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

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**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 the probability that a loan is funded, the amount a borrower requests, the interest rate at which a loan is funded, and the probability of default. The key to my empirical strategy is that there was initially substantial variability in states' interest rate caps. Prosper borrowers from different states faced different caps ranging from 6 to 36%. A behind-the-scenes change in loan origination suddenly increased the cap to 36% in all but one state. This change, which was not pre-announced and which changed nothing else for lenders and borrowers, created "treatment" states whose caps rose and few control states whose caps remained unchanged. I find that higher interest rate caps increased the probability that a loan was funded, especially if its

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borrower was risky and had been previously just "outside the money." I do not find that borrowers change the loan amounts that they request or that their probability of default rises. The interest rate paid for all loans, however, rises slightly probably because online lending is imperfectly integrated with credit markets.

# 1 Introduction

Legislated caps on interest rates, known as usury laws, are one of the oldest forms of market regulation known to exist. Usury laws inspire great debate, as do other forms of government intervention in financial markets. Opponents argue that interest rate caps exclude higher risk borrowers from being able to obtain credit or develop a credit history. Proponents argue that caps reduce the interest rates a given borrower pays because lenders have market power. Proponents also argue that interest rate caps prevent naive borrowers from agreeing to loan terms on which they will eventually be forced to default. This paper empirically examines each of these arguments using a sudden and unforeseen shift in the interest rate caps affecting online borrowers. This paper's main advantage is clean identification of the effects of usury laws, facilitated by an exogenous change that allows me to rule out confounding phenomena. In addition, I not only use data that are very detailed, but I also observe virtually all the information observed by lenders. This allows me to eliminate selection problems that have plagued previous work on usury laws.

Economic theory suggests that interest rate caps may affect credit markets through various channels. First, higher caps should make lending to higher risk borrowers profitable. That is, higher caps may result in credit being extended to borrowers who were previously denied credit. Second, because the riskiness of a loan depends on its size and not just the identity of the borrower, higher caps may cause a given borrower to request larger loans. Third, higher caps may increase the probability that borrowers default on loans, particularly if the caps were "protecting naive borrowers from themselves"–that is, preventing borrowers from agreeing to loan terms that they could not manage financially. Finally, theory suggests a couple of routes by which interest rate caps could reduce the price paid for any given loan. If lenders have market power, then usury laws could decrease the interest rates charged by 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 be in inelastic supply. If they are, then the price of credit will rise with the cap simply because the greater number of borrowers who request loans under a higher cap will drive up demand for credit.

I use new data from Prosper.com, the largest online person-to-person loan marketplace in the U.S. I observe full details on the universe of Prosper's loan requests, loan originations, and loan repayments. 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, and state's caps varied between 6 and 36%. On April 15, 2008, however, a formal change in Prosper's loan origination suddenly changed its borrowers' interest rate cap to 36% in all states but one. In other words, borrowers in most states faced sudden increases in their interest rate cap, and borrowers in a few states faced no change. (In addition to the one state whose low cap did not change in April 2008, there were a few states that already had caps of 36%.)

Given the experiment just described, the natural empirical strategy is differences-in-differences. That is, I simultaneously compare across time (before and after April 15, 2008) and across states (treated and control states). The strategy is richer than one may think at first glance, however, because the states' initial conditions varied. Some states' borrowers saw their cap rise by only 11%; others saw their cap rise by as much as 30%. I exploit this variation to estimate non-linear effects of a percentage point change in the interest rate cap. Moreover, we expect the effect of an interest rate cap to depend on a borrower's riskiness. I estimate fully heterogeneous effects–specifically, I allow the effect to vary with a borrower's credit grade.

I empirically examine each of the theoretical predictions described above. My first cut at the data is a very simple comparison of outcomes before and after April 2008. For instance, I investigate whether a high risk borrower's loan request is more likely to be funded after his rate cap rises to 36%. Following this simple but fairly revealing analysis, I exploit the full richness of the data in order to test the predictions while accounting for endogeneity and selection (see below). 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. Interestingly, I find that the largest increase in funding probability is experienced by borrowers who were previously just "outside the money" in their state. To address concerns that the amount requested by a borrower might be endogenous to the interest rate cap, I investigate whether borrowers request larger loans following the increase in the cap. I find that they do not. I also find that, for a given type of borrower and loan amount, defaults do not rise following the increase in interest rate caps. Finally, I analyze whether a given borrower pays a higher price for credit when his interest rate cap rises. I find zero to small increases (no more than 50 basis points) in interest rates paid.

Recall that there are two theoretical reasons why interest rates might rise–in this case, very slightly–with interest rate caps. I look for direct evidence of the second explanation: an inelastic supply of lenders. For instance, I investigate whether rates go up for low risk borrowers in states that initially had 36% caps–borrowers who would be unaffected by the April 2008 change if the supply of credit were perfectly elastic. My results suggest that the supply of lenders on Prosper is slightly inelastic. Since Prosper's borrowers do not constitute a large share of total U.S. borrowing, these findings suggest that Prosper is imperfectly integrated into credit markets. This is not altogether surprising because it operates online and most of its lenders are individuals rather than institutional investors.

Selection problems have plagued previous studies of usury laws. Specifically, if an individual's decision to apply for a loan depends on his expectation that the loan will be funded, and if the probability that a loan is funded depends on the maximum interest rate he is allowed to pay, then the composition of individuals applying for loans changes when the cap changes. Estimates of the effects of interest rate caps are, therefore, often confounded with the changing composition of borrowers (selection).

I am able to control much better for selection than previous authors because of a unique feature of Prosper's data. Because the marketplace is online and virtually everything about a loan that appears online is recorded in the data, I observe essentially all the information that potential lenders observe. (The only information I do not observe is information that might have been exchanged in Prosper-related chatrooms.) Thus, selection of borrowers on unobservables is–at most–a very minor issue. So long as I control sufficiently flexibly for observable characteristics of borrowers and their loan requests, selection should not be a problem. This is in contrast to previous studies in which loans were typically originated through in-person interviews of applicants, in which loan officers could observe a great deal of information that the econometrician missed.

The rich and numerous Prosper loan data allow me to fully exercise the power of a differences-in-differences strategy. I control for time effects that are constant across states, state effects that are constant across time, credit grade effects, state-by-credit grade effects, time-by-credit grade effects, and so on through all the "two-way" effects.

The remainder of the paper is organized as follows. Section 2 provides background information on usury laws, reviews the related literature and introduces Prosper's natural experiment. Section 3 describes the data. I motivate and describe my empirical strategy in section 4. Basic evidence from a simple first cut of the data is presented in Section 5. Section 6 presents regression-based analysis in which I account for selection and endogeneity. I conclude in section 7.

# 2 Background

## 2.1 The Origins of Usury Laws and Their Evolution in the American Legal System[1]

The earliest references to usury laws are in Hammurabi's code of 1800 B.C.(ancient Mesopotamia) and the ban on interest rates which shows up three times in the five books of Moses. In both Exodus and the book of Luke, strong limitations on the rates that lenders can charge are advocated.[2] In the modern era, usury laws are one of the only topics on which Adam Smith was skeptical of a "laissez-faire" solution. In a famous exchange with the philosopher Jeremy Bentham, Smith refused to rule out the necessity of legal ceilings on interest rates.[3] Usury laws made their way into the American legal system through the American colonies, which adopted existing English usury statutes.[4] The proliferation of loan-sharking in the 19th century stimulated the evolution of lending institutions such as credit unions that were intended to be non-predatory. Credit unions are often based on a common employer, church, or social organization. However, such institutions left many borrowers without credit and policy makers sought a way to make consumer lending profitable without being predatory.

The Uniform Small Loan Law, enacted in 1916, was intended to address the problem. The law created a class of lenders authorized to charge rates significantly in excess of the general usury caps in return for the lenders' agreement to be regulated.[5] Each usury law was

[1]The description of the evolution of usury laws in the American legal system is based on Renuart and Keest (2005), section 3.2.

[2]For historical reviews of usury laws see Frierson (1969), Homer and Sylla (2005), Shanks (1967) and Glaeser and Scheinkman (1998).

[3]The letters written by Bentham were published in Bentham (1818). For a detailed description of the correspondence as well as early philosophers' thoughts on usury see Persky (2007).

[4]See section 1 in Masciandaro (2001) for a review of usury laws in Europe.

[5]For example, the regulation imposed on consumer credit sales was mainly through restrictions on finance

aimed at a particular type of creditor, a particular type of transaction, or some combination of the two.[6] However, as the number of special credit laws in each state became unwieldy, attempts were made to consolidate the laws into simplified statutes.[7]

During the 1970s, inflation drove commercial market rates above 20%, rates significantly higher than most usury ceilings. Fearing that consumer credit would dry up, the federal government preempted many states' usury limitations, subject to the proviso that a state could opt out of the federal preemption if it enacted legislation proclaiming that the state did not want the federal preemption to apply to it. The federal action thus forced states to reconsider their often complex sets of general and special statutes. In the 1980s, states followed one of several paths. Some states repealed their general usury caps entirely, allowing parties who were not regulated by special usury laws to contract on any agreed rate. Other states modified their general usury laws so that caps would fluctuate with a published market interest rate. Most states, however, simply raised their interest caps to a point that did not bind traditional lenders. In the last decade, most legal activity can be described as a trench warfare between state and city-level attempts to curb what they believe is predatory lending and federal attempts to preempt this activity. The rapid growth of the payday loan industry in the past 15 years is an example for how usury laws are constantly challenged or legally evaded. Written with short terms and for occasional use, payday loans are exempted from many state and local usury laws. Usury laws are now being extended even to payday loans: 17 states have recently prohibited them outright or effectively banned them through interest rate caps.[8]

## 2.2 The Debate

The debate over government constraints on the maximum price of credit has attracted the attention of philosophers, legal scholars and economists, and they have made a number of arguments. Most arguments in favor of usury laws are focused on consumer protection. One

charges.

[6]The types of creditors that were distinguished by the law are depository lenders such as banks and credit unions, and non-depository lenders such as licensed lenders or retail sellers. The types of credit that are discerned by the law are loan vs. credit sales, open end vs. closed-end credit (e.g. credit for an auto sale transaction vs. bank credit card) and secured vs. unsecured credit. See Pridgen (2003) for a practitioner guide that exemplifies the variety of usury laws.

[7]The most recognized attempt at consolidating usury laws is the Uniform Consumer Credit Code originally approved in 1968. Ten states have adopted forms of it, whereas other states have adopted portions of it.

