ADVERSE SELECTION ON MATURITY: EVIDENCE FROM ON-LINE CONSUMER CREDIT*

ANDREW HERTZBERG†, ANDRES LIBERMAN‡, AND DANIEL PARAVISINI††

Abstract. We provide evidence that borrowers with private negative information about their future creditworthiness select into long term debt. Our estimation relies on comparing the repayment behavior of two groups of observationally equivalent borrowers that took identical 36-month loans, but where only one of the groups is *selected on maturity*, in the sense that it chose the 36-month loan when a 60-month maturity option was also available. We find that borrowers who choose the short maturity loan when the long maturity loan is available default less, have higher future credit ratings, and pre-pay at a lower rate. Thus, borrowers who self-select into the long maturity loans are unconstrained in the short run but exhibit worse repayment behavior in the future. Our findings imply that loan maturity can be used to screen borrowers in consumer credit markets with asymmetric information.

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† Columbia University. Email: ah2692@gsb.columbia.edu.
‡ New York University. Email: aliberma@stern.nyu.edu.
†† London School of Economics. Email: D.Paravisini@lse.ac.uk.
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I. Introduction

Adverse selection can limit the effectiveness with which credit markets allow households to efficiently allocate consumption over time. Indeed, credit rationing can arise in equilibrium when borrowers possess private information about their own creditworthiness. The normal function of the price mechanism to equate supply and demand breaks down because lenders recognize that raising the loan interest rate will discourage the best quality borrowers from taking a loan. Lenders may mitigate adverse selection by screening observationally equivalent borrowers using a menu of loan contract terms. In theory, lenders can screen borrowers using loan amount (Stiglitz and Weiss (1981)), collateral (Bester (1987)), their willingness to agree to joint liability (Van Tassel (1999)), and by selecting a combination of coupon rates and points (Stanton and Wallace (1998)). We investigate empirically the role of loan maturity as a screening device, proposed theoretically first in Flannery (1986) and Diamond (1991).

We begin by developing a stylized model of consumer credit choice to explain how and, importantly, *when* maturity can be used to screen borrowers. In our framework, borrowers have private information about their creditworthiness in the short and long run. In this setting, long maturity debt allows households to smooth consumption over time and provides borrowers with insurance against future shocks to their creditworthiness and income.[1] When borrowers are better informed about their own creditworthiness the competitive equilibrium will offer borrowers a menu of contracts that they self select into based on their unobservable creditworthiness. Better types can credibly separate themselves by either borrowing less (Stiglitz and Weiss (1981)) or by taking shorter maturity loans. The last result parallels the Rothschild and Stiglitz (1976) result that low risk types distinguish themselves by taking less insurance. Our model demonstrates that maturity rather than quantity will be the optimal screening device when the power of a borrower's private information to predict default is increasing over time from origination.

[1]Athreya (2008) develops a model in which unsecured debt provides insurance through default. Evidence of the insurance motive of debt is present in Mahoney (2012) and Dobbie and Song (2014).

The present paper provides the first evidence that loan maturity can be used to screen borrowers in consumer credit markets. In particular, we document evidence of adverse selection on maturity: borrowers who have a privately observed lower willingness or ability to repay in the future select into long maturity loans. The main challenge in measuring how ex ante selection on maturity affects ex post repayment performance stems from the fact that maturity itself affects borrower behavior. Suppose, for example, we observe two observationally equivalent groups of borrowers, A and B, the first with a short term loan at a rate r_{ST} and the second with a long term loan at a rate r_{LT}. Suppose that borrowers in group B, that have the long term loan, default at a rate higher than the borrowers in group A with the short term loans. Although this difference in default rates may be due to adverse selection on maturity, it may also be due to the effect that a different maturity, installment amount, and interest rate have on the borrowers' repayment behavior. For this reason, identifying empirically the consequences of selection on repayment requires comparing how selected and unselected borrower samples behave when facing the *same* contract.

To illustrate this point and provide a motivation for our empircal strategy below, consider the idealized setting depicted in Figure 1. Suppose we observe two prospective groups of borrowers, A and B, before they take a loan. Group A is offered only a short maturity loan at a rate of r_{ST}. The default rate of these borrowers is γ_A^{ST}. Group B is offered two options: the same short maturity loan as group A, and a long maturity loan for the same amount at a rate of r_{LT}. Group B borrowers that choose the short term (long term) loan default at a rate γ_B^{ST} (γ_B^{LT}). Borrowers from group B who take the short term loan are *selected on maturity*: they could have taken a long term loan, but chose not to. Group A borrowers, in contrast, are an *unselected* group. Further, both group A and group B short term borrowers face identical loan terms (interest rate, amount, and maturity). Thus, any difference in the repayment of the short term loans between group B and group A borrowers, $\gamma_B^{ST} - \gamma_A^{ST}$, must be driven by the selection induced by the long maturity loan. In particular, $\gamma_B^{ST} - \gamma_A^{ST} < 0$ would indicate that less creditworthy borrowers select into the long maturity loan.

