

The Effects of Usury Laws: Evidence from the Online Loan Market*

Oren Rigbi[†]
Ben-Gurion University of the Negev

May 2011

Abstract

Usury laws cap the interest rates that lenders can charge. Using data from Prosper.com (an online lending marketplace), I show how interest rate caps affect: 1) the probability that a loan is funded; 2) the amount a borrower requests; 3) the interest rate at which a loan is funded; and 4) loan repayments. The key to my empirical strategy is that there initially was substantial variability in states' interest rate caps, according to which Prosper borrowers from different states faced caps ranging from 6 to 36%. A behind-the-scenes change in loan origination, however, suddenly increased the cap to 36% in all but one state. This change, which was not pre-announced, creates "treatment" states where caps rose and a few control states where caps remained unchanged. I find that higher interest rate caps increase the probability that a loan will be funded, especially if the borrower is risky and previously was just "outside the money." I do not find, however, that borrowers change the loan amounts they request or that their probability of default rises. On the other hand, the interest rate paid rises slightly, probably because online lending is substantially, yet imperfectly, integrated with the general credit market.

1 Introduction

Legislated caps on interest rates, known as usury laws, are one of the oldest forms of market regulation,¹ and have inspired great debate throughout history. Opponents argue

*This is a revised version of Chapter 1 of my 2009 Stanford University dissertation. I am particularly indebted to my advisors Liran Einav, Caroline Hoxby and Ran Abramitzky for their encouragement and continuous support. I also benefited from discussions with Itai Ater, Danny Cohen-Zada, Mark Gradstein, Ginger Jin, Jon Levin, Yotam Margalit, Christopher Peterson, Monika Piazzesi, Yaniv Yedid-Levi, participants at several conferences, as well as seminar participants at various universities. Neale Mahoney kindly provided individual exemption data. I gratefully acknowledge financial support from the Stanford Olin Law and Economics Program, the Shultz Graduate Student Fellowship, the SIEPR Fellowship, the European Community's Seventh Programme (grant no. 249232) and the Israel Science Foundation (grant No. 1255/10).

[†]Mailing Address: Department of Economics, Ben-Gurion University of the Negev. P.O.B 653, Beer-Sheva 84105, Israel. Email: origbi@bgu.ac.il.

¹For historical reviews of usury laws see Homer and Sylla (2005) and Glaeser and Scheinkman (1998). Benmelech and Moskowitz (2010) study the political economy of usury laws and provide a detailed description of their evolution in America during the 19th century.

that interest rate caps exclude higher risk borrowers from obtaining credit or developing a credit history, simply because lenders will not lend if it is unprofitable. As a result, borrowers may resort to illegal loan sharks and face financial distress. On the other hand, proponents, taking consumer protection into account, contend that caps reduce the price paid by a given borrower by limiting the market power of lenders,² and also that they prevent naive borrowers from agreeing to loan terms on which they will eventually be forced to default.³

The early empirical literature examining this debate nearly always finds a positive correlation between the interest rate cap and the amount of credit given. The effects on other outcomes, however, such as the amount requested, the price paid by borrowers, and loan repayment have mostly been inconclusive even though the quality of the data and the sophistication of the econometric methods used significantly improved over time.⁴ More recent empirical studies have focused on the effects of access to credit at very high interest rates, known also as payday loans. The main findings of these works is that obtaining credit at high rates has not alleviated economic hardship, but rather has had adverse effects on loan repayments, the ability to pay for other services, and job performance (Melzer (2009), Skiba and Tobacman (2009) and Carrell and Zinman (2008)). Another recent contribution is Benmelech and Moskowitz (2010) who demonstrate that imposing interest rate restrictions has hurt the financially weak rather than help them.

This paper informs the debate by exploiting an exogenous increase in the interest rate cap that had affected some, but not all, online borrowers. Relying on an exogenous increase rules out alternative explanations for the effect of interest rate caps that are based on the endogenous formation of legislated caps. It also allows me to isolate generic time effects because some borrowers were unaffected by the change. Furthermore, I can eliminate selection problems that have plagued previous work on usury laws because the data used include virtually all information observed by lenders.

²E.g. Brown (1992) and Rougeau (1996).

³See Wallace (1976). Hyperbolic discounting has been suggested to explain this behavior (Laibson (1997)).

⁴While the earliest papers used state-level data (Goudzwaard (1968) and Shay (1970)), later ones used individual-level data and account for the truncation in the price paid (Greer (1974, 1975), Villegas (1982, 1989) and Alessie, Hochguertel, and Weber (2005)).

Economic theory suggests that interest rate caps may affect credit markets through various channels. First, higher caps make lending to higher risk borrowers profitable by extending credit to some borrowers who were previously denied it. Second, because the riskiness of a loan depends on its size, and not just on the identity of the borrower, higher caps may cause a given borrower to request a larger loan. Third, higher caps may increase the probability that borrowers default on loans, particularly if the caps were preventing borrowers from agreeing to loan terms they could not manage financially. Finally, theory suggests that interest rate caps can actually reduce the price paid for any given loan. For example, if lenders possess market power, then usury laws might decrease the interest rates charged, shifting the market toward the price that would be obtained in the absence of market power. Even if lenders have no market power, they may still be inelastic in their supply of credit. If so, then the price of credit will rise with the cap simply because a greater number of borrowers who request loans under the higher cap will drive up the demand for credit.

This study is based on recent data from Prosper.com, the largest online person-to-person loan marketplace in the U.S. These data allow me to observe full details on the universe of Prosper's loan requests, loan originations, and loan repayments. Furthermore, Prosper's recent history provides an informative natural experiment. Prior to April 15, 2008, a Prosper borrower's interest rate cap was governed by his state's usury law, which generally varied between 6 and 36%. On April 15, 2008, however, a formal change in Prosper's loan origination suddenly placed its borrowers' interest rate cap at 36% in all but one state (Texas).⁵ In other words, borrowers in most states faced a sudden increase in their interest rate cap, and borrowers in a few states faced no change - either because their cap remained lower than 36% after April 2008 or it already was 36% before April 2008.

I use a differences-in-differences approach which entails comparing several out-

⁵In Texas the maximum interest rate caps have remained at 10% and 18%, depending on the purpose of the loan. To the best of my knowledge, there was no legal reason preventing Prosper from increasing the maximum interest rate for Texas borrowers to 36%. One possible explanation for the decision to keep a lower cap is that Prosper, or Prosper's charting renting bank partner, had informal discussions with bank regulators in Texas and were concerned that circumventing Texas law was more politically risky than circumventing the laws of other states.

come variables across time (before and after April 15, 2008) and across states (treated and control states). This strategy is richer than one might think at first glance because the states' initial conditions varied. Some states' borrowers saw their cap rise by only 11%, while others saw it rise by as much as 30%. I exploit this variation to estimate the effect of a change in the interest rate cap. Moreover, since the effect of an interest rate cap may depend on a borrower's riskiness, I can estimate effects that are heterogenous in the riskiness of the borrower.

One major concern when studying the effects of interest rate caps is selection because estimates of the effects may be confounded with the changing composition of borrowers. For example, an individual's decision to apply for a loan depends on his expectation that the loan will be funded. Moreover, if the probability that a loan is funded depends on the maximum interest rate the borrower is allowed to pay, then the composition of individuals applying for loans changes when the cap changes. I am able to control for selection since virtually all of the information that potential lenders observe is recorded in the data. This is in contrast to previous studies in which loans were typically originated through in-person interviews of applicants, so that loan officers could observe a great deal of information that the econometrician missed.

My first cut at the data is a very simple comparison of outcomes before and after April 2008. Then, I test the theoretical predictions while accounting for endogeneity and selection. In particular, I analyze whether a given loan is more likely to be funded if its borrower was previously risky enough to be restricted by his state's interest rate cap. As predicted by economic theory, I find that the largest increase in funding probability is experienced by borrowers who were previously just "outside the money" in their state. I also address the concern that the amount requested by a borrower might be endogenous to the interest rate cap by investigating whether borrowers request larger loans following an increase in the cap. My findings show that they mostly do not. Additionally, I analyze whether a given borrower pays a higher price for credit when his interest rate cap rises, and find either zero or small increases (no more than 90 basis points) in interest rates paid. Finally, for any given borrower, loan repayment patterns, such as default, do not change following an increase in interest rate caps.

The finding that relates interest rate caps to the price of credit is then used to determine whether the supply of credit on Prosper is perfectly elastic. This is done by investigating whether interest rates go up. Essentially no group of borrowers should pay more if the supply of credit were perfectly elastic. However, my results suggest that the supply of credit on Prosper is at least slightly inelastic. Furthermore, given the low concentration levels of lenders at Prosper, the explanation mentioned earlier of an increase in interest rates based on lender's market power is implausible. Because Prosper's borrowers constitute a tiny share of total U.S. borrowing, these findings suggest that Prosper is substantially, yet imperfectly, integrated into credit markets. This is not altogether surprising, because it operates online and most of its lenders are individual rather than institutional investors.

The remainder of the paper is organized as follows. Section 2 introduces Prosper and describes the data. In Section 3, I motivate and describe my empirical strategy. In Section 4 I present basic evidence from a simple first cut of the data. Section 5 presents a regression-based analysis, accounting for selection and endogeneity. In Section 6, I examine the robustness of the findings, and Section 7 discusses these findings and concludes.

2 Environment and Data

On Prosper's online platform lenders and borrowers interact and originate fixed rate unsecured consumer loans of \$1,000 to \$25,000. In order to borrow money, the borrower posts a listing indicating the amount requested and the maximum interest rate he is willing to pay, subject to usury limitations. Prosper adds verified financial information gathered from credit bureaus, and the borrower can also add non-verified information. Lenders browse listings and can bid on portions of them, knowing that a loan is originated only if it is fully funded. The loans are fully amortized in monthly payments over three years. Borrowers are subject to late fees and can suffer a substantial reduction in their credit score.

