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 exploits discontinuities in loan eligibility to test for moral hazard and adverse selection in the payday-loan market. Regression discontinuity and regression kink approaches suggest that payday borrowers are *less* likely to default on larger loans. A $50 larger payday loan leads to a 17 to 33 percent drop in the probability of default. Conversely, there is economically and statistically significant adverse selection into larger payday loans when loan eligibility is held constant. Payday borrowers who choose a $50 larger loan are 16 to 47 percent more likely to default.

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Theory has long emphasized the importance of private information in explaining credit-market failures. Information asymmetries and the resulting credit constraints 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.

This paper provides new evidence on the empirical relevance of asymmetric information using administrative data from the payday-lending market. Payday loans are short-term loans of $100 to $500. Loan fees average $15 to $20 per $100 of principal, implying an annual percentage rate (APR) of over 400 percent. Despite these high interest rates, payday lenders have more storefronts in the United States than McDonald's and Starbucks combined, with nearly 19 million households receiving a payday loan in 2010 (Skiba and Tobacman, 2011). The payday-loan market is also extremely high risk, with more than 19 percent of initial loans in our sample ending in default.

Payday borrowers are particularly vulnerable to market failures due to their low incomes and poor credit histories. 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). Payday loans also have the unique feature that delinquencies are not reported to traditional credit-rating agencies, and default comes with few penalties outside of calls from debt collection agencies. Theory suggests that asymmetric-information problems are exacerbated by precisely these kinds of commitment problems (Athreya, Tam and Young, 2009; Chatterjee et al., 2007; Livshits, MacGee, and Tertilt, 2010; White, 2007; White, 2009).

We identify the impact of moral hazard in the payday-loan market using two separate empirical models. The first exploits discontinuities in the relationship between borrower pay and loan eligibility to estimate a regression discontinuity design. Many payday lenders offer loans in \$50 increments up to but not exceeding half of an individual's biweekly pay. As a result, there are loan-eligibility cutoffs around which very similar borrowers are offered different size loans. These institutional features allow us to attribute any discontinuous relationship between loan outcomes and pay at the loan-eligibility cutoffs to the causal impact of loan size. Our second empirical model uses a discontinuous change in slope relating borrower pay to loan eligibility to estimate a regression kink design. In this separate sample of states, payday lenders offer loans in continuous increments that are no larger than half of a borrower's biweekly pay, capping loans for all borrowers at a state-mandated limit of either \$300 or \$500. The fact that loan amounts are offered in continuous increments up to these caps implies that there is a discontinuous change in the slope relating loan eligibility and biweekly pay at each loan cap. We use this discontinuous change in the slope to provide a second set of moral-hazard estimates. As the correlation between default and loan size combines the selection and incentive effects of loan size, we can, under reasonable assumptions, obtain an estimate of adverse selection by subtracting our moral-hazard estimates from the cross-sectional coefficient relating loan size and borrower default.

We begin our empirical analysis by documenting credit constraints among payday borrowers. Using our regression discontinuity strategy, we find that a \$50 increase in payday credit leads to a \$19.73 to \$22.02 increase in average loan size. Thus, payday borrowers borrow 39 to 44 cents per additional dollar of credit. These estimates are larger than previous findings using data from different types of debtors, likely reflecting the fact that payday borrowers are particularly credit constrained. For example, the typical credit-card holder consumes 10 to 14 cents out of every additional dollar of credit (Gross and Souleles, 2002), while the typical financially constrained household consumes 20 to 40 cents out of every additional dollar in tax-rebate amount (Johnson, Parker, and Souleles, 2006).

Surprisingly, both our regression discontinuity and regression kink empirical strategies suggest that relaxing these credit constraints lowers the probability that a payday borrower defaults. A \$50 increase in payday-loan size leads to a 4.4 to 6.4 percentage point decrease in the probability of default in our regression discontinuity strategy sample, a 22 to 33 percent decrease. Using our regression kink design, we find that a $50 increase in payday-loan size lowers the probability of default by 1.6 to 4.6 percentage points, a 17 to 23 percent decrease. The finding that larger loans lower the rate of default is surprising given the prominence of moral hazard in the theoretical literature and the empirical relevance of moral hazard in other consumer-lending markets (Adams, Einav, and Levin, 2009).

Conversely, we find economically and statistically significant adverse selection into larger payday loans. In our OLS results, which combine both adverse selection and moral hazard, a $50 increase in loan size is associated with a 1.0 to 2.3 percentage point increase in the probability of default in our regression discontinuity sample. Taken together with our estimates of moral hazard, this suggests that borrowers who *choose* a loan that is $50 larger are 5.4 to 8.7 percentage points more likely to default, a 28 to 44 percent increase. In our regression kink sample, the OLS results suggest that borrowers who choose a $50 larger loan are 16 to 47 percent more likely to default. Our results are therefore consistent with the view that adverse selection alone can lead to credit constraints in equilibrium.

We conclude our analysis by examining two key threats to our interpretation of the regression discontinuity and regression kink estimates. The first threat is that 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. The second threat to our identification strategy is that our empirical design is misspecified. To ensure that our estimates identify discontinuities that exist solely due to institutional factors, we replicate our empirical results in a set of states where loan size is not a discontinuous function of income.

Our work fits into an important empirical literature estimating moral hazard and adverse selection in credit markets in the United States (Ausubel, 1991; Edelberg, 2003; Edelberg, 2004) and abroad (Klonner and Rai, 2006; Karlan and Zinman, 2009). Ausubel (1999), for example, uses randomized credit-card offers to show that a 1 percent increase in introductory interest rate increases the probability of delinquency by 1.2 percentage points and the probability of bankruptcy by 0.4 percentage points. Adams, Einav, and Levin (2009) exploit exogenous variation in price and minimum down payments to identify moral hazard and adverse selection in an automobile-loan market. Adams, Einav, and Levin (2009) estimate that for a given auto-loan borrower, a $1,000 increase in loan size increases the probability of default by 16 percent. Individuals who borrow an extra $1,000 for unobservable reasons have an 18 percent higher rate of default than those who do not. Also related is Melzer and Morgan (2010), who find adverse selection into bank overdraft services when payday lending is available.

This paper complements this literature in three ways. First, the characteristics of the borrowers make this a particularly important population for which to study credit dynamics. As previously discussed, payday borrowers are particularly vulnerable to market failures given their low incomes and poor credit histories. Payday borrowers apply for payday loans precisely when they have exhausted traditional credit options. In fact, 80 percent of payday-loan applicants have no available credit on credit cards when they apply for a payday loan (Bhutta, Skiba, and Tobacman, 2012). Second, the institutional features of the payday-loan market allow for a particularly sharp research design. Adams, Einav, and Levin (2009), whose work is most closely related to ours, use price and down-payment variation across time, credit categories, and regions to identify the impact of moral hazard. Their empirical design therefore relies on having controlled for all sources of endogenous variation. In contrast, we focus on two transparent and well-identified sources of variation in payday-loan size to identify moral hazard. 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 in 2008. In comparison, the subprime automobile-loan market totaled approximately $50 billion in 2006 (J.D. Power and Associates, 2007).

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., Parker, 1999; Souleles, 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.

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, Strain, and Seblani, 2012). 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 I provides background on our institutional setting and describes our data. Section II reviews the theoretical framework that motivates our empirical analysis. Section III describes our empirical strategy. Section IV presents our results. Section V discusses potential mechanisms through which larger payday loans lower the probability of default. Section VI concludes.

I. Data and Institutional Setting

Payday loans are small, short-term loans collateralized with a personal check. 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.[1] 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.

[1]While some lenders use credit scores to screen applicants, none of the firms in our sample use risk-based pricing and all borrowers pay the same finance charge. See Agarwal, Skiba, and Tobacman (2009) for more information on the subprime credit-scoring process.

Payday-loan eligibility is typically a discontinuous function of net pay, with the precise eligibility rules varying across firms and states. In our data, loan-eligibility rules take two forms. In the first form, loans are offered in \$50 increments that are no larger than half of a borrower's biweekly pay. Thus, loan eligibility increases discontinuously by \$50 at each \$100 pay interval. Stores using this rule form our regression discontinuity sample. A second set of stores offer loans in continuous increments that are no larger than half of a borrower's biweekly pay, capping loans for all borrowers at a state-mandated limit of either \$300 or \$500. The fact that loan amounts are offered in continuous increments implies that there are no discontinuous jumps in loan eligibility. Instead, there is a discontinuous change in the slope relating loan eligibility and biweekly pay at the loan-limit amount. Stores using this eligibility rule form our regression kink sample.

