Information Asymmetries in Consumer Credit Markets: Evidence from Payday Lending*

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Abstract

Information asymmetries are prominent in theory but difficult to estimate. This paper presents a new empirical test for moral hazard and adverse selection that exploits sharp discontinuities in borrowers' eligibility for payday loans. Surprisingly, we find no evidence of moral hazard. If anything, an exogenous increase in credit lowers the probability that a borrower defaults. On the other hand, there is evidence of significant adverse selection into larger payday loans. Borrowers who chose $50 larger loans are 7.8 to 9.0 percentage points more likely to default, a 46 to 52 percent increase.

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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 administrative data from two payday lenders. We identify the impact of moral hazard in our sample by exploiting the fact that payday loan amounts are a discontinuous function of a borrower's 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 loans of different sizes. This institutional rule allows us to attribute any discontinuous relationship between loan outcomes and net pay to the causal impact of loan size. As a cross-sectional regression of default on loan size combines the selection and incentive effects of loan size, we obtain an estimate of selection by subtracting our regression discontinuity estimate from the cross-sectional coefficient on loan size.

Our empirical analysis begins by documenting significant credit constraints among payday borrowers. The loan-eligibility cutoffs are highly predictive of average loan size across a number of specifications. Each \$50 increase in credit leads to a \$23 to \$25 increase in average loan size.

Next, we use our regression discontinuity design to estimate the causal impact of additional credit on default. Surprisingly, we find a negative relationship between available credit

[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.

and default that is significant at the 10 percent level. Our point estimates suggest that borrowers earning just above an eligibility cutoff are 2.915 to 3.136 percentage points less likely to default compared to borrowers earning just below an eligibility cutoff. The implied second stage estimates suggest that a $50 increase in loan size lead to a 5.8 to 6.8 percentage point decrease in the probability of default, a 34 to 40 percent decrease. Most importantly, the relative precision of our results allows us to rule out all but the smallest impact of moral hazard. A separate difference-in-differences strategy yields similar estimates.

Finally we estimate the extent of adverse selection into larger loans. In our OLS results, a $50 increase in loan size is associated with a 2.15 percentage point increase in the probability of default in our baseline results, and a 2.00 percentage point increase controlling for demographic characteristics. Taken together with our estimates of moral hazard, this suggests that borrowers who *choose* a loan that is $50 larger are 7.8 to 9.0 percentage points more likely to default, a 46 to 52 percent increase.

We conclude our analysis by testing two key threats to our interpretation of the results. First, individuals may opt out of borrowing if they are not eligible for a sufficiently large 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. Second, our empirical design may be misspecified. To ensure that our estimates identify discontinuities that exist due to institutional factors, we replicate our empirical results in a set of states where loan size is not a discontinuous function of income. We find no evidence of a relationship between loan size and income or default and income around the loan-eligibility cutoffs in states where loan size is not institutionally set to be a discontinuous function of pay.

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. They find that individuals who borrow an extra $1,000 for unobservable reasons have an 18 percent higher rate of default than those who do 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). Also related is the work of Melzer and Morgan (2010), who find results consistent with adverse selection into bank overdraft services when payday lending is available.

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 in which 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 $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, 2011) 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, 2011), and lead to increased difficulty paying mortgage, rent, and utility bills (Melzer, 2011).

The remainder of the paper is structured as follows. Section 2 provides background on our institutional setting and describes our data. Section 3 reviews the theoretical framework 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 designate that day as the loan's due date. The customer writes a check for the amount of the loan plus a finance charge that is typically \$15 to \$18 per \$100 borrowed.[2] The lender agrees to hold the check until the next payday, typically for 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.

Important to our analysis is the fact that 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 for one pay period. 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 paid biweekly is depicted in Figure 1. Note that Tennessee payday lenders only offer loans up to \$200, while payday lenders in all other states

[2] While some lenders use subprime credit scores to screen applicants, there is no risk-based pricing in this market and all borrowers pay the same finance charge.

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

Because default precludes subsequent borrowing, our main analysis restricts our sample to the first loan made to each individual. Within each pay frequency (biweekly, monthly, semimonthly, weekly) 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, allows a more straightforward presentation of the result. This group also makes up nearly 50 percent of the sample and results are identical if we include all borrowers. Finally, we restrict our analysis to borrowers with valid income data and drop individuals with incomes in the top or bottom 1 percent of the sample, leaving us borrowers with biweekly earnings between $200 and $1,800. This leaves us with 4,621 observations from Firm A and 8,624 observations from Firm B.

Summary statistics for our core sample are displayed in Table 1. The average borrower at Firm A is more likely to be female and black, is 37 years old, has a biweekly income of $715.83, and has a checking account balance of $227.06. The mean first loan is for $190.24. The typical borrower at Firm B is also 37 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, where Firm B operates, cap loans at $500 rather and $200.

Ten percent of borrowers default on their first loan at Firm A, and 39 percent default

[3]Firm A offers loans in continuous amounts in the other 13 states in which it operates. We drop these states from our main analysis as we have no way of separately identifying the impact of incentives when available credit is determined continuously. We use these states' data in falsification tests discussed in Section 5.4. Ohio and Tennessee offer loans in a discontinuous manner due to a legacy policy. Firm B operates in eight other states where complete data are not yet available.

[4]Lenders use credit scores to screen applicants. These are distinct from FICO scores and are computed by a third party called Teletrack. For more information on this subprime credit scoring process, see Agarwal, Skiba and Tobacman (2009).

during the sample period. Default rates at Firm B are even higher: 21 percent of borrowers default on their first loan, and 61 percent default during the sample period.

3 Conceptual Framework

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 version of the model (e.g., adverse incentives), individual borrowers are more likely to default on larger or more expensive loans. This assumption is motivated by at least two considerations. 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 do. 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 obligation 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 our 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

[5] Models of asymmetric information typically assume limited commitment by borrowers, the idea that borrowers always have the option for personal bankruptcy. 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).

this section. Further research will be needed to truly disentangle the mechanisms through which our results operate in the payday market.

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):

$$D_i = \alpha + \gamma L_i + \varepsilon_i \tag{1}$$

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 potential inference problem 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:

$$E[\varepsilon_i|pay_i = c_l + \Delta]_{\Delta \to 0^+} = E[\varepsilon_i|pay_i = c_l - \Delta]_{\Delta \to 0^+} \tag{2}$$

where pay_i is an individual's net pay and c_l is the eligibility cutoff for loan size l. Equation (2) 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). 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 (2).[6]

Formally, let loan size L_i be a smooth function of an individual's pay with a discontinuous

[6]This approach assumes that loan eligibility impacts default only through loan size. 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. Note that Adams et al. (2009) use the same assumption to identify the impact of moral hazard in the subprime auto loan market.

jump at each loan-eligibility cutoff c_l:

$$L_i = f(pay_i) + \sum_{l=100}^{500} \lambda_l (pay_i \geq c_l) + \eta_i \tag{3}$$

where λ_l measures the contemporaneous increase in debt in response to a credit 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. In our empirical analysis we experiment with several functional forms of $f(pay_i)$, including a seventh-order polynomial, a linear spline, and a local linear regression. In all specifications we include a separate trend for Tennessee after the \$200 loan limit to account for the fact that Tennessee caps loans at a lower limit than other states do. To address potential concerns about discreteness in pay, we cluster our standard errors at the net-pay level (Lee and Card, 2008). 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 essentially unchanged.