Prosper, being the lender issuing loans, originally had to comply with each state's

small-loan usury laws. However, starting on April 15, 2008 a collaboration with a national bank allowed Prosper to increase the interest rate cap to 36%, regardless of the borrower's state of residence.⁶ Notably, Prosper did not advertise that collaboration in advance, only informing borrowers and lenders on the day of the change as to its subsequent effects.

The data contains information on listings, loans, loan repayments, and marketplace participants, all of which can be found on Prosper's web site and can be used by its participants.⁷ On each listing the data available includes verified financial information obtained from credit reports generated by the credit bureaus and some non-verified information that the borrower provides. The verified information includes: the borrower's credit grade letter (based on his credit score), past and current delinquencies, past and current negative public records, credit lines, and state of residence.⁸ The non-verified information includes the borrower's purpose for the loan, employment status, income, and loan narrative. I also use the interest rate cap that applies to the loan and the monthly repayments. Most states have a fixed cap, but others let their cap fluctuate with the federal funds rate, and a few conditions the cap on the amount or purpose of the loan.⁹

During its start-up period, Prosper improved lenders' ability to screen high-risk borrowers by changing the information it provided (Freedman and Jin (2011) and Miller (2010)). To avoid a period with any major changes, I focus only on October 30, 2007 to September 30, 2008.¹⁰ The cap increase on April 15, 2008, the event I exploit, is nicely centered in this period.

Figure 1 graphs the numbers of listings and loans originated with Prosper by

⁶Collaborating with a national bank allowed Prosper to take advantage of a 1978 Supreme Court decision, *Marquette National Bank of Minneapolis v. First Omaha Service Corp.*, that permits national banks to export their lender status from their home state to other states, thereby preempting the usury laws of the borrower's home state.

⁷Questions and answers between lenders and borrowers are the only pieces of information that can be included in the listing's web page and that are not observed in my data. The decision to post them is up to the borrower.

⁸See online Appendix A.1 for a detailed description of the variables included in the analysis.

⁹For example, the interest rate restriction in California is 19.2% for loans up to \$2550, and 36% for loans in the range \$2,550-\$25,000. In Texas there is a limit of 18% on business loans and 10% on loans intended for other purposes. In Arkansas the interest rate cap is set at 6% higher than the federal funds rate.

¹⁰The data cover repayments paid until March 2010. Thus, I use information about the first 18 monthly repayments.

month and risk category.¹¹ April 15, 2008 is delineated by the vertical line. The monthly number of low- and medium-risk listings increases moderately, while the number of high-risk listings fluctuates. Also, the number of loans rises in 2008 until the April 15 change, and decreases slightly afterwards for all borrowers' risk categories.

Table 1 reports the descriptive statistics for the main variables. While nearly 70% of those requesting a loan are high-risk borrowers, only one third of those whose loan request is funded are high risk. On average, a borrower requests nearly \$7,500, and has a probability of 8.7% of getting funded at an APR of 18.56%. Three-quarters of the borrowers did not miss a single monthly repayment within the first 18 months of the loan. Table A.1 in the online Appendix summarizes the variables provided by the borrowers.

3 Empirical Strategy

As mentioned earlier, the empirical strategy used here exploits the exogenous shock to the maximum allowed interest rate that occurred on April 15, 2008 to identify its causal effects using a differences-in-differences approach. For my purposes, this change creates differences across both treated and non-treated states. The point at time in which the shock occurred adds an additional difference in the time dimension.

The change on April 15 creates a natural division of states into treatment and control groups. The control group includes the states that experienced no change in their cap. The treated states are divided into three groups based on the interest rate cap clusters shown in Figure 2. States that had an interest rate cap of 24-25% are labeled as *Low Intensity Treatment* and states with a cap of 16-21% and 6-12% are labeled as *Medium* and *High Intensity Treatments*, respectively. In addition, I include a set of week and a set of state indicators, thereby controlling for any effect that is constant within a week or within a state. Including a listing's characteristics also controls for other

¹¹While Prosper assigns a credit grade letter to each borrower based on the score provided by the credit bureau, I bundle credit grade letters into three risk categories to simplify the analysis. I refer to borrowers with credit scores greater than 720 as low-risk borrowers, borrowers with credit scores in the range 640-719 as medium-risk borrowers, and borrowers with credit scores in the range 520-639 as high-risk borrowers.

differences between listings. The resulting specification is

$$\begin{aligned}
Y_i = & \beta_0 + \beta_1 \cdot \textit{After} * \textit{Low Intensity Treat.}_i + \\
& \beta_2 \cdot \textit{After} * \textit{Med. Intensity Treat.}_i + \\
& \beta_3 \cdot \textit{After} * \textit{High Intensity Treat.}_i + \\
& \beta_4 \cdot \textit{Week}_i + \beta_5 \cdot \textit{State}_i + \beta_6 \cdot X_i + \epsilon_i
\end{aligned} \tag{1}$$

where Y_i is the outcome variable on which the treatment effect is estimated. $\textit{After} * \textit{Low}$ (or $\textit{Med.}$ or \textit{High}) $\textit{Intensity Treat.}_i$ indicate whether listing i was posted after April 15, 2008 and whether the borrower resides in a treated state with a pre-change interest rate restriction in the range of 24-25%, 16-21% or 6-12%, respectively. \textit{Week}_i and \textit{State}_i are vectors of week and state dummies. X_i is a vector with the characteristics of the listing. The coefficients β_1, β_2 and β_3 are the average low, medium, and high-intensity treatment effects on the outcome variable Y .

Since economic theory suggests that the treatment effect may depend on the risk level associated with a borrower,¹² the baseline specification I use throughout the analysis is an extension of equation (1) which allows the treatment effects to be heterogenous in the risk category of the borrowers.¹³ This enables me to exploit differences in time, state, and the risk category of borrowers to estimate the treatment effect of interest rate caps.

Most previous studies on the effects of usury laws that are based on individual-level data do not adequately account for selection: namely, the dependence between the ceiling interest rate and the attributes of potential borrowers. As a result, a researcher may incorrectly conclude that an increase in the cap reduces a listing's funding probability. In reality, an elevated cap increases the funding probability, but adversely changes the composition of listings. Unlike previous studies, my data include virtually all the

¹²The treatment effect may also depend on the slope of the supply curve. I discuss this issue below.

¹³The data are rich enough to allow for a greater degree of heterogeneity. Prosper assigns one of seven credit grade letters to each borrower, whereas the analysis is based on three risk categories, each consisting of several credit grade letters. I present the results for the analysis based on three risk categories because it involves fewer parameters of interest and provide the same qualitative insights as the analysis based on credit grade letters. Online Appendix B presents the estimation results when treatment effects are heterogenous in credit grade letters.

information that lenders may have observed in the process of making their bidding decision. Thereby, controlling for the information on borrowers in a flexible way enables me to account for selection.

In the following sections of the paper I focus on the effect of interest rate restrictions on four groups of outcome variables. Below I list these variables and the estimation method used:

1. The probability of a listing being funded - since the dependent variable indicates whether a listing was funded, a Probit model is appropriate for the estimation.
2. The amount a borrower requests - since the amount requested is a continuous variable that ranges from \$1,000 to \$25,000, I use a two-sided Tobit model.
3. The APR a borrower pays - because the APR paid by a borrower is a continuous variable that is bound to be below the interest rate cap, it is most natural to use a one-sided Tobit model.
4. The probability of default - although the default variable is binary, a linear probability model is used for its analysis in order overcome the problem of separation.¹⁴

This empirical strategy will be used to test whether the Prosper marketplace is perfectly integrated with the consumer loan market. If Prosper were perfectly integrated, that is facing a perfectly elastic supply curve, then the April 15 change would have been expected to redirect credit from other loan markets into the Prosper marketplace. Thus, I propose two tests for the null hypothesis that the supply curve of credit is perfectly elastic. One is based on the treatment effect on the APR; the other is based on the effect on the funding probability. The intuition behind these tests is that a perfectly elastic supply curve implies a zero price effect of the April 15 change, regardless of the treatment intensity and the risk category. It further implies that the funding probability

¹⁴Since the dependent variable is binary, the first model that comes to mind is a Probit model. Yet, some of the categories that had very few loans originated prior to April 15, 2008 had no variability in their default indicator variable. As a result, the dependent variable of these loans is perfectly predicted by the treatment intensity and risk categories dummies. This problem is known as quasi-complete separation (Zorn, 2005). Estimation of a Probit model results in very high estimates for some parameters and their standard deviations. Hence, I use a linear probability model instead.

is unchanged in categories that were unaffected by the treatment. Specifically, borrowers in the control group, and in treatment groups that were not bounded under the original cap, should not have experienced a change in their funding probability. Nonetheless, these tests are limited, because they do not distinguish between an upward sloping supply curve and generic time effects that affect differently the risk categories within any treatment group.

4 Basic Evidence

In considering differences over time and between treatment and control groups, and differentiating between risk categories, I focus on a narrow time window consisting of one month before and one month after the change. This reduces the potential effect of a generic time trend. I analyze both the control group and the group of borrowers that experienced the largest treatment, namely going from an interest rate cap in the 6-12% range to an interest rate cap of 36%.¹⁵ Within each group, I consider the three risk categories, and present the main variables of interest for each (number of listings and loans, the funding probability as well as the mean values of the amount requested, the price paid by borrowers, and the default probability).