[8]See Stegman and Faris (2003).

argument is that usury laws limit the market power of lenders. People often express this argument informally when they say that usury laws provide borrowers credit at a "fair" price.[9] Another argument is paternalistic: usury laws prevent naive borrowers from entering into loan contracts that reduce their utility under rational expectations, even though they did not expect it themselves.[10]

Opponents of usury laws argue that the reason some borrowers face high rates is not market power but their risk: lenders will not lend if it is unprofitable. In other words, these opponents argue that interest rate caps inefficiently exclude some borrowers from getting credit. Being deprived of credit not only has immediate effects on the borrower, it also affects his ability to build a credit history, which is essential to his being able to borrow at lower rates in the future. Opponents of usury laws also argue that borrowers who are denied credit from legal lenders resort to illegal loan sharks or other dubious means of obtaining liquidity. These means may have far worse consequences than a conventional loan with an interest rate that a consumer advocate would describe as "high."[11]

### 2.3 Previous Empirical Evidence

The longstanding debate and the substantial variability in interest rate caps across states have generated a large body of research on the effects of usury laws. Most of the literature dates from the 1970s and early 1980s when, partly because of inflation, there was significant popular dissatisfaction with usury laws. In early work, Blitz and Long (1965) apply economic theory to usury regulation and generate testable predictions. Building on their analysis, Goudzwaard (1968) and Shay (1970) use aggregate data on 32 states to study the correlation between interest rate caps and the risk accepted by consumer finance companies. In 1971, the National Commission on Consumer Finance collected data from a large national lender. Greer(1974, 1975) uses that data to study the effect of caps on the risk undertaken by lenders, lending activity, and rejection rates. Greer focuses on how the cap affects aggregate indicators, not individual loan requests. Villegas(1982, 1989) employs individual-level data from the Consumer

[9]E.g. Brown (1992). This argument was supported by the view that when lenders have market power, usury laws redress the unequal bargaining power between borrowers and lenders; for example, see Durkin (1992), Hayeck (1996) and Rougeau (1996).

[10]See Wallace (1976).

[11]See Finchler (1993), Oeltjen (1975) and Waterman (1979).

Expenditure Survey from 1972 and 1983 to study the correlations between interest rate caps, the price of credit, and access to credit. Villegas accounts for truncation in the dependent variable using a tobit model, which is an improvement over previous studies, but his data do not contain sufficient variation to allow him to separate the effect of the cap from the potentially confounding effect of the risk associated with the borrower's location. In addition, his data are insufficient rich to account for selection of borrowers into the sample. Alessie et al. (2005) employ a rich individual-level data set covering loan requests received by a large Italian provider of consumer credit in 1995 through 1999. They analyze the imposition of a single interest rate cap in Italy in 1997. Compared to my study, they can control much less well for time effects (since the cap changes identically for all borrowers at a point in time) and can control less well for selection (since they have only some of the information lenders observe).

Results from the previous literature nearly always indicate that lower rate caps reduce the amount of credit given. Effects on other outcomes, such as the amount requested or the rate at which a loan is funded, are less unanimous.

### 2.4 Prosper.com

Prosper is an online platform for lenders and borrowers to interact and originate fixed rate, unsecured consumer loans of $1,000 to $25,000. Loans are originated through a uniform price auction. In order to borrow money a borrower posts a listing in which he indicates the amount requested and the maximum interest rate he is willing to pay subject to the usury limitations. Prosper adds objective financial information to the listing that it gathers from credit bureaus: the borrower's credit grade letter, delinquencies, public records, and credit line history. In addition, the borrower is asked to indicate his intended use of the loan and to describe his financial situation. Borrowers can also post a photograph.

Lenders may bid on portions of the listing. A lender's bid consists of an amount that is greater than $50 and the minimum interest rate the lender agrees to be paid for the loan. Lenders can browse listings manually or use pre-defined or customized portfolio plans. A portfolio plan is an automatic bidding tool that bids on listings that match a specific bidding criteria.[12] A typical bidding criteria is a function of the verified financial information from credit reporting agencies that is provided by Prosper. A loan is originated only if it is fully funded by lenders. If prospective lenders' bids exceed the size of the loan request, then the bids with the lowest interest rates win, and the interest rate paid by the borrower is the highest interest rate among the winning bids. Loans are fully amortized in monthly payments over three years. Borrowers who are late face late fees. A loan that is more than one month late is turned over to a collection agency and is sold to a debt buyer after three months. If a borrower's loan is sold to a debt buyer, he is suspended from Prosper and his credit score is penalized substantially.

When Prosper began, it was formally the finance lender that issues the loans, and it must comply with each state's small loan usury laws.[13] However, starting on April 15, 2008, Prosper began collaborating with a national bank that took over the formal role as the issuing finance lender. This "back office" change is invisible to Prosper's lenders and borrowers. Despite its invisibility, the collaboration allows 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. It should be emphasized that Prosper did not advertise the collaboration in any way in advance. Prosper simply informed its borrowers and lenders in an email and updated its blog with a relevant post.[14]

Despite Prosper's short history, it has already received scholarly attention. Freedman and Jin (2008) investigate the firm's conduct an in-depth investigation of the business model employed by Prosper. They study the marketplace and its user interface. They predict how the online marketplace will evolve as an alternative to traditional consumer credit markets. Other researchers exploit the fact that Prosper borrowers are allowed to post photographs with their listings. Pope and Sydnor (2008) and Ravina (2008) investigate whether lenders

[12] The use of automatic bidding engine has been introduced into the web site on October 30, 2007. In its original version, lenders could pick one of several pre-defined portfolio plans. The bidding engine has been upgraded on February 23, 2008 allowing lenders to define their own plans. Prosper declines to provide the penetration rate of portfolio plans. Yet, anecdotal evidence suggests that more than 50% of the bids are generated through automatic engines.

[13] For a full description of Prosper's regulatory status see Prosper's S1 Form at *http://www.sec.gov/Archives/edgar/data/*1416265/000110465907078072/*a*07 − 27421_1*s*1.*htm*.

[14] See *http://blog.prosper.com/*2008/04/15/*site − update −* %*e*2%80%93 *− april −* 15 − 2008/ for the post. Prosper did not release a press release as at that time it was already available nationwide.

discriminate on the basis of race, gender, or physical attractiveness (using the photographs to classify borrowers on these characteristics). They find evidence of discrimination against racial minorities, against the overweight, and in favor of women, especially those whose appearance is rated above average.

# 3 Data

The data I use include information on listings, loans, loan repayments and marketplace participants. Most importantly, almost all the information that is presented in the web site and that can be used by participants is available.[15] When a borrower or a lender registers its web site, Prosper verifies his identity using his social security number and bank account information. It then obtains his credit reports from the credit bureaus. The data available on each listing includes the verified financial information that Prosper obtains and some non-verified information that the borrower supplies. The borrower must indicate the amount he is requesting and the maximum interest rate he is willing to pay. This rate must be below the rate cap that is legal, given his state of residence. 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.[16] The unverified information includes the borrower's purpose for the loan, employment status, income, expenditure report, and photograph (if he wishes to post one).[17] I also use the interest rate the borrower agrees to pay, his repayments by month, and the interest rate cap that applies to his loan. While most states have a fixed cap, others let their cap move with the federal funds rate, and a few condition the cap on the amount or purpose of the loan.[18] In order to eliminate selection on

[15]Questions and answers between lenders and borrowers are the only pieces of information that are included in the listing's web page and that are not available. A borrower can choose to post the question and his answer in the listing's web page. An additional source of information that was easily accessible to participants is a forum that was integrated into the web site until November 2007. The forum was created to facilitate information and experience sharing between borrowers and lenders. Since then, the content of the forum was deleted and the permission to upload new postings was severely restricted. Discussions from the old forum are not available. Since the data I use starts at the end of October 2007, the deletion of the forums is not a concern.

[16]See the Appendix for a detailed description of the variables included in the analysis.

[17]Upon loan origination, Prosper verifies the correctness of some of the non-verified information for a small fraction of the borrowers.

[18]For example, the interest rate restriction in California is 19.2% for loans up to $2550, and 36% for loans in the range $2550-$25000. In Texas there is a limitation of 18% on business loans and 10% on loans intended for other purposes. In Arkansas the interest rate cap set at 6% higher than the federal funds rate.

the observables, I incorporate all verified information in the analysis and maximum feasible amount of the self-reported information.[19] I not only use all of the verified variables and borrower reported variables, mainly in categorical form for maximum flexibility. I also include indicators for words and phrases that commonly appear in borrowers' statements about the purpose for their loan.

During its start-up period, Prosper made several changes to its user interface and the information provided in each listing's web page. Freedman and Jin (2008) document that the generated better screening of high-risk borrowers. In order to avoid the period of major changes, I focus my analysis on the period that begins on October 30, 2007 and ends on September 30, 2008. The event I exploit (the cap increases on April 15, 2008) is nicely centered in this period.

Figure 1 graphs the numbers of listings and loans, by month, originated at Prosper. For ease of presentation, I aggregate the credit grades (AA, A, B, C, D, E, HR) into three groups: super prime, prime, and sub prime.[20] The vertical line marks April 15, 2008. The monthly numbers of super prime and prime listings increase moderately while the numbers of sub prime listings fluctuate. The number of loans falls in 2007 and rises in 2008 for all the groups of borrowers. In addition, the number of listings and loans increase for all groups after the April 15, 2008 change.

Figure 2 combines the number of listings and loans–that is, the probability that a listing is funded. The funding probability exhibits an overall tendency to decrease for low-risk borrowers and to increase for high-risk borrowers. Figure 3 presents the average amount requested by borrowers in listings and loans. It shows a modest decrease over time for all credit score classes. Finally, Figure 4 shows the APR of loans that were funded increased over time, especially for high-risk borrowers.

In Table 1, I present descriptive statistics for all the verifiable information on Prosper. Each variable is shown for the three credit score groups and the full sample of borrowers.

[19] The proliferation of automatic bidding tools that are less compatible with the borrowers' narrative or with the photo posted by borrowers, suggests that they are not that crucial for the analysis. Yet, I incorporate them into the analysis.

[20] Prosper assigns each potential borrower to one of seven credit grade letters. The borrower's credit grade letter is based on the credit score provided by the credit bureau. Each credit grade letter corresponds to a 40 points credit score interval. Credit grade AA corresponds to a credit score greater than 760. Credit grade A corresponds to credit score in the range 720-759. The remaining credit grades are - B, C, D, E, and HR. The corresponding credit score intervals are - 680-719, 640-679, 600-639, 560-599 and 520-559.

Unsurprisingly, most of the financial indicators are significantly worse for sub prime borrowers. In addition, the credit line variables suggest that borrowers with lower credit scores are more credit constrained: they have fewer credit lines, lower revolving credit balances, and higher utilization of their credit lines.

In Table 2, I present descriptive statistics for all the variables provided by the borrowers. Since all these variables are indicators, I report only their means. Approximately 40% of potential borrowers state that they plan to use the loan for debt consolidation. Borrowers in lower credit score groups have lower reported income and are less likely to be fully employed.[21]

# 4 Empirical Strategy

The empirical strategy taken in this research exploits the exogenous shock that occurred on April 15, 2008 to find the treatment effects of a change in the maximum allowed interest rate. At the core of the empirical strategy is a comparison of the outcomes of two similar borrowers that face different interest rate caps due to posting their loan requests before versus after the change. This empirical strategy can be labeled as differences-in-differences; An exogenous shock to the maximum allowed interest rate in some states generates differences across treated and non-treated states. An additional difference in the time dimension is created by the point in time in which the shock occurred.