Our specific data come from three large payday lenders. Lending information is available from January 2000 through July 2004 in 15 states for the first firm in our data (hereafter Firm A), from January 2008 through April 2010 in two states for the second firm in our data (hereafter Firm B), and from January 2008 through June 2011 in two states for the third firm in our data (hereafter Firm C).[2] We combine these data with records of repayment and default from each firm. 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. Our data from Firms B and C are more sparse, only including information on each borrower's income, home address, and age.

Our regression discontinuity sample consists of all initial loans made in four states that offer loans in \$50 increments. This sample includes Firm A stores in Ohio and Tennessee and Firm B stores in Kansas and Missouri. We restrict our analysis to borrowers paid biweekly or semimonthly, who make up nearly 70 percent of all borrowers, to allow a more straightforward presentation of the regression discontinuity results. Results are nearly identical including all borrowers. Finally,

[2]Our data spans periods both before and after the Great Recession. Our regression discontinuity sample is too small to provide estimates by period. Our regression kink estimates are nearly identical for both Firm A and Firm C, whose data span both time periods.

we restrict our regression discontinuity analysis to borrowers earning within \$100 of a loan-eligibility cutoff, or borrowers who make between \$100 and \$500 in Tennessee, which limits loans at \$200, and between \$100 and \$1,100 in the other three states in our sample. These restrictions leave us with 2,350 observations from Firm A and 7,123 observations from Firm B.

Columns 1 and 2 of Table 1 present summary statistics for the two firms in our regression discontinuity sample. Weighting the mean from each firm by the number of borrowers, the typical borrower borrows \$226.71 (including fees) in his first transaction and earns \$682.39 every two weeks. Nineteen and a half percent of borrowers default on their first loan, with the rate being more than ten percentage points higher for borrowers at Firm B. The higher rate of default may be due, at least in part, to these loans being made during the Great Recession. The more detailed data from Firm A show that 28.3 percent of borrowers are male and 77.8 percent are black, although these numbers vary widely across store locations. Just under 27 percent of payday borrowers in our regression discontinuity sample own a home, 25.3 percent use direct deposit, and 2.4 percent have their wages garnished by a creditor.

Our regression kink sample consists of all initial loans made in four states that offer loans in \$1 or \$10 increments. This sample includes Firm A stores in Alabama, Colorado, Florida, Georgia, Indiana, Louisiana, Missouri, North Carolina, Oklahoma, and Texas. The sample for Firm C includes stores in California and Oklahoma. Stores in California limit loans at \$300, while all other states limit loans at \$500. Following our regression discontinuity sample, we restrict our regression kink analysis to borrowers paid biweekly or semimonthly. We also drop borrowers making less than \$100 each biweekly pay period and those making more than \$1,000 than the amount necessary to qualify for the largest available payday loan. Thus, we include borrowers making between \$100 and \$1,600 in California and \$100 and \$2,000 in all other states in our regression kink sample. These restrictions leave us with 91,806 observations from Firm A and 38,311 observations from Firm C.

Columns 3 and 4 of Table 1 present summary statistics for our regression kink sample. Weighting the mean from each firm by the number of borrowers, the typical borrower in our regression kink sample borrows \$267.64 in his first transaction, \$40.93 more than in our regression discontinuity sample, and earns \$840.92 every two weeks, \$158.53 more. Borrowers in our regression kink sample also default at a rate of 12.3 percent, more than seven percentage points less than the regression discontinuity sample. Borrowers in the regression kink sample are also less likely to be black, have lower credit scores, and are more likely to own a home than borrowers in the regression discontinuity sample. The positive selection into our regression kink sample is due to the sample including borrowers earning between \$100 and either \$1,600 or \$2,000 every two weeks, as opposed to our regression discontinuity sample that only includes borrowers earning between \$100 and \$1,100. Moreover, our regression kink sample includes more borrowers from Firm A, whose data is drawn from before the Great Recession when default rates were lower for all payday-lending firms.

II. 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.[3] In the moral-hazard version of the model, individual borrowers are more likely to default on larger or more expensive loans. The underlying behavioral mechanisms consistent with these moral-hazard models span situations whereby individuals have a great deal of control over their default decisions (e.g. strategic default) to situations where individuals have relatively little control and default is due largely to unexpected shocks. For instance, payday borrowers may have less incentive to repay a larger loan even when they have the ability to do so. This can happen if the penalties of default increase less quickly than the benefits of default. Borrowers will therefore be more likely to voluntarily default as the loan amount increases. This can lead to credit constraints in the payday-loan market because borrowers will not

[3] Models of asymmetric information typically assume limited commitment by borrowers, or the idea that borrowers always have the option of personal bankruptcy. An emerging literature suggests that asymmetric-information issues are no longer relevant when limited commitment can be fully resolved (Chatterjee et al., 2007; White, 2007; Athreya, Tam, and Young, 2009; White, 2009; Livshits, MacGee, and Tertilt, 2010).

internalize the full increase in default costs that come with larger loan sizes, with lenders needing to cap loan sizes to prevent overborrowing. In this scenario, improved collection or repayment schemes can help relax credit constraints for all payday borrowers.

In models of adverse selection, borrowers at a high risk of default choose larger loans. Adverse selection may result from forward-looking borrowers anticipating the high likelihood of default and therefore choosing larger and more valuable loans. Conversely, payday borrowers that are more illiquid today and more in need of a larger loan may also be more likely to be illiquid later and have trouble with repayment. Adverse selection of either kind will lead to credit constraints in the payday-loan market whenever lenders cannot observe a borrower's risk type, as lenders will need to deny credit to both high- and low-risk types. In this scenario, credit scoring and information coordination can help mitigate selection problems and increase the supply of credit to low-risk borrowers.

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 consequences of moral hazard and adverse selection in our setting. Our estimates will likely reflect a number of the mechanisms discussed above. In Section V, we will explore which of these mechanisms is most plausible given the pattern of results.

III. Empirical Strategy

We estimate two empirical models to identify the impact of moral hazard in the payday-loan market. The first empirical model exploits discontinuities in the relationship between net pay and loan eligibility to estimate a regression discontinuity design. The second empirical model uses loan limits to estimate a regression kink design.[4]

[4] A third empirical 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 can use the interaction of an indicator variable for a borrower residing in 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

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., moral hazard). The problem for inference is that if individuals select a loan size because of important unobserved determinants of later outcomes, such estimates may be biased. In particular, it is plausible that people who select larger loans have a different probability of default even if loan size was held 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 first strategy is that this bias can be overcome if the conditional distribution of unobserved determinants of default $E[\varepsilon_i|pay_i]$ trends smoothly through the loan-eligibility cutoffs used by payday lenders. In this scenario, the distribution of unobserved characteristics of individuals who just barely qualified for a larger loan is the same as the distribution among those who just barely did not qualify:

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

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

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

cutoff identifies the impact of moral hazard. Online Appendix Table 1 reports these difference-in-difference results. The results are qualitatively similar to our preferred regression discontinuity and regression kink estimates.

discontinuous jump at each of nine loan-eligibility cutoffs c_l:

$$L_i = f(pay_i) + \sum_{l=100}^{500} \lambda_l \mathbb{1}\{pay_i \geq c_l\} + \eta_i \tag{3}$$

where λ_l measures the effect of loan eligibility on loan size at each of the nine cutoffs. λ_l can be interpreted as the marginal propensity to borrow estimated by Gross and Souleles (2002) and others at each eligibility cutoff. 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. The two-stage least squares regression controls for the underlying relationship between pay and both default and loan size using $f(pay_i)$, and instruments for loan size using loan eligibility $\mathbb{1}\{pay_i \geq c_l\}$ at each cutoff l.

In practice, the functional form of $f(pay_i)$ is unknown. In our empirical analysis, we experiment with several functional forms to control for borrower pay, including a seventh-order polynomial, a linear spline, and a local linear regression. To address potential concerns about discreteness in pay, we cluster our standard errors by pay (Lee and Card, 2008). We also control for month-, year-, and state-of-loan effects in all specifications. Adding controls for age, gender, race, baseline credit score, and baseline checking-account balance leaves the results essentially unchanged.