The results presented in Table 2 reveal that the funding probability decreases in the control group for all risk categories, whereas a reverse pattern is exhibited in the treatment group. If the supply curve is upward sloping, then the different patterns between the treatment and control groups can be explained by a de facto shift in aggregate demand that results from an increase in the maximum allowed rate in the treatment groups. These findings can also be interpreted as evidence for different time effects across the two groups. The average amount requested does not change significantly over time within any group. The APR observed in the control group changes slightly over time, but the APR observed in the treatment group rises in all risk categories. The higher APR might be the result of a change in the composition of borrowers, because high

¹⁵These patterns carry through even by widening the time window. In addition, including more treatment groups does not provide additional insights.

risk borrowers with potentially different observed characteristics may have their loans funded under the elevated cap. While the default probability exhibits a mixed pattern in the control group, it jumps significantly in the treatment group.

5 Empirical Analysis

The comparison presented in Section 4 does not account for changes in the composition of listings posted under different interest rate caps. The analysis in this section is based on an extension of equation (1) which does allow for the estimated treatment effects to be heterogenous in the three risk categories.¹⁶ The vector of listing characteristics includes the financial information provided by Prosper with the effect of different variables relaxed to be nonlinear and the non-verified information provided by the borrower. Online Appendices A.1 - A.3 contain a detailed description of the variables that constitute the vector of the listing characteristics.

I first consider how the probability of a listing being funded is affected by the April 15 change using a Probit model in which the dependent variable is an indicator of whether a listing was funded. Table 3 presents the estimated treatment effects. In addition to the regression results, the table displays the empirical funding probabilities prior to the April 15 change as a benchmark. The coefficients should be interpreted as the expected increase in the funding probability. For example, increasing the cap from 24-25% to 36% increases the funding probability by up to 0.06. The increments in the funding probabilities of listings from the treatment groups with caps of 6-12% and 16-21% depend on the risk category and are bounded from above at 0.53 and 0.17, respectively.

Table 3 provides two insights. First, the treatment effect is significantly positive in those categories that could have benefited from an increase in the cap; that is, categories with an interest rate restriction which was more binding than that of their counterpart control group categories. Second, the largest treatment effect within a treat-

¹⁶The estimation results obtained when the treatment effects are allowed to be heterogenous in the seven credit grades are presented in Tables B.1 - B.4 in online Appendix B. These results exhibit similar patterns to those found here.

ment intensity group is for those in a different risk category. Here I define a risk category to be *restricted* if the interest rate cap is lower than the average interest rate in the control group for the same risk category before the April 15 change. For example, high- and medium- risk borrowers were *restricted* under a cap of 16-21% because their average APR in the control group was 26% and 20.1%, respectively. This suggests that there are likely to be positive treatment effects for these risk categories as a result of the increase in the cap from 16-21% to 36%. Furthermore, the finding that the largest treatment effects in different treatment groups are not estimated for the same risk category reflects the fact that the borrowers with the greatest share of their market interest rate falling between the original cap and 36% is not always the high risk one.

The amount that a borrower requests in a listing is a major determinant of the probability of getting funded. The regression results reveal that for the average listing, the funding probability decreases by 4.8% for a 1% increase in the amount requested.¹⁷ Although I control for the requested amount in the funding probability analysis, it may be endogenous in the sense that borrowers will tailor the amount they request according to the interest rate cap. I address this possibility by estimating the treatment effect on the amount a borrower requests. I use a two-sided Tobit model, because the amount requested must be in the range of \$1,000 - \$25,000. Table 4 indicates that the treatment effects are insignificant in eight out of the nine categories analyzed. Nonetheless, I can still reject the null hypothesis of a zero treatment effect (P-value = 0.001).

Next, I investigate the effect of the interest rate cap on the APR. One natural way to do this would be to use a loan's APR as the dependent variable. However, ignoring non-funded listings generates a problem of selection on the dependent variable, because the APR is bound to be below some value. Therefore, I use the information embodied not only in loans but also in non-funded listings. The dependent variable is the APR for loans and the interest rate cap for non-funded listings. The model is a one-sided Tobit model, implicitly assuming that the non-funded listings would have been funded under a higher interest rate cap. Specifically, the cap on non-funded listings is treated as a

¹⁷The specification used allows for non-linear effects of the amount requested on the funding probability. The effect is allowed to differ over the quartiles of the amount. The corresponding z-stats. are in the range of 31.6-38.2.

lower bound on the APR.

Given the high share of risky loan requests, a major concern is that the “true” APR distribution suffers from fat tails which would make the estimates sensitive to the proportion and to the extent to which the listings are censored. I resolve this problem by choosing a predicted funding probability of 0.1 to be the threshold below which observations are eliminated.¹⁸ The results in Table 5 suggest that the treatment effects on the interest rate in general are close to zero. While the effect is significantly positive in four out of the seven categories, none exceed 0.9%. These findings imply that an increase in the cap is not likely to cause any change in the APR, or at the most only a minor increase.

The raw data presented in Table 2 suggest that riskier loans are originated after the increase in the cap. I investigate whether, conditional on observables, riskier loans indeed are originated after the change. Specifically, I consider two loans with identical characteristics, one originated before the change and thus facing a lower cap and the other originated after the change and facing a 36% interest rate cap. I then use information about the first 18 payments for all originated loans. Because repayment history in general is complicated, I simplify the analysis by estimating the treatment effect on the default probability. In order to account for a selection of loans based on observables, I include each loan’s predicted funding probability as a regressor, using the estimates presented in Table 3.¹⁹ A zero treatment effect can be expected, because I condition on the characteristics of the loan, and find that the interest rate is nearly unchanged. Table 6 presents estimates from a linear probability model. These results indicate that, conditional on observables, there is no association between loan riskiness and the interest rate cap. The only exception is in the category of medium-risk loans that experienced

¹⁸I use observations from before April 15, 2008 to estimate a Probit model of the probability of a listing being funded. The model estimates are then used to predict the funding probability of listings posted before and after April 15. Focusing on listings with a funding probability greater than 0.1 eliminates 83,825 listings of which only 2,656 were funded. I also experiment with values different than 0.1 for the threshold such as 0.05, 0.2, 0.3 and 0.4. I find that after eliminating listings with low funding probability, the estimated treatment effects in categories with very few observations are sensitive to the threshold chosen. The estimation results are, nonetheless, robust in those categories with more observations, mainly the lower-risk categories.

¹⁹I use the Murphy-Topel standard errors adjustment to account for the fact that the funding probability is a generated regressor. See Hardin (2002) for details on this adjustment.

high treatment intensity. In this category, loan requests were unlikely to get funded before the April 15 change (only ten of which were funded, and none defaulted). The treatment effect there is 0.17. Furthermore, I cannot reject the null hypothesis, that all treatment effects are zero (P-value = 0.12).

I now invoke the positive correlation test that is common in the insurance markets literature in order to evaluate the extent of selection of borrowers based on variables that are unobservable to lenders. The basic idea of this test is that, conditional on observables, a positive correlation between the amount of insurance purchased and the ex-post occurrence of insured risk implies information asymmetry.²⁰ Thus, the finding that, conditional on observables, there is no correlation between loan repayment and the interest rate cap suggests that selection on unobservables is not a problem in evaluating other effects of interest rate caps.²¹

To test the null hypothesis that the supply curve is perfectly elastic, I implement the tests described in Section 5 using the treatment effects just estimated. I conduct two tests. In the first test, I examine whether treatment effects for all risk categories in the treatment groups, as well as changes in the APR in the control group, are zero.²² For the second one, I assume that low-risk borrowers were not *restricted* in the low- and medium-treatment intensity groups. I then perform a joint test for zero treatment effects in those non-restricted categories, and for a zero change in the funding probability in the control group. I reject both tests and conclude that the supply curve for credit is not perfectly elastic (The P-values of both tests are less than 0.001).

²⁰See Chiappori and Salanie (2000) for the original statement of the test.

²¹Iyer, Khwaja, Luttmer, and Shue (2010) demonstrate that lenders in Prosper infer borrower's credit worthiness using the non-verified information provided by the borrower. This ability of the lenders to use non-verified information supports the finding presented here of no information asymmetry.

²²Given that lenders' HHI is 0.04% and that the largest lender is responsible for only 0.8% of the credit allocated, the explanation mentioned earlier for positive treatment effects on the APR that is based on lenders' market power is unconvincing.

6 Robustness Tests

In each subsection below, I describe a specific concern, the test conducted to address it, and the result.²³

6.1 Linear Treatment Effects

My empirical approach estimates the fully non-linear effects of a change in the interest rate cap. One disadvantage of this approach, however, is that the treatment effect is assumed to be constant within a treatment group. I can verify that the findings are robust to this assumption by estimating a linear treatment effect specification. That is, a specification in which the treatment effect of a cap increase from 6 to 36% is constrained to be exactly twice the effect of a cap increase from 21 to 36%. The results of estimating this linear treatment effect specification for each of the outcome variables are presented in Table 7. The coefficients in the table are the estimated treatment effects for a 10% increase in the interest rate cap. According to the table, the funding probability of low-risk borrowers is not expected to significantly change following an increase in the cap, whereas the funding probability of high-risk borrowers is expected to increase by nearly 4 percentage points following a 10% increase in the cap. In addition, the estimated treatment effects reveal that the amount requested is expected to increase only for medium-risk borrowers, and the APR to decrease slightly for high-risk borrowers only. The default probability is not expected to change for any of the risk categories. While there are a few differences here from the non-linear specification estimates presented in Tables 3 - 6, most of the insights carry through to the linear effect specification.

6.2 Generic Time Effects

The differences-in-differences method identifies causal effects if one assumes that conditional on covariates, the time trend is similar across groups. However, the time trend may vary across states, and lenders may take this variation into account. For example, information regarding elevated financial stress in the auto industry that was revealed

²³Full results for the tests discussed in Sections 6.2 - 6.5 are available upon request.

around April 2008 may adversely affect Michigan borrowers. Thus, if the estimation is performed as if the time trend is state invariant, then the estimated treatment effects will underestimate the effect of the cap increase in Michigan from the original 25% to 36%. I suggest two ways to address this concern.