## 4.1 The Basic Estimation strategy

The April 15 change created two control groups. The first group consists of states for which the maximum interest rate was 36% before the change and thus has not changed subsequently. The second group contains the state of Texas, in which legal limitations prevented an increase in the maximum rate. In addition, multiple treatment groups were created - a state that had a cap of 6% experienced a greater change than a state that had a cap of 21%. I reduce the number of control and treatment groups by merging categories. The control group includes states in which the cap has not changed. I divide treated states into three groups based on the interest rate cap clusters observed in Figure 5. I label states that had an interest rate cap of 24-25% as states with *Low Intensity Treatment*. States with cap of 16-21% and 6-12%

[21] I describe the variables presented in Tables 1-2.

are labeled as states with *Medium* and *High Intensity Treatments*, respectively. The equation corresponding to the groups defined above is -

$$\begin{aligned} Y_{it} = \beta_0 + \beta_1 \cdot I_t^{After} + \beta_2 \cdot I_i^{Low\ Intensity\ Treat.} + \beta_3 \cdot I_i^{Med.\ Intensity\ Treat.} + \beta_4 \cdot I_i^{High\ Intensity\ Treat.} + \\ \beta_5 \cdot I_t^{After} \cdot I_i^{Low\ Intensity\ Treat.} + \beta_6 \cdot I_t^{After} \cdot I_i^{Med.\ Intensity\ Treat.} + \\ \beta_7 \cdot I_t^{After} \cdot I_i^{High\ Intensity\ Treat.} + \epsilon_{it} \end{aligned} \tag{1}$$

Where $I_t^{After}$ indicates whether listing $i$ was posted after April 15, 2008. $I_i^{Low\ Intensity\ Treat.}$, $I_i^{Med.\ Intensity\ Treat.}$ and $I_i^{High\ Intensity\ Treat.}$ indicate if the borrower who posted listing $i$ resides in a treated state that had its pre-change interest rate restriction in the range of 24-25%, 16-21% and 6-12%, respectively.

I enrich the specification by including a set of week and state indicators. Thus, I control for any effect that is constant within a week (rather than within the time period before or after the change) or that is constant within a state (rather than within a group of states that had a similar cap before the change). It should be noted that the new specification makes redundant the previously included indicators for after the change and for each type of treatment group. In addition, the inclusion of a listing's characteristics control for other differences between listings. The resulting equation is -

$$\begin{aligned} Y_{it} = &\gamma_0 + \gamma_1 \cdot I_t^{Week} + \gamma_2 \cdot I_i^{State} + \gamma_3 \cdot I_t^{After} \cdot I_i^{Low\ Intensity\ Treat.} + \\ &\gamma_4 \cdot I_t^{After} \cdot I_i^{Med.\ Intensity\ Treat.} + \gamma_5 \cdot I_t^{After} \cdot I_i^{High\ Intensity\ Treat.} + \gamma_6 \cdot h(X_{it}) + \epsilon_{it} \end{aligned} \tag{2}$$

Where $I_t^{Week}$ is a vector of week dummies, and $I_i^{State}$ is a vector of state dummies. The omitted week is naturally taken to be the week just before the April 15 change. The omitted group of states consists of all states in the control group. $X_{it}$ is a vector with the characteristics of the listing.

## 4.2 Heterogenous Treatment Effects

The empirical strategy described so far has focused on estimating the average treatment effect. Nonetheless, economic theory suggests that the treatment effect may depend on the risk level associated with a borrower. For example, a high-risk borrower that is bound by the interest rate cap is expected to have a greater treatment effect than a low-risk borrower for whom the interest rate cap is not restricted.[22]

The empirical strategy is extended to account for heterogenous treatment effects based on borrowers' risk levels - a continuous latent variable. I use the credit grade letter assigned by Prosper to each potential borrower as a proxy for the risk level. Credit grades can serve as good proxies since they are salient to lenders and are used as a natural way to sort borrowers. The result is the following extended estimation equation -

$$\begin{aligned}
Y_{it} =& \eta_0 + \eta_1 \cdot I_t^{Week} + \eta_2 \cdot I_i^{State} + \sum_{g \in (A,B,...,HR)} \eta_{Grade\ g} \cdot I_i^{Grade\ g} + \\
& \sum_{j \in (High,Med.,Low)} \eta_{After,j\ Intensity\ Treat.} \cdot I_t^{After} \cdot I_i^{j\ Intensity\ Treat.} + \\
& \sum_{g \in (A,B,...,HR)} \eta_{After,Grade\ g} \cdot I_t^{After} \cdot I_i^{Grade\ g} + \\
& \sum_{\substack{j \in (High,Med.,Low) \\ g \in (AA,A,B,...,HR)}} \eta_{j\ Intensity\ Treat.,Grade\ g} \cdot I_i^{j\ Intensity\ Treat.} \cdot I_i^{Grade\ g} + \\
& \sum_{\substack{j \in (High,Med.,Low) \\ g \in (A,B,...,HR)}} \eta_{After,j\ Intensity\ Treat.,Grade\ g} \cdot I_t^{After} \cdot I_i^{j\ Intensity\ Treat.} \cdot I_i^{Grade\ g} + \eta_3 \cdot h(X_{it}) + \epsilon_{it}
\end{aligned} \tag{3}$$

Where the first difference effects - on time, state and grade - are included in the first line of the equation. The second through the fourth lines of the equation contain the second differences. The fifth line contains the main parameters of interest - the effects of the interest rate restriction on each treatment group and credit grade combination.

## 4.3 Selection into the Pool of Borrowers

The most serious problem with previous studies of the effects of usury laws that are based on individual-level data is that they do not credibly solve the problem of selection into the pool of potential borrowers. The following example illustrates the problem. Consider a potential

[22]The treatment effect may depend on the slope of the supply curve. I devote Section 4.5 to discuss this issue.

borrower that faces a cap of 12%. If he has low expectations to be funded, he may find it suboptimal to post his listing due to the opportunity cost associated with the time it takes to post a listing (approximately 30 minutes). Alternatively, the increase in the ceiling rate may drive new borrowers ("entrants") to post their listings because they have higher expectations to be funded. Nonetheless, "entrants" likely have a lower funding probability than "incumbents". As a result, a researcher may wrongly conclude that the increase in the interest rate cap results in a decrease in a listing's funding probability whereas in reality, any listing has a higher funding probability, but the composition of listings has changed.

Typically, the econometrician observes data on individuals that apply for loans before and after the change. Hence, he can control for the different characteristics of potential borrowers. If he were to do it perfectly, then he would be able to separate between the effect of the change in the cap, and the effect of a change in the pool of borrowers. In reality, however, there is a gap between the information utilized by lenders and the information observed by the econometrician. This gap restricts the econometrician's ability to correctly perform the counterfactual of predicting the outcome under the original interest rate cap of "entrants" that are observed only after the change. As a result, selection usually is not fully controlled.

Luckily, I am in a unique position since the data set in hand contains almost every piece of information that lenders could have observed in the process of making their bidding decision. Put another way, any information that I do not observe could not have affected all the lenders anyway. Therefore, controlling the information on borrowers in a flexible way would eliminate selection. Ideally, I would like to estimate the probability of a potential borrower to post a listing. Yet, I observe only listings that were actually posted, a fact that make the estimation of the posting probability infeasible. However, since potential borrowers' decision to post a listing is affected by their probability to be funded, I can use the data to control for the probability of a listing to be funded, and by this eliminate the selection problem.

### 4.4 Outcomes

In the following section I investigate the effect of interest rate restrictions on four outcome variables: 1) The probability of a listing to be funded; 2) The amount a borrower requests; 3) The APR a borrower pays; 4) The probability of default. Even though some of the variables are determined simultaneously, I estimate the effects of the interest rate cap separately.

First, the probability of a listing to be funded is a binary variable. This suggests that a probit specification is appropriate for the estimation. Second, the amount requested by a borrower is a continuous variable that ranges from \$1,000 to \$25,000. Hence, I use a two-sided tobit model to estimate how interest rate restriction affects the amount requested. Third, the APR paid by a borrower is a continuous variable that is bound to be below the interest rate cap. Hence, it is most natural to use a one-side tobit specification. Fourth, a borrower is considered as defaulting on a loan if he misses at least one of the first three payments. Thus, a probit specification is used for the analysis of defaults.

### 4.5 General Equilibrium Effects

Since its introduction, Prosper has mediated loans for a total amount exceeding \$175,000,000. This large amount only accounts for less than 0.0025% of the US consumer credit loans.[23] Hence, if Prosper had been perfectly integrated into the consumer loans market, then the April 15 change would have been expected to redirect credit from other loan markets into the Prosper marketplace. If this were the case, borrowers would not be expected to pay a higher APR as a result of the higher volume of loan requests. However, it is not at all clear that Prosper is indeed perfectly integrated into other consumer loan markets. Specifically, Prosper's position as a relatively new marketplace that operates in an online setting may make its integration process into other credit markets slower. The distinction of whether Prosper is well or poorly integrated into consumer loan markets is reflected by the slope of the credit supply curve in the Prosper marketplace; a perfectly elastic supply curve is the manifestation of being well integrated, whereas an increasing supply curve indicates that this marketplace, to some extent, operates separately from other consumer loan markets.

I conduct two tests of the null hypothesis that the supply curve is perfectly elastic. The first test is based on the treatment effect of the price paid by borrowers. The second test is based on the treatment effect of the funding probability. A perfectly elastic supply curve implies a zero price effect of the April 15 change on all categories. Furthermore, it implies that the funding probability is unchanged in categories that were unaffected by the treatment.

[23] See the Federal Reserve Statistical Release on consumer credit - http://www.federalreserve.gov/releases/g19/Current/.

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.

The tests described are limited since they do not distinguish between upward sloping supply curve and generic time effects. Namely, a rejection of the null can be interpreted as evidence for an upward sloping supply curve if I assume that nothing else occurred during that time that affected differently two credit grades within any treatment group. Alternatively, a rejection of the null is to be considered as evidence for generic time effect under the assumption of perfectly elastic supply curve.

# 5 Basic Evidence

This section presents basic evidence for the various effects of usury laws. The evidence presented provides very similar insights as the analysis performed in Section 6, despite the descriptive nature of the evidence. Following the empirical approach taken, I focus on differences over time and between treatment and control groups. In addition, I differentiate between credit grades. I abstract from potential time effects by focusing on the month before and the month after the change. I present data on the control group and on the group of borrowers that experienced the largest treatment - a change from interest rate cap in the 6-12% range to an interest rate cap of 36%. Within each group, I only present data on a subset of the credit grades.[24] For each category, I present the main outcome variables of interest - number of listings and loans together with the mean values of the amount requested, the funding probability, and a proxy for default. In addition, the estimated predicted default probabilities of loans and listings are presented.[25]

I present the results in Table 3. The table reveals a greater increase in the number of listings posted in the treatment group. The funding probability in the control group decreases in all credit grades, whereas the reversed pattern is exhibited in the treatment group. If the supply curve is believed to be upward sloping, the different patterns between the treatment

[24]The patterns presented are carried through if I consider two month - one before and one after the change - that are further away from each other. In addition, inclusion of more treatment groups or more credit grades do not provide additional insights.

