age of 36.8 and average biweekly net pay of \$822.78. Borrowers take out somewhat larger first loans as both Kansas and Missouri set loans caps 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 our sample period. At Firm $B$, 23 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. In the 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 at a higher ex ante risk of default view the likelihood of repayment as lower and, as a result, choose larger loans. Because lenders cannot observe a borrower's risk type, adverse selection leads not only to loan caps, but also 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 obligation first. Or, borrowers may wish to maintain access to higher credit lines and choose not to default on those loans. To lead to credit constraints in equilibrium, however, the net impact of selection and incentives must lead to a positive correlation between loan default and the size 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 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 adverse selection and incentives mean in our context. Our model incorporates both selection and incentive effects and operates under a handful of intuitive assumptions.

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 $\theta$. 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, $\varepsilon$.

Conditional on the realization of $\varepsilon$, 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 increasing in loan amount $L$, as the firm may pursue debtors more aggressively when they owe more money. We also assume that disutility is increasing in the maximum loan amount available $\bar{L}$, as borrowers value access to larger loans more than access to smaller loans. Finally, we assume that default is more costly for borrowers with higher $\theta$ ($\frac{\partial D}{\partial \theta} > 0$).

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 implies that for each borrower there is a $\varepsilon = \varepsilon^*(L, \bar{L}, \theta)$ where she is indifferent between repaying the loan and default. 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 adverse incentive 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, incentives are adverse 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 $\bar{L}(-\frac{\partial C_2}{\partial L}\frac{\partial L}{\partial \bar{L}} R < -\frac{\partial D}{\partial L}\frac{\partial L}{\partial \bar{L}} - \frac{\partial D}{\partial \bar{L}})$. Incentives are therefore advantageous if $-\frac{\partial C_2}{\partial L}\frac{\partial L}{\partial \bar{L}} R \geq -\frac{\partial D}{\partial L}\frac{\partial L}{\partial \bar{L}} - \frac{\partial D}{\partial \bar{L}}$.

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

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

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

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

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 a loan amount greater than the maximum loan amount, $L \geq \bar{L}$, 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 at 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 $\theta$ (e.g. the selection effect) from the causal impact of loan size on default holding $\theta$ constant (e.g. the incentive effect). Next we describe our empirical strategy to do exactly this.

## 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 ($L_i$):

$$D_i = \alpha + \gamma L_i + \varepsilon_i \quad (4)$$

The parameter of interest is $\gamma$, which measures the causal effect of loan size on default (e.g. the 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 had 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 $\gamma$ 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 were 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 ($\varepsilon_i$). 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 $\varepsilon_i$ is by assumption continuous at the cutoffs, the coefficient $\gamma$ 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 $c_l$:

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

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 provides similar results.

The identified second stage parameter measures the average causal effect for individuals induced into a larger loan by earning just above a cutoff. In the context of our model, this is the impact of a change in loan amount ($L$) generated by variation in the largest available loan ($\bar{L}$), holding borrower characteristics ($\theta$) constant. 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 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 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 the 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 would invalidate our empirical design.

A simple extension of our approach allows us to estimate the magnitude of selection in our sample. A cross-sectional regression of default on loan size combines selection and the effect of incentives. 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 eligibility cutoffs have a different marginal return to credit than other borrowers. Given that the eligibility cutoffs span borrowers with biweekly incomes from \$200 to \$1000, we do not see this as a serious concern.

An alternative approach to estimating the extent of selection in our sample is 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 in Appendix Table 3.

# 5 Results

## 5.1 First-Stage

First-stage results are presented graphically in Figure 2, which plots average loan amounts in $25 income bins and predicted loan amount from a regression relating loan size to nine loan-eligibility indicators and a quadratic in net pay.[5] 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 formal estimates for the figure just described. We regress loan amount on the maximum loan an individual is eligible for (the "offer curve") and month, year, and state effects. Columns 1 and 2 in Table 2 present results controlling for linear and quadratic controls in net pay respectively. We scale all first-stage results so that they can be interpreted as the increase in loan size for each additional dollar of credit offered. For each additional dollar of available credit, individuals take out loans that are 45.8 to 49.0 cents larger. Column 3 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 (0.927, se=0.084). This suggests that borrowers with very low incomes may be more liquidity constrained than wealthier borrowers. Otherwise there are no obvious trends across the nine cutoffs. Appendix Table 3 presents results including borrowers paid weekly, semimonthly and monthly (Columns 1 through 3); borrowers from Firm $A$ only (Columns 4 through 6); and borrowers from Firm $A$ controlling for gender, race, credit score and savings (Columns 7 through 10). The results are identical to those in Table 2.