As with any regression discontinuity approach, one threat to a causal interpretation of our 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 D we evaluate this possibility in two ways: (1) by testing whether the density of borrowers is a continuous function of loan-eligibility cutoffs, and (2) 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 regression discontinuity 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, we do not find a relationship between loan size and income or default and income around the loan-eligibility cutoffs in these states.

Finally, our regression discontinuity 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, Einav, and Levin (2009) use the same assumption to identify the impact of moral hazard in the subprime auto-loan market.

To complement our regression discontinuity strategy, our second statistical approach exploits loan limits in states that offer payday loans in relatively continuous amounts. In these states, payday lenders offer loans in continuous increments that are no larger than half of a borrower's biweekly pay up to a state-mandated limit of either \$300 or \$500. The fact that loan amounts are offered in continuous increments up to these caps implies that there is a discontinuous change in the slope relating loan eligibility and biweekly pay at each loan cap. We use this discontinuous change in the slope to provide a second set of moral-hazard estimates.

Formally, let loan size L_i be a smooth function of an individual's pay with a discontinuous change in the slope after the largest available loan in a state c_{max}:

$$L_i = pay_i + \pi \mathbb{1}\{pay_i \geq c_{max}\} \cdot pay_i + \eta_i \tag{4}$$

where π measures the effect of the loan limit on the relationship between earnings and loan size. Under a number of assumptions, including a monotonicity condition analogous to the standard instrumental-variables framework (Angrist, Imbens, and Rubin, 1996), we can use Equation (4) as the first stage to provide a second set of moral-hazard estimates. The two-stage least squares regression controls for the underlying relationship between pay and both default and loan size using pay_i, and instruments for loan size using the change in slope at the loan cap $\mathbb{1}\{pay_i \geq c_{max}\}$. The identified two-stage least squares parameter is a weighted average of marginal effects, where the weights are proportional to the magnitude of the individual-specific kinks (see Card et al. (2012) for additional details).

There are two important assumptions necessary to interpret our regression kink estimates as causal. Following our regression discontinuity design, the conditional distribution of unobserved determinants of default $E[\varepsilon_i|pay_i]$ must trend smoothly through the loan caps used by payday lenders. In addition, the conditional distribution of unobserved determinants $E[\varepsilon_i|pay_i]$ must be continuously differentiable in pay. In practice, these assumptions imply that borrowers cannot precisely change their income, while allowing for other less extreme forms of endogeneity such as borrowers having imperfect control over their preborrowing earnings.

Similar to our regression discontinuity approach, the identifying assumptions required by the regression kink design generate strong predictions for the distribution of predetermined covariates around the loan caps. Following our robustness checks for our regression discontinuity design, we test our regression kink design in two ways: (1) by testing whether the density of borrowers is a continuous function of kink point, and (2) by examining the continuity of observable borrower characteristics at the kink point. There is no evidence that the number of borrowers changes at the kink point, with the results from Section D ruling out even modest selection in or out of the sample around the kink point. However, there are some small changes in the observable characteristics of borrowers around the kink points. Thus, our regression kink estimates should be interpreted with these changes in mind.

A simple extension of our regression discontinuity and regression kink approach, first pioneered by Adams, Einav, and Levin (2009), allows us to 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. There are nine cutoffs in our sample and this assumption would be violated if borrowers right around these eligibility cutoffs have a different marginal return to credit than other borrowers.

IV. Results

A. *The Impact of Loan Eligibility on Loan Amount*

Figures 1A - 1C present regression discontinuity estimates of the impact of loan eligibility on loan amount. 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 1A plots fitted values from a regression of loan size on a seventh-order polynomial in net pay. That is, the fitted values for Figure 1A come from the following specification:

$$L_i = \alpha_0 + \sum_{l=100}^{500} \alpha_{1l} \mathbb{1}\{pay_i \geq c_l\} + \sum_{p=1}^{7} \beta_{1p} pay_i^p + \varepsilon_i \tag{5}$$

where α_{1l} is the effect of having a biweekly income above the cutoff for each loan size l.

Figure 1B plots fitted values from a linear spline specification:

$$L_i = \alpha_0 + \sum_{l=100}^{500} \big(\alpha_{1l} \mathbb{1}\{pay_i \geq c_l\} + \beta_{1l} \mathbb{1}\{pay_i \geq c_l\} \cdot (pay_i - c_l)\big) + \varepsilon_i \tag{6}$$

Figure 1C stacks data from each cutoff and controls for pay with a linear trend interacted with the loan-eligibility cutoff:

$$\widehat{L_i} = \alpha_0 + \alpha_1 \mathbb{1}\{pay_i \geq c\} + \beta_1 (pay_i - c) + \beta_2 \big(\mathbb{1}\{pay_i \geq c\} \cdot (pay_i - c)\big) + \varepsilon_i \tag{7}$$

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 1C plots residualized loan amounts $\widehat{L_i}$ from a regression of raw loan size on cutoff fixed effects. All three figures exclude borrowers from Tennessee earning more than \$500.

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, 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 1A, columns 1 and 2 control for income using a seventh-order polynomial in net pay. Columns 3 and 4, corresponding to Figure 1B, control for income using a linear spline. Columns 5 and 6 present results that are analogous to Figure 1C, where we stack data from each cutoff and control for income using a linear trend and a linear trend interacted with earning above the loan-eligibility cutoff. The dependent variable is raw loan amount. All specifications control for month-, year-, and state-of-loan effects, with columns 5 and 6 adding controls for cutoff fixed effects. Columns 2, 4, and 6 also control for age, race, gender, credit score, checking-account balance, home ownership, direct-deposit status, and garnishment status. Observations from Firm B only control for age, the only demographic characteristic available. All specifications restrict the effect of each loan cutoff to have the same impact on loan size, and cluster standard errors at the pay level.

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 $22.02 more than borrowers with earnings just below a cutoff. Adding controls for age, race, gender, marital status, credit score, and checking-account balance leaves the results essentially unchanged. Controlling for income with a linear spline specification, the effect is $21.91. Stacking data from each cutoff the effect is $19.63.

Our regression discontinuity estimates therefore imply that individuals in the payday market borrow 39 to 44 cents out of every additional dollar of available credit. Perhaps unsurprisingly, this suggests that payday borrowers are much more liquidity constrained than other individuals in the United States. For instance, Gross and Souleles (2002) find that a $1 increase in a credit-card holder's limit raises card spending by 10 to 14 cents, and Johnson, Parker, and Souleles (2006) find that households immediately consumed 20 to 40 cents for every \$1 increase in their 2001 tax rebate.

Figure 2 plots average loan size and biweekly pay for first-time payday borrowers in our regression kink sample. The sample consists of borrowers living in states offering payday loans in \$1 or \$10 increments who are paid biweekly or semimonthly. We restrict the sample to borrowers earning more than \$100, and less than the kink point plus \$1000. The smoothed line controls for pay interacted with being eligible for the maximum loan size in a state c_{max}. That is, the fitted values for Figure 2 come from the following local linear specification:

$$L_i = \alpha_0 + \alpha_1 (pay_i - c_{max}) + \beta_1 \mathbb{1}\{pay_i \geq c_{max}\} \cdot (pay_i - c_{max}) + \varepsilon_i \tag{8}$$

estimated separately for borrowers in states with a \$300 and \$500 maximum loan size.

As expected given the loan-eligibility formula, Figure 2 shows very clear kinks in the empirical relationship between average loan size and biweekly earnings, with a sharp decrease in slope as earnings pass the loan-limit threshold. However, the relationship between loan amount and earnings before the kink is less than the 0.5 predicted by the loan-eligibility formula, again suggesting that not all borrowers take out the maximum loan available. Loan size is also increasing in earnings after the kink point, suggesting that there is a slight positive relationship between underlying loan demand and earnings.[5]

Table 3 presents formal regression kink estimates controlling for month-, year-

[5]Online Appendix Table 2 presents results estimating the association between borrower characteristics and loan choice in our regression discontinuity and regression kink samples. The dependent variable for each regression is an indicator for choosing the largest available loan. Thirty-three percent of borrowers in our regression discontinuity sample choose the largest available loan, as do 28 percent of borrowers in the regression kink sample. Online Appendix Table 2 shows that an additional hundred dollars of biweekly pay is associated with a 7.4 to 7.6 percentage point decrease in the probability of choosing the largest loan in the regression discontinuity sample, and a 0.6 percentage point decrease in the regression kink sample. In both samples, borrowers who are older, white, and male are more likely to choose a larger loan. Borrowers with higher credit scores and lower checking-account balances are also somewhat more likely to choose larger loans, though not all point estimates are statistically significant.