First, I enrich the specification with macro variables that can capture differences in time trends across states. I focus on variables that are posted regularly at the state level and that are potentially reflected in the time trend if they are not included. These include the monthly unemployment rate, the change in the unemployment rate, and the quarterly per person number of bankruptcy filings. I then compare the estimates of the treatment effect on the funding probability in a specification that includes the macro variables with the estimates of the baseline specification (presented in Table 3). I find that the two sets of estimates are almost identical, and that among the three macro variables included, only the coefficient on the bankruptcy variable is significant.

Second, in order for the treatment effects not to capture generic time trends, I focus on a narrow window around April 15, 2008. If the window is narrow enough, then the time effects will be redundant. However, too narrow time window can bring about insufficient variation in the data needed to estimate the baseline specification. In addition, a reduction in the number of observations yields noisier estimates. Hence, I use a time window of one month before and after April 15, 2008. This time window has enough variation to allow me to estimate the baseline specification. While the estimates obtained in the restricted sample are noisier, the overall pattern is similar. Thus, I conclude that the state invariant time effect assumption is not restrictive.

6.3 Amount-Dependent Interest Rate Cap

While in most states the interest rate caps are fixed, certain states restrict their caps to being amount-dependent.²⁴ As a result, the observed interest rate cap partially reflects

²⁴For example, the interest rate cap in California is 19.2% for loans of \$1000-\$2550 and 36% for loans of higher amounts. Other states with amount dependent caps are Arizona, Kentucky, Maine, Massachusetts, Minnesota and New Hampshire.

borrowers' responses through adjusting the amount they request.²⁵ I resolve this problem by forming simulating instruments for the amount requested. My goal is to exploit the variation in state interest rate caps, but not the variation generated by the endogenous decisions on the amount requested. Therefore, I instrument for the amount requested with the value of this variable if the decision about that amount were not affected by the interest rate cap. I do this using post-April 15 data, when no state had a cap that was amount-dependent.²⁶ Addressing endogeneity in the amount requested is important because borrowers from multi-cap states posted 22% (27%) of the listings (loans) in Prosper prior to April 15. Nonetheless, the use of simulated instruments only changes the association of borrowers with their original treatment group for 1.85% (1.32%) of the pre-April 15 listings (loans). Therefore, it is not surprising that the differences between the treatment effects estimated using simulated instruments and the treatment effects estimated in the baseline specification are tiny.

6.4 Listing Reposting

Borrowers at Prosper are allowed to repost a listing if the auction on their previous listing has ended and it was not fully funded, or if they withdrew their listing before the auction ended. A borrower who reposts a listing can change the content of the listing that he generated, including the amount requested, the maximum rate he is willing to pay, his photo, and his narrative. As a result, one might expect that the observed listings are heterogenous with respect to a borrower's ability to optimally position his loan request. Although some borrowers have their first listing optimally positioned and phrased, others need to repost their listing several times to achieve optimality. I want to disentangle the effect of the interest rate cap from the effect of the borrower's ability

²⁵This relationship between the amount requested and the interest cap creates a problem of endogeneity because these two variables are simultaneously determined. As a result, the cap is endogenous when the dependent variable is the amount requested. Furthermore, for the other dependent variables, a borrower may alter the amount he requests in order for a different cap to be applied, making the amount requested endogenous.

²⁶I estimate a Tobit model to determine how loan request characteristics and state dummies affect the amount requested using only post-April 15 data (excluding borrowers from Texas) in which the cap is set to 36% for all borrowers. The model estimates allow to predict the amount requested by borrowers prior to this date. These simulated amounts are used to determine the cap to be applied prior to April 15 for borrowers from multi-cap states. Finally, I re-estimate the baseline specification using the simulated amounts and interest rate caps.

to assemble an attractive listing. To do so, I account for this heterogeneity by focusing only on listings that are more likely to be optimally positioned. Specifically, I define a borrower’s “repost episode” as the sequence of listings posted by the same borrower in which each (except for the first) is posted a short time after the previous listing has expired or was withdrawn. I then only include the latest listing in each repost episode. I repeat the analysis for various thresholds, each constituting an upper limit on what defines a short time.²⁷

I estimate the baseline specification on the listing funding dummy as the dependent variable, restricting the sample to only the latest listing in each repost episode. The estimated treatment effects are almost identical to those estimated using the full sample. This suggests that the findings are robust to this specific institutional detail.

6.5 Placebo Test

One might suspect that the driving force of the findings is not the increase in the interest rate cap that took place at Prosper, but rather some other unobserved change. If this is the case, then the documented effects can be at most only partially attributed to the cap increase. In order to test whether a different change with similar effects has taken place, I conduct a placebo test. I use data from either before or after April 15, 2008, randomly draw a date, and estimate the baseline specification on the listing funding dummy as if the unobserved change had taken place on that date.²⁸ I conduct 200 repetitions of this procedure, and find that, on average, only 0.69 of the nine estimated treatment effects on the funding probability are significant at the 5% level. Eight of the treatment effects are found to be significant when the change is assumed to occur on April 15, 2008.²⁹ Comparing these two figures suggests that it is unlikely that another unobserved change

²⁷I experiment with 12, 24, 48, 72, 120 and 240 as the maximum number of hours between a listing expiration or withdrawal and posting of a listing by the same borrower. These figures define repost episodes.

²⁸Technically, I assign the value 1 to the variable *After* if a listing was posted after the randomly drawn date. Drawing a date that is very close to the beginning or the end of the sample period results in zero loans in some categories. As a result, the treatment effect on the funding probability for those categories cannot be estimated using a Probit model. Hence, I restrict the unobserved change to occur on 12/15/2007-03/15/2008 or 05/15/2008-08/15/2008.

²⁹The number of significant treatment effects estimated in the placebo test ranges from zero to three. Out of 200 repetitions performed, no significant treatment effect was found in 51.5% of the repetitions. In 31%, 14% and 3.5% of the repetitions, the number of significant treatment effects are 1, 2 and 3, respectively.

drives the findings.

7 Discussion and Conclusions

Access to credit is considered to be a main springboard to economic development. The historic evolution of usury laws has positioned them as a government intervention in credit markets that is required to protect consumers from excessively high interest rates. This paper uses detailed individual-level data to evaluate the validity of this claim in the online person-to-person credit market. I examine the effects of interest rate restrictions on the marketplace by analyzing a change that increased to 36% the maximum interest rate charged to a borrower in all but one US state. The main challenges of my study are the selection of borrowers into the sample and the isolation of causal effects from generic time effects that are unrelated to the rate change. Borrowers can be selected into the sample based on information that is observed or unobserved by lenders. Selection based on observables is taken into account by incorporating the information that lenders have when making their lending decisions. Selection based on unobservables (i.e. asymmetric information) is not an issue, as the positive correlation test suggests. Isolating causal effects from generic time effects is accomplished by using a control group that did not experience a change in its interest rate cap. The major contribution of this research lies in its ability to identify the causal effects of interest rate restrictions.

Four main findings can be drawn from the present study: *(i)* borrowers who were restricted under their original cap benefited from the increase in the cap and that the marginal borrower benefited most. *(ii)* any individual borrower was expected to pay at most only a slightly higher price for credit issued under a higher interest rate restriction. *(iii)* repayment patterns remained unchanged. *(iv)* Prosper is substantially, yet imperfectly, integrated with other credit markets.

It is important to bear in mind, that the experiment studied here is based on an increase in the interest rate cap in a single credit market, rather than a simultaneous increase in the cap in many credit markets.³⁰ Nonetheless, I show below how it is

³⁰A simultaneous change is defined as a change in an interest rate cap that occurs concurrently in many credit

possible to use the findings to hypothesize what the effects of a simultaneous change in the cap on loan repayment would be. Having the cap increased in a single credit market might have an adverse effect on the default probability in that credit market relative to the default probability in other credit markets. This would be so because a borrower facing higher future payments for a loan originated in the credit market with the higher cap would opt to default on this loan, rather than on a loan with a lower cap which is likely to be cheaper. Thus, the effect of a simultaneous change depends on how caps in different credit markets co-vary. If these caps are positively correlated across states, then a simultaneous increase in the caps is less likely to change the relative price of credit between credit markets within a state. Therefore, the treatment effect on the default probability that I estimate here (when the cap in other credit markets remain unchanged) constitutes an upper bound on the effect of a simultaneous increase on the default probability. However, if caps are negatively correlated or uncorrelated, then the effect of a simultaneous increase in the cap on the default probability in a single credit market would depend on changes in the relative price of credit in that credit market compared to its price in other credit markets.

To evaluate the extent to which regulation is correlated across credit markets, I use data on credit card interest rate caps and on individual bankruptcy exemptions.³¹ I find weak correlations between the interest cap used in Prosper before the April 15 change and the credit card interest rate cap (0.093) and individual bankruptcy exemption (-0.028).³² These weak correlations suggest that the effect of a simultaneous increase in the caps in many markets might differ from the effect estimated in this paper.

It should be emphasized that, in a sense, the lenders at Prosper exhibit erroneous risk assessment. I assume that the conducts Prosper and the consumer credit market markets in the same direction and magnitude.

³¹Gropp, Scholz, and White (1997) demonstrate the link between personal bankruptcy laws and credit prices. Data on credit card interest rate caps are taken from the American Bankers Association. Data on exemption was compiled by Mahoney (2009) based on the bankruptcy exemptions featured in Elias (2006). Bankruptcy exemption data for each state indicate the average exemption that individuals from that state are entitled to if they choose to file for bankruptcy. Individual exemptions are calculated based on households' self reported balance sheets from the 2005 Panel Survey of Income Dynamics.