In addition to establishing the validity of our empirical design, our first-stage results provide new evidence that individuals in the subprime credit market are liquidity constrained. If borrowers were not constrained, the marginal propensity to borrow from an increase in credit should be zero. To put the magnitude of our estimates in context, Gross and Souleles (2002) find that a $1 increase

[5] Lee and Lemieux (forthcoming) propose a formal test for optimal bin width based on the idea that if the bins are "narrow enough" there should not be a systematic relationship between the outcome variable and the running variable within each bin. Results from this test are presented in Appendix Table 4. We add a set of interactions between the bin dummies and the running variable to a base regression of loan amount on the set of bin dummies, then whether the interactions are jointly significant. The interaction terms are not jointly significant in any of the bin widths attempted, so we settle on a relatively "narrow" width of $25, which may result in somewhat more noise in our figures.

in a card holder's limit raises credit card spending by 10 to 14 cents, and Johnson et al. (2006) find that households immediately consumed 20 to 40 cents for every dollar in their 2001 tax rebate.

### 5.2 Incentive Effect

Reduced form results of the causal impact of loan size on default are presented graphically in Figure 3. We plot the average probability of default and predicted probability of default from a regression relating default to nine loan-eligibility indicators and a quadratic in net pay. We use a bin size of $50 because the default data have more idiosyncratic variation than the loan data. The change in the probability of default around the loan-eligibility cutoffs is the reduced form effect of increasing a borrower's credit limit. When pay is less than $300, there is only a weak relationship between the eligibility cutoffs and default. This is not surprising given how few individuals we observe around these cutoffs. On the other hand, there is evidence that loan size is negatively associated with the probability of default for pay levels over $300. There are large decreases in the fraction of borrowers in default at five of the cutoffs in this range, little change at three of the cutoffs, and evidence of an increase in default at only one cutoff.

Table 3 reports formal results from our instrumental variables specification. These estimates represent the causal effect of loan amount holding selection constant. The dependent variable is an indicator variable equal to one if a borrower defaulted on her first loan. 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. Column 1 presents results using the maximum loan an individual is eligible for as an instrumental variable for loan amount, controlling for a linear trend in net pay and month, year, and state effects. Column 2 adds control for a quadratic in net pay. Column 3 instruments for loan size using a set of nine indicator variables equal to one if a borrower is eligible for a particular loan (equivalent to Column 3 of our first-stage results).

Our results are remarkably consistent across specifications. After accounting for selection, borrowers are 3.7 to 4.2 percentage points less likely to default for each additional $100 lent. Controlling for a borrower's gender, race, credit score, and savings balance leaves the results unchanged. We consider these results evidence of advantageous incentives.

While not ruled out by theory, the finding of advantageous incentives is perhaps surprising given the emphasis on adverse incentives (e.g. moral hazard) among policymakers and in the theoretical literature. There are a number of plausible mechanisms through which loan size could lead to a lower probability of default. First, because defaulting precludes borrowers from obtaining additional loans, borrowers may wish to maintain access to higher credit lines and choose not to default on larger loans. Second, borrowers eligible for larger loans may be able to substitute away from more expensive credit options.

To shed some light on which of these mechanisms is more empirically relevant, we test the heterogeneity of the incentive effect across borrowers. We hypothesize that borrowers with lower discount rates value access to future credit more than other borrowers. In this scenario, the expansion of credit will have a larger impact on more patient borrowers. In practice, we do not have direct evidence on discount rates. Instead we split the sample by pre-loan credit score, pre-loan checking balance, age, and gender. There is evidence that borrowers with higher credit scores and checking balances are likely to have lower discount rates, and that patience is increasing in age (e.g. Gilman, 1976; Harrison and Williams, 2002; Dohmen, Falk, Human and Sunde, forthcoming; Meier and Sprenger, 2010). To the best of our knowledge, there is no evidence that discount rate differs by gender.

Our second hypothesis is that the same borrowers we have identified as being more patient - those with higher credit scores, higher checking account balances, and those who are older - are likely to have access to less expensive forms of credit than other borrowers. In this scenario, the expansion of credit will have a smaller impact on these borrowers. Splitting the sample by credit score, checkings balance, and age therefore provides a simple test of which of our two plausible mechanisms drives our results.

Table 4 presents results for each of our mutually exclusive subgroups. The sample is restricted to borrowers at Firm $A$ as we lack demographic information for borrowers at Firm $B$. For each group, we split the sample into two groups using the median value of the sample. We regress default on loan amount fully interacted with our group indicator variable. We instrument the interacted loan amount variables with the maximum loan an individual is eligible for, which is also fully interacted with our group indicator variable. In all regressions we control for a quadratic trend in net pay and state, month, and year effects, with cluster standard errors at the net pay level.

The incentive effect is significantly larger for borrowers who have higher ex-ante credit scores, higher checking balances, and who are older. Borrowers with higher credit scores are 4.9 percentage points less likely to default than borrowers with lower credit scores for every additional $100 lent. Borrowers with higher checking balances are 1.7 percentage points less likely to default, and borrowers over 40 are 2.3 percentage points less likely to default. All of the differences are significant at the one percent level. The incentive response does not appear to differ by gender. In sum, the incentive effect is consistently larger among groups likely to have lower discount rates. These results are consistent with the hypothesis that the incentive effect is driven by the dynamic incentives created by a higher future credit line.