, and state-of-loan effects. For borrowers in states capping loans at $300, loan amount increases by 29.4 cents for each additional dollar of earnings before the kink point, compared to only 3.7 cents after the kink point. In $500 cap states, loan amount increases by 28.6 cents for each additional dollar of earnings before the kink point, compared to only 3.5 cents after the kink point.

B. *Moral Hazard*

Figures 3A - C plot default and biweekly pay for payday borrowers in our regression discontinuity sample. These figures represent the reduced-form impact of loan eligibility on default. Following the first-stage regression discontinuity 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 plots residualized default rates after stacking data from each cutoff and controlling for income using 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. In fact, default appears to be somewhat lower for borrowers with earnings just above loan cutoffs.

Table 4 presents formal two-stage least squares estimates of the causal impact of an additional dollar in loan amount on default. These two-stage least squares estimates pool information across all loan-eligibility cutoffs and are therefore more precise than the reduced-form results presented in Figure 3. All specifications instrument for loan amount using the maximum eligible loan, and control for month-, year-, and state-of-loan effects. Columns 5 and 6 control for cutoff fixed effects, with columns 2, 4, and 6 controlling for age, race, gender, credit score, checking-account balance, home ownership, direct-deposit status, and garnishment status. Observations from Firm B control for age, the only available demographic characteristic. All specifications restrict the effect of each loan cutoff to have the same impact on loan size and cluster standard errors at the pay level. The dependent variable in each specification is an indicator variable equal to one if the debtor defaults on their payday loan. We multiply all estimates by 100 so that each coefficient can be interpreted as the percentage change in the probability of default.

Our regression discontinuity results from Table 4 suggest that a larger loan decreases the probability that a payday borrower defaults on his first loan. Controlling for income using a seventh-order polynomial, a \$1 larger loan is associated with a 0.127 percentage point decrease in default. This implies that a \$50 larger loan (e.g. the typical increase in loan eligibility) is associated with a 6.35 percentage point decrease in default, a 32 percent decrease from the mean default rate of 19.47 percent. Controlling for income with a linear spline specification, a \$50 larger loan lowers default by 6.05 percentage points, a 31 percent decrease. Stacking data from each cutoff, the effect of a \$50 larger loan is 4.35 percentage points, a 22 percent drop in the probability of default in our regression discontinuity sample.

Figure 4 plots default and biweekly pay for payday borrowers in our regression kink sample. Following the first-stage results, there is a clear kink in the empirical relationship between default and biweekly earnings, with a sharp decrease in slope as earnings pass the loan-limit threshold. This pattern is consistent with larger loans decreasing the probability of default.

Table 5 presents formal two-stage least squares estimates of the causal impact of an additional dollar in loan amount on default using our regression kink design. We instrument for loan amount using the interaction between pay and the kink point, and use a local linear control specification to control for pay. We also control for month-, year-, and state-of-loan effects, and multiply all estimates by 100. In our regression kink specification, a \$1 larger loan is associated with a 0.09 percentage point decrease in the probability of default at the \$300 cutoff and a 0.03 percentage point decrease in the probability of default at the \$500 cutoff. This implies that a \$50 larger loan is associated with a 4.55 percentage point decrease in the probability of default at the \$300 cutoff, a 21.9 percent drop, and a 1.60 percentage point decrease in the probability of default at the \$500 cutoff, a 17.2 percent drop. It is worth emphasizing the similarity of our regression discontinuity and regression kink point estimates given the very different samples and identification strategies.

Table 6 and Table 7 report regression discontinuity and regression kink estimates interacted with borrower age, gender, race, baseline home ownership, baseline credit score, and baseline checking-account balance. We focus on our regression kink results, where the larger sample size allows for increased precision. We follow our earlier specifications by controlling for a local linear trend in pay and month-, year-, and state-of-loan effects. We instrument for loan size using the triple interaction of pay with the loan kink point and the relevant borrower characteristic. We dichotomize all borrower characteristics by splitting the sample at the median. Finally, we restrict our attention to the \$500 kink point, as we do not have information on borrower characteristics for borrowers in the \$300 kink point states.

The effect of loan size on default is larger for borrowers who are younger and who are male. A \$50 increase in loan size decreases the probability that a borrower under 40 defaults by 2.2 percentage points, compared to only 0.6 percentage points for borrowers over 40. A \$50 increase in loan size also decreases the probability that a male borrower under 40 defaults by 1.65 percentage points, compared to only 0.05 percentage points for female borrowers. However, both younger borrowers and male borrowers are more likely to default in general, implying that the relative effect of loan size on default is comparable between the different groups.

More striking is the lack of difference between borrowers with high and low baseline credit scores and high and low baseline checking-account balances. In both cases, the interaction term is economically small and not statistically significant. This suggests that the impact of loan size on repayment behavior is similar across high- and low-risk individuals. This pattern of results is also consistent with the regression kink estimates from Table 5 showing similar impacts at the \$300 and \$500 kink points, despite large differences in the type of borrowers on those margins.

C. *Adverse Selection*

Table 8 presents OLS estimates relating default to loan size. 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 discussed in Section III, the magnitude of adverse selection is the coefficient from our OLS regressions minus the impact of moral hazard implied by Tables 4 and 5.

Following our earlier results, the dependent variable is an indicator variable equal to one if a loan ends in default. All specifications control for month-, year-, and state-of-loan effects. We report robust standard errors in parentheses and multiply all coefficients and standard errors by 100. Columns 1 and 5 present our baseline results using data from both firms in our sample and no controls other than month-, year-, and state-of-loan fixed effects. Columns 2 and 6 add controls for net pay, columns 3 and 7 add controls for age, race gender, marriage, credit score, and checking-account balance, and columns 4 and 8 add controls for the maximum loan a borrower is eligible for. Observations from Firms B and C only control for age and the maximum loan available, as other demographic controls are not available.

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. Scaling the estimates to be equivalent to our two-stage least squares results, a $50 increase in loan size is associated with a 1.0 percentage point increase in the probability of default in our regression discontinuity sample, and a 0.4 percentage point increase in the probability of default in our regression kink sample. Controlling only for biweekly pay, a $50 increase in loan size is associated with a 2.3 percentage point increase in the probability of default in our regression discontinuity sample, and a 1.3 percentage point increase in our regression kink sample. Controlling for borrower characteristics and loan eligibility yields similar results to those that control for pay only.

Taken together with our moral-hazard estimates discussed above, our results from Table 8 imply that borrowers who *select* a $50 larger loan are 5.4 to 8.65 percentage points more likely to default on their first payday loan in our regression discontinuity sample, and 2.00 to 5.85 percentage points more likely to default in our regression kink sample. These represent a 28 to 44 percent increase in the probability of default in our discontinuity sample, and a 16 to 47 percent increase in our regression kink sample. The precision of both our two-stage least squares and OLS estimates result in our adverse-selection estimates also being highly statistically significant, with p-values of less than 0.001 across all specifications.

D. *Specification Checks*

This section presents results from a series of specification checks for our regression discontinuity and regression kink estimates. First, we test the assumption that individuals do not selectively borrow based on loan eligibility. Second, we replicate our results in states without the discontinuity as a more general falsification test.

Our first set of specification checks examines 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.

Online Appendix Figure 1A plots observable borrower characteristics and biweekly pay for borrowers in our regression discontinuity sample. Following our earlier results, we also plot predicted lines controlling for a seventh-order polynomial in pay, a linear spline in pay, and a local linear line stacking data from each eligibility cutoff. There is little evidence of the type of systematic selection that would bias our results. Borrower characteristics appear to trend smoothly through each cutoff.