One key threat to a causal interpretation of our regression discontinuity 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 test points to the kind of selective borrowing that invalidates our empirical design.

A more general threat is the possibility that our empirical design is misspecified. To ensure that our estimates identify actual discontinuities in loan size and default that exist due to institutional factors, we replicate our empirical specifications in a set of states where loan size is not a discontinuous function of income. Consistent with our empirical design, in states where loan size is not institutionally set to be a discontinuous function of pay, we do not find a relationship between loan size and income or default and income around the loan-eligibility cutoffs.

Under the above assumptions, we can use Equation (3) as the first stage to estimate the average causal effect for individuals induced into a larger loan by earning an amount just above a cutoff. In this discontinuity framework, γ is identified using within-state variation around each cutoff.

An alternative strategy to estimate the impact of moral hazard exploits the fact that payday loans in Tennessee are capped at \$200. As a result, there is a trend break in the relationship between net pay and maximum loan size in Tennessee. Specifically, we use the interaction of an indicator variable for a borrower being from Tennessee and being eligible for a \$200 loan with net pay as an instrumental variable. The differences in state trends in loan amounts and default after the \$200 cutoff identifies γ. We use this difference-in-difference approach as an additional check of our regression discontinuity estimates.

A simple extension of our approach, first pioneered by Adams et al. (2009), allows us to also estimate the magnitude of selection in our sample. Recall that a cross-sectional regression of default on loan size combines both selection and incentive effects. By subtracting our estimate of moral hazard from the cross-sectional coefficient on loan size, we obtain an estimate of selection. It is important to note that this approach assumes that our estimate of moral hazard is the relevant estimate for the full population. This assumption would be violated if borrowers right around the nine eligibility cutoffs have a different marginal return to credit than other borrowers.

5 Results

5.1 The Impact of Loan Eligibility on Loan Amount

The effect of loan eligibility on loan amount is presented graphically in Figures 2A-C. Each figure plots average loan amounts in \$25 income bins for the first loans of borrowers with biweekly take-home pay between \$100 and \$1,100. Figure 2A plots fitted values from a regression of loan size on a seventh-order polynomial in net pay, an indicator for living in Tennessee, and a separate quadratic trend for Tennessee borrowers eligible for a \$200 loan. That is, the fitted values for Figure 2A come from the following specification:

$$L_i = \alpha_0 + \sum_{l=100}^{500} \alpha_{1l} I_{pay_i \geq c_l} + \sum_{p=1}^{7} \beta_{1p} pay_i^p + \alpha_2 I_{TN} + \sum_{j=1}^{2} \beta_{2j}(I_{TN} \times I_{pay_i \geq 400} \times pay_i^j) + \varepsilon_i \quad (4)$$

where α_{1l} is the effect of having a biweekly income above the cutoff for each loan size l. Note that we include a quadratic trend for borrower's in Tennessee with incomes above \$400 to account for the state's \$200 cap.

Figure 2B plots fitted values from a linear spline specification that allows the impact of pay to vary between each cutoff:

$$L_i = \alpha_0 + \sum_{l=100}^{500} \left(\alpha_1 I_{pay_i \geq c_l} + \beta_1 (I_{pay_i \geq c_l} \times pay_i)\right) + \alpha_2 I_{TN} + \beta_2 (I_{TN} \times I_{pay_i \geq 400} \times pay_i) + \varepsilon_i \quad (5)$$

where $\alpha_l - \alpha_{l-50} + 2 \times l \times (\beta_l - \beta_{l-50})$ is the effect of having an income above the cutoff for each loan size l. Note that we include a separate linear trend for borrowers in Tennessee with incomes above \$400 to account for the state's \$200 cap.

Our final specification displayed in Figure 2C stacks data from each cutoff and controls for pay using a local linear specification interacted with the loan-eligibility cutoff:

$$\widehat{L_i} = \alpha_0 + \alpha_1 I_{pay_i \geq c} + \beta_1 (pay_i - c) + \beta_2 \left(I_{pay_i \geq c} \times (pay_i - c)\right) + \varepsilon_i \quad (6)$$

where α_1 is the impact of having an income above the loan-eligibility cutoff. To normalize the loan amounts across the nine cutoffs, Figure 2C plots residualized loan amounts $\widehat{L_i}$ from a regression of raw loan size on a set of indicator variables for each cutoff instead of raw loan amount L_i. The formal estimates in Table 2 instead use raw loan amounts L_i as a dependent variable controlling for a set of indicators for which cutoff each observation is drawn from.

Loan eligibility is highly predictive of average loan size across all three specifications. While average loan amount is approximately constant between each two consecutive cutoffs (and after the \$200 loan cutoffs in Tennessee), the typical loan increases approximately \$25 at each \$50 eligibility cutoff. It is also interesting to note that at lower cutoffs, borrowers take out loans that are near the maximum allowed level. The average loan size for borrowers earning just above the \$100 cutoff is at or just above \$100. In contrast, the typical debtor around higher cutoffs takes out loans that are significantly less than the maximum loan amount. The average loan size at the \$500 cutoff, for example, is just over \$300.

Table 2 presents formal estimates for the figures just described. The sample consists of first loans for borrowers with biweekly take-home pay between \$100 and \$1,100. Analogous to Figure 2A, the first column controls for income using a seventh-order polynomial in net pay with a separate quadratic trend for Tennessee above the \$200 loan cutoff. Column 2, corresponding to Figure 2B, controls for income using a linear spline with a separate linear trend for Tennessee after the \$200 cutoff. Finally, column 3 presents results that are analogous to Figure 2C, where we stack data from each cutoff and control for income using a local linear trend and a linear trend interacted with being earning above the loan-eligibility cutoff. The local linear specification in column 3 uses a bandwidth of \$100, the largest possible.[7] Column 3 also includes a set of indicator variables for the cutoff that each observation is drawn from. All specifications also control for month-by-year and state-of-loan effects, with standard errors clustered at the pay level. The dependent variable in each specification is raw loan amount.

Consistent with the graphical evidence, loan eligibility is highly predictive of loan amount. Controlling for income using a seventh-order polynomial, borrowers with earnings just above a loan cutoff borrow \$22.33 more than borrowers with earnings just below a cutoff. Controlling for income with a linear spline specification, the amount is \$24.90. Stacking data from each cutoff and controlling for income using our local linear specification, borrowers earning just above the loan-eligibility cutoff take out loans that are \$23.02 more than those of borrowers earning just below the cutoff. These estimates imply that for each additional dollar of credit extended, borrowers take out loans that are 46 to 50 cents larger.