### 5.3 OLS and Selection Results

Table 5 reports results from an OLS model of default on borrower characteristics. These estimates combine the impact of incentives and selection. The impact of adverse selection alone is the coefficient from our OLS regressions minus the coefficient from our IV results in Table 3. The dependent variable is an indicator variable equal to one if a borrower defaulted on her first loan. We report robust standard errors in parentheses and multiply all coefficients and standard errors by 100 so that our coefficients can again be interpreted as the percentage point change in default associated with a $1 larger loan.

Column 1 of Table 5 presents results controlling for net pay and state, month, and year effects. Column 2 adds a quadratic in net pay. Consistent with theoretical foundations of information economics, loan amount is positively associated with default risk in both specifications. For each additional $100 lent, borrowers are 3.8 percentage points more likely to default. Taken together with our estimates in Table 3, our OLS results imply that for every $100 increase in loan size selected by an individual, she is 6.9 to 8.0 percentage points more likely to default. This suggests that, unlike moral hazard, adverse selection is a serious concern even in a market that specializes in financing high-risk borrowers.

Thus far we have assumed that the OLS estimates include only the effects of selection and incentives. In practice there may be other determinants of loan size. For example, firms may discriminate based on observable characteristics, offering larger loans to borrowers less likely to default. Column 3 presents results using the sample of borrowers with demographic controls to insure that the change in sample is unimportant, while Column 4 adds controls for gender, race, credit score and checking account balance. Adding demographic controls does not change the coefficient on loan amount. This is consistent with the idea that the cross-sectional correlation between loan amount and default is driven only by borrower selection and incentives.

Appendix Table 3 presents results controlling for an individual's eligibility category. If the eligibility categories control for all variation in what loans are available, this provides another estimate of the selection effect. For each additional $100 lent, borrowers are 4.4 percentage points more likely to default. While the point estimates are smaller than our results in Table 5, this provides additional evidence that the selection effect is larger than the naïve cross-sectional relationship between loan size and default would suggest.

### 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 examining the conditional density of borrowers around the cutoff, and by testing whether the observable characteristics of borrowers trends smoothly through the cutoff. 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.

Column 1 of Table 6 tests for potential kinks in the density of observations at each loan-eligibility cutoff. 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. To control for the secular jumps in density that we would expect in any state (e.g. from employers setting wages around multiples of $100), we also include a set of control states that offer loans in continuous amounts. In these control states there is no reason to expect discontinuous changes in density around the eligibility cutoffs. The inclusion of these states allows us to isolate changes in density due to strategic manipulations to the loan-eligibility cutoffs in our treatment states.

Specifically, we use the first loan made to borrowers in states where Firm $A$ offers continuous loans. We drop individuals with incomes in the top or bottom 1 percent of the original sample, and drop loans made in Michigan as there are fewer than 1,000 observations during our sample period. This provides 108,286 loans in 13 states as a control. We then collapse the data into equal-sized bins of $25 for each state. The key variables in our data are the fraction of observations in each $25 bin, and the net pay amount that the bins are centered around. We regress the fraction of observations in each bin on the maximum loan a borrower is eligible for (set to zero in the control states) and a set of indicator variables for earning above each $100 cutoff. The set of indicator variables will control for the secular jumps in density that we would expect in any state, while our offer curve variable will identify any changes in density due to strategic manipulations in loan-eligibility guidelines. To ease the interpretation of our results, we multiply coefficients and standard errors by 10,000.

Column 1 of Table 6 presents results of this test controlling for a quadratic in net pay and month, year and state of loan effects. This is analogous to Column 2 of our first-stage results. Our results do not suggest an unexpected jump in the density function around the eligibility cutoffs. The coefficient on the offer curve variable is small (0.009, se = 0.018) and not statistically significant. In unreported results, we allow the estimated effect of each discontinuity 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 ($p$-value = 0.313).

Columns 2 through 6 of Table 6 present reduced form estimates for predetermined characteristics of borrowers. If there is a discontinuous change at the cutoffs, that would indicate that borrowers who were eligible for larger loans differ in a way that would invalidate our research design. Each entry is from a regression of a predetermined characteristic on the maximum loan amount a borrower is eligible for, a quadratic in net pay, and state, month, and year effects. We multiply coefficients and standard errors by 100 to make the coefficients easier to interpret. This specification is identical to Column 2 of our first-stage results in Table 2. Borrowers eligible for larger loans are somewhat more likely to be older and less likely to be male, though both results are only statistically significant at the ten percent level. On the other hand, borrowers eligible for larger loans are no more likely to have a higher credit score or checking account balance. Results are identical if we allow the effect to vary by cutoff. Given the mixed signs and general lack of statistical significance on both of our robustness checks, we interpret our results as showing no clear evidence that our identifying assumption is violated.