³²Similarly, Benmelech and Moskowitz (2010) find a weak relationship between 19th century usury and bankruptcy laws.

operate in are similar, and that the consumer credit market is efficient, and therefore it assesses risk properly. Thus, the fact that 39.3% of the loan requests are claimed to be used for debt consolidation can be explained by cheaper credit being allocated in Proper because of missing risk assessment by Prosper’s lenders. Still, as long as the risk of loan requests in the treatment and control groups is assessed with the same magnitude of error, the differences-in-differences approach guarantees that the findings here would hold even if the risk of borrowers at Prosper were assessed properly.³³

When examining the paper’s findings, one must take into consideration the effects of the financial crisis that began in the end of 2007 and that had significantly deepened in September 2008. I use data on loan requests from November 2007 - September 2008, and data on loan repayments that up to April 2010. Hence, the effects of the crisis on loan origination is presumably constant over the time period used for the analysis. On the other hand, the aggravation in the crisis from September 2008 may dominate any cross-section heterogeneity in the default rate, and may invalidate the finding that there is no selection on unobservables.

Although Prosper is a new and unique marketplace, my findings are at least indicative of the effects of usury laws in other credit markets, especially those with similar market structure. The main takeaway point from this inquiry is that in a system with both restricted and essentially unrestricted, imperfectly integrated markets, interest rate restrictions in a sub-market do not seem to make much difference.

References

- ALESSIE, R., S. HOCHGUERTEL, AND G. WEBER (2005): “Consumer Credit: Evidence From Italian Micro Data,” *Journal of the European Economic Association*, 3(1), 144–178.
- BENMELECH, E., AND T. J. MOSKOWITZ (2010): “The Political Economy of Financial Regulation: Evidence from U.S. State Usury Laws in the 19th Century,” *The Journal*

³³Note that this insight is carried through even if the magnitude of erroneous risk assessment is not persistent over time, as reflected by lenders’ learning found in Freedman and Jin (2011).

of Finance, 65(3), 1029–1073.

BROWN, J. L. (1992): “An Argument Evaluating Price Controls on Bank credit Cards in Light of Certain Reemerging Common Law Doctrines,” *Georgia State University Law Review*, 9, 797–819.

CARRELL, S., AND J. ZINMAN (2008): “In Harms Way? Payday Loan Access and Military Personnel Performance,” *Working Paper*.

CHIAPPORI, P.-A., AND B. SALANIE (2000): “Testing for Asymmetric Information in Insurance Markets,” *Journal of Political Economy*, 108(1), 56–78.

ELIAS, S. (2006): *The New Bankruptcy: Will it Work for You?* Nolo Press.

FREEDMAN, S., AND G. Z. JIN (2011): “Learning by Doing with Asymmetric Information: Evidence from Prosper.com,” *NBER Working Paper No. 16855*.

GLAESER, E. L., AND J. SCHEINKMAN (1998): “Neither A Borrower Nor A Lender Be: An Economic Analysis of Interest Restrictions and Usury Laws,” *The Journal of Law and Economics*, 41(1), 1–36.

GOUDZWAARD, M. B. (1968): “Price Ceilings and Credit Rationing,” *Journal of Finance*, 23(1), 177–185.

GREER, D. F. (1974): “Rate Ceilings, Market Structure, and the Supply of Finance Company Personal Loans,” *Journal of Finance*, 29(5), 1362–1382.

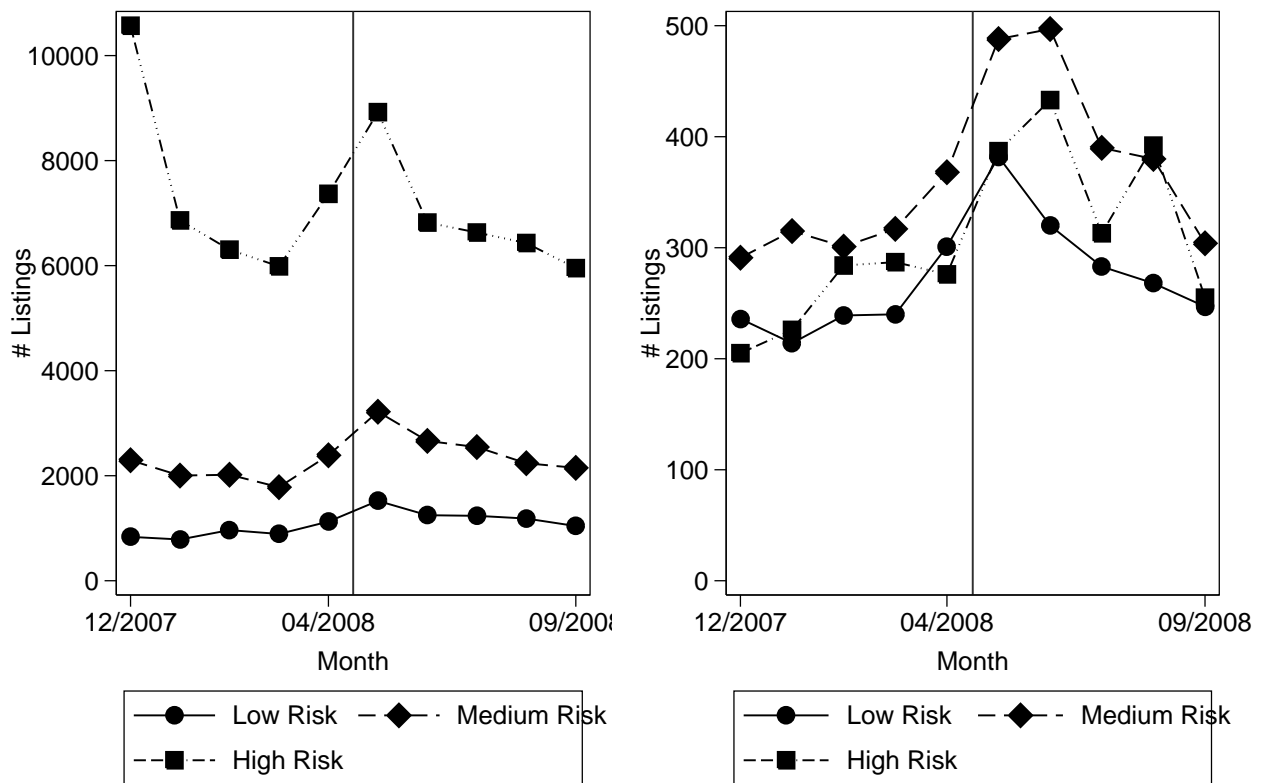
GROPP, R., J. K. SCHOLZ, AND M. J. WHITE (1997): “Personal Bankruptcy and Credit Supply and Demand,” *The Quarterly Journal of Economics*, 112(1), 217–251.

HARDIN, J. W. (2002): “The Robust Variance Estimator for Two-Stage Models,” *The Stata Journal*, 2(3), 253–266.

HOMER, S., AND R. SYLLA (2005): *A History of Interest Rates*. John Wiley & Sons Inc., fourth edn.

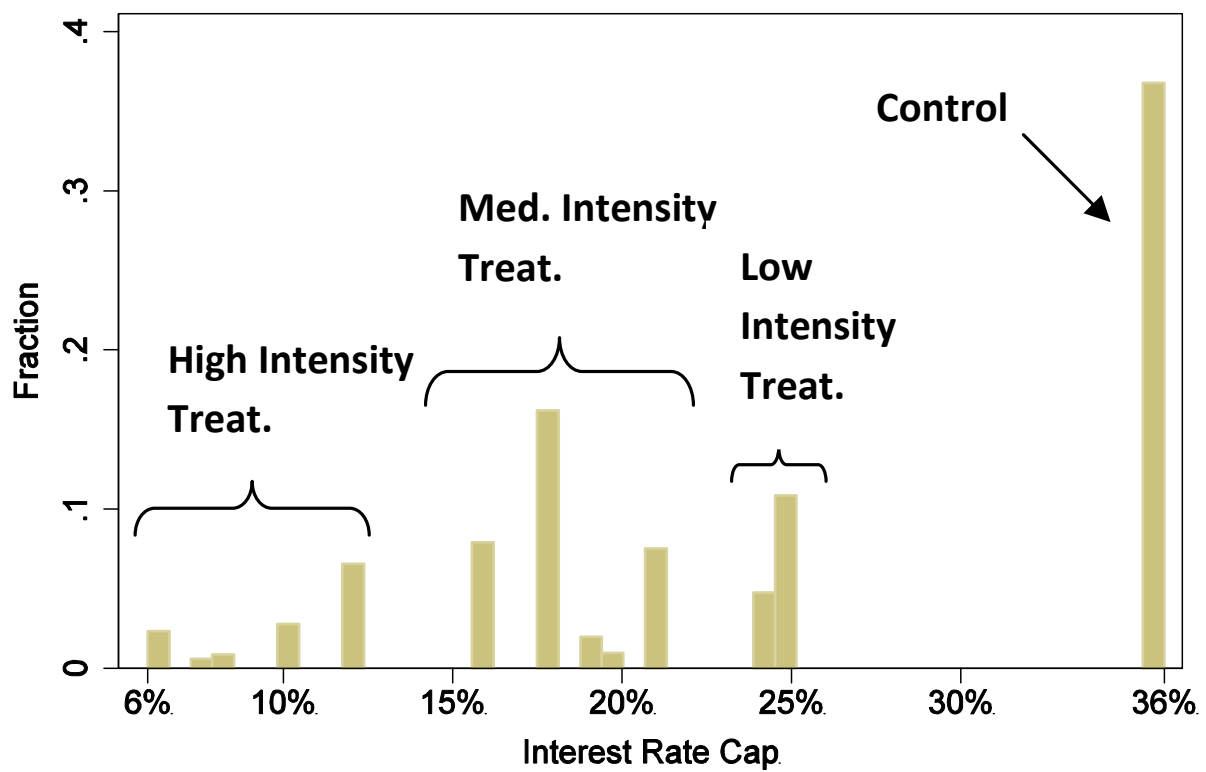
- IYER, R., A. KHWAJA, E. LUTTMER, AND K. SHUE (2010): “Inferring Asset Quality: Determining Borrower Creditworthiness in Peer-to-Peer Lending Markets,” *NBER Working Paper No. 15242*.
- LAIBSON, D. (1997): “Golden Egges and Hyperbolic Discounting,” *The Quarterly Journal of Economics*, 62, 443–477.
- MAHONEY, N. (2009): “Bankruptcy, Unpaid Care, and Health Insurance Coverage,” Discussion paper, Stanford University.
- MELZER, B. T. (2009): “The Real Costs of Credit Access: Evidence from the Payday Lending Market,” *Quarterly Journal of Economics* (*forthcoming*).
- MILLER, S. (2010): “Information and Default in Consumer Credit Markets: Evidence from a Natural Experiment,” Discussion paper, University of Illinois.
- ROUGEAU, V. D. (1996): “Rediscovering Usury: An Argument For Legal Controls On Credit Card Interest Rates,” *University of Colorado Law Review*, 67, 1–46.
- SHAY, R. P. (1970): “Factors Affecting Price, Volume, and Credit Risk in the Consumer Finance Industry,” *Journal of Finance*, 25(2), 503–515.
- SKIBA, P. M., AND J. B. TOBACMAN (2009): “Do Payday Loans Cause Bankruptcy?,” Discussion paper, University of Pennsylvania.
- VILLEGAS, D. J. (1982): “An Analysis Of The Impact Of Interest Rate Ceilings,” *Journal of Finance*, 37(4), 941–954.
- WALLACE, G. J. (1976): “The Uses Of Usury: Low Rate Ceilings Examined,” *Boston University Law Review*, 56, 451–497.
- ZORN, C. (2005): “A Solution to Separation in Binary Response Models,” *Political Analysis*, 13(2), 157–170.