These estimates are considerably larger than those previously reported. Gross and Souleles (2002) find that a \$1 increase in a credit card holder's limit raises card spending by just 10 to 14 cents, and Johnson et al. (2006) find that financially constrained households consumed 20 to 40 cents for every \$1 increase in their 2001 tax rebate.

5.2 Moral Hazard

Reduced form results of the causal impact of loan eligibility on default are presented graphically in Figures 3A-C. Following the first stage results, each figure plots average loan amounts in \$25 income bins for the first loans of borrowers with biweekly take-home pay between \$100 and \$1,100. Figure 3A plots fitted values controlling for income using a seventh-order polynomial. Figure 3B plots fitted values using a linear spline. Figure 3C presents results stacking data from each cutoff and controlling for income using a local linear trend and a linear trend interacted with earning above the loan-eligibility cutoff. In sharp contrast to previous research, there is no evidence of moral hazard in our setting. If anything, default appears to be somewhat lower for borrowers with earnings just above loan cutoffs, though the imprecision of our data makes definitive conclusions difficult.

Table 3 presents formal reduced form estimates of the relationship between loan eligibility and default. As with the first stage results, column 1 controls for income using a seventh-

[7] Appendix Figure 1 presents results using a range of bandwidths. Point estimates are slightly smaller with smaller bandwidths, with bandwidths below \$60 yielding estimates that are no longer statistically significant.

order polynomial. Column 2 controls for income using a linear spline income. Column 3 stacks data from each cutoff and controls for a local linear trend in income, a linear trend interacted with earning above the loan-eligibility cutoff, and indicators for which cutoff each observation is drawn from. All specifications also control for month-by-year and state-of-loan effects, with standard errors clustered at the pay level. The dependent variable in each specification is an indicator variable equal to one if the debtor defaults on the first payday loan. We multiply all estimates by 100 so that each coefficient can be interpreted as the percentage change in the probability of default.

Across all three specifications there is a negative relationship between available credit and default. While the raw differences reported in Figures 3A-3C are not statistically significant, the addition of month-by-year and state-of-loan effects sharpens the precision of our estimates such that each estimate is significant at the 10 percent level. Our point estimates suggest that borrowers earning just above an eligibility cutoff, and therefore eligible for an additional \$50 in payday credit, are 2.915 to 3.136 percentage points less likely to default. The implied second stage estimates suggest that a \$50 increase in loan size leads to a 5.8 to 6.8 percentage point decrease in the probability of default. Taken at face value, these impacts are a 34 to 40 percent decrease in the probability of default. Most importantly, the relative precision of our results allow us to rule out all but the smallest impact of moral hazard.

Table 4 presents our polynomial results from column 1 separately by baseline credit score, age, and gender. The samples for the credit score and gender results are restricted to borrowers at Firm A, as we lack that demographic information for borrowers at Firm B. Controls include gender, age, credit score, a seventh-order polynomial in income, and month-by-year and state-of-loan 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 years old. For every additional \$50 of available credit, borrowers with above-median credit scores are 2.128 percentage points less likely to default than borrowers with lower credit scores are. Borrowers over 40 are 1.494 percentage points less likely to default for every additional \$50 of available credit than borrowers under 40 are. The effect of loan size does not appear to differ significantly with gender. None of the estimates suggest a significant role for moral hazard.

While not ruled out by theory, our estimates suggesting little to no moral hazard in our setting are surprising. We therefore test the robustness of our estimates in three ways. First, we experiment with smaller bandwidths on our local linear specification from column 3. Figure 4 presents point estimates and 95 percent confidence intervals from our local linear design using bandwidths ranging from \$100 down to \$20. While the point estimates remain remarkably constant across bandwidths, our local linear estimates are no longer statistically significant when the bandwidth is larger than \$80.

Next, in Table 5, we test the impact of loan eligibility of longer term measures of default. Following Table 3, the first column presents results controlling for income using a seventh-order polynomial, the second using a linear spline, and the third using a local linear design. We use indicators for default in the 6 months after a debtor's first loan, in the 12 months after a debtor's first loan, and ever in our sample period. The point estimates are negative across all three specifications and definitions of default, though not statistically significant.

Finally, we estimate the impact of loan amount on default using a separate identification strategy that exploits the fact that all borrowers in Tennessee who earn above \$400 are constrained by the \$200 loan limit in that state. To do this, we use a difference-in-difference approach that compares the probability of default for borrowers earning more than \$400 living in Tennessee and borrowers earning more than \$400 living outside of Tennessee. Specifically, we instrument for loan size using a linear trend in income interacted with an indicator variable for living in Tennessee and being eligible for a \$200 loan. We control for a seventh-order polynomial in net pay, month-by-year effects and state-of-loan effects.

Our difference-in-difference results are presented in Table 6.[8] The results are smaller in magnitude than our regression discontinuity approach, but again indicate little to no impact of moral hazard. A \$50 increase in loan amount is associated with (0.034×50) 1.7 percentage point decrease in the probability of default. While perhaps less convincing than our regression discontinuity estimates, our difference-in-difference approach provides additional evidence that there is little to no moral hazard in our sample.

While the imprecision of our robustness checks make definitive conclusions difficult, we see our estimates as suggesting that moral hazard does not play a significant role in our setting. If anything, loan eligibility is associated with a decrease in the probability of default. There are a number of possible explanations for this result. Under non-exclusive contracting, individuals borrowing from multiple sources may choose to pay down the largest loan obligation first. As a result, the now larger payday loan takes precedence over other debts. Alternatively, borrowers may wish to maintain access to higher credit lines and choose not to default on those loans. In this scenario, it is not the loan amount itself but the larger credit line that changes the probability of default. Finally, given the mixed significance of

[8]First stage estimates for our difference-in-difference strategy are available in Appendix Table 1.

our robustness checks, it is possible that there is neither a positive or negative relationship between available credit and default in our sample. Unfortunately, our data does not allow for a conclusive test of these possibilities.

5.3 Adverse Selection

Next we explore the extent of adverse selection into larger payday loans. OLS results relating loan size to default are presented in Table 7. Recall that 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 magnitude of adverse selection is the coefficient from our OLS regressions minus the impact of moral hazard implied by Tables 2 and 3.

As before, 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 $50 increase in loan size is associated with a 2.15 percentage point increase in the probability of default in our baseline results, and a 2.00 percentage point increase controlling for demographic characteristics. This suggests that borrowers who *select* a $50 larger loan are 7.8 to 9.0 percentage points more likely to default, more than a 40 percent increase from the mean rate of default.[9]

[9] 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 each loan-eligibility group (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. Including loan-eligibility indicators increases the coefficient on loan amount, implying that there is little to no moral hazard in our setting and that adverse selection drives the cross-sectional relationship between loan amount and default.