Online Appendix Table 3 presents formal results testing whether observable baseline characteristics trend smoothly through the loan-eligibility cutoffs. We regress each baseline characteristic on the maximum loan for which a borrower is eligible, controlling for income and month-, year-, and state-of-loan effects. Consistent with the results from Online Appendix Figure 1A, none of the point estimates are statistically significant in any of the three specifications we consider.

Online Appendix Figure 1B plots the number of borrowers and biweekly pay for our regression discontinuity sample. The bottom row of Online Appendix Table 3 presents formal estimates testing whether the number of borrowers trends smoothly through the loan-eligibility cutoffs. Specifically, we regress the number of borrowers in each $10 bin on a seventh-order polynomial in pay, a linear spline in pay, and local linear in pay stacking data from each cutoff. Consistent with our identifying assumptions, none of these specifications suggest that the number of borrowers changes with loan eligibility. Results are identical across a range of specifications and choice of binwidth.

Online Appendix Figure 2A and Online Appendix Table 4 present results testing whether observable characteristics trend smoothly in our regression kink sample.

Following our earlier results, we also plot predicted lines controlling for pay interacted with the kink point. The results from Online Appendix Figure 2A suggest that the fraction of borrowers who are black trends down after the kink point. There are also changes in direct deposit and garnishment. Conversely, gender, credit score, checking-account balance, home ownership, and age all appear to trend smoothly through the kink point. Formal estimates available in Online Appendix Table 4 further suggest we cannot rule out economically small differences at the kink point for a number of characteristics. Thus, our regression kink estimates should be interpreted with this caveat in mind.

Online Appendix Figure 2B plots the number of borrowers and biweekly pay for our regression kink sample. We also plot a predicted line from a seventh-order polynomial interacted with the kink point, the polynomial order that has the lowest Akaike criterion. The bottom row of Online Appendix Table 4 presents formal estimates from the same specification. Following Card et al. (2012) we report the coefficient and standard error on the linear interaction term. There is no evidence that the number of borrowers changes at the kink point, with the results from Online Appendix Table 4 ruling out even modest selection in or out of the sample around the kink point.

We conclude this section by considering a more general falsification test of our regression discontinuity design. 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 our regression kink sample, where loan size is not a discontinuous function of income before the kink point. 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 a large sample of 101,026 borrowers.

The first-stage estimates from our falsification test are presented in Online Appendix Figure 3 with corresponding regression results in Online Appendix Table 5. 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. Loan amount trends smoothly through each cutoff, with the first-stage point estimates ranging from 1.65

to 2.75, with none of the point estimates reaching statistical significance.

Reduced-form estimates from our falsification test are presented in Online Appendix Figure 4 and Online Appendix Table 6. Again, there is no evidence of an economically or statistically significant relationship between pay and default in the falsification sample. Default trends smoothly through each cutoff, with none of the two-stage least squares estimates suggesting a statistically significant relationship between loan size and default.

V. Discussion

This paper has presented evidence that larger payday-loan amounts decrease the probability of payday-loan default. This is a surprising result given the prominence of moral hazard in the theoretical literature and the empirical relevance of moral hazard in other consumer-lending markets (e.g., Adams, Einav, and Levin (2009)). There are at least five potential reasons why moral hazard is not empirically relevant in payday lending.

First, it is possible that borrowers repay larger loans to maintain a larger credit line in the future. In this scenario, the marginal benefit of a higher credit line tomorrow is larger than the marginal benefit of defaulting on a larger loan today. This scenario also assumes that it is prohibitively costly for borrowers to increase their credit line in other ways, such as increasing earnings to qualify for a larger loan or petitioning the lender for an exemption. Payday firms in our sample report that they offer these types of exemptions on second loans, suggesting that this mechanism is unlikely to play an important role in explaining our results.

Second, borrowers may fear more aggressive collection efforts if they default on a larger loan. If lenders are able to increase the cost of default sufficiently, the marginal cost of default may increase faster with loan size than the marginal benefit. Conversely, the payday firms in our sample have no official policy of pursuing larger loans more aggressively, and there is no evidence that payday lenders are more effective at collecting larger loans in our sample. However, we are unable to rule out differences in borrower beliefs regarding collection efforts.

Third, larger loans may increase the ability of borrowers to repay in the future.

For example, if electricity or telephone service is shut off, the time and expense to restart service can exceed the payday-loan fees. A larger payday loan may also allow an individual to fix her car and stay employed, or pay rent or her mortgage and avoid eviction or foreclosure. Consistent with this mechanism, approximately one-half of payday borrowers report that they plan to use their loan for bills, emergencies, transportation expenses, food or to repay another debt (Bertrand and Morse, 2011). In a separate sample, approximately one-half of payday borrowers report that they plan to use their loan to deal with an unexpected expense shock, while another fifth report that they plan to use their loan to deal with an unexpected income shock. Only one-third of payday borrowers plan to use their loan for a discretionary expense (Elliehausen and Lawrence, 2001).

Fourth, it is possible that individuals who do not qualify for a large enough loan substitute toward even more costly forms of credit which makes it more difficult to repay. Many sources of short-term credit are more expensive than payday loans, including overdraft charges on a checking account, returned check fees, credit-card late fees, and automobile-title loans. Consistent with this explanation, Skiba and Tobacman (2011) find that rejected payday-loan applicants are more likely to take out a pawn loan. This is likely because 80 percent of payday applicants have precisely $0 in available credit-card liquidity at the time of application, with 90 percent having less than $300 in liquidity when they apply (Bhutta, Skiba, and Tobacman, 2012).

Finally, our results are consistent with a number of alternative models of decision-making.[6] For instance, if borrowers suffer from limited attention, they may be more likely to repay larger loans due to their increased salience. Forward-looking borrowers suffering from limited attention problems may also be more likely to set reminders or seek commitment devices to repay larger loans (O'Donoghue and Rabin, 2001). It is also possible that payday borrowers discount smaller dollar amounts more than larger amounts (e.g. the magnitude effect discussed by Loewenstein and Prelec (1992)).

[6] Campbell et al. (2011) discuss behavioral anomalies in the payday-loan market. See Rabin (1998) and DellaVigna (2009) for a broader discussion of potential deviations from the neoclassical model of decision-making.

VI. Conclusion

This paper exploits sharp discontinuities in loan eligibility to test for moral hazard and adverse selection in the payday-loan market, one of the largest sources of subprime credit in the United States. Both regression discontinuity and regression kink approaches suggest that payday-loan borrowers are *less* likely to default when offered a larger loan. A $50 larger payday loan leads to a 17 to 33 percent drop in the probability of default on the first loan. Conversely, we find evidence of economically and statistically significant adverse selection into larger payday loans when loan eligibility is held constant. Payday borrowers who choose a $50 larger loan are 16 to 44 percent more likely to default on the first loan.

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 continue to explore new identification strategies as we have done here. Our work also highlights the significant adverse-selection problems facing firms in the payday-loan 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 the payday-loan market are still unknown, as we cannot say with certainty what is driving our effects. A better understanding of which model of decision-making best characterizes the behavior of credit constrained borrowers 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

	RD Sample		RK Sample		All Borrowers		
	Firm A	Firm B	Firm A	Firm C	Firm A	Firm B	Firm C
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loan Amount	190.936	238.614	285.866	223.918	283.933	257.738	228.426
Biweekly Pay	580.827	715.852	790.856	961.133	809.250	822.658	1229.444
Default	0.112	0.222	0.090	0.202	0.090	0.210	0.187
Ever Default	0.369	0.616	0.343	0.643	0.342	0.608	0.618
Age	36.508	35.710	35.482	35.808	35.619	36.609	37.084
Male	0.283	–	0.335	–	0.336	–	–
White	0.089	–	0.110	–	0.110	–	–
Black	0.777	–	0.496	–	0.509	–	–
Checkings	207.166	–	272.456	–	275.147	–	–
Credit Score	513.171	–	443.155	–	446.470	–	–
Home Owner	0.270	–	0.321	–	0.323	–	–
Direct Deposit	0.413	–	0.428	–	0.429	–	–
Garnishment Flag	0.025	–	0.027	–	0.027	–	–
Observations	2,350	7,123	91,790	38,235	96,679	8,607	50,092

Notes: This table reports summary statistics. The regression discontinuity (RD) sample consists of first-time payday-loan borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly earning between $100 and $1100 every two weeks. The regression kink (RK) sample consists of first-time payday-loan borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly earning more than $100 and within $1000 of a kink point. All borrowers are paid biweekly or semimonthly. Firm A data are available for 2000 to 2004. Firm B data are available for 2008 to 2010. Firm C data are available for 2008 to 2012. Default is an indicator for bounced payment on the first loan. Ever default is an indicator for ever bouncing a payment. Checkings balance is reported via the most recent bank statement. Credit score is a subprime credit score calculated at the time of application by a third-party credit-scoring agency called Teletrack. Direct deposit is an indicator for having one's paycheck directly deposited into a checking account. Garnishment is an indicator for a creditor currently garnishing a portion of one's wages. See text for additional details on the sample and variable construction.