Figure 1: The Number of Listings and Loans Over Time



The left panel shows the number of listings and the right panel shows the number of loans. Borrowers are clustered by risk categories: credit scores above 720 are low risk borrowers; credit scores in the range 640-719 are medium risk; credit scores below 640 but above 520 are high risk. The vertical line marks April 15, 2008, the date when the maximum interest rate allowed was set at 36% in all states. The activity that is shown on April 2008 corresponds to the period that starts on March 15, 2008 and ends on April 14, 2008.

Figure 2: Fraction of Listings Posted Under Various Interest Rate Caps - Before 04/15/2008



This figure shows the distribution of interest rate caps in listings posted before April 15, 2008 and divides these caps among the treatment and control groups.

Table 1: Summary Statistics

	<u>General</u>	<u>Risk Category</u>			
		<u>Total</u>	<u>Low</u>	<u>Medium</u>	<u>High</u>
# Listings		114902	11483	24898	78521
# Loans		9969	2878	3860	3231
<u>Listing Characteristics</u>					
		<u>Mean</u>	<u>Std.</u>	<u>10%</u>	<u>90%</u>
Amount		7417	6380	1800	17000
Open For Duration		0.87	0.34	0	1
Amount Delinquent		3394	14419	0	9030
Current Delinquencies		2.96	4.63	0	9
Delinquencies Last 7 Years		9.91	15.97	0	30
Public Records Last Year		0.07	0.33	0	0
Public Records Last 10 Years		0.60	1.16	0	2
Inquiries Last 6 Month		3.71	4.49	0	9
Bank Card Utilization		0.64	0.42	0	1
Current Credit Lines		8.86	6.28	2	17
Revolving Credit Balance		14359	35360	0	33951
Total Credit Lines		26.82	14.93	9	46
<u>Loan Outcomes</u>					
Pr(Funding)		0.087			
APR		18.56%	8.41%	9.00%	34%
Pr(All First 18 Payments Paid)		0.752			
Pr(18th Payment Paid)		0.938			
# Late Payments		0.907	2.663	0	4
Pr(Default)		0.194			

The table contains summary statistics of listing characteristics and various repayment variables. General variables are the numbers of loans and listings, presented separately for the full sample and for each of the three risk categories. Listing characteristics are variables that are included in the verified section of the listing such as the amount requested by the borrower and the number of delinquencies the borrower suffered in the last seven years. The loan outcome category includes the probability of being funded, a loan APR, and variables summarizing the borrower's repayments in the first 18 payments. Repayment variables include an indicator of whether a borrower paid each of the first 18 payments; an indicator of whether the borrower paid the 18th payment; the number of missing payments in the first 18 payments (censored from above at 4); and the probability of default. The summary statistics for each variable are the mean, the standard deviation, and the 10th and 90th percentiles of its distribution.

Table 2: The Effects of Usury Laws - Basic Evidence

Control						
03/15/2008 - 04/14/2008						
Ceiling Rate 36%						
Risk Category	Num. Listings	Num. Loans	Prob. Funding	Avg. Amount	APR	Prob. Default
Low	468	127	0.271	14891	0.141	0.138
Medium	991	185	0.187	10484	0.201	0.209
High	3132	210	0.067	6029	0.260	0.332

High Intensity Treatment						
03/15/2008 - 04/14/2008						
Ceiling Rate 6%-12%						
Risk Category	Num. Listings	Num. Loans	Prob. Funding	Avg. Amount	APR	Prob. Default
Low	158	31	0.196	10811	0.085	0.000
Medium	249	6	0.024	9816	0.084	0.000
High	986	3	0.003	5598	0.084	0.000

04/15/2008 - 05/14/2008						
Ceiling Rate 36%						
Risk Category	Num. Listings	Num. Loans	Prob. Funding	Avg. Amount	APR	Prob. Default
Low	678	155	0.229	14180	0.146	0.173
Medium	1358	189	0.139	10480	0.197	0.205
High	3496	101	0.029	6022	0.256	0.257

04/15/2008 - 05/14/2008						
Original Ceiling Rate 6%-12% (Now 36%)						
Risk Category	Num. Listings	Num. Loans	Prob. Funding	Avg. Amount	APR	Prob. Default
Low	224	67	0.299	12370	0.141	0.077
Medium	467	86	0.184	9078	0.211	0.244
High	1565	85	0.054	5352	0.287	0.232

The left two panels correspond to the control group (states that experienced no change in their interest rate cap around April 15, 2008). The right panels correspond to the high-intensity treatment group (states subject to interest rate caps in the range of 6-12% before April 15, 2008). The upper panels refer to lending activity on 03/15/2008-04/14/2008 and the lower panels refer to lending activity on 04/15/2008-05/14/2008. Each panel presents the number of listings, number of loans, probability of funding, average amount requested, APR, and the probability of default in the first 18 months.

Table 3: Treatment Effect Estimates on a Listing Funding - Probit Model

Dep. Variable - Funding Indicator, Probit Estimates				
Treatment Intensity	Risk Category	Baseline Pr(Funding)	Marg. Eff.	Std. Err.
Low	Low	0.345	-0.001	(.006)
	Medium	0.210	0.013	(.005)
	High	0.032	0.060	(.008)
Medium	Low	0.263	0.012	(.006)
	Medium	0.126	0.045	(.007)
	High	0.011	0.171	(.014)
High	Low	0.176	0.125	(.023)
	Medium	0.010	0.532	(.054)
	High	0.004	0.256	(.031)
Pseudo R-Square			0.274	
Num. Obs.			114686	

The table contains estimation results from a Probit regression that explores the effect of interest rate restrictions on a dummy variable indicating whether a listing was funded. The set of control variables includes the characteristics of the loan request as well as week and state fixed effects. For each category, the funding probability in the five months prior to the experiment is presented as a benchmark. Marginal effects and standard errors clustered by state and week are presented.

Table 4: Treatment Effect Estimates on the Amount Requested - Tobit Model

Dep. Variable - log(Amount Requested), Tobit Estimates				
Treatment Intensity	Risk Category	Baseline Amount Req.	Marg. Eff.	Std. Err.
Low	Low	13045	-0.0001	(.0003)
	Medium	9715	0.0000	(.0002)
	High	5747	-0.0003	(.0001)
Medium	Low	13438	-0.0001	(.0003)
	Medium	9852	0.0002	(.0001)
	High	5723	0.0000	(.0001)
High	Low	10968	0.0005	(.0003)
	Medium	9166	0.0007	(.0002)
	High	5854	-0.0003	(.0001)
Pseudo R-Square			0.134	
Num. Obs.			114699	

In this Tobit regression the dependent variable is the logarithm of the amount requested. The set of control variables includes the characteristics of the loan request as well as week and state fixed effects. For each category, the average amount requested in the five months prior to the experiment is a benchmark. Marginal effects and standard errors clustered by state and week are presented.

Table 5: Treatment Effect Estimates on the Paid Interest Rates (APR) - Tobit Model

Dep. Variable - APR, Tobit Estimates				
Treatment Intensity	Risk Category	Baseline Avg. APR	Marg. Eff.	Std. Err.
Low	Low	0.115	0.009	(.003)
	Medium	0.164	0.005	(.002)
	High	0.200	-0.003	(.003)
Medium	Low	0.103	0.008	(.002)
	Medium	0.138	0.005	(.002)
	High	0.161	-0.032	(.008)
High	Low	0.082	-0.007	(.004)
Pseudo R-Square			0.276	
Num. Obs.			30764	

In this Tobit regression the dependent variable is the APR. If a listing was not funded, I use its interest rate cap as a lower bound on the APR. The sample is restricted to loan requests that are predicted to get funded with a probability greater than 0.1. The set of control variables includes the characteristics of the loan request as well as week and state fixed effects. For each category, the average APR of loans originated over the five months prior to the experiment is presented as a benchmark. Marginal effects and standard errors clustered by state and week are presented.