5.4 Specification Checks

This section presents results from a series of specification checks. Our first set of specification checks test the assumption that individuals do not selectively borrow based on the eligibility cutoffs. Our second set of specification checks estimates the impact of the loan cutoffs in states that do not have them.

Our first set of specification checks examine the assumption that individuals eligible for larger loans are not more or less 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 trend smoothly through the cutoffs, and by testing the density of borrowers around the cutoffs.

Table 8 tests whether observable baseline characteristics trend smoothly through the loan-eligibility cutoffs. Here we only discuss results from Firm A, where the richness of our data allows for more convincing checks of our identifying assumptions. 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 for which a borrower is eligible, 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 significant 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 8 as showing no clear evidence that our identifying assumption is violated.

Our second specification 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 15 states in which Firm A operates, including 13 states that offer loans in continuous amounts, into equal-sized bins.[10] The key variables in our data are the fraction

[10] The states in which loans are available in continuous loans are Alabama, Colorado, Florida, Georgia,

of observations in each bin, and the net pay amount around which the bins are centered. We then regress the fraction of observations in each bin on the maximum loan for which a borrower is eligible and a quadratic in net pay. We control for the \$100 bin that each observation falls into to control for wage-setting effects unrelated to the type of selective borrowing for which we are testing. 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.

Table 9 presents results for whether the frequency of borrowers changes at the eligibility cutoffs. We present results using bins ranging in width from \$10 to \$50 to ensure that our results are robust to this choice. 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 results available upon request, we allow the estimated effect of each cutoff to vary. The coefficients on the eligibility indicators are small and inconsistent in sign, and we cannot reject the null hypothesis that the indicator variables are jointly equal to zero at any bin width.

Our second set of specification checks conducts a more general falsification test. To ensure that our estimates identify discontinuities in loan size and default that exist due to institutional rules determining loan eligibility, we replicate our main results in the 13 states where loan size is not a discontinuous function of income. As in the rest of our results, we restrict this falsification sample to biweekly borrowers with take-home pay between \$100 and \$1,100. These restrictions leave us with 86,254 borrowers in 13 states.

The first stage estimates for our falsification sample are presented in Table 10 and Figure 5. Following the main results, each figure plots average loan amounts in \$25 income bins for the first loans of borrowers with biweekly take-home pay between \$100 and \$1,100. As in the rest of our results, we control for $f(pay_i)$ using a seventh-order polynomial, a linear spline, and a local linear regression that is allowed to vary on either side of the loan-eligibility cutoff. The formal results in Table 9 also control for month-by-year and state-of-loan effects, with standard errors clustered at the pay level. There is no evidence of an economically or statistically significant relationship between income and loan size in our falsification sample of states where loan size is not institutionally set to be a discontinuous function of pay. The first stage point estimates are small, ranging from \$-0.74 to \$3.59, and not statistically significant.

Reduced form estimates for our falsification sample are presented in Table 11 and Figure

Illinois, Indiana, Kentucky, Louisiana, Missouri, North Carolina, Oklahoma, Texas, and Utah.

6. Again, there is no evidence of an economically or statistically significant relationship between income and default in the falsification sample. The reduced form estimates range from $-0.401 to $0.104 and are not statistically significant.

6 Conclusion

This paper has documented severe credit constraints among payday borrowers. Surprisingly, relaxing these credit constraints does not lead to increased default rates. Our preferred regression discontinuity estimates suggest that a $50 increase in credit availability *decreases* the probability that a borrower defaults by 2.9 to 3.1 percentage points. This positive within-borrower impact of additional credit is more than offset by adverse selection into larger loans. Borrowers who *choose* $50 larger loans are 7.8 to 9.0 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.

Given the emphasis placed on moral hazard by policymakers and within the theoretical literature, our results are somewhat surprising. We hope that our findings spur new work estimating the impact of moral hazard in other settings and using new identification strategies, while helping guide future theoretical work on credit market failures. Our work also highlights the significant adverse selection problems facing 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 toward 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		

Notes: This table reports summary statistics for two payday lending firms. Columns 1 and 2 are 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.

Table 2
First Stage Results
Effect of Loan Cutoffs on Loan Amount

	7th Order Polynomial (1)	Linear Spline (2)	Local Linear (3)
Loan Cutoff	22.330***	24.900***	23.020***
	(5.023)	(3.524)	(8.245)
F-Statistic	19.765	49.925	7.794
Observations	11258	11258	16603

Notes: This table reports first stage estimates. The sample is the first loan for borrowers paid biweekly who are paid between $100 and $1,100 each pay period. The dependent variable is the dollar amount of the loan. Column 1 controls for a seventh-order polynomial in net pay and a separate quadratic trend for Tennessee after the $200 loan cutoff. Column 2 controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. We report the average change in loan amount across the nine cutoffs. Tennessee is allowed to have a separate linear trend after the $200 cutoff. Column 3 stacks data from each cutoff and controls for net pay using a local linear regression, a local linear regression interacted with the loan cutoff, and indicators for each cutoff. The local linear regression uses a bandwidth of $100. Tennessee cutoffs over $200 are excluded. All regressions control for month-by-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-cutoff indicators are jointly equal to zero. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3
Effect of Loan Cutoffs on Default

	7th Order Polynomial (1)	Linear Spline (2)	Local Linear (3)
Loan Cutoff	-3.136^{**}	-2.915^{*}	-2.926^{**}
	(1.547)	(1.642)	(1.305)
Observations	11258	11258	16603

Notes: This table reports reduced form estimates. The sample is the first loan for borrowers paid biweekly who are paid between $100 and $1,100 each pay period. The dependent variable is an indicator variable for default on the first loan, multiplied by 100. Column 1 controls for a seventh-order polynomial in net pay and a separate quadratic trend for Tennessee after the $200 loan cutoff. Column 2 controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. We report the average change in loan amount across the nine cutoffs. Tennessee is allowed to have a separate linear trend after the $200 cutoff. Column 3 stacks data from each cutoff and controls for net pay using a local linear regression, a local linear regression interacted with the loan cutoff, and indicators for each cutoff. The local linear regression uses a bandwidth of $100. Tennessee cutoffs over $200 are excluded. All regressions control for month-by-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 4
Reduced Form Effects by Subgroup

	Good Credit	Poor Credit	High Savings	Low Savings	Older	Younger	Male	Female
Loan Amount	−2.128***	0.062	−1.710***	−0.848	−1.494***	−0.311	−1.245*	−1.195*
	(0.538)	(0.544)	(0.524)	(0.550)	(0.398)	(0.404)	(0.640)	(0.639)
Observations	1767	1815	1082	2498	3990	7267	712	1774

Notes: This table reports reduced form estimates. The sample is the first loan for borrowers paid biweekly who are paid between \$100 and \$1,100 each pay period. Results by age include both Firm A and Firm B. All other results include only Firm A. The dependent variable is an indicator variable for default on the first loan, multiplied by 100. We control for a seventh-order polynomial in net pay, month-by-year effects, state-of-loan effects, and group. We report coefficients on the loan cutoff interacted with group status. We also control for a separate quadratic trend for Tennessee after the \$200 loan cutoff. 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 5
Effect of Loan Cutoffs on Long Term Default