[25]The predicted default probabilities are based on a probit model that is estimated separately for each credit grade. The dependent variable indicates whether the borrower missed at least one out of the first three payments. The loan characteristics are being used as regressors. I use the regression coefficients to extrapolate predicted values for listings that were not funded.

and control groups can be explained by the de facto shift in the aggregate demand as a result of the increase in the maximum allowed rate in the treatment groups. In addition, these findings can be interpreted as evidence for different time effects across the two groups.

The average amount requested does not exhibit major changes over time within any group. The APR observed in the control group is slightly changed over time. On the contrary, the APR observed in the control group is elevated for the lower credit scores. One possible reason for the higher APR is the change in the composition of borrowers since high risk borrowers with potentially different observed characteristics have their loans funded under the elevated cap. The default probability increases over time both in the control and in the treatment groups. Yet, the low number of loans in the treatment group prior to the change suggests that any comparison between the groups is not informative. Finally, loan actual default probabilities correspond to the overall risk of loans, whereas, loan predicted default probabilities correspond only to the observed risk of loans. Therefore, the difference between the two probabilities should be interpreted as an indication of whether loans unobserved risk is greater or less than the observed risk of loans. The table does not demonstrate a clear pattern regarding which component of the risk is of greater magnitude.

## 6 Empirical Analysis

While the evidence presented in Section 5 does not account for changes in the composition of listings posted under different interest rate caps, the analysis presented below utilizes the richness of the data and emphasizes the need to account for potential selection. The analysis is based on variants of equation ( 3) that are tailored to the specificities of each outcome variable of interest.

I begin with studying how the probability of a listing to be funded is affected by the change. I employ a probit model based on equation ( 3) with the dependent variable being an indicator for whether a listing was funded. I present the treatment effects in Table 4. The four specifications differ by the extent and the flexibility in which selection is accounted for. The first specification does not account for any characteristics of the listing apart from the the borrower's state and the week of posting. The second specification includes financial information provided by Prosper as described in the first part of the Appendix. The third specification relaxes the linearity of key financial variables as described in the second part of the Appendix by allowing their effect to change over the variable distribution. Information provided by the borrower on his financial situation and the purpose of the loan as described in the third part of the Appendix is controlled in the fourth specification. I supplement the table by including the empirical funding probabilities prior to the change as a benchmark. In order to ease the understanding of the table, Figure 6 contains a graphical representation of the results from the fourth specification. In the graph the point estimates and the 95% confidence interval for each category treatment effect are drawn.

The same qualitative results are observed in all specifications, even though additional regressors are added over specifications. The coefficients should be interpreted as the expected increase in the funding probability. An increase in the cap from 24-25% to 36% is to increase the funding probability by up to 0.2. The expected increments in the funding probabilities of listings from the treatment groups that had a cap of 6-12% and 16-21% are 0.75 and 0.4, respectively. The table provides two insights. First, the treatment effect is significantly positive in categories that could have benefited from an increase in their cap. That is, categories that had an interest rate restriction that was more binding than the interest rate restriction in their counterpart control group categories. Second, the greatest treatment effect within a treatment group is estimated for one of the two highest credit grades that were restricted under the treatment group's original interest rate cap. For this purpose, I define a credit grade to be restricted under an interest rate cap if the cap is lower than the control group average interest rate for the same credit grade before the April 15 change. Below, I provide graphical illustrations for these insights.

Figure 7 contains the average APR for credit grades in the control and the treatment groups before April 15, 2008. The average APR in the high intensity treatment and the control group are, loosely speaking, distinguishable even for credit grade AA. This implies that the interest rate cap in high intensity treated states had been binding before the change for AA borrowers more than it was in control states. Thus, the treatment effect is positive and significant for all credit grades in the high intensity treatment. Similarly, the gap between the control group and the medium treatment group widen for credit grade B, implying a positive treatment effect for credit grades lower than A in the medium intensity treatment group.

Figure 8 contains the average APR in the control group before the April 15 change for all credit grades together with horizontal lines that mark the highest cap in each treatment group. The figure allows to identify for each treatment group which are the two highest restricted credit grades. For example, the highest cap in the low intensity treatment group is 12%. According to the figure credit grades A and B are the two highest restricted credit grades under this cap. The greatest treatment effect in the high intensity treatment group is, therefore, achieved in credit grade B.

The amount a borrower requests in a listing is a major determinant of its probability to get funded. The coefficients on the amount requested are not presented in Table 4. The parameter estimates of specification 3 reveal that for the average listing, the funding probability decreases by 4.5% for a 1% increase in the amount requested.[26] Even though I control for the amount in the analysis of the treatment effect of the amount requested, one might be concerned that the amount requested is endogenous in the sense that borrowers tailor the amount they request according to the interest rate cap. I address this concern by estimating the treatment effect on the amount a borrower requests, and present it in Table 5. I use a two-sided tobit model since the amount requested is bound to be in the range of $1,000 - $25,000 and present the marginal effects.[27] The same four specifications that were employed to study the treatment effect on the funding probability are estimated. The results are robust to the specification chosen. The treatment effect is found to be insignificant in 16 out of the 21 categories analyzed. Yet, I can reject the null hypothesis of zero treatment effect (P-value < 0.001). I provide a graphical illustration of the last specification estimates and 95% confidence interval in Figure 9.

I proceed by investigating the effect of interest rate cap on the APR. The specification employed is the one previously used. A natural way to estimate the treatment effect is to use a loan's APR as the dependent variable. The problem, however, is that ignoring non-funded listings generates a problem of selection because the APR is bound to be below some

[26]Specification 4 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 31.2-38.1.

[27]The tobit parameters ($\beta$ and the standard deviation of the tobit error term $\sigma$) are estimated through maximum likelihood. The estimated coefficients are then used to estimate the marginal effect on the latent APR conditional on the latent APR being below the interest rate cap - $\frac{\partial E[y|y<Cap,x]}{\partial x_k} = \beta_k(1-\lambda(\frac{x\beta}{\sigma})[(\frac{x\beta}{\sigma})+\lambda(\frac{x\beta}{\sigma})])$. Where $\lambda(\cdot) = \frac{\phi(\cdot)}{\Phi(\cdot)}$ is the inverse Mills ratio. The marginal effects are estimated at the sample average point. Standard errors are estimated using the delta method.

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 being used is a one-sided tobit model. The implicit assumption employed is that non-funded listings would have been funded under a higher interest rate cap. That is, their cap is treated as a lower bound on the APR. A major concern is the low number of loans observed before the April 15 change in some categories. The small number makes the treatment effect estimation in these categories being less reliable due to the high proportion of censored observation. Therefore, I restrict the analysis to categories that had at least 15 loans and had less than 98.5% of the observations censored. I present the marginal effects in table 6. I provide a graphical illustration of point estimates and the 95% confidence interval in Figure 10. The analysis suggests that the treatment effect on the interest rate is positive, yet, it is of small magnitude, less than $\frac{1}{2}$%. The negative estimated treatment effects in the lower credit grades is not predicted by the theory and suggests that a higher threshold might be appropriate.

In what follows, I investigate whether riskier loans are originated if one controls for the characteristics of the loan. The sample includes loans that are observed in at least the first three payments. A zero treatment effect can be expected since I condition on the characteristics of the loan. A non-zero treatment effect can, however, be interpreted as evidence for a change in the extent of adverse selection. That is, a change in the importance of the unobserved characteristics of a borrower on the performance of his loan. I present the estimates of a linear probability model in Table 7.[28] The point estimates and the 95% confidence interval of the fourth specification are drawn in Figure 11. The insignificance of the treatment effect coefficients is robust to the specification used - only two out of the 21 estimated treatment effects are significant. Hence, I cannot reject the hypothesis that all the treatment effects are zero. (P-value = 0.35) The analysis implies that following the change, lenders face similar

[28]The first model that comes to mind is a probit model. Since I use only information on the first three payments, there are categories that have not faced even a single default, and therefore their outcome is perfectly predicted by the category's dummy. This problem is know as *Separation* (see Zorn (2005)). Most categories that generate this problem are those with low interest rate caps and few loans. Estimation of binary response models with separation raises several issues, thus I choose to use a linear probability model. As a robustness check for my choice in a linear probability model, I estimate an aggregated model in which I aggregate the three treatment groups together. This aggregated model does not suffer from separation and can be estimated using a probit model. I compare the model estimates obtained from a probit model and from a linear probability model. I find that the estimates are qualitatively the same.

difficulties to screen potential borrowers that are not credit-worthy.

I use the estimated treatment effects to test the null hypothesis that the supply curve is perfectly elastic by implementing the tests described in Section 4.5. I conduct two tests. In the first, I test whether treatment effects for all credit grades in the treatment groups as well as changes in the APR in the control group are all 0. The second test uses the treatment effect on the funding probability. Specifically, I assume that as reflected in Figure 6, borrowers with credit grade AA were not restricted in low and medium treatment intensity groups. Similarly, borrowers with credit grade A were not restricted in the low intensity treatment group. I perform a joint test for zero treatment effects in those non-restricted categories and a zero change in the funding probability in the control group. I reject both tests and conclude that the supply curve of credit is not perfectly elastic. (The P-values of both tests is less than 0.001)

# 7 Conclusions

Access to credit has been considered as a main springboard to economic development. The evolution of usury laws throughout history has positioned them as a government intervention in the credit markets that is required in order to protect consumers from usury. This paper uses detailed individual-level data to evaluate the validity of the claims in a new yet fast-growing credit market, the online person-to-person market. The evaluation takes the form of studying the effects of interest rate restrictions on the marketplace by utilizing a change that has increased the maximum interest rate allowed for a borrower to pay up to 36% in all the states but one.

The main contribution of this research lies in its ability to identify the causal effects of interest rate caps. The increase in the maximum allowed interest rate enables me to overcome challenges that previous research has not been able to fully resolve. The two main challenges addressed here are borrowers' selection into the sample and the isolation of causal effects from generic time effects that are unrelated to the change. The main pieces of evidence reveal that borrowers who were restricted under their original cap benefit from the change and the marginal borrower benefits the most. In addition, borrowers are not expected to pay a much higher price for credit that is issued under a higher interest rate restriction. The main takeaway point from this inquiry is that interest rate restrictions do not seem to deliver the outcomes that have been their main premise. An additional contribution of this work is the evidence it provides regarding the imperfect integration of the studied credit market with other credit markets.

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# 8 Appendix - Variable Descriptions

## 8.1 Verified Variables

Below I describe the variables that are included in the borrower's credit report and are being used throughout the analysis.