Table 2
Regression Discontinuity Estimates of the Effect of Loan Eligibility on Loan Amount

	Polynomial		Linear Spline		Local Linear	
	(1)	(2)	(3)	(4)	(5)	(6)
Loan Eligibility	22.021***	22.067***	21.906***	21.946***	19.633***	19.678***
	(2.887)	(2.902)	(2.911)	(2.930)	(4.365)	(2.523)
Age		0.026		0.025		0.020
		(0.083)		(0.083)		(0.083)
Black		−12.250***		−12.382***		−12.527***
		(4.613)		(4.610)		(4.606)
Male		−2.217		−2.045		−2.084
		(4.084)		(4.086)		(4.095)
Credit Score		−0.016*		−0.016*		−0.015*
		(0.009)		(0.009)		(0.009)
Checkings		0.007*		0.007*		0.007
		(0.004)		(0.004)		(0.004)
Home Owner		3.822		3.857		4.053
		(4.823)		(4.837)		(4.829)
Direct Deposit		0.869		0.814		0.885
		(3.416)		(3.412)		(3.402)
Garnishment Flag		7.809		8.111		8.804
		(14.244)		(14.237)		(14.141)
Observations	9,473	9,473	9,473	9,473	9,473	9,473

Notes: This table reports regression discontinuity estimates of the impact of a \$50 increase in loan eligibility on loan amount. The sample consists of first-time payday-loan borrowers living in states offering payday loans in \$50 increments who are paid biweekly or semimonthly earning between \$100 and \$1100 every two weeks. Columns 1-2 control for a seventh-order polynomial in net pay. Columns 3-4 control for a linear spline in net pay. Columns 5-6 stack data from each cutoff and control for net pay using a linear regression interacted with the loan cutoff. The dependent variable is the dollar amount of the borrower's first loan. Loan eligibility indicates a \$50 increase in payday-loan eligibility. All regressions control for month-, year-, and state-of-loan effects. Column 5 also controls for cutoff fixed effects. Standard errors are clustered by pay. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3
Regression Kink Estimates of the
Effect of Loan Eligibility on Loan Amount

	$300 Cutoff		$500 Cutoff	
	(1)	(2)	(3)	(4)
Pay x Loan Cap	-0.257^{***}	-0.256^{***}	-0.251^{***}	-0.249^{***}
	(0.005)	(0.005)	(0.004)	(0.004)
Pay	0.294^{***}	0.291^{***}	0.286^{***}	0.283^{***}
	(0.005)	(0.005)	(0.002)	(0.002)
Age		0.423^{***}		0.013
		(0.030)		(0.037)
Black		–		0.483
				(1.185)
Male		–		-4.810^{***}
				(1.261)
Credit Score		–		-0.006^{*}
				(0.003)
Checkings		–		0.004^{***}
				(0.001)
Home Owner		–		11.669^{***}
				(1.416)
Direct Deposit		–		0.424
				(0.972)
Garnishment		–		-1.154
				(4.045)
Observations	33,259	33,259	96,766	96,766

Notes: This table reports regression kink estimates of the impact of loan eligibility interacted with pay on loan amount. The sample consists of first-time payday-loan borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly earning more than $100 and within $1000 of a kink point. Loan amount is limited to half of net pay up to the loan limit. Columns 1-2 include states with a $300 loan limit. Columns 3-4 include states with a $500 loan limit. The dependent variable is the dollar amount of the borrower's first loan. Loan cap is an indicator for eligibility for the largest loan available in a state. All regressions control for month-, year-, and state-of-loan effects. Standard errors are clustered by pay. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 4
Regression Discontinuity Estimates of the Effect of Loan Amount on Default

	Polynomial		Linear Spline		Local Linear	
	(1)	(2)	(3)	(4)	(5)	(6)
Loan Amount	−0.127**	−0.113**	−0.121**	−0.123**	−0.087*	−0.098**
	(0.053)	(0.050)	(0.052)	(0.052)	(0.050)	(0.051)
Age		−0.462***		−0.461***		−0.462***
		(0.035)		(0.035)		(0.035)
Black		−1.507		−1.628		−1.336
		(2.190)		(2.210)		(2.193)
Male		1.931		1.976		2.183
		(1.958)		(1.968)		(1.935)
Credit Score		−0.044***		−0.044***		−0.044***
		(0.004)		(0.004)		(0.004)
Checkings		0.001		0.001		0.000
		(0.002)		(0.002)		(0.002)
Home Owner		−1.406		−1.429		−1.525
		(2.044)		(2.064)		(2.027)
Direct Deposit		0.123		0.180		0.252
		(1.671)		(1.683)		(1.655)
Garnishment Flag		16.075**		16.542**		16.149**
		(8.094)		(8.151)		(8.205)
Observations	9,473	9,473	9,473	9,473	9,473	9,473

Notes: This table reports regression discontinuity estimates of loan amount on default. The sample consists of first-time payday-loan borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly earning between $100 and $1100 every two weeks. Columns 1-2 control for a seventh-order polynomial in net pay. Columns 3-4 control for a linear spline in net pay. Columns 5-6 stack data from each cutoff and control for net pay using a linear regression interacted with the loan cutoff. The dependent variable is an indicator for bouncing a check on the first loan. All regressions instrument for loan amount using loan eligibility and control for month-, year-, and state-of-loan effects. Columns 5 and 6 also control for cutoff fixed effects. Standard errors are clustered by pay. Coefficients and standard errors are multiplied by 100. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 5
Regression Kink Estimates of the
Effect of Loan Amount on Default

	\$300 Cutoff		\$500 Cutoff	
	(1)	(2)	(3)	(4)
Loan Amount	-0.091^{***}	-0.088^{***}	-0.032^{***}	-0.028^{***}
	(0.017)	(0.017)	(0.004)	(0.004)
Pay	-0.009^{***}	-0.007^{***}	0.001	0.003^{***}
	(0.002)	(0.001)	(0.001)	(0.001)
Age		-0.381^{***}		-0.238^{***}
		(0.023)		(0.009)
Black		–		3.111^{***}
				(0.276)
Male		–		1.789^{***}
				(0.294)
Credit Score		–		-0.019^{***}
				(0.001)
Checkings		–		-0.001^{***}
				(0.000)
Home Owner		–		-0.956^{***}
				(0.355)
Direct Deposit		–		-2.733^{***}
				(0.273)
Garnishment		–		0.302
				(1.125)
Observations	33,259	33,259	96,766	96,766

Notes: This table reports regression kink estimates of loan amount on default. The sample consists of first-time payday-loan borrowers living in states offering payday loans in \$1 or \$10 increments who are paid biweekly or semimonthly earning more than \$100 and within \$1000 of a kink point. Columns 1-2 include states with a \$300 loan limit. Columns 3-4 include states with a \$500 loan limit. The dependent variable is an indicator for default on the first loan. All regressions instrument for loan amount using an indicator for eligibility for the largest loan available in a state and control for month-, year-, and state-of-loan effects. Coefficients and robust standard errors are multiplied by 100. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 6
Regression Discontinuity Subsample Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loan Amount	−0.120**	−0.527	0.032	−0.008	0.007	0.095	0.274
	(0.051)	(0.450)	(0.101)	(0.073)	(0.100)	(0.149)	(0.668)
Loan Amount x Over 40		−0.039					
		(0.042)					
Loan Amount x High Credit Sc.			0.020				
			(0.028)				
Loan Amount x High Checking				−0.021			
				(0.025)			
Loan Amount x Male					0.011		
					(0.037)		
Loan Amount x Black						−0.006	
						(0.037)	
Loan Amount x Home Owner							−0.006
							(0.062)
Observations	9,473	9,443	2,165	2,274	1,316	1,316	1,160