	7th Order Polynomial (1)	Linear Spline (2)	Local Linear (3)
Default Ever	−1.915	−1.740	−1.415
	(1.872)	(1.913)	(1.511)
	11258	11258	16603
Default in 6 Months	−1.777	−1.255	−1.401
	(1.896)	(1.930)	(1.525)
	11258	11258	16603
Default in 12 Months	−2.261	−2.392	−1.836
	(1.909)	(1.931)	(1.555)
	11258	11258	16603

Notes: This table reports reduced form estimates. The sample is the first loan for borrowers paid biweekly who are paid between \$100 and \$1,100 each pay period. The dependent variable is an indicator variable for default in the first 6 months, 12 months, or default on any loan in the sample period, each multiplied by 100. Column 1 controls for a seventh-order polynomial in net pay and a separate quadratic trend for Tennessee after the \$200 loan cutoff. Column 2 controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. We report the average change in loan amount across the nine cutoffs. Tennessee is allowed to have a separate linear trend after the \$200 cutoff. Column 3 stacks data from each cutoff and controls for net pay using a local linear regression, a local linear regression interacted with the loan cutoff, and indicators for each cutoff. The local linear regression uses a bandwidth of \$100. Tennessee cutoffs over \$200 are excluded. All regressions control for month-by-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 6
Difference in Differences
Second Stage Results

	(1)
Loan Amount	-0.034^{*}
	(0.019)
Observations	11258

Notes: This table reports two-stage least squares estimates. The sample is the first loan for borrowers paid biweekly who are paid between $100 and $1,100 each pay period. The dependent variable is an indicator variable for default on the first loan, multiplied by 100. We instrument for loan size using a linear trend in net pay interacted with a borrower being from Tennessee and being eligible for a $200 loan. The regression also controls for a seventh-order polynomial in net pay, month-by-year effects, 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 7
The Correlation Between Loan Amount and Default

	1	2	3
Loan Amount	0.043***	0.039***	0.040***
	(0.004)	(0.007)	(0.007)
Net Pay	−0.036***	−0.020*	−0.016
	(0.009)	(0.011)	(0.011)
Net Pay Sq	0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)
Age			−0.213***
			(0.038)
Male			0.954
			(1.247)
Black			2.058
			(1.453)
Credit Score			−0.031***
			(0.011)
Credit Score Sq			0.000
			(0.000)
Checking Balance			−0.004***
			(0.001)
Checking Balance Sq			0.000**
			(0.000)
R^2	0.054	0.042	0.093
Observations	11258	4124	4124

Notes: This table reports OLS estimates. The sample is the first loan for borrowers paid biweekly who are paid between $100 and $1,100 each pay period. Column 1 includes borrowers at both Firm *A* and Firm *B*. Columns 2 and 3 include borrowers at Firm *A* only. The dependent variable is an indicator variable equal to one if the borrower defaults, multiplied by 100. All regressions control for month-by-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 8
Test of Quasi-Random Assignment

	7th Order Polynomial (1)	Linear Spline (2)	Local Linear (3)
Age	0.710	1.077**	0.681
	(0.448)	(0.498)	(0.496)
	11441	11441	16826
Black	−0.014	0.005	−0.010
	(0.034)	(0.034)	(0.030)
	4307	4307	4036
Male	0.009	−0.007	0.009
	(0.028)	(0.027)	(0.025)
	4307	4307	4036
Married	0.016	0.013	0.016*
	(0.011)	(0.011)	(0.009)
	4307	4307	4036
Credit Score	2.292	8.739	0.153
	(11.520)	(11.506)	(10.506)
	3653	3653	3414
Checking Balance	48.037	44.859	51.361
	(36.820)	(35.208)	(32.962)
	4127	4127	3785

Notes: This table reports reduced form estimates. The sample is the first loan for borrowers paid biweekly who are paid between $100 and $1,100 each pay period. Age includes both Firm A and Firm B. All other baseline results include only Firm A. Each dependent variable is multiplied by 100. Column 1 controls for a seventh-order polynomial in net pay and a separate quadratic trend for Tennessee after the $200 loan cutoff. Column 2 controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. We report the average change in loan amount across the nine cutoffs. Tennessee is allowed to have a separate linear trend after the $200 cutoff. Column 3 stacks data from each cutoff and controls for net pay using a local linear regression, a local linear regression interacted with the loan cutoff, and indicators for each cutoff. The local linear regression uses a bandwidth of $100. Tennessee cutoffs over $200 are excluded. All regressions control for month-by-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 9
Density Test of
Quasi-Random Assignment

Bin Size	Estimate
10	0.004
	(0.006)
	1941
20	0.008
	(0.013)
	990
25	0.010
	(0.017)
	794
33	0.010
	(0.024)
	600
50	0.017
	(0.040)
	405

Notes: 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 the main text. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 10
First Stage Falsification Test

	7th Order Polynomial (1)	Linear Spline (2)	Local Linear (3)
Loan Cutoff	−0.733	2.587	−0.740
	(1.959)	(1.719)	(3.753)
F-Statistic	0.140	2.265	0.039
Observations	84896	84896	159777

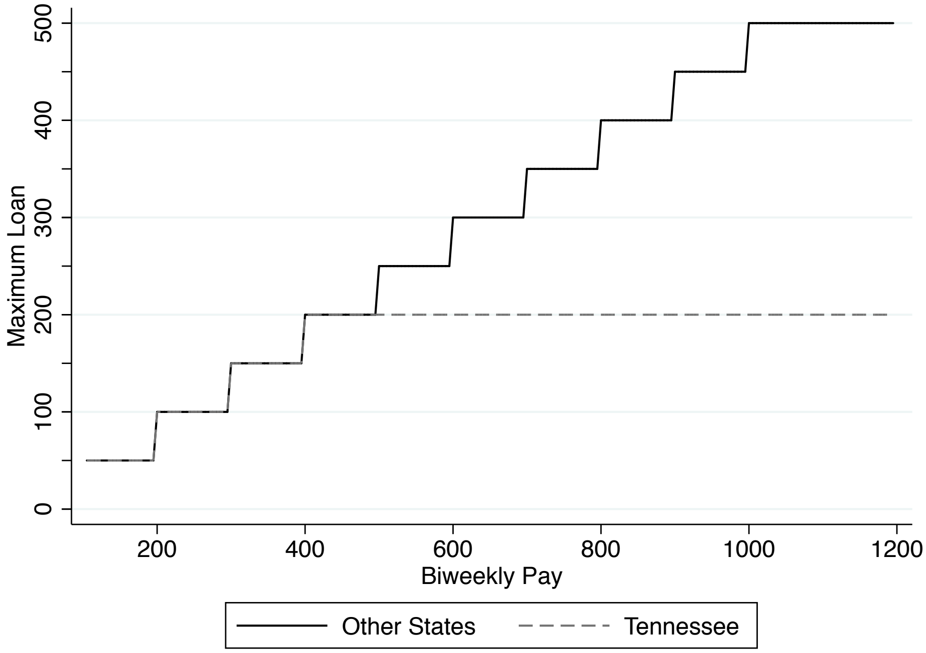
Notes: This table reports first stage estimates. The sample is the first loan for borrowers paid biweekly who are paid between $100 and $1,100 each pay period in states that do not have loan cutoffs (Alabama, Colorado, Florida, Georgia, Illinois, Indiana, Kentucky, Louisiana, Missouri, North Carolina, Oklahoma, Texas, and Utah). The dependent variable is the dollar amount of the loan. Column 1 controls for a seventh-order polynomial in net pay. Column 2 controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. We report the average change in loan amount across the nine cutoffs. Column 3 stacks data from each cutoff and controls for net pay using a local linear regression, a local linear regression interacted with the loan cutoff, and indicators for each cutoff. The local linear regression uses a bandwidth of $100. All regressions control for month-by-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 cutoff indicators are jointly equal to zero. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 11
Reduced Form Falsification Test