- Auction Open For Duration - A dummy that equals to 1 if the borrower chooses to end the auction when its duration ends and not before (the advantage of keeping the auction open is that the interest rate decreases if there is excess funding)
- Home Owner - A dummy that equals to 1 if the borrower is a home owner
- Amount Delinquent - The monetary amount delinquent
- Current Delinquencies - number of account on 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

## 8.2 Variables Relaxed to Account for Non Linear Effect

Some of the specifications used allow for non linear effects of several variables. In such cases, the effects depend on the value of the variable. The thresholds used to allow for different effects 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

### 8.3 Non-Verified Variables

The list below is a partial list of variables that are self-reported by borrowers. These variables are being included in some specifications -

- 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

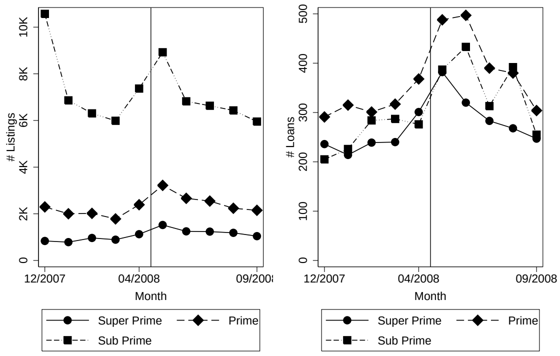


Figure 1: The Number of Listing and Loans Over Time

The figure graphs the monthly number of listing and loans. The left figure corresponds to the number of listings, and the right for the number loans. Within each graph, borrowers are clustered by their credit score. I define borrowers with credit score above 720 as super prime borrowers. Borrowers with credit score in the range 640-719 as prime borrowers and borrowers with credit score below 640 but above 520 are defined as sub prime borrowers. The vertical line marks the April 15 change in which the maximum interest rate allowed for a borrower to pay was set at 36% in all the states. Since the change occurred in the middle of a month I relabel time accordingly. For example, the activity that is marked on April 2008 corresponds to the time period that starts on March 15, 2008 and ends on April 14, 2008. In addition, I omit the number of listings and loans from 10/30/2007-11/14/2007 because this time period includes less than a month-long od data.

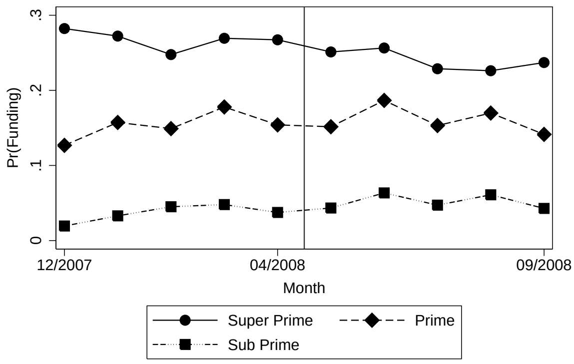


Figure 2: Probability of Funding Over Time

The figure graphs the change over time in the funding probability of listings. Listings are clustered into groups based on their credit score. Borrowers are clustered by their credit score. I define borrowers with credit score above 720 as super prime borrowers. Borrowers with credit score in the range 640-719 as prime borrowers and borrowers with credit score below 640 but above 520 are defined as sub prime borrowers. The vertical line marks the April 15 change in which the maximum interest rate allowed for a borrower to pay was set at 36% in all the states. Since this change occurred in the middle of a month I relabel time accordingly. For example, the activity that is marked on April 2008 corresponds to the time period that starts on March 15, 2008 and ends on April 14, 2008. In addition, I omit the number of listings and loans from 10/30/2007-11/14/2007 because this time period includes less than a month-long od data.

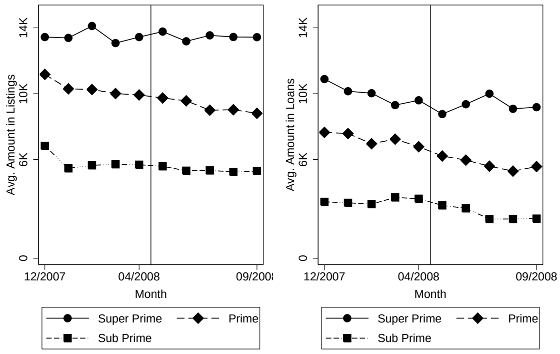


Figure 3: Average Amount Requested Over Time

The figure contains the monthly average amount requested by borrowers. The left graph presents the amount requested in listings, while the right corresponds to loans. Within each graph, borrowers are clustered by their credit score. I define borrowers with credit score above 720 as super prime borrowers. Borrowers with credit score in the range 640-719 as prime borrowers and borrowers with credit score below 640 but above 520 are defined as sub prime borrowers. The vertical line marks the April 15 change in which the maximum interest rate allowed for a borrower to pay was set at 36% in all the states. Since the change occurred in the middle of a month I relabel time accordingly. For example, the activity that is marked on April 2008 corresponds to the time period that starts on March 15, 2008 and ends on April 14, 2008. In addition, I omit the number of listings and loans from 10/30/2007-11/14/2007 because this time period includes less than a month-long od data.

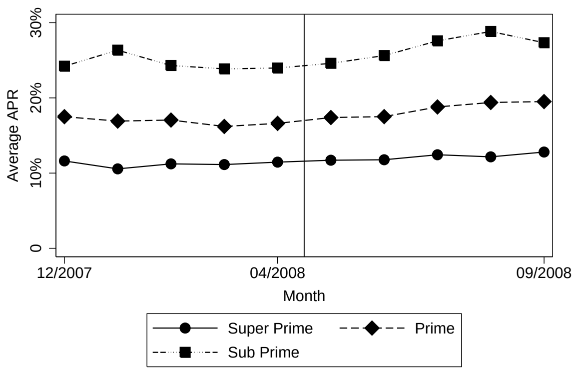


Figure 4: Average Interest Rate Over Time

The figure contains the monthly average APR paid by borrowers. Borrowers are clustered by their credit score. I define borrowers with credit score above 720 as super prime borrowers. Borrowers with credit score in the range 640-719 as prime borrowers and borrowers with credit score below 640 but above 520 are defined as sub prime borrowers. The vertical line marks the April 15 change in which the maximum interest rate allowed for a borrower to pay was set at 36% in all the states. Since the change occurred in the middle of a month I relabel time accordingly. For example, the activity that is marked on April 2008 corresponds to the time period that starts on March 15, 2008 and ends on April 14, 2008. In addition, I omit the number of listings and loans from 10/30/2007-11/14/2007 because this time period includes less than a month-long od data.

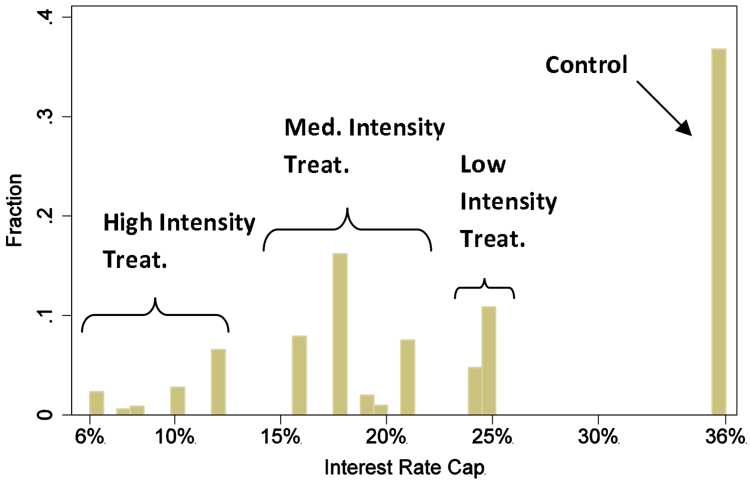


Figure 5: Fraction of Listings Posted Under Various Interest Rate Caps - Before 04/15/2008

The figure contains the distribution of interest rate caps in listings posted before April 15, 2008 and maps interest rate caps into the different treatment and control groups.

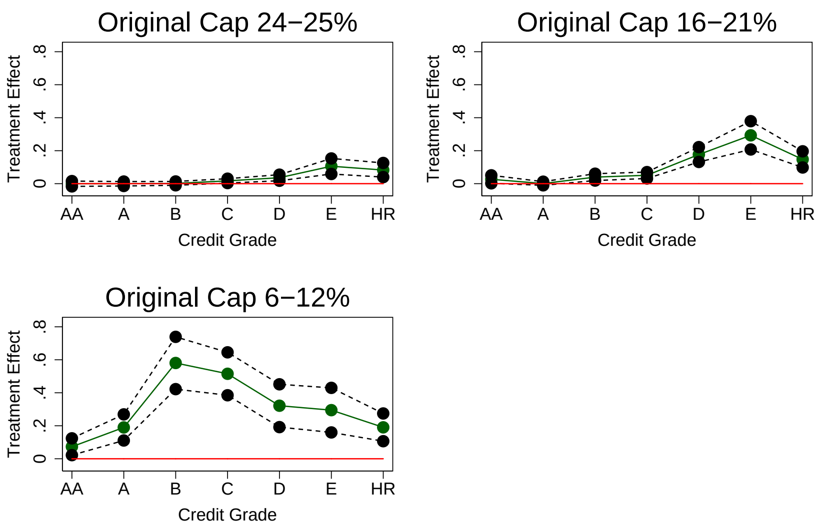


Figure 6: Treatment Effect on the Funding Probability

The figure contains a graphical representation of the treatment effect on the funding probability. The point estimates and the 95% confidence intervals of the treatment effect for any treatment group - credit grade combination based on column 4 of Table 4 are included in the graph.

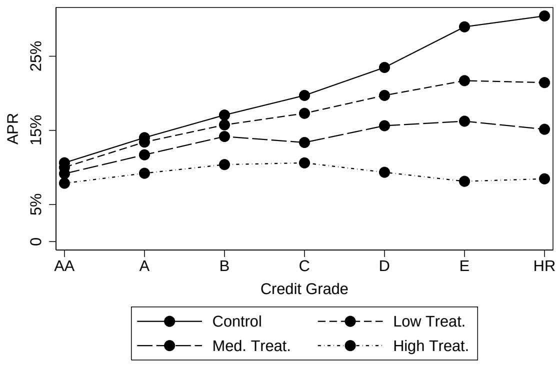


Figure 7: Average APR by Treatment Before prior to 04/15/2008

The figure contains the average APR in loans originated before the change. Each line corresponds to a different treatment or control group.

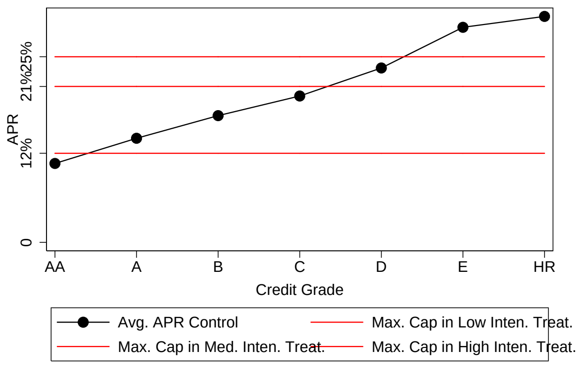


Figure 8: Average APR in Control Group Before the Change

The figure contains the average APR observed in the control group before the change. The three horizontal lines in 12%, 21% and 25% correspond to the highest interest rate allowed in the high, medium and low intensity treatment groups.