## 6 Conclusion

This paper has shown evidence of adverse selection and advantageous incentives in a subprime consumer loan market. While borrowers who select loans that are \$100 larger are 6.9 to 8.0 percentage points more likely to default, individual borrowers are 3.7 to 4.2 percentage points less likely to default for each additional \$100 lent. The incentive effect is concentrated among borrowers who are likely to have low discount rates, consistent with the idea that future loan terms have a large impact on borrower behavior.

Our finding of advantageous incentives is notable given the emphasis on moral hazard by both policymakers and 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 | 37.30 | 4,691 | 36.80 | 10,189 |
| Loan Amount | 191.52 | 4,691 | 254.83 | 10,191 |
| Net Biweekly Pay | 716.33 | 4,626 | 822.78 | 8,624 |
| Default on First Loan | 0.10 | 4,691 | 0.23 | 10,191 |
| Default on Any Loan | 0.39 | 4,691 | 0.61 | 10,191 |
| Male | 0.30 | 2,810 | | |
| White | 0.18 | 2,638 | | |
| Black | 0.81 | 2,638 | | |
| Credit Score | 550.07 | 4,095 | | |
| Checking Balance | 228.27 | 4,599 | | |

This table reports summary statistics for two payday lending firms. The sample is first loans made to borrowers with valid pay data who are paid biweekly. We drop individuals with incomes in the top or bottom 1 percent of the sample.

**Table 2**
**First Stage Results**

| | 1 | 2 | 3 |
|---|---|---|---|
| Offer Curve | 0.458*** | 0.490*** | |
| | (0.018) | (0.018) | |
| $100 Cutoff | | | 0.927*** |
| | | | (0.084) |
| $150 Cutoff | | | 0.674*** |
| | | | (0.063) |
| $200 Cutoff | | | 0.727*** |
| | | | (0.054) |
| $250 Cutoff | | | 0.516*** |
| | | | (0.064) |
| $300 Cutoff | | | 0.494*** |
| | | | (0.090) |
| $350 Cutoff | | | 0.280*** |
| | | | (0.091) |
| $400 Cutoff | | | 0.684*** |
| | | | (0.097) |
| $450 Cutoff | | | 0.390*** |
| | | | (0.114) |
| $500 Cutoff | | | 0.669*** |
| | | | (0.126) |
| Linear in Net Pay | Y | Y | Y |
| Quadratic in Net Pay | N | Y | Y |
| F-Statistic | 654.87 | 764.05 | 151.56 |
| Observations | 13246 | 13246 | 13246 |

This table reports first stage estimates. The sample is first loans made to borrowers with valid pay data who are paid biweekly. We drop individuals with incomes in the top or bottom 1 percent of the sample. The dependent variable is the dollar amount of the loan. Coefficients are scaled so that they can be interpreted as the increase in loan size for each additional dollar of credit offered. All regressions control for month, year and state of loan effects. Standard errors are clustered at the net pay level. We report the F-statistic for the null hypothesis that the loan eligibility indicators are jointly equal to zero. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

**Table 3**
**IV Results**

| | 1 | 2 | 3 |
|---|---|---|---|
| Loan Amount | $-0.041^{***}$ | $-0.037^{***}$ | $-0.042^{***}$ |
| | (0.014) | (0.014) | (0.014) |
| Offer Curve | Y | Y | N |
| Eligibility Indicators | N | N | Y |
| Linear Net Pay | Y | Y | Y |
| Quadratic Net Pay | N | Y | N |
| Observations | 13246 | 13246 | 13246 |

This table reports instrumental variable estimates. The sample is first loans made to borrowers with valid pay data who are paid biweekly. We drop individuals with incomes in the top or bottom 1 percent of the sample. The dependent variable is an indicator variable equal to one if the borrower defaults. We report the coefficient on loan amount, instrumented for using the offer curve or a set of loan eligibility indicators. Coefficients and standard errors are multiplied by 100. All regressions control for net pay and month, year and state of loan effects. 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**
**IV Results by Subgroup**

| | Good Credit | Poor Credit | High Savings | Low Savings | Older | Younger | Male | Female |
|---|---|---|---|---|---|---|---|---|
| Loan Amount | −0.058*** | −0.009 | −0.047*** | −0.030* | −0.047*** | −0.024 | −0.029 | −0.026 |
| | (0.016) | (0.016) | (0.016) | (0.017) | (0.016) | (0.016) | (0.020) | (0.020) |
| Offer Curve | Y | Y | Y | Y | Y | Y | Y | Y |
| Quadratic Net Pay | Y | Y | Y | Y | Y | Y | Y | Y |
| Observations | 2025 | 2016 | 1281 | 2741 | 1847 | 2779 | 829 | 1941 |