	7th Order Polynomial (1)	Linear Spline (2)	Local Linear (3)
Loan Cutoff	0.104	−0.401	0.096
	(0.364)	(0.445)	(0.356)
Observations	86254	86254	162241

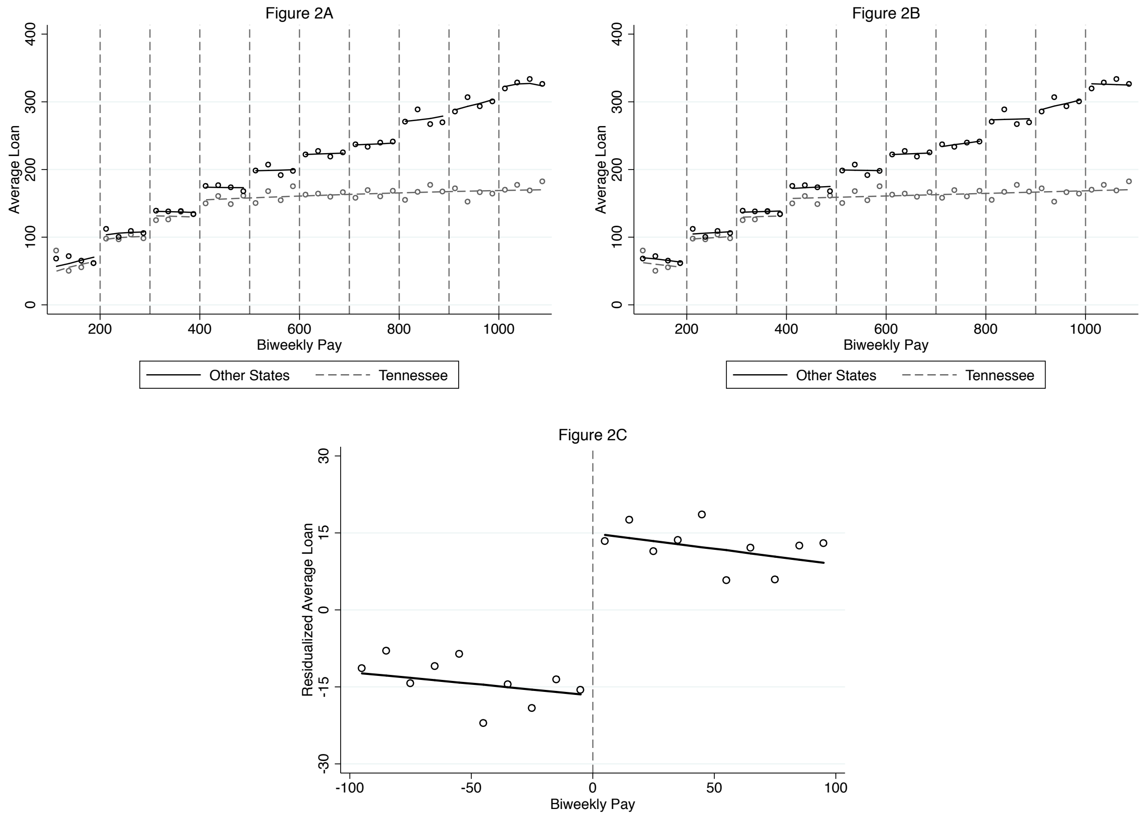
Notes: This table reports reduced form estimates. The sample is the first loan for borrowers paid biweekly who are paid between \$100 and \$1,100 each pay period in states that do not have loan cutoffs (Alabama, Colorado, Florida, Georgia, Illinois, Indiana, Kentucky, Louisiana, Missouri, North Carolina, Oklahoma, Texas, and Utah). The dependent variable is default on the first loan, multiplied by 100. Column 1 controls for a seventh-order polynomial in net pay and a separate quadratic trend for Tennessee after the \$200 loan cutoff. Column 2 controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. We report the average change in loan amount across the nine cutoffs. Tennessee is allowed to have a separate linear trend after the \$200 cutoff. Column 3 stacks data from each cutoff and controls for net pay using a local linear regression, a local linear regression interacted with the loan cutoff, and indicators for each cutoff. The local linear regression uses a bandwidth of \$100. Tennessee cutoffs over \$200 are excluded. All regressions control for month-by-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 cutoff indicators are jointly equal to zero. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Figure 1
Loan Cutoff Rule