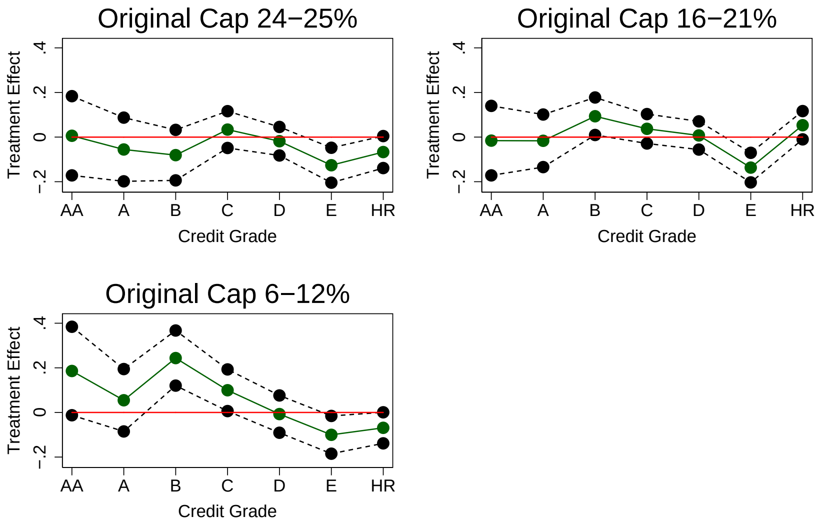


Figure 9: Treatment Effect on the Amount Requested

The figure contains a graphical representation of the treatment effect on the amount requested. The point estimates and the 95% confidence intervals of the treatment effect for any treatment group - credit grade combination based on column 4 of Table 5 are included in the graph.

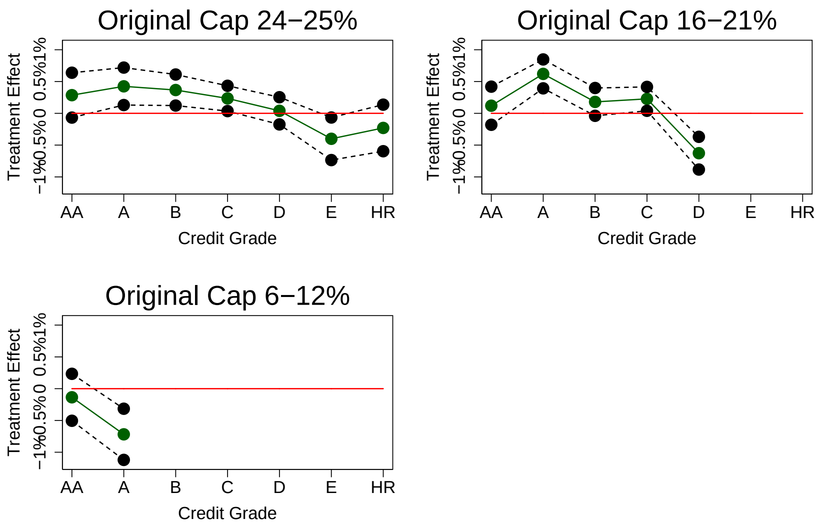


Figure 10: Treatment Effect on the APR

The figure contains a graphical representation of the treatment effect on the APR. The point estimates of the marginal effect and the 95% confidence interval for any treatment group - credit grade combination with more than 15 loans are presented. The graph is based on column 4 of Table 6.

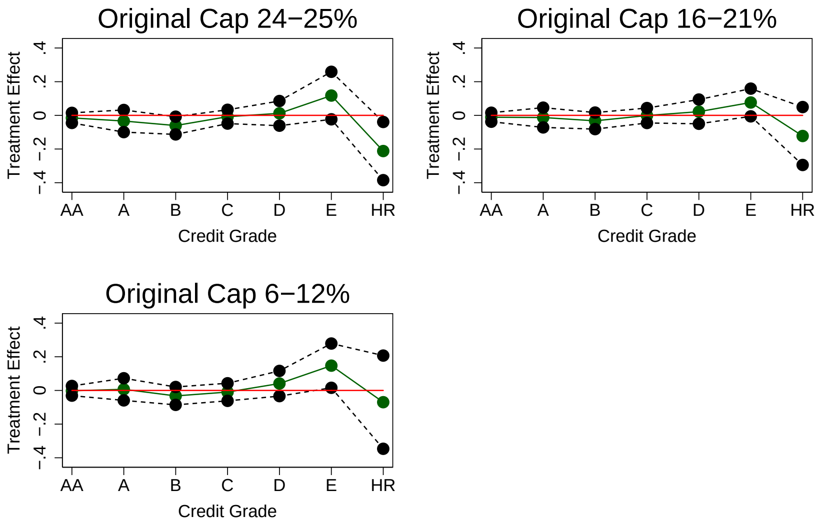


Figure 11: Treatment Effect on the Default

The figure contains a graphical representation of the treatment effect on default. The point estimates and the 95% confidence intervals of the treatment effect for any treatment group - credit grade combination based on column 4 of Table 7 are included in the graph.

| | Super Prime | | | | Prime | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Std. | 10% | 90% | Mean | Std. | 10% | 90% |
| General | | | | | | | | |
| # Listings | 11483 | | | | 24898 | | | |
| # Loans | 2878 | | | | 3860 | | | |
| Listing Characteristics | | | | | | | | |
| Amount | 13470 | 8074 | 3000 | 25000 | 9816 | 6284 | 3000 | 20000 |
| Open For Duration | 0.94 | 0.23 | 1 | 1 | 0.92 | 0.28 | 1 | 1 |
| Amount Delinquent | 922 | 19973 | 0 | 142 | 1557 | 7990 | 0 | 3183 |
| Current Delinquencies | 0.30 | 0.96 | 0 | 1 | 0.91 | 2.07 | 0 | 3 |
| Delinquencies Last 7 Years | 1.75 | 6.22 | 0 | 4 | 4.92 | 10.40 | 0 | 16 |
| Public Records Last Year | 0.02 | 0.21 | 0 | 0 | 0.04 | 0.23 | 0 | 0 |
| Public Records Last 10 Years | 0.24 | 0.85 | 0 | 1 | 0.38 | 0.86 | 0 | 1 |
| Inquiries Last 6 Month | 2.12 | 2.87 | 0 | 5 | 2.97 | 3.54 | 0 | 7 |
| Bank Card Utilization | 0.35 | 0.29 | 0 | 1 | 0.60 | 0.33 | 0 | 1 |
| Current Credit Lines | 11.65 | 6.50 | 4 | 20 | 11.00 | 6.56 | 4 | 20 |
| Revolving Credit Balance | 28749 | 60327 | 154 | 74467 | 24950 | 46843 | 272 | 62638 |
| Total Credit Lines | 28.24 | 15.06 | 11 | 48 | 28.26 | 15.37 | 10 | 48 |
| Listing Outcomes | | | | | | | | |
| Pr(Funding) | 0.251 | | | | 0.155 | | | |
| APR | 11.70% | 4.67% | 7.00% | 17.45% | 17.70% | 6.07% | 11.05% | 26% |
| Pr(Default) | 0.015 | | | | 0.026 | | | |

| | Sub Prime | | | | Total | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Std. | 10% | 90% | Mean | Std. | 10% | 90% |
| General | | | | | | | | |
| # Listings | 78521 | | | | 114902 | | | |
| # Loans | 3231 | | | | 9969 | | | |
| Listing Characteristics | | | | | | | | |
| Amount | 5771 | 5256 | 1500 | 12500 | 7417 | 6380 | 1800 | 17000 |
| Open For Duration | 0.84 | 0.36 | 0 | 1 | 0.87 | 0.34 | 0 | 1 |
| Amount Delinquent | 4337 | 14926 | 0 | 11510 | 3394 | 14419 | 0 | 9030 |
| Current Delinquencies | 4.00 | 5.14 | 0 | 10 | 2.96 | 4.63 | 0 | 9 |
| Delinquencies Last 7 Years | 12.68 | 17.55 | 0 | 35 | 9.91 | 15.97 | 0 | 30 |
| Public Records Last Year | 0.09 | 0.37 | 0 | 0 | 0.07 | 0.33 | 0 | 0 |
| Public Records Last 10 Years | 0.72 | 1.26 | 0 | 2 | 0.60 | 1.16 | 0 | 2 |
| Inquiries Last 6 Month | 4.18 | 4.85 | 0 | 10 | 3.71 | 4.49 | 0 | 9 |
| Bank Card Utilization | 0.70 | 0.45 | 0 | 1 | 0.64 | 0.42 | 0 | 1 |
| Current Credit Lines | 7.77 | 5.84 | 2 | 16 | 8.86 | 6.28 | 2 | 17 |
| Revolving Credit Balance | 8895 | 22495 | 0 | 21370 | 14359 | 35360 | 0 | 33951 |
| Total Credit Lines | 26.16 | 14.72 | 9 | 45 | 26.82 | 14.93 | 9 | 46 |
| Listing Outcomes | | | | | | | | |
| Pr(Funding) | 0.041 | | | | 0.087 | | | |
| APR | 25.68% | 7.82% | 15.50% | 35.00% | 18.56% | 8.41% | 9.00% | 34% |
| Pr(Default) | 0.062 | | | | 0.034 | | | |

Table 1: Summary Statistics - Prosper Provided Information

The tables contains summary statistics of the variables Prosper provides. The variables are divided into three categories - general, listing characteristics and listing outcomes. General variables are the numbers of loans and listings. The category of listing characteristics contains variables that are included in the verified part of the listing such as the amount requested by the borrower and the number of delinquencies the borrower suffered from in the last seven years. The listing outcome category includes variables such as a listing's probability to be funded and the probability that the borrower defaults on a loan in case it is funded. A loan is defined as default if the borrowers missed at least one of the first three payments. The summary statistics are calculated separately for each of the three defined group of borrowers as well as for the full sample combined. The statistics that are presented for each variable are mean, standard deviation and the $10^{th}$ and $90^{th}$ percentiles of its distribution.

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

Table 2: Summary Statistics - Borrower's Provided Information

The table contains summary statistics for the information borrowers may provide that is used in the analysis. I report the mean value for each of them since they are all dummy variables. 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 income and the last group indicates the borrower's employment status.