This table reports pooled instrumental variable estimates for mutually exclusive subsamples for Firm A. The sample is first loans made to borrowers with valid pay data who are paid biweekly. We drop individuals with incomes in the top or bottom 1 percent of the sample. The dependent variable is an indicator variable equal to one if the borrower defaults. We report the coefficient on loan liability interacted by sample, instrumented for using the offer curve. Coefficients and standard errors are multiplied by 100. The number of observations in the pooled regression is also reported. High credit and checking are defined as having a credit score or checking account balance at or above the median. All regressions control for net pay and month, year and state of loan effects. Standard errors are clustered at the net pay level. The reported number of observations vary as not all demographic controls are available for every borrower. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

**Table 5**
**OLS Results**

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Loan Amount | 0.038*** | 0.039*** | 0.032*** | 0.033*** |
| | (0.003) | (0.003) | (0.006) | (0.006) |
| Net Pay | −0.017*** | −0.031*** | −0.027*** | −0.023*** |
| | (0.001) | (0.004) | (0.006) | (0.006) |
| Net Pay Sq | | 0.000*** | 0.000*** | 0.000*** |
| | | (0.000) | (0.000) | (0.000) |
| Age | | | | −0.218*** |
| | | | | (0.037) |
| Male | | | | 0.635 |
| | | | | (1.135) |
| Black | | | | 2.102 |
| | | | | (1.295) |
| Credit Score | | | | −0.034*** |
| | | | | (0.003) |
| Checking Balance | | | | −0.005*** |
| | | | | (0.001) |
| $R^2$ | 0.046 | 0.047 | 0.028 | 0.077 |
| Observations | 13246 | 13246 | 4622 | 4622 |

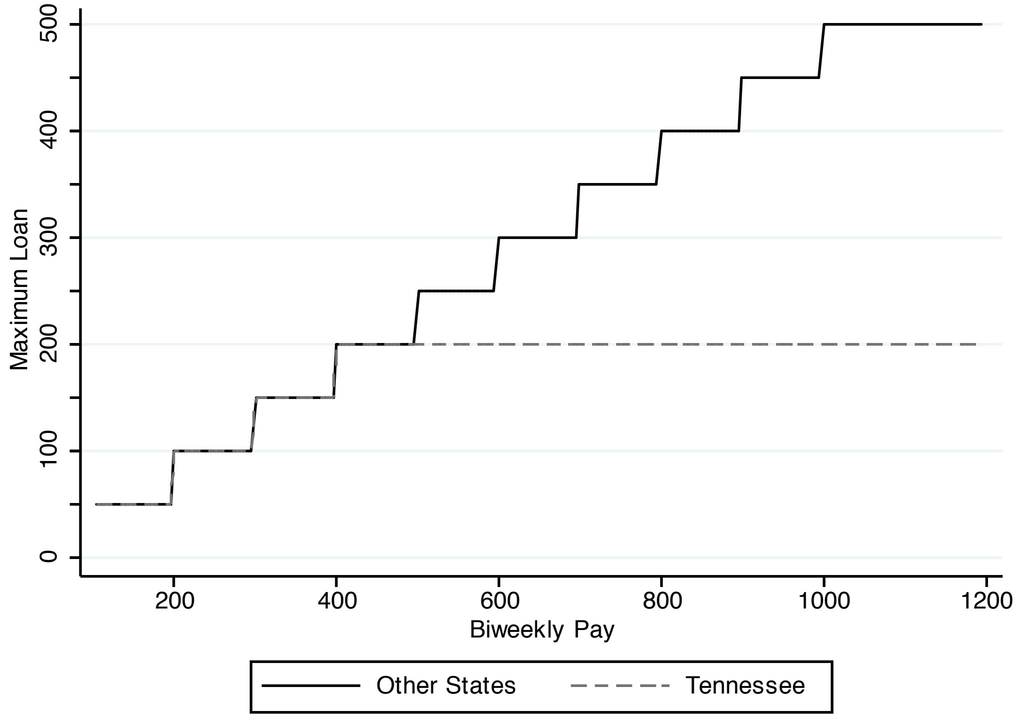
This table reports OLS estimates. The sample is first loans made to borrowers with valid pay data who are paid biweekly. We drop individuals with incomes in the top or bottom 1 percent of the sample. The dependent variable is an indicator variable equal to one if the borrower defaults. Coefficients and standard errors are multiplied by 100. All regressions control for month, year and state of loan effects. Robust standard errors are reported. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

**Table 6**
**Continuity of Borrower Characteristics**

| | Density | Age | Male | Black | Credit | Savings |
|---|---|---|---|---|---|---|
| Loan Amount | 0.009 | 0.484* | −0.030* | 0.001 | −0.306 | 7.375 |
| | (0.018) | (0.294) | (0.017) | (0.016) | (4.726) | (7.178) |
| Observations | 779 | 4622 | 2770 | 2602 | 4036 | 3878 |