Table 6: Treatment Effect Estimates on the Default Probability

Dep. Variable - Default Indicator, OLS Estimates				
Treatment Intensity	Risk Category	Baseline Pr(Default)	Coef.	Std. Err.
Low	Low	0.115	0.021	(.033)
	Medium	0.168	0.027	(.034)
	High	0.204	0.080	(.047)
Medium	Low	0.108	-0.020	(.028)
	Medium	0.147	0.037	(.032)
	High	0.150	0.093	(.05)
High	Low	0.028	0.028	(.04)
	Medium	0.000	0.171	(.063)
	High	0.400	-0.050	(.11)
R-Square			0.0997	
Num. Obs.			9850	

The table contains the coefficients estimated in a linear probability model in which the dependent variable indicates a default loan. The set of control variables includes the characteristics of the loan request, week and state fixed effects and the predicted funding probability. For each category, the default probability within the first 18 months after origination for loans made over the five months prior to the experiment is the benchmark. Standard errors in parenthesis are clustered by state and week. I use the Murphy-Topel adjustment to account for the fact that the funding probability, a generated regressor, is included as a regressor.

Table 7: Linear Treatment Effect Estimates for a 10% Cap Increase

Dep. Var:	(1)			(2)			(3)			(4)		
	Funding Indicator	log(Amount Requested)	APR	Default Indicator								
Risk Category	Marg. Eff.	Std. Err.	Marg. Eff.	Std. Err.	Marg. Eff.	Std. Err.	Marg. Eff.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
Low	0.014	(.002)	0.014	(.022)	0.002	(.001)	-0.122	(.14)				
Medium	0.026	(.002)	0.036	(.014)	0.000	(.001)	0.268	(.18)				
High	0.039	(.002)	-0.003	(.012)	-0.009	(.002)	0.309	(.24)				
Pseudo R-Square	0.270		0.136		0.274		0.098					
Num. Obs.	114686		114699		30899		9850					

The treatment effects shown here are restricted to be linear within each risk category, in contrast to the previous regressions presented where the effects were restricted to be constant within each treatment intensity category. The coefficients correspond to an increase of 10% in the interest rate restriction: for example, a 10% increase in the interest rate cap is expected to increase the funding probability of low-risk borrowers by 1.4%. Regression (1) is a Probit regression; regressions (2)-(3) are Tobit regressions; regression (4) is a linear regression.

Appendix A

Data Appendix

A.1 Verified Variables

Below I describe the variables that are included in the borrower's credit report and provided by Prosper.

- Auction Open For Duration - A dummy that is equal to 1 if the borrower chooses to end the auction when its duration ends and not before
- Home Owner - A dummy that is equal to 1 if the borrower is a home owner
- Amount Delinquent - The monetary amount delinquent
- Current Delinquencies - Number of accounts in which the borrower is currently late on payment
- Delinquencies in Last 7 Years - Number of 90+ days delinquencies in the last 7 years
- Public Records Last Year - Number of negative public records in the borrower's credit report in the last 12 months
- Public Records Last 10 Years - Number of negative public records in the borrower's credit report in the last 10 years
- Inquiries Last 6 Months - Number of inquiries made by creditors to view the borrower's credit report in the last 6 months
- Bank Card Utilization - The percentage of available revolving credit that is utilized
- Current of Credit Lines in Last 6 Months - Number of reported credit lines in the last 6 months

- Revolving Credit Balance - Sum of balance on all open revolving credit lines in the last 6 months
- Total Credit Lines - The total number of credit lines appearing in the credit report

A.2 Variables Relaxed to Account for Non-Linear Effects

In estimating the treatment effects presented in Tables 3-7, I allow for non-linear relations of several of the verified financial information variables presented in Appendix A.1. In such cases, the slope depends on the value of the variable. The thresholds used to allow for non-linear relations are based on the variables' median or quartiles values. Specifically, the variables and the thresholds used are:

- Amount Requested - with thresholds at \$3000, \$5000 and \$10000
- Current Delinquencies - with a threshold at 1 current delinquency
- Delinquencies in Last 7 Years - with a threshold at 3 delinquencies
- Inquiries Last 6 Months - with a threshold at 2 recent inquiries
- Bank Card Utilization - with a threshold at a utilization ratio of 0.75
- Current of Credit Lines in Last 6 Months - with thresholds at 4, 8 and 12 current credit lines

A.3 Non-Verified Variables

Below I provide a partial list of variables that are self-reported by borrowers. These variables are used as control variables in the regressions presented in Tables 3-7. The average values of these variables in the full sample and in each borrower's risk category are presented in Table A.1.

- Dummy variables for the inclusion of each of the following words/phrases in the listings' title
 - help, credit card, debt, consolidate, start business, real estate, student, school, tuition, medical, doctor, fresh start, good guy

- Dummy variables for reported income within the following income ranges - up to \$25K, \$25K – \$50K, \$50K – \$75K, \$75K – \$100K and \$100K+
- Dummy variables for each of the following employment statuses - not employed, retired, part time, self employed and full time

Table A.1: Summary Statistics - Borrower's Self-Reported Information

	Risk Category			
	Total	Low	Medium	High
<u>Key Word Appears in the Listing's Title</u>				
Help	0.090	0.047	0.068	0.103
Credit Card	0.200	0.194	0.245	0.186
Debt	0.151	0.112	0.144	0.159
Consolidate	0.009	0.008	0.008	0.009
Start Business	0.031	0.022	0.036	0.031
Real Estate	0.026	0.013	0.020	0.030
Student	0.002	0.001	0.002	0.002
School	0.016	0.015	0.019	0.015
Tuition	0.005	0.012	0.007	0.003
Medical	0.019	0.016	0.016	0.020
Doctor	0.008	0.002	0.004	0.010
Fresh	0.006	0.004	0.005	0.006
Good Guy	0.0001	0.0002	0.0000	0.0001
<u>Loan Category</u>				
Debt Consolidation	0.393	0.315	0.415	0.397
Home Improvement Loan	0.033	0.063	0.043	0.026
Business Loan	0.131	0.257	0.185	0.096
Personal Loan	0.179	0.151	0.146	0.194
Student Loan	0.031	0.031	0.026	0.032
Auto Loan	0.021	0.024	0.018	0.022
Other	0.073	0.085	0.063	0.075
<u>Income</u>				
\$1-24,999	0.143	0.078	0.103	0.165
\$25,000-49,999	0.398	0.265	0.327	0.440
\$50,000-74,999	0.224	0.223	0.246	0.217
\$75,000-99,999	0.085	0.132	0.116	0.069
\$100,000+	0.070	0.168	0.111	0.043
<u>Employment</u>				
Full Time	0.822	0.743	0.802	0.840
Not Employed	0.020	0.020	0.019	0.020
Part Time	0.035	0.030	0.030	0.038
Retired	0.031	0.033	0.033	0.030
Self-Employed	0.092	0.173	0.117	0.072

The table contains summary statistics for the unverified variables provided by borrowers that are used in the analysis. I report the mean values for the full sample and for the three risk categories. The variables are divided into four groups. In the first group, a variable gets the value 1 if the key word is contained within the listing's title. The second group contains indicators for the purpose of the loan. The third group indicates the range of the borrower's annual income and the last group indicates the borrower's employment status.

Appendix B

Analysis Appendix

The tables included in this appendix present the estimated effects when the treatment effects are allowed to be heterogenous in the borrower's seven credit grade letters. This analysis represents a greater degree of heterogeneity relative to the specification used in the paper, where the treatment effects are allowed to be heterogenous in the borrower's three risk categories. Interestingly, as reflected in the tables below, allowing for a greater degree of heterogeneity does not provide additional insights.

Table B.1: Credit Grade Heterogenous Treatment Effect Estimates on a Listing Funding - Probit Model

Dep. Variable - Funding Indicator, Probit Estimates	Five Months Before		(1)	
	Num. Listings	Pr(Funding)	dF/dx	z-stat.
After*Low Intensity Treat.*Grade AA	270	0.415	0.000	-(0.06)
After*Low Intensity Treat.*Grade A	335	0.290	0.000	-(0.07)
After*Low Intensity Treat.*Grade B	496	0.300	0.002	(0.31)
After*Low Intensity Treat.*Grade C	1117	0.170	0.017	(3.09)
After*Low Intensity Treat.*Grade D	1332	0.093	0.036	(5.41)
After*Low Intensity Treat.*Grade E	1644	0.027	0.106	(7.46)
After*Low Intensity Treat.*Grade HR	2944	0.008	0.083	(6.12)
After*Med. Intensity Treat.*Grade AA	583	0.298	0.026	(2.82)
After*Med. Intensity Treat.*Grade A	726	0.234	0.001	(0.18)
After*Med. Intensity Treat.*Grade B	1206	0.166	0.040	(5.32)
After*Med. Intensity Treat.*Grade C	1881	0.101	0.051	(7.67)
After*Med. Intensity Treat.*Grade D	2575	0.025	0.177	(13.87)
After*Med. Intensity Treat.*Grade E	2520	0.007	0.293	(12.07)
After*Med. Intensity Treat.*Grade HR	5410	0.006	0.147	(10.21)
After*High Intensity Treat.*Grade AA	254	0.299	0.073	(4.48)
After*High Intensity Treat.*Grade A	382	0.094	0.190	(8.53)
After*High Intensity Treat.*Grade B	374	0.016	0.581	(10.65)
After*High Intensity Treat.*Grade C	741	0.007	0.515	(12.34)
After*High Intensity Treat.*Grade D	957	0.007	0.322	(8.74)
After*High Intensity Treat.*Grade E	1054	0.005	0.295	(7.76)
After*High Intensity Treat.*Grade HR	2526	0.002	0.191	(7.97)
Pseudo R-Square			0.298	
Num. Obs.			114686	

The table contains estimation result from a Probit regression that explores the effect of interest rate restrictions on a listing's funding probability. The table is an extension of Table 3 in the sense that the estimated treatment effects are now heterogenous in the borrower's credit grade letter. The set of control variables includes the characteristics of the loan request as well as week and state fixed effects. For each category, the funding probability in the five months prior to the experiment is presented as a benchmark. Marginal effects and standard errors clustered by state and week are presented.