**Control**

| | | | | Avg. Amount | | | | Predicted Default | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | 03/15/2008 - 04/14/2008, Ceiling Rate 36% | | | | | | | | |
| Before | Grade | # Listungs | # Loans | Listings | Loans | Pr(Funding) | APR | Loans | Listings | Actaul Default |
| | AA | 206 | 67 | 14842 | 11279 | 0.325 | 0.107 | 0.000 | 0.056 | 0.000 |
| | C | 626 | 96 | 9422 | 6497 | 0.153 | 0.198 | 0.030 | 0.047 | 0.032 |
| | HR | 1414 | 51 | 5000 | 2514 | 0.036 | 0.312 | 0.123 | 0.189 | 0.059 |
| | | 04/15/2008 - 05/14/2008, Ceiling Rate 36% | | | | | | | | |
| After | Grade | # Listungs | # Loans | Listings | Loans | Pr(Funding) | APR | Loans | Listings | Actaul Default |
| | AA | 306 | 76 | 14759 | 10158 | 0.248 | 0.117 | 0.000 | 0.112 | 0.000 |
| | C | 807 | 107 | 9338 | 5636 | 0.133 | 0.172 | 0.030 | 0.046 | 0.038 |
| | HR | 1613 | 26 | 5216 | 2056 | 0.016 | 0.280 | 0.122 | 0.191 | 0.231 |

**High Intensity Treatment**

| | | | Avg. Amount | | | | Predicted Default | | |
|---|---|---|---|---|---|---|---|---|---|
| | 03/15/2008 - 04/14/2008, Ceiling Rate 6%-12% | | | | | | | | |
| Grade | # Listungs | # Loans | Listings | Loans | Pr(Funding) | APR | Loans | Listings | Actaul Default |
| AA | 66 | 23 | 11890 | 6959 | 0.348 | 0.076 | 0.000 | 0.000 | 0.000 |
| C | 164 | 4 | 9093 | 3200 | 0.024 | 0.106 | 0.003 | 0.052 | 0.000 |
| HR | 518 | 1 | 5156 | 1500 | 0.002 | 0.110 | 0.156 | 0.184 | 0.000 |
| | 04/15/2008 - 05/14/2008, Original Ceiling Rate 6%-12% (Now 36%) | | | | | | | | |
| Grade | # Listungs | # Loans | Listings | Loans | Pr(Funding) | APR | Loans | Listings | Actaul Default |
| AA | 94 | 38 | 11362 | 6574 | 0.404 | 0.086 | 0.000 | 0.121 | 0.000 |
| C | 301 | 48 | 8242 | 4631 | 0.159 | 0.203 | 0.044 | 0.047 | 0.021 |
| HR | 802 | 25 | 4625 | 1733 | 0.031 | 0.294 | 0.119 | 0.180 | 0.167 |

Table 3: Basic Evidence

The figure consists of four panels. The left two panels correspond to the control group. Namely, listings that were posted in states that experienced no change in their interest rate cap around April 15, 2008. The right panels correspond to the high intensity treatment group. That is, states that were subject to interest rate cap in the range 6-12% before April 15, 2008. The upper panels refer to activity on 03/15/2008-04/14/2008, the lower panels refer to activity that took place on 04/15/2008-05/14/2008. Each panel contains statistics on credit grades AA, C and HR. For each credit grade the number of listings, number of loans, average amount requested in listings and loans, probability of funding, average APR and actual default probability are presented. In addition, a default model is used to predict the average default probability of loans and listings. For the purpose of the model, a loan is defined as default if at least one of the first three payments were missed.

| Dep. Variable - Funding Indicator, Probit Estimates | Five Months Before 15-Apr-08 | | (1) | | (2) | | (3) | | (4) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Num. Listings | Pr(Funding) | dF/dx | z-stat. | dF/dx | z-stat. | dF/dx | z-stat. | dF/dx | z-stat. |
| After*Low Intensity Treat.*Grade AA | 270 | 0.415 | 0.000 | -(0.02) | -0.004 | -(0.52) | -0.004 | -(0.47) | 0.000 | -(0.06) |
| After*Low Intensity Treat.*Grade A | 335 | 0.290 | 0.003 | (0.24) | -0.002 | -(0.26) | -0.003 | -(0.38) | 0.000 | -(0.07) |
| After*Low Intensity Treat.*Grade B | 496 | 0.300 | 0.003 | (0.32) | 0.000 | -(0.07) | 0.000 | -(0.07) | 0.002 | (0.31) |
| After*Low Intensity Treat.*Grade C | 1117 | 0.170 | 0.020 | (2.18) | 0.018 | (3.04) | 0.018 | (2.95) | 0.017 | (3.09) |
| After*Low Intensity Treat.*Grade D | 1332 | 0.093 | 0.046 | (4.33) | 0.040 | (5.41) | 0.041 | (5.54) | 0.036 | (5.41) |
| After*Low Intensity Treat.*Grade E | 1644 | 0.027 | 0.121 | (6.61) | 0.108 | (7.25) | 0.111 | (7.36) | 0.106 | (7.46) |
| After*Low Intensity Treat.*Grade HR | 2944 | 0.008 | 0.117 | (6.31) | 0.090 | (6.35) | 0.093 | (6.51) | 0.083 | (6.12) |
| After*Med. Intensity Treat.*Grade AA | 583 | 0.298 | 0.035 | (2.66) | 0.028 | (2.83) | 0.027 | (2.75) | 0.026 | (2.82) |
| After*Med. Intensity Treat.*Grade A | 726 | 0.234 | 0.004 | (0.42) | 0.000 | (0.03) | -0.001 | -(0.13) | 0.001 | (0.18) |
| After*Med. Intensity Treat.*Grade B | 1206 | 0.166 | 0.043 | (4.05) | 0.037 | (4.72) | 0.038 | (4.83) | 0.040 | (5.32) |
| After*Med. Intensity Treat.*Grade C | 1881 | 0.101 | 0.047 | (5.10) | 0.055 | (7.73) | 0.053 | (7.55) | 0.051 | (7.67) |
| After*Med. Intensity Treat.*Grade D | 2575 | 0.025 | 0.177 | (12.02) | 0.184 | (13.89) | 0.184 | (13.96) | 0.177 | (13.87) |
| After*Med. Intensity Treat.*Grade E | 2520 | 0.007 | 0.318 | (11.69) | 0.291 | (11.94) | 0.298 | (12.04) | 0.293 | (12.07) |
| After*Med. Intensity Treat.*Grade HR | 5410 | 0.006 | 0.159 | (9.33) | 0.159 | (10.48) | 0.161 | (10.54) | 0.147 | (10.21) |
| After*High Intensity Treat.*Grade AA | 254 | 0.299 | 0.055 | (3.04) | 0.068 | (4.13) | 0.069 | (4.18) | 0.073 | (4.48) |
| After*High Intensity Treat.*Grade A | 382 | 0.094 | 0.162 | (6.88) | 0.183 | (8.02) | 0.181 | (7.99) | 0.190 | (8.53) |
| After*High Intensity Treat.*Grade B | 374 | 0.016 | 0.483 | (10.04) | 0.580 | (10.99) | 0.577 | (10.91) | 0.581 | (10.65) |
| After*High Intensity Treat.*Grade C | 741 | 0.007 | 0.422 | (10.67) | 0.511 | (12.07) | 0.515 | (12.07) | 0.515 | (12.34) |
| After*High Intensity Treat.*Grade D | 957 | 0.007 | 0.328 | (9.15) | 0.339 | (9.08) | 0.347 | (9.18) | 0.322 | (8.74) |
| After*High Intensity Treat.*Grade E | 1054 | 0.005 | 0.326 | (7.73) | 0.308 | (7.93) | 0.305 | (7.84) | 0.295 | (7.76) |
| After*High Intensity Treat.*Grade HR | 2526 | 0.002 | 0.225 | (7.75) | 0.206 | (8.17) | 0.208 | (8.19) | 0.191 | (7.97) |
| State and Week F.E. | | | ✓ | | ✓ | | ✓ | | ✓ | |
| Verified Information | | | | | ✓ | | ✓ | | ✓ | |
| Discretezation of Financial Information | | | | | | | ✓ | | ✓ | |
| Non-Verified Information | | | | | | | | | ✓ | |
| Pseudo R-Square | | | 0.156 | | 0.186 | | 0.277 | | 0.298 | |
| Num. Obs. | | | 114902 | | 114699 | | 114699 | | 114686 | |

Table 4: Treatment Effects Estimates on a Listing Funding - Probit Model

The table contains estimation results from probit regressions that explore the factors that affect listing's funding probability. The first two columns contain number of listings and the funding probability in the five months before April 15 change in each of the main group of interest. For each regression, the coefficients on the heterogenous treatment effects are presented. Standard errors in parenthesis are clustered by state and week.

| Dep. Variable - log(Amount Requested) Tobit Estimates | Five Month Before 15-Apr-08 | | (1) | | (2) | | (3) | | (4) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Num. Listings | Average Amount Req. | Coef. | Std. Err. | Coef. | Std. Err. | Coef. | Std. Err. | Coef. | Std. Err. |
| After*Low Intensity Treat.*Grade AA | 270 | 13640 | -0.018 | (0.098) | 0.010 | (0.095) | 0.016 | (0.095) | 0.006 | (0.091) |
| After*Low Intensity Treat.*Grade A | 335 | 12566 | -0.108 | (0.079) | -0.096 | (0.076) | -0.091 | (0.076) | -0.055 | (0.073) |
| After*Low Intensity Treat.*Grade B | 496 | 11728 | -0.099 | (0.062) | -0.092 | (0.060) | -0.090 | (0.060) | -0.081 | (0.058) |
| After*Low Intensity Treat.*Grade C | 1117 | 8821 | 0.049 | (0.046) | 0.047 | (0.045) | 0.044 | (0.045) | 0.034 | (0.042) |
| After*Low Intensity Treat.*Grade D | 1332 | 7176 | 0.016 | (0.035) | 0.000 | (0.034) | -0.001 | (0.034) | -0.018 | (0.033) |
| After*Low Intensity Treat.*Grade E | 1644 | 5377 | -0.103 | (0.044) | -0.122 | (0.042) | -0.124 | (0.042) | -0.126 | (0.040) |
| After*Low Intensity Treat.*Grade HR | 2944 | 5306 | -0.070 | (0.040) | -0.068 | (0.038) | -0.068 | (0.038) | -0.067 | (0.037) |
| After*Med. Intensity Treat.*Grade AA | 583 | 13461 | 0.003 | (0.087) | 0.002 | (0.084) | 0.002 | (0.084) | -0.016 | (0.079) |
| After*Med. Intensity Treat.*Grade A | 726 | 13419 | -0.061 | (0.067) | -0.017 | (0.064) | -0.019 | (0.063) | -0.017 | (0.060) |
| After*Med. Intensity Treat.*Grade B | 1206 | 11224 | 0.092 | (0.045) | 0.094 | (0.043) | 0.102 | (0.044) | 0.094 | (0.043) |
| After*Med. Intensity Treat.*Grade C | 1881 | 8972 | 0.081 | (0.037) | 0.056 | (0.036) | 0.057 | (0.036) | 0.037 | (0.034) |
| After*Med. Intensity Treat.*Grade D | 2575 | 7588 | 0.012 | (0.037) | 0.004 | (0.035) | 0.005 | (0.035) | 0.007 | (0.032) |
| After*Med. Intensity Treat.*Grade E | 2520 | 5637 | -0.126 | (0.038) | -0.138 | (0.036) | -0.139 | (0.036) | -0.137 | (0.034) |
| After*Med. Intensity Treat.*Grade HR | 5410 | 4875 | 0.064 | (0.036) | 0.064 | (0.034) | 0.062 | (0.034) | 0.053 | (0.032) |
| After*High Intensity Treat.*Grade AA | 254 | 11354 | 0.222 | (0.108) | 0.209 | (0.103) | 0.211 | (0.102) | 0.186 | (0.101) |
| After*High Intensity Treat.*Grade A | 382 | 10712 | 0.089 | (0.078) | 0.105 | (0.076) | 0.108 | (0.076) | 0.055 | (0.071) |
| After*High Intensity Treat.*Grade B | 374 | 10311 | 0.230 | (0.067) | 0.269 | (0.066) | 0.264 | (0.066) | 0.244 | (0.063) |
| After*High Intensity Treat.*Grade C | 741 | 8588 | 0.149 | (0.052) | 0.118 | (0.051) | 0.116 | (0.051) | 0.100 | (0.048) |
| After*High Intensity Treat.*Grade D | 957 | 7322 | -0.003 | (0.047) | -0.011 | (0.045) | -0.003 | (0.045) | -0.007 | (0.043) |
| After*High Intensity Treat.*Grade E | 1054 | 5760 | -0.078 | (0.047) | -0.098 | (0.045) | -0.099 | (0.045) | -0.100 | (0.043) |
| After*High Intensity Treat.*Grade HR | 2526 | 5337 | -0.064 | (0.039) | -0.068 | (0.038) | -0.068 | (0.038) | -0.069 | (0.036) |
| State and Week F.E. | | | ✓ | | ✓ | | ✓ | | ✓ | |
| Verified Information | | | | | ✓ | | ✓ | | ✓ | |
| Discretezation of Financial Information | | | | | | | ✓ | | ✓ | |
| Non-Verified Information | | | | | | | | | ✓ | |
| Pseudo R-Square | | | 0.105 | | 0.12 | | 0.12 | | 0.147 | |
| Num. Obs. | | | 114902 | | 114699 | | 114699 | | 114699 | |