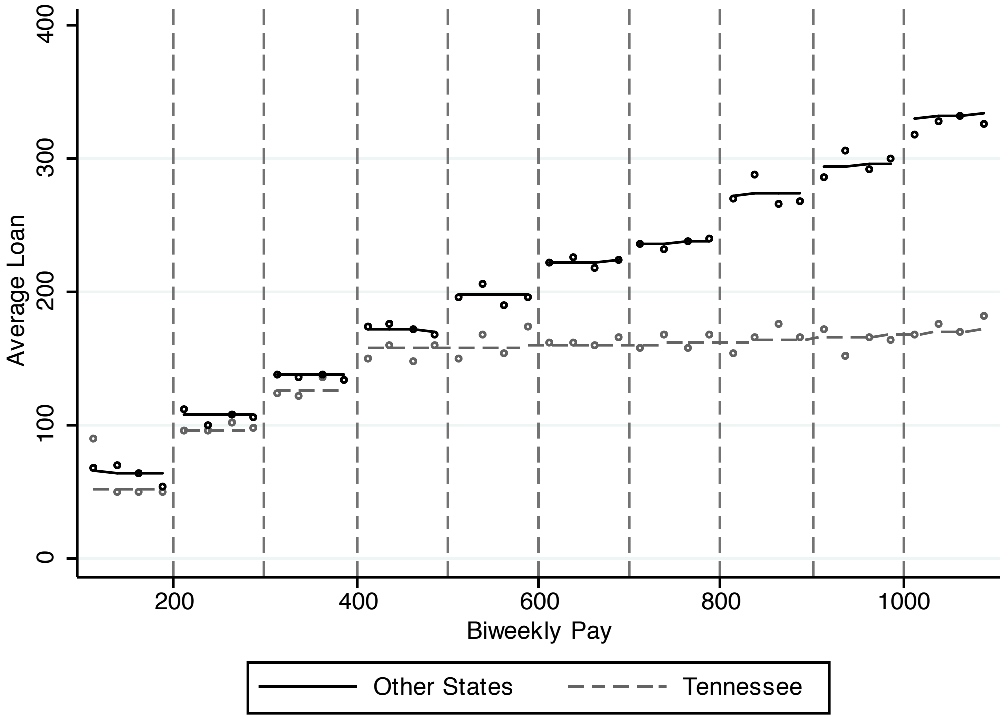
This table reports reduced form estimates for the conditional density function and available predetermined characteristics for Firm A. The sample is first loans made between 1/2000 and 7/2004 to borrowers with valid pay data who are paid biweekly. We drop individuals with incomes in the top or bottom 1 percent of the sample. The dependent variable is listed at the top of the column. For column 1, the dependent variable is the fraction of borrowers in a $25 wide bin. We report the coefficient on the offer curve variable. Coefficients and standard errors are multiplied by 10,000 in Column 1, and 100 in Columns 2 through 7. All regressions control for a quadratic in net pay, and month, year and state of loan effects. Column 1 also controls for a set of indicator variables for earning above each $100 threshold. Standard errors are clustered at the net pay level in Columns 2 through 7. The reported number of observations vary as not all demographic controls are available for every borrower. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

**Figure 1**
**Maximum Loan Amount**



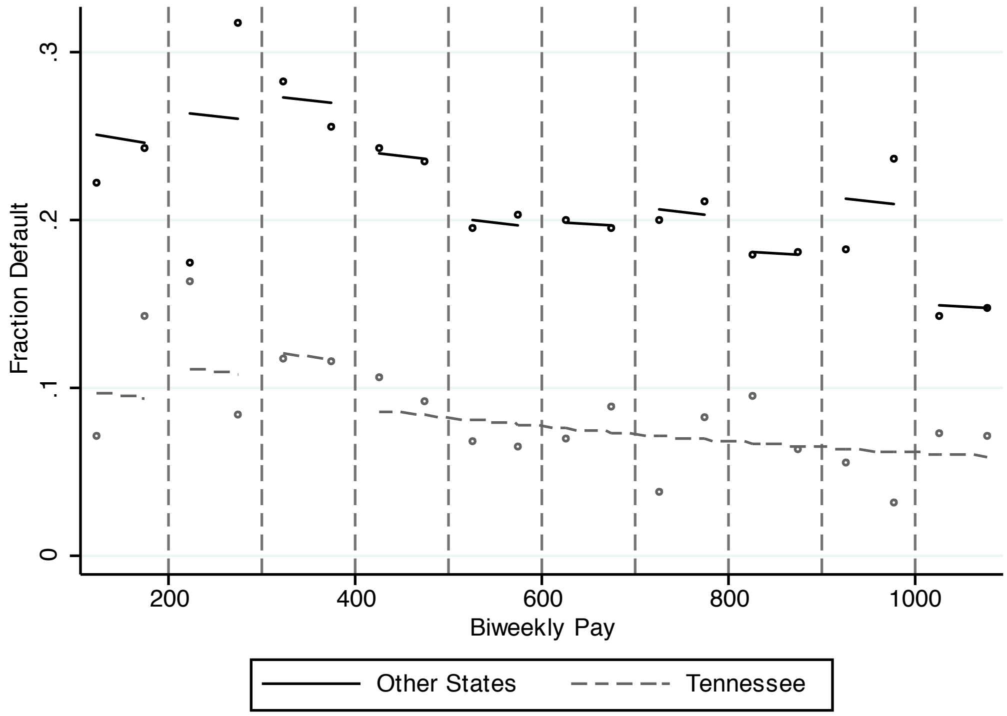

This figure illustrates the loan eligibility rule used by firms in our sample. Individuals are eligible for loans up to but not exceeding half of net pay.