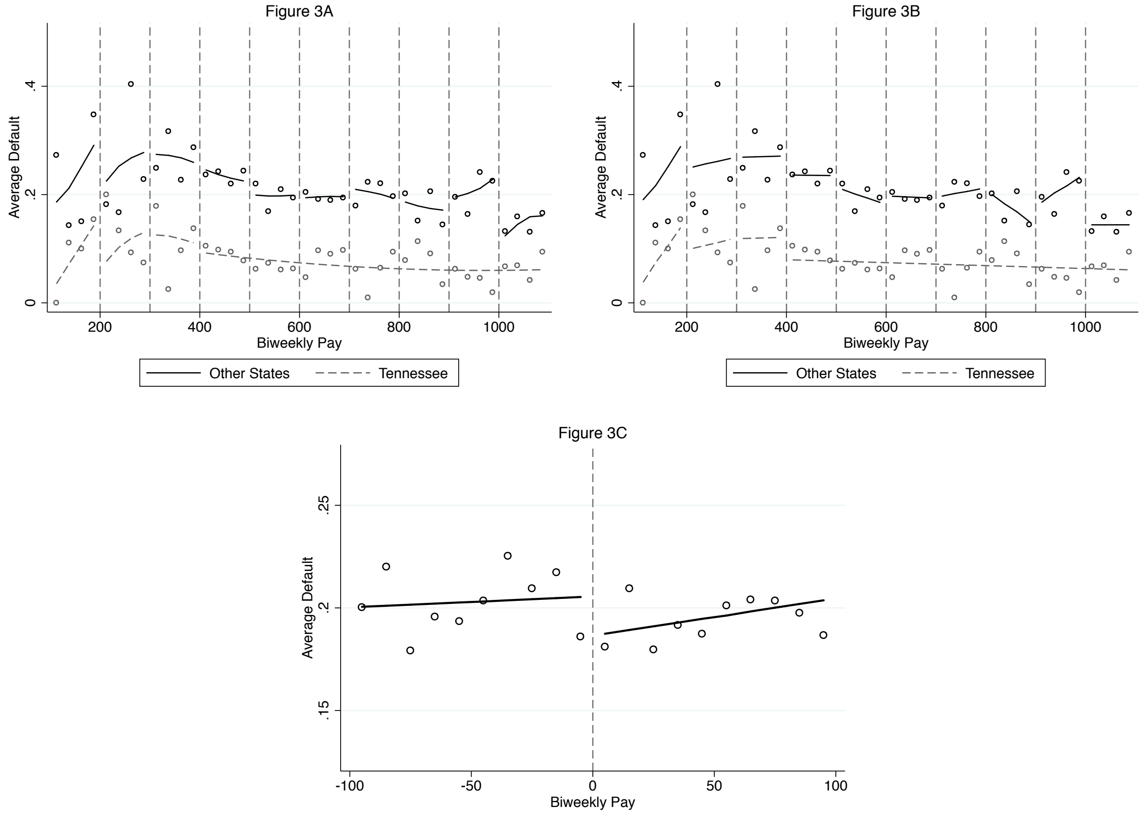
Notes: This figure illustrates the loan cutoff rule used by firms in our sample. Individuals are eligible for loans up to but not exceeding half of biweekly pay.

Figure 2
Effect of Loan Cutoffs on Loan Amount



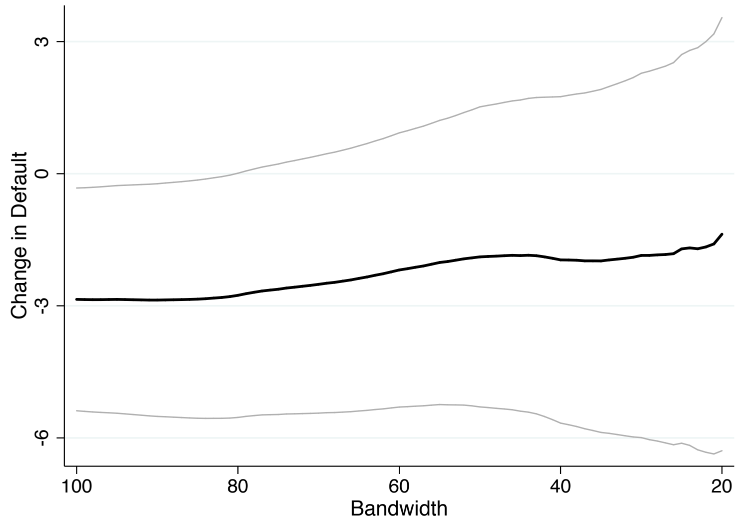
Notes: These figures plot average loan size for first-time borrowers. The smoothed line in Figure A controls for a seventh-order polynomial in net pay and a separate quadratic trend for Tennessee after the $200 loan cutoff. Figure B controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. Tennessee is allowed to have a separate linear trend after the $200 cutoff. Figure C stacks data from each cutoff and controls for net pay using a local linear regression and a local linear regression interacted with the loan cutoff. Tennessee cutoffs over $200 are excluded. Additional details are in the main text.

Figure 3
Effect of Loan Cutoffs on Default



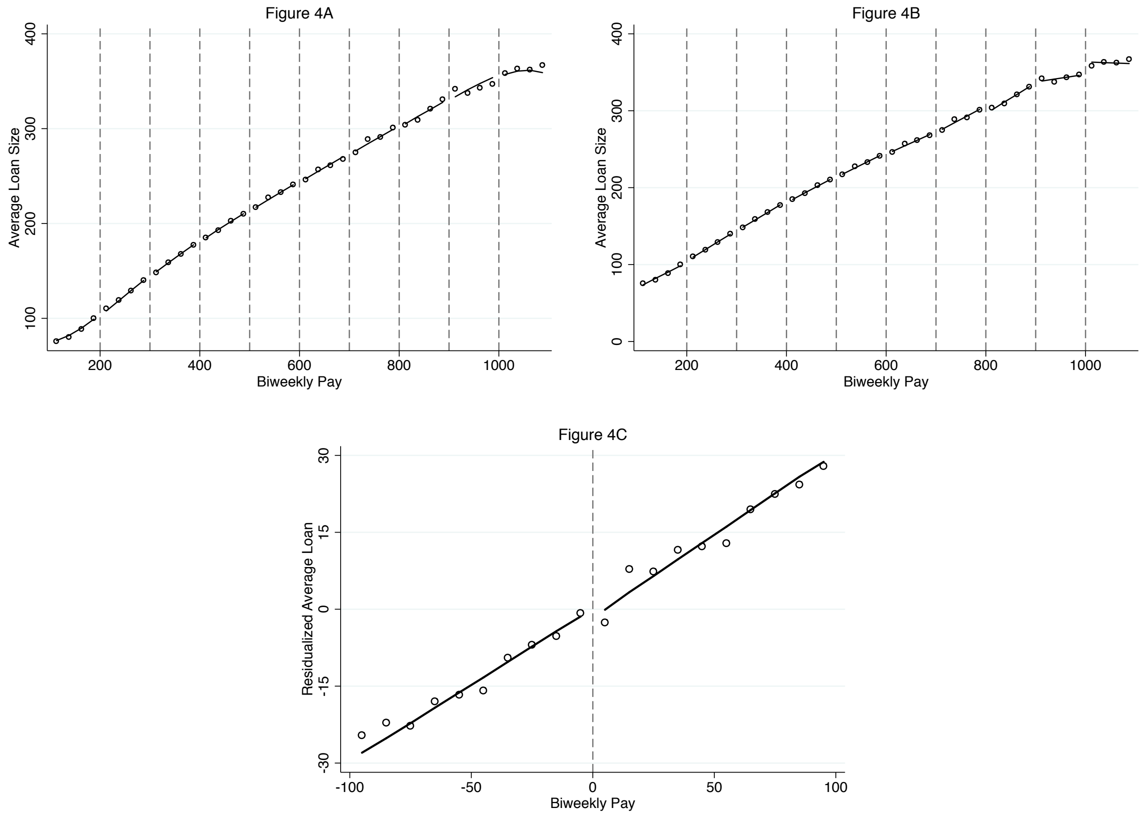
Notes: These figures plot average default for first-time borrowers. The smoothed line in Figure A controls for a seventh-order polynomial in net pay and a separate quadratic trend for Tennessee after the $200 loan cutoff. Figure B controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. Tennessee is allowed to have a separate linear trend after the $200 cutoff. Figure C stacks data from each cutoff and controls for net pay using a local linear regression and a local linear regression interacted with the loan cutoff. Tennessee cutoffs over $200 are excluded. Additional details are in the main text.