Table B.2: Credit Grade Heterogenous Treatment Effects Estimate on the Amount Requested - Tobit Model

Dep. Variable - log(Amount Requested) Tobit Estimates				
	Five Month Before			
	April 15, 2008			(1)
	Num. Listings	Average Amount Req.	dF/dx	z-stat.
After*Low Intensity Treat.*Grade AA	270	13640	0.0000	(.07)
After*Low Intensity Treat.*Grade A	335	12566	-0.0003	-(.76)
After*Low Intensity Treat.*Grade B	496	11728	-0.0004	-(1.39)
After*Low Intensity Treat.*Grade C	1117	8821	0.0002	(.8)
After*Low Intensity Treat.*Grade D	1332	7176	-0.0001	-(.56)
After*Low Intensity Treat.*Grade E	1644	5377	-0.0006	-(3.14)
After*Low Intensity Treat.*Grade HR	2944	5306	-0.0003	-(1.83)
After*Med. Intensity Treat.*Grade AA	583	13461	-0.0001	-(.2)
After*Med. Intensity Treat.*Grade A	726	13419	-0.0001	-(.28)
After*Med. Intensity Treat.*Grade B	1206	11224	0.0005	(2.19)
After*Med. Intensity Treat.*Grade C	1881	8972	0.0002	(1.11)
After*Med. Intensity Treat.*Grade D	2575	7588	0.0000	(.23)
After*Med. Intensity Treat.*Grade E	2520	5637	-0.0007	-(4.01)
After*Med. Intensity Treat.*Grade HR	5410	4875	0.0003	(1.65)
After*High Intensity Treat.*Grade AA	254	11354	0.0009	(1.85)
After*High Intensity Treat.*Grade A	382	10712	0.0003	(.77)
After*High Intensity Treat.*Grade B	374	10311	0.0012	(3.92)
After*High Intensity Treat.*Grade C	741	8588	0.0005	(2.1)
After*High Intensity Treat.*Grade D	957	7322	0.0000	-(.17)
After*High Intensity Treat.*Grade E	1054	5760	-0.0005	-(2.31)
After*High Intensity Treat.*Grade HR	2526	5337	-0.0003	-(1.93)
Pseudo R-Square				0.147
Num. Obs.				114699

The table contains the coefficients estimated in the Tobit regressions in which the dependent variable is the logarithm of the amount requested. The table is an extension of Table 4 in the sense that the estimated treatment effects are now heterogenous in the borrower's credit grade letter. The set of control variables includes the characteristics of the loan request as well as week and state fixed effects. For each category, the average amount requested in the five months prior to the experiment is a benchmark. Marginal effects and standard errors clustered by state and week are presented.

Table B.3: Credit Grade Heterogenous Treatment Effect Estimate on the Paid Interest Rates (APR) - Tobit Model

Dep. Variable - APR, Tobit Estimates	Five Months Before April 15, 2008		(1)	
	Num Loans	Avg. APR	dF/dx	z-stat.
After*Low Intensity Treat.*Grade AA	113	0.098	0.008	(1.89)
After*Low Intensity Treat.*Grade A	107	0.133	0.010	(2.75)
After*Low Intensity Treat.*Grade B	155	0.156	0.007	(2.54)
After*Low Intensity Treat.*Grade C	197	0.171	0.005	(2.09)
After*Low Intensity Treat.*Grade D	113	0.197	0.004	(1.43)
After*Low Intensity Treat.*Grade E	19	0.216	0.003	(.51)
After*Low Intensity Treat.*Grade HR	3	0.213	0.006	(.52)
After*Med. Intensity Treat.*Grade AA	184	0.091	0.003	(.81)
After*Med. Intensity Treat.*Grade A	173	0.115	0.013	(4.7)
After*Med. Intensity Treat.*Grade B	189	0.140	0.005	(1.91)
After*Med. Intensity Treat.*Grade C	163	0.135	0.008	(3.51)
After*Med. Intensity Treat.*Grade D	22	0.161	-0.011	-(2.37)
After*Med. Intensity Treat.*Grade HR	2	0.162	0.016	(1.36)
After*High Intensity Treat.*Grade AA	77	0.079	0.003	(.59)
After*High Intensity Treat.*Grade A	26	0.091	-0.006	-(1.26)
After*High Intensity Treat.*Grade C	1	0.110	-0.005	-(.25)
Pseudo R-Square			0.311	
Num. Obs.			41803	

The table contains the marginal effects found in the Tobit regressions in which the dependent variable is the APR. The table is an extension of Table 5 in the sense that the estimated treatment effects are now heterogenous in the borrower's credit grade letter. The sample is restricted to loan requests that are predicted to funded with a probability greater than 0.1. The set of control variables includes the characteristics of the loan request as well as week and state fixed effects. For each category, the average APR of loans originated over the five months prior to the experiment is presented as a benchmark. Marginal effects and standard errors clustered by state and week are presented.

Table B.4: Credit Grade Heterogenous Treatment Effect Estimates on Repayment Variables

Dependent Variable -	All First 18 Payments were Paid		18th Payment was Paid		# of Late Payments		Default	
	OLS Regression (1)	Std. Err.	OLS Regression (2)	Std. Err.	dF/dX (3)	z-stat.	OLS Regression (4)	Std. Err.
After*Low Intensity Treat.*Grade AA	0.052	(.049)	0.011	(.032)	-0.227	-(1.41)	-0.031	(.042)
After*Low Intensity Treat.*Grade A	-0.071	(.056)	-0.008	(.028)	0.346	(1.03)	0.071	(.051)
After*Low Intensity Treat.*Grade B	0.034	(.053)	0.041	(.032)	-0.138	(-.94)	-0.008	(.049)
After*Low Intensity Treat.*Grade C	-0.034	(.05)	0.044	(.032)	0.068	(.41)	0.045	(.044)
After*Low Intensity Treat.*Grade D	-0.002	(.064)	0.033	(.038)	-0.017	(-.1)	0.014	(.061)
After*Low Intensity Treat.*Grade E	-0.235	(.095)	-0.052	(.054)	0.771	(1.31)	0.201	(.091)
After*Low Intensity Treat.*Grade HR	-0.123	(.133)	-0.057	(.069)	0.158	(.47)	0.068	(.123)
After*Med. Intensity Treat.*Grade AA	0.101	(.042)	0.034	(.028)	-0.437	(-5.6)	-0.078	(.036)
After*Med. Intensity Treat.*Grade A	-0.014	(.047)	0.018	(.027)	0.166	(.69)	0.041	(.044)
After*Med. Intensity Treat.*Grade B	-0.022	(.051)	0.012	(.031)	-0.031	(-.2)	0.027	(.047)
After*Med. Intensity Treat.*Grade C	-0.018	(.048)	0.004	(.029)	0.060	(.35)	0.033	(.042)
After*Med. Intensity Treat.*Grade D	-0.085	(.068)	-0.056	(.031)	0.125	(.46)	0.040	(.063)
After*Med. Intensity Treat.*Grade E	-0.077	(.119)	0.040	(.1)	0.217	(.43)	0.113	(.107)
After*Med. Intensity Treat.*Grade HR	-0.253	(.095)	-0.064	(.052)	0.961	(1.33)	0.156	(.087)
After*High Intensity Treat.*Grade AA	0.033	(.046)	-0.018	(.026)	-0.394	(-2.94)	-0.056	(.043)
After*High Intensity Treat.*Grade A	-0.067	(.074)	-0.005	(.045)	1.146	(.69)	0.073	(.065)
After*High Intensity Treat.*Grade B	-0.196	(.086)	-0.082	(.046)	0.203	(1.04)	0.122	(.078)
After*High Intensity Treat.*Grade C	-0.225	(.08)	-0.090	(.042)	0.335	(1.71)	0.158	(.075)
After*High Intensity Treat.*Grade D	0.201	(.23)	0.260	(.209)	-0.440	(-2.5)	-0.279	(.214)
After*High Intensity Treat.*Grade E	-0.183	(.207)	0.146	(.224)	0.429	(.54)	0.096	(.188)
After*High Intensity Treat.*Grade HR	-0.055	(.165)	-0.023	(.09)	-0.182	(-.72)	-0.015	(.172)
Pseudo R-Square	0.1143		0.0285		0.093		0.1058	
Num. Obs.	9850		9850		9850		9850	

The table contains the coefficients estimated in regressions focused on loan repayment. The dependent variables in the first and the second columns indicate if the borrower paid the first 18 payments, and whether he paid the 18th payment, respectively. The dependent variable in the third column is the number of late payments taking the values 0,...,4. A loan is assigned the value 4 if the borrower is 4 or more payments behind. The dependent variable in the last column is a default indicator. A borrower is defined as having defaulted on a loan if he is more than 4 months behind in payments. A linear probability model is used whenever the dependent variable is a binary variable (columns 1,2 and 4). A Poisson model is used when the dependent variable is the number of late payments. Column 4 is an extension of Table 6 in the sense that the estimated treatment effects are now heterogenous in the borrower's credit grade letter. Standard errors in parenthesis are clustered by state and week, and are adjusted using the Murphy-Topel adjustment to account for the fact that the funding probability, a generated regressor, is included as a regressor.