**Figure 2**
**First Stage Results**



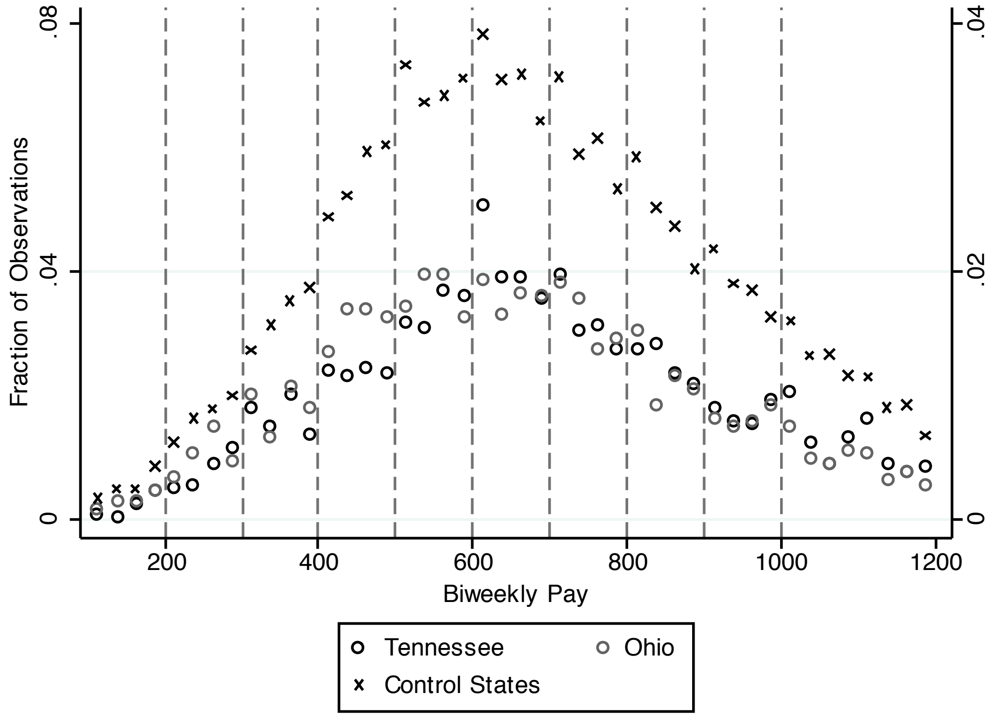

This figure plots the average first loan of first time borrowers and the predicted average loan from a regression of loan amount on biweekly pay and set of loan eligibility indicators. Bin width is $25.

**Figure 3**
**Reduced Form Results**




This figure plots the average rate of default for first time borrowers and the predicted rate of default from a regression of loan amount on biweekly pay and set of loan eligibility indicators. Bin width is \$50.

**Figure 4**
**Density of Observations**




This figure plots the fraction of observations observed in each $25 bin. Control states include all states in Firm A that made more than 1,000 loans and that offer continuous loan amounts. The control states are plotted on the right axis while Tennessee and Ohio are plotted on the left axis.

**Appendix Table 1**
**First Stage Robustness Checks**

| | All Pay Frequencies | | | Firm A | | | Firm A with controls | | |
|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 1 | 2 | 3 | 1 | 2 | 3 |
| Offer Curve | 0.580*** | 0.573*** | | 0.540*** | 0.544*** | | 0.535*** | 0.539*** | |
| | (0.008) | (0.008) | | (0.021) | (0.024) | | (0.021) | (0.024) | |
| $100 Cutoff | | | 0.816*** | | | 1.012*** | | | 1.022*** |
| | | | (0.070) | | | (0.120) | | | (0.120) |
| $150 Cutoff | | | 0.684*** | | | 0.621*** | | | 0.604*** |
| | | | (0.049) | | | (0.082) | | | (0.084) |
| $200 Cutoff | | | 0.798*** | | | 0.623*** | | | 0.623*** |
| | | | (0.036) | | | (0.064) | | | (0.064) |
| $250 Cutoff | | | 0.502*** | | | 0.460*** | | | 0.444*** |
| | | | (0.052) | | | (0.098) | | | (0.097) |
| $300 Cutoff | | | 0.478*** | | | 0.619*** | | | 0.620*** |
| | | | (0.080) | | | (0.114) | | | (0.112) |
| $350 Cutoff | | | 0.282*** | | | 0.267* | | | 0.251* |
| | | | (0.087) | | | (0.142) | | | (0.140) |
| $400 Cutoff | | | 0.679*** | | | 0.715*** | | | 0.710*** |
| | | | (0.085) | | | (0.194) | | | (0.191) |
| $450 Cutoff | | | 0.400*** | | | 0.508* | | | 0.527** |
| | | | (0.098) | | | (0.260) | | | (0.255) |
| $500 Cutoff | | | 1.013*** | | | 0.697*** | | | 0.685*** |
| | | | (0.093) | | | (0.264) | | | (0.262) |
| Linear in Net Pay | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Quadratic in Net Pay | N | Y | Y | N | Y | Y | N | Y | Y |
| F-Statistic | 5874.31 | 5035.25 | 783.45 | 681.38 | 526.06 | 98.50 | 665.09 | 514.21 | 95.80 |
| Observations | 23163 | 23163 | 23163 | 4622 | 4622 | 4622 | 4622 | 4622 | 4622 |