Table 5: Treatment Effect Estimates on the Amount Requested - Tobit Model

The table contains the coefficients estimated in tobit regressions in which the dependent variable is the logarithm of the amount requested. The first two columns contain the number of listings and the average amount requested in listings posted in the five months prior to the change for each of the main groups (3 treatments * 7 credit grades). I present only coefficients of the heterogenous treatment effects for the three treatment groups. Standard errors in parenthesis are clustered by state and week.

| Dep. Variable - APR, Tobit Estimates | Five Months Before 15-Apr-08 | | (1) | | (2) | | (3) | | (4) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Num Loans | Avg. APR | dF/dx | z-stat. | dF/dx | z-stat. | dF/dx | z-stat. | dF/dx | z-stat. |
| After*Low Intensity Treat.*Grade AA | 112 | 0.100 | 0.003 | (1.58) | 0.004 | (2.23) | 0.004 | (2.12) | 0.003 | (1.59) |
| After*Low Intensity Treat.*Grade A | 97 | 0.135 | 0.003 | (2.34) | 0.004 | (2.96) | 0.005 | (3.24) | 0.004 | (2.83) |
| After*Low Intensity Treat.*Grade B | 149 | 0.156 | 0.004 | (2.8) | 0.004 | (3.49) | 0.004 | (3.48) | 0.004 | (2.94) |
| After*Low Intensity Treat.*Grade C | 190 | 0.172 | 0.003 | (2.58) | 0.002 | (2.35) | 0.003 | (2.43) | 0.002 | (2.31) |
| After*Low Intensity Treat.*Grade D | 124 | 0.195 | 0.001 | (.87) | 0.000 | (.14) | 0.000 | (.1) | 0.000 | (.36) |
| After*Low Intensity Treat.*Grade E | 44 | 0.218 | -0.003 | -(1.97) | -0.004 | -(2.38) | -0.004 | -(2.36) | -0.004 | -(2.35) |
| After*Low Intensity Treat.*Grade HR | 24 | 0.221 | -0.003 | -(1.86) | -0.003 | -(1.62) | -0.003 | -(1.62) | -0.002 | -(1.23) |
| After*Med. Intensity Treat.*Grade AA | 174 | 0.092 | 0.002 | (1.47) | 0.001 | (.97) | 0.001 | (.99) | 0.001 | (.78) |
| After*Med. Intensity Treat.*Grade A | 170 | 0.118 | 0.006 | (5.1) | 0.006 | (5.58) | 0.007 | (5.79) | 0.006 | (5.34) |
| After*Med. Intensity Treat.*Grade B | 200 | 0.142 | 0.003 | (2.98) | 0.003 | (2.3) | 0.002 | (2.02) | 0.002 | (1.61) |
| After*Med. Intensity Treat.*Grade C | 190 | 0.133 | 0.004 | (4.08) | 0.002 | (2.18) | 0.002 | (2.35) | 0.002 | (2.38) |
| After*Med. Intensity Treat.*Grade D | 65 | 0.165 | -0.004 | -(3.44) | -0.007 | -(5.13) | -0.007 | -(5.05) | -0.006 | -(4.77) |
| After*High Intensity Treat.*Grade AA | 76 | 0.080 | 0.003 | (1.8) | 0.000 | -(.05) | 0.000 | -(.11) | -0.001 | -(.72) |
| After*High Intensity Treat.*Grade A | 36 | 0.096 | -0.002 | -(1.13) | -0.006 | -(2.87) | -0.006 | -(2.9) | -0.007 | -(3.5) |
| State and Week F.E. | | | ✓ | | ✓ | | ✓ | | ✓ | |
| Verified Information | | | | | ✓ | | ✓ | | ✓ | |
| Discretezation of Financial Information | | | | | | | ✓ | | ✓ | |
| Non-Verified Information | | | | | | | | | ✓ | |
| Pseudo R-Square | | | 0.221 | | 0.392 | | 0.403 | | 0.441 | |
| Num. Obs. | | | 83534 | | 83375 | | 83375 | | 83375 | |

Table 6: Treatment Effect Estimates on the Paid Interest rate (APR) - Tobit Model

The table contains the marginal effects found in tobit regressions in which the dependent variable is the APR. The first two columns present the number of loans and the average APR paid in each group in the five months before the change. In case that a listing was not funded, I use its interest rate cap as a lower bound on the APR. I present only coefficients of the heterogenous treatment effects for the three treatment groups. Only treatment group - credit grade categories with at least 15 loans are presented. Standard errors in parenthesis are clustered by state and week.

| Dep. Variable - Default Indicator, Linear Probability Model Estimates | Five Months Before 15-Apr-08 | | (1) | | (2) | | (3) | | (4) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Num. Loans | Pr(Default) | Coef. | Std. Err. | Coef. | Std. Err. | Coef. | Std. Err. | Coef. | Std. Err. |
| After*Low Intensity Treat.*Grade AA | 104 | 0.010 | -0.013 | (0.015) | -0.009 | (0.015) | -0.008 | (0.015) | -0.015 | (0.015) |
| After*Low Intensity Treat.*Grade A | 95 | 0.021 | -0.030 | (0.033) | -0.031 | (0.033) | -0.033 | (0.034) | -0.034 | (0.034) |
| After*Low Intensity Treat.*Grade B | 146 | 0.034 | -0.055 | (0.027) | -0.058 | (0.028) | -0.061 | (0.028) | -0.061 | (0.027) |
| After*Low Intensity Treat.*Grade C | 184 | 0.016 | -0.014 | (0.022) | -0.012 | (0.021) | -0.010 | (0.021) | -0.008 | (0.021) |
| After*Low Intensity Treat.*Grade D | 121 | 0.033 | 0.017 | (0.038) | 0.011 | (0.038) | 0.010 | (0.038) | 0.012 | (0.037) |
| After*Low Intensity Treat.*Grade E | 43 | 0.023 | 0.138 | (0.073) | 0.125 | (0.074) | 0.118 | (0.073) | 0.118 | (0.072) |
| After*Low Intensity Treat.*Grade HR | 24 | 0.083 | -0.215 | (0.088) | -0.212 | (0.088) | -0.217 | (0.088) | -0.212 | (0.088) |
| After*Med. Intensity Treat.*Grade AA | 165 | 0.006 | -0.008 | (0.013) | -0.009 | (0.013) | -0.009 | (0.014) | -0.011 | (0.014) |
| After*Med. Intensity Treat.*Grade A | 167 | 0.012 | -0.018 | (0.030) | -0.014 | (0.030) | -0.013 | (0.030) | -0.013 | (0.030) |
| After*Med. Intensity Treat.*Grade B | 197 | 0.010 | -0.022 | (0.025) | -0.026 | (0.025) | -0.029 | (0.025) | -0.032 | (0.025) |
| After*Med. Intensity Treat.*Grade C | 186 | 0.022 | 0.006 | (0.023) | 0.000 | (0.023) | 0.000 | (0.023) | -0.001 | (0.023) |
| After*Med. Intensity Treat.*Grade D | 61 | 0.016 | 0.026 | (0.037) | 0.020 | (0.036) | 0.021 | (0.036) | 0.022 | (0.037) |
| After*Med. Intensity Treat.*Grade E | 13 | 0.000 | 0.095 | (0.043) | 0.083 | (0.044) | 0.077 | (0.044) | 0.076 | (0.042) |
| After*Med. Intensity Treat.*Grade HR | 30 | 0.067 | -0.121 | (0.087) | -0.124 | (0.088) | -0.125 | (0.087) | -0.123 | (0.088) |
| After*High Intensity Treat.*Grade AA | 74 | 0.000 | 0.004 | (0.013) | 0.004 | (0.013) | 0.005 | (0.014) | -0.002 | (0.015) |
| After*High Intensity Treat.*Grade A | 33 | 0.000 | 0.013 | (0.034) | 0.010 | (0.033) | 0.012 | (0.034) | 0.007 | (0.034) |
| After*High Intensity Treat.*Grade B | 6 | 0.000 | -0.009 | (0.027) | -0.020 | (0.027) | -0.023 | (0.027) | -0.033 | (0.027) |
| After*High Intensity Treat.*Grade C | 4 | 0.000 | 0.015 | (0.028) | 0.005 | (0.027) | -0.006 | (0.027) | -0.010 | (0.027) |
| After*High Intensity Treat.*Grade D | 6 | 0.000 | 0.043 | (0.036) | 0.041 | (0.036) | 0.042 | (0.036) | 0.041 | (0.038) |
| After*High Intensity Treat.*Grade E | 4 | 0.000 | 0.131 | (0.069) | 0.135 | (0.069) | 0.135 | (0.068) | 0.147 | (0.067) |
| After*High Intensity Treat.*Grade HR | 5 | 0.200 | -0.090 | (0.143) | -0.078 | (0.141) | -0.076 | (0.141) | -0.070 | (0.141) |
| State and Week F.E. | | | ✓ | | ✓ | | ✓ | | ✓ | |
| Verified Information | | | | | ✓ | | ✓ | | ✓ | |
| Discretezation of Financial Information | | | | | | | ✓ | | ✓ | |
| Non-Verified Information | | | | | | | | | ✓ | |
| Pseudo R-Square | | | 0.038 | | 0.046 | | 0.049 | | 0.054 | |
| Num. Obs. | | | 6677 | | 6671 | | 6671 | | 6671 | |

Table 7: Treatment Effect Estimates on a Default Dummy - Linear Probability Model

The table contains the coefficients found in linear probability regressions in which the dependent variable is the an indicator for a default loan. A loan is defined as default if the borrower was late in at least one of the first three payments. The first two columns present the number of loans and the proportion of default loans in each group in the five months before the change. I present only coefficients of the heterogenous treatment effects for the three treatment groups. Standard errors in parenthesis are clustered by state and week.