Notes: This table reports regression discontinuity estimates interacted with borrower characteristics. The sample consists of first-time payday-loan borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly earning between $100 and $1100 every two weeks who report information on the relevant characteristic. The dependent variable is an indicator for bouncing a check on the first loan. All regressions instrument for loan amount using loan eligibility interacted with the relevant characteristic and control for a seventh-order polynomial in net pay, the borrower characteristic, and month-, year-, and state-of-loan effects. Standard errors are clustered by pay. Coefficients and standard errors are multiplied by 100. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 7
Regression Kink Subsample Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loan Amount	−0.032***	−0.044***	−0.029***	−0.034***	−0.010	−0.014**	−0.053***
	(0.004)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.013)
Loan Amount x Over 40		0.032***					
		(0.008)					
Loan Amount x High Credit Sc.			−0.001				
			(0.008)				
Loan Amount x High Checking				0.012			
				(0.008)			
Loan Amount x Male					−0.023*		
					(0.012)		
Loan Amount x Black						−0.012	
						(0.012)	
Loan Amount x Home Owner							0.020
							(0.016)
Observations	96,766	96,631	91,261	89,844	40,878	40,878	34,133

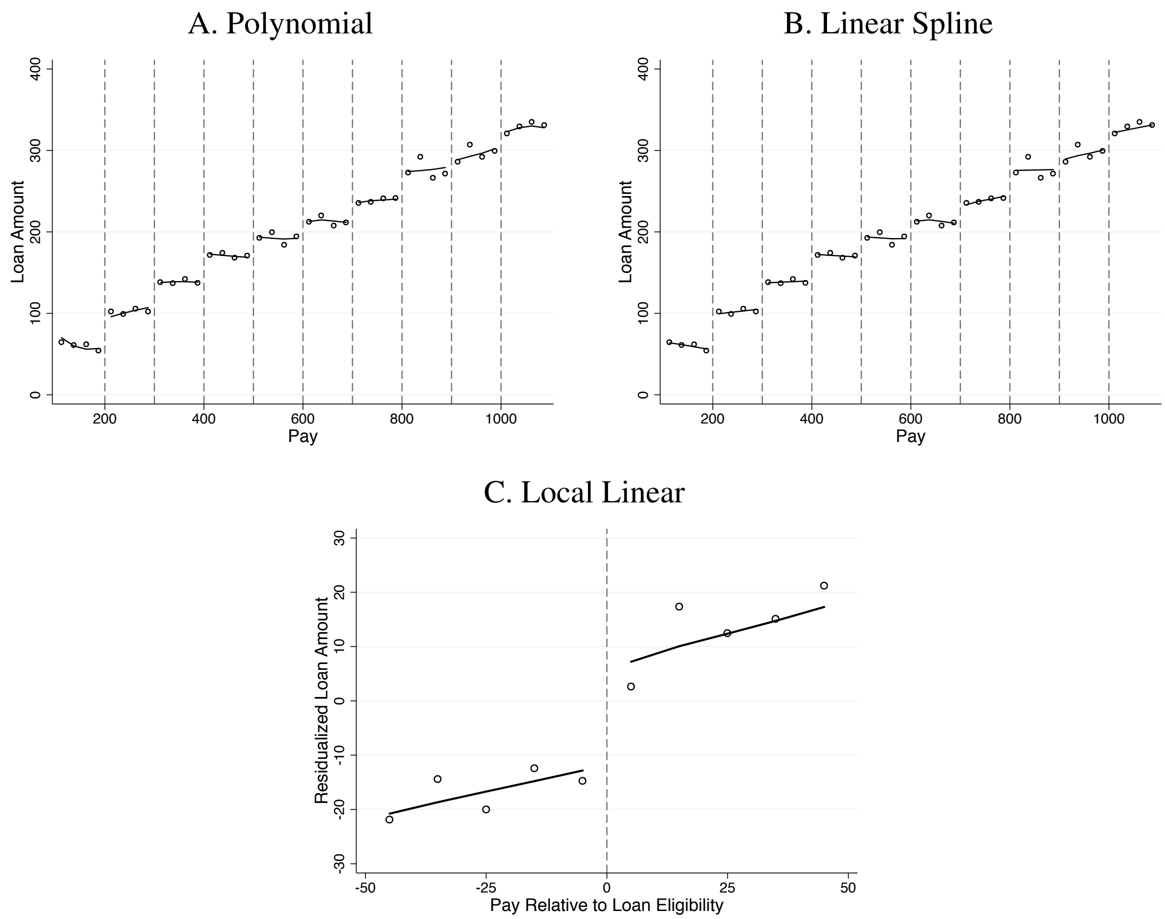
Notes: This table reports regression kink estimates interacted with borrower characteristics. The sample consists of first-time payday-loan borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly earning more than $100 and within $1000 of a kink point who report information on the relevant characteristic. The dependent variable is an indicator for bouncing a check on the first loan. All regressions instrument for loan amount using an indicator for eligibility for the largest loan available in a state interacted with characteristic listed in the left-most column, and control for pay interacted with the listed characteristic, the listed characteristic, and month-, year-, and state-of-loan effects. Coefficients and robust standard errors are multiplied by 100. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 8
OLS Estimates of the Effect of Loan Amount on Default

	RD Sample				RK Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Loan Amount	0.020***	0.046***	0.045***	0.047***	0.008***	0.026***	0.027***	0.029***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)
Biweekly Pay		−0.023***	−0.019***	0.009		−0.012***	−0.009***	−0.007***
		(0.002)	(0.002)	(0.008)		(0.000)	(0.000)	(0.000)
Age			−0.466***	−0.465***			−0.303***	−0.303***
			(0.033)	(0.033)			(0.008)	(0.008)
Black			0.472	0.408			3.151***	3.161***
			(1.997)	(1.997)			(0.266)	(0.266)
Male			2.337	2.440			2.432***	2.511***
			(1.878)	(1.880)			(0.285)	(0.285)
Credit Score			−0.041***	−0.042***			−0.018***	−0.018***
			(0.004)	(0.004)			(0.001)	(0.001)
Checkings			−0.001	−0.001			−0.001***	−0.001***
			(0.002)	(0.002)			(0.000)	(0.000)
Home Owner			−2.011	−2.053			−1.276***	−1.277***
			(1.905)	(1.907)			(0.340)	(0.340)
Direct Deposit			0.082	−0.175			−2.377***	−2.221***
			(1.576)	(1.575)			(0.258)	(0.258)
Garnishment			15.522*	15.410*			0.155	0.119
			(8.191)	(8.261)			(1.076)	(1.076)
Loan Eligibility				−2.943***				−0.525***
				(0.873)				(0.081)
R^2	0.028	0.039	0.066	0.067	0.037	0.048	0.066	0.067
Observations	9,473	9,473	9,473	9,473	130,025	130,025	130,025	130,025