Figure 4
Robustness of RD Reduced Form Estimates



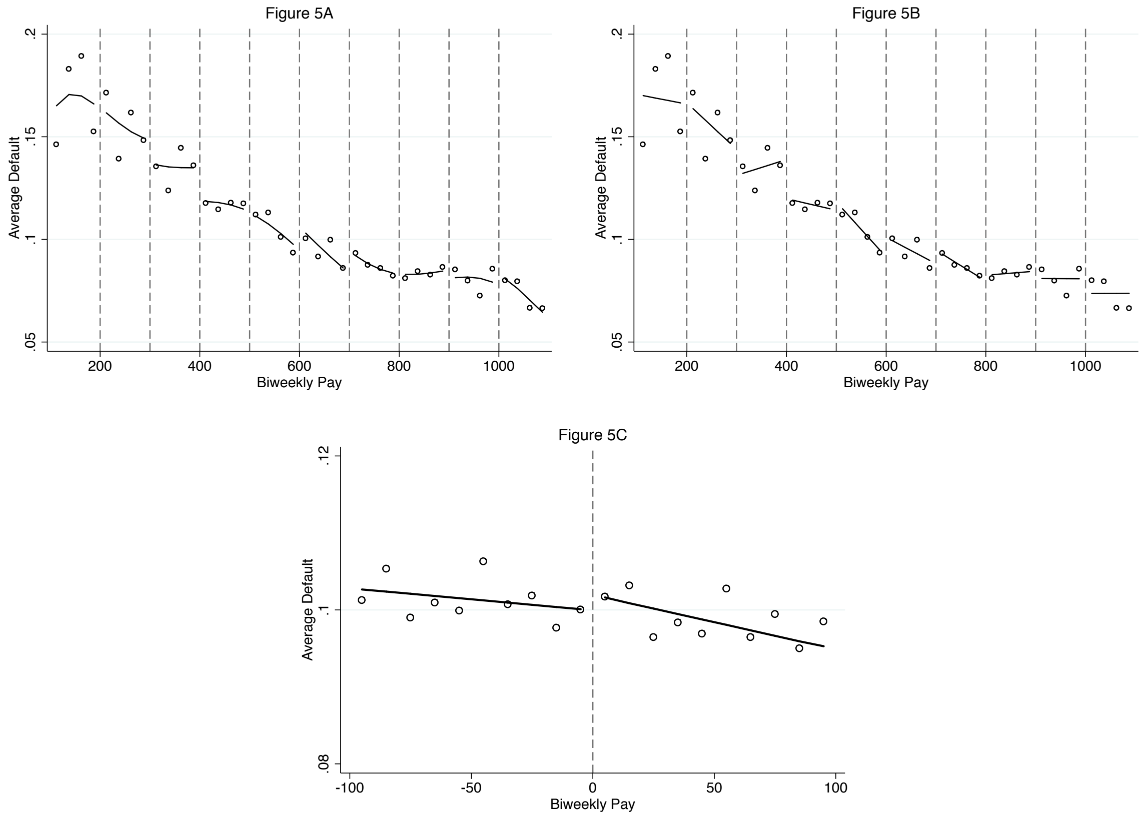
Notes: This figure plots regression discontinuity estimates using different bandwidths. We stack data from each cutoff and control for net pay using a local linear regression and a local linear regression interacted with the loan cutoff. Tennessee cutoffs over $200 are excluded.

Figure 5
First Stage Falsification Test



Notes: These figures plot average default for first-time borrowers as in Figure 2 except for states that do not have a loan cutoffs (Alabama, Colorado, Florida, Georgia, Illinois, Indiana, Kentucky, Louisiana, Missouri, North Carolina, Oklahoma, Texas, and Utah). The smoothed line in Figure A controls for a seventh-order polynomial in net pay. Figure B controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. Figure C stacks data from each cutoff and controls for net pay using a local linear regression and a local linear regression interacted with the loan cutoff. Additional details are in the main text.

Figure 6
Reduced Form Falsification Test



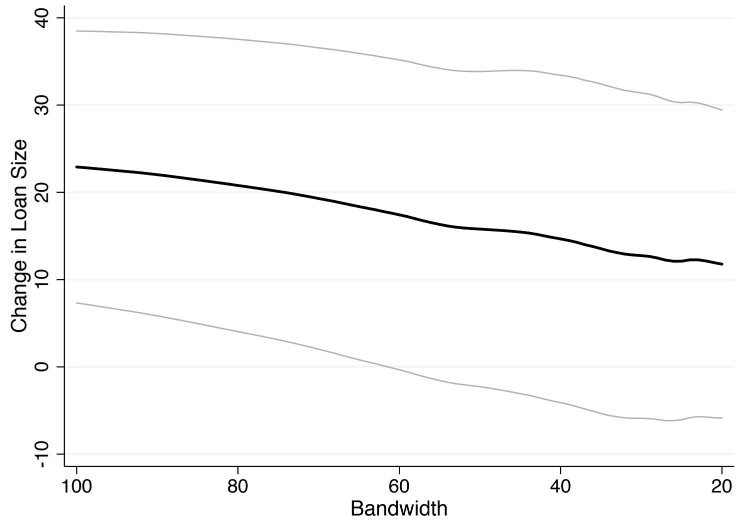
Notes: These figures plot average default for first-time borrowers as in Figure 3 except for states without loan cutoffs (Alabama, Colorado, Florida, Georgia, Illinois, Indiana, Kentucky, Louisiana, Missouri, North Carolina, Oklahoma, Texas, and Utah). The smoothed line in Figure A controls for a seventh-order polynomial in net pay. Figure B controls for a linear spline in net pay while allowing the effect of each loan cutoff to be different. Figure C stacks data from each cutoff and controls for net pay using a local linear regression and a local linear regression interacted with the loan cutoff. Additional details are in the main text.

Appendix Table 1
Difference in Differences
First Stage Results

	(1)
Loan Amount	-0.131^{***}
	(0.004)
Observations	11258

Notes: This table reports first stage estimates. The sample is the first loan for borrowers paid biweekly who are paid between \$100 and \$1,100 each pay period. The dependent variable is loan amount. We report the coefficient from a linear trend in net pay interacted with a borrower being from Tennessee and being eligible for a \$200 loan. The regression also controls for a seventh-order polynomial in net pay, month-by-year effects, 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 Figure 1
Robustness of RD First Stage Estimates



Notes: This figure plots first stage regression discontinuity estimates using different bandwidths. We stack data from each cutoff and control for net pay using a local linear regression and a local linear regression interacted with the loan cutoff. Tennessee cutoffs over $200 are excluded.