This table reports first stage estimates. The sample is first loans made to borrowers with valid pay data who are paid biweekly. We drop individuals with incomes in the top or bottom 1 percent of the sample. The dependent variable is the dollar amount of the loan. Coefficients are scaled so that they can be interpreted as the increase in loan size for each additional dollar of credit offered. All regressions control for month, year and state of loan effects. Standard errors are clustered at the net pay level. We report the F-statistic for the null hypothesis that the loan eligibility indicators are jointly equal to zero. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

**Appendix Table 2**
**IV Robustness Checks**

| | All Pay Frequencies | | | Firm A | | | Firm A with controls | | |
|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 1 | 2 | 3 | 1 | 2 | 3 |
| Loan Amount | −0.042*** | −0.042*** | −0.044*** | −0.039*** | −0.033** | −0.029** | −0.036*** | −0.030** | −0.026* |
| | (0.004) | (0.004) | (0.004) | (0.015) | (0.015) | (0.015) | (0.014) | (0.015) | (0.015) |
| Offer Curve | Y | Y | N | Y | Y | N | Y | Y | N |
| Eligibility Indicators | N | N | Y | N | N | Y | N | N | Y |
| Linear Net Pay | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Quadratic Net Pay | N | Y | N | N | Y | N | N | Y | N |
| Observations | 23163 | 23163 | 23163 | 4622 | 4622 | 4622 | 4622 | 4622 | 4622 |

This table reports instrumental variable estimates. The sample is first loans made to borrowers with valid pay data. We drop individuals with incomes in the top or bottom 1 percent of the sample. The dependent variable is an indicator variable equal to one if the borrower defaults. We report the coefficient on loan amount, instrumented for using the offer curve or a set of loan eligibility indicators. Coefficients and standard errors are multiplied by 100. All regressions control for net pay and month, year and state of loan effects. 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 3**
**OLS Results Within Loan Eligibility Groups**

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Loan Amount | 0.044*** | 0.044*** | 0.044*** | 0.044*** |
| | (0.003) | (0.003) | (0.006) | (0.006) |
| Net Pay | −0.005*** | −0.004 | −0.017** | −0.015* |
| | (0.002) | (0.007) | (0.009) | (0.009) |
| Net Pay Sq | | 0.000 | 0.000 | 0.000* |
| | | (0.000) | (0.000) | (0.000) |
| Age | | | | −0.212*** |
| | | | | (0.037) |
| Male | | | | 0.569 |
| | | | | (1.128) |
| Black | | | | 2.205* |
| | | | | (1.292) |
| Credit Score | | | | −0.034*** |
| | | | | (0.003) |
| Checking Balance | | | | −0.005*** |
| | | | | (0.001) |
| $R^2$ | 0.051 | 0.051 | 0.034 | 0.082 |
| Observations | 13246 | 13246 | 4622 | 4622 |

This table reports OLS estimates controlling for the maximum loan a borrower is eligible for. The sample is first loans made to borrowers with valid pay data who are paid biweekly. We drop individuals with incomes in the top or bottom 1 percent of the sample. The dependent variable is an indicator variable equal to one if the borrower defaults. Coefficients and standard errors are multiplied by 100. All regressions control for month, year and state of loan effects. Robust standard errors are reported. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

**Appendix Table 4**
**Bin Width Test**

| Bin Size | p-value |
|---|---|
| 10 | 0.901 |
| 20 | 0.572 |
| 33 | 0.217 |
| 50 | 0.103 |
| 100 | 0.529 |

This table reports results of the optimal bin width. The sample is first loans made to borrowers with valid pay data who are paid biweekly. We drop individuals with incomes in the top or bottom 1 percent of the sample. Each column reports results of a regression of where we add a set of interactions between the bin dummies and the running variable to a base regression of the outcome variable on the set of bin dummies. The p-value is from the test of whether the interactions are jointly significant.