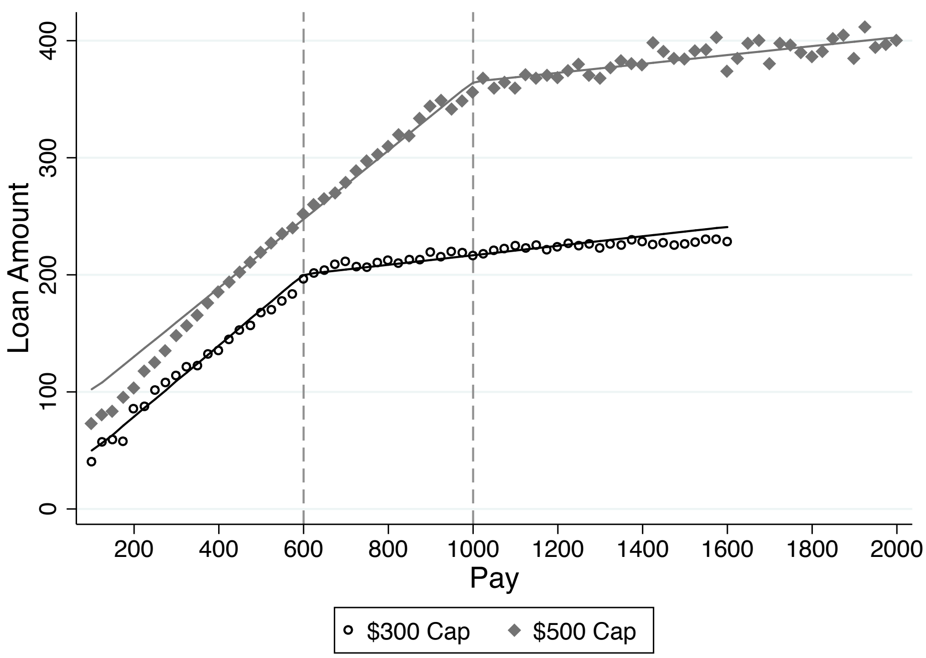
Notes: This table reports OLS estimates of the cross-sectional correlation between loan amount and default. The regression discontinuity (RD) sample consists of first-time payday-loan borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly earning between $100 and $1100 every two weeks. The regression kink (RK) sample consists of first-time payday-loan borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly earning more than $100 and within $1000 of a kink point. All regressions control for month-, year-, and state-of-loan effects. Coefficients and robust standard errors are multiplied by 100. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Figure 1
Loan Eligibility and Loan Amount in the Regression Discontinuity Sample



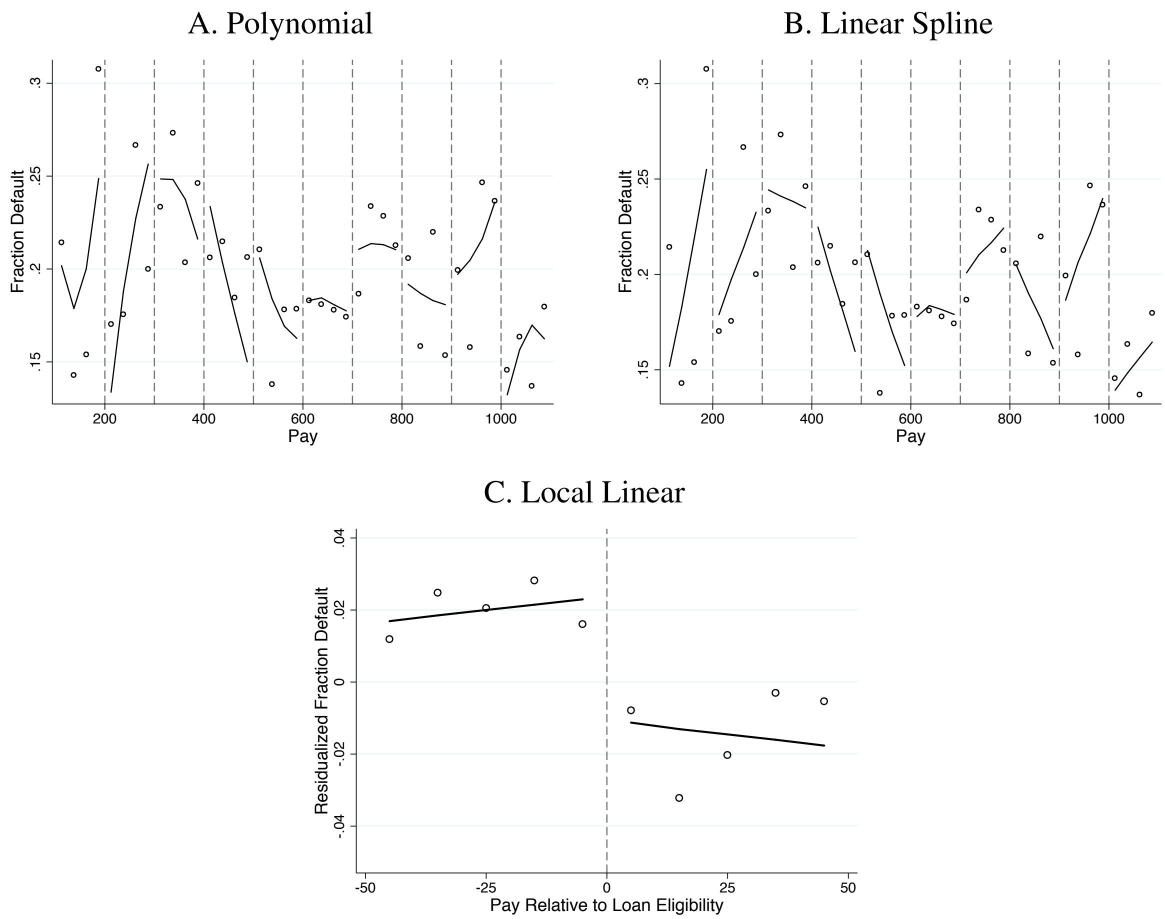
Notes: These figures plot average loan size and biweekly pay for first-time payday borrowers in our regression discontinuity sample. The sample consists of borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly between $100 and $1100. The smoothed line in Figure A controls for a seventh-order polynomial in net pay. Figure B controls for a linear spline in net pay. Figure C stacks data from each cutoff and controls for net pay using a linear regression and a linear regression interacted with the loan cutoff. See text for additional details.

Figure 2
Loan Eligibility and Loan Amount in the Regression Kink Sample



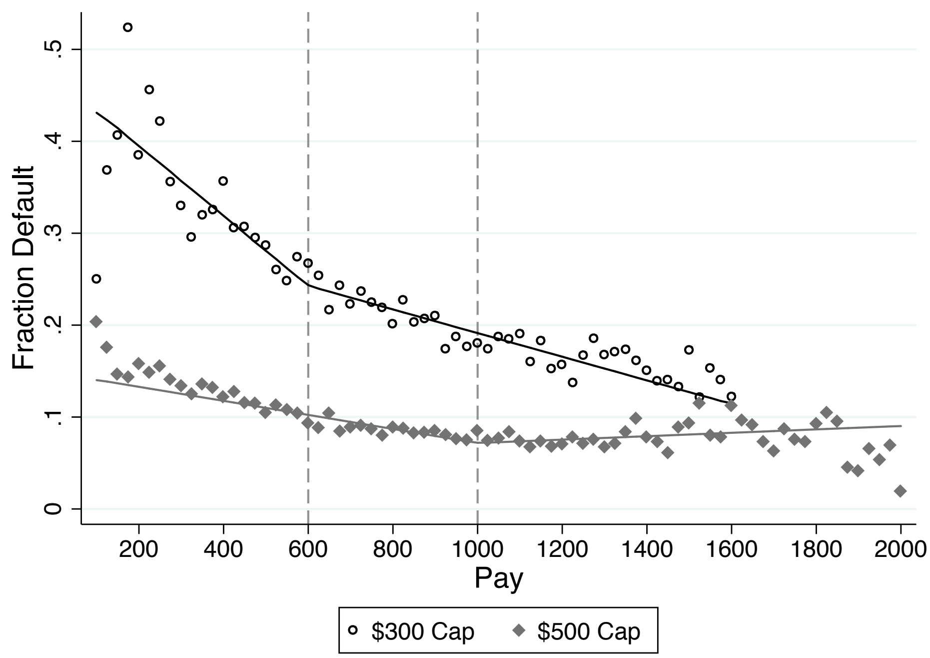
Notes: This figure plots average loan size and biweekly pay for first-time payday borrowers in our regression kink sample. The sample consists of borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly and paid more than $100 and within $1000 of a kink point. The smoothed line controls for pay interacted with being eligible for the maximum loan size in a state. See text for additional details.

Figure 3
Loan Eligibility and Default in the Regression Discontinuity Sample



Notes: These figures plot average default and biweekly pay for first-time payday borrowers in our regression discontinuity sample. The sample consists of borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly between $100 and $1100. 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. Figure C stacks data from each cutoff and controls for net pay using a linear regression and a linear regression interacted with the loan cutoff. See text for additional details.

Figure 4
Loan Eligibility and Default in the Regression Kink Sample



Notes: This figure plots default and biweekly pay for first-time payday borrowers in our regression kink sample. The sample consists of borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly and paid more than $100 and within $1000 of a kink point. The smoothed line controls for pay interacted with being eligible for the maximum loan size in a state. See text for additional details.