We exploit the staggered roll-out of long maturity loans by an online lending platform, Lending Club (hereafter, LC), as an empirical setting that closely resembles this idealized setting. When a borrower applies for a loan at LC she is assigned to a narrow risk category based on FICO score and other observable characteristics. Each borrower in a risk category is offered the same menu of loan choices that specifies the interest rate for any loan amount. Loans are available in $25 increments between $1,000 and $35,000 in either short—36 months—or long maturities—60 months. In January 2013 the long maturity loan was available only for amounts above $16,000. During 2013 the long term loan availability in the menu was expanded twice: 1) to loans amounts between $12,000 and $16,000 in March 2013, and 2) to loan amounts between $10,000 and $12,000 in July 2013. Crucially for our analysis, the terms of all other previously available menu items were unchanged within each risk category during this time, and the roll-out was not advertised on the LC website or accompanied by any additional marketing campaign.[2] Borrowers would only notice the new options once they began applying for a loan.

Our empirical strategy compares the default rate of short term loans between $10,000 and $16,000 issued before and after the availability of the long maturity option at the corresponding amount, which approximate groups A and B of the idealized setting of Figure 1, respectively. For example, borrowers choosing a 36-month $10,000 loan *before* July 2013 resemble those in group A of Figure: these borrowers did not have a long term option in the menu at the time of making the choice. Borrowers choosing a 36-month $10,000 loan *after* July 2013 resemble borrowers in group B: they chose the 36-month loan when a longer maturity loan was available, and are thus a sample selected on maturity. Simple before-after comparisons are potentially biased due to time-of-origination shocks. To account for these shocks we estimate a difference-in-differences specification that exploits the staggered roll-out of the long term loans, and that uses short term loans of amounts just above and just below the $10,000 to $16,000

[2]According to the information reported in the website Internet Archive, LC's website at the time of the expansion advertised that 60 month loans were available for amounts above $16,000 until November 2013.

to construct counterfactuals. We validate the identification assumptions behind our empirical strategy by documenting that the bulk of self-selection into long maturity loans occurred among borrowers that would have borrowed between $10,000 and $16,000. We find, for example, that the number of short maturity loans between $10,000 and $16,000 dropped by 17% after the long maturity loans become available, relative to loans issued at amounts just above and below this interval. Further, the decline was permanent and occurred the same month the 36-month loan appeared in the menu for the corresponding amount. Finally, to perform comparisons between observationally equivalent borrowers, our specifications include, month-of-origination, risk category, and 4-point FICO range fixed-effect.[3]

We find that the average default rate of short maturity loans decreases by 1.2 percentage points, and the average future FICO score increases by 2.5, when a long maturity loan was available at the time of origination. This implies that borrowers who are less creditworthy endogenously *self-select* out of short-term loans and into long term ones. Since the increase in performance of 36-month loans is due to the 17% of borrowers that self-select into long maturity, the estimates imply that the borrowers that selected out of the 36-month loan would have had a default rate that is 6.8 percentage points higher (1.2/0.17) than the default rate of the average 36-month borrower in our sample (9.2%). These results indicate that selection on maturity provides a powerful device for identifying, among a pool of observationally identical borrowers, those with the poorest repayment prospects.[4]

[3]Our empirical setting has several additional advantages that underline the robustness of our estimates. First, loans offered on the LC platform are funded by investors at the terms set by LC's pricing algorithm. These terms compare favorably to other investments of similar risk, thereby ensuring that all loans are filled. Moreover all terms of the 36-month loans including interest rate remain unchanged during our sample period. This rules out that selection is occurring based on supply side screening decisions or by reverse causality of loan terms impacting default. Second, LC's revenue is derived from an upfront origination fee that varies between 1.1 and 5% of a borrower's loan amount, which is subtracted from the amount borrowed. Thus, borrowers who took a short maturity loan could not costlessly refinance their loans and swap them for long maturity loans after the expansion. This ensures that the pool of borrowers who select the short maturity loan prior to the expansion is not impacted by the expansion itself.

[4]We perform a battery of robustness tests to ensure our results are not simply capturing time-of-origination varying differences in creditworthiness for loans of different amounts. First, our results are unchanged when we reduce our sample to a narrower bandwidth around the affected amounts. Second, we perform a placebo test as if the change in the expansion occurred at a different

As emphasized in the motivation of the empirical approach, measuring selection requires comparing the performance across borrowers with identical contract terms. Thus, our empirical strategy allows us to measure what the default probability of the borrowers that self-select into the 60-month loan would have been if they had taken the 36-month loan instead. We find that this default probability is much higher than that of the average 36-month borrower. One potential interpretation of this result is that these borrowers that self-select into 60-month loans were liquidity constrained and forced into default because they could not pay the larger installment amounts of the short-term contract. We discuss several pieces of evidence that indicate this interpretation cannot explain the observed default behavior of the selected sample of borrowers, and that in contrast, support the adverse selection interpretation of the results.

First, the average difference in installment amounts between 60-month and 36-month loans in the sample is around $200 per month. Since the average LC borrower has $6,000 of unused revolving credit and interest rates on consumer lending were flat during our sample period, it is very unlikely that the additional $200 in installment amount of the 36-month loan could have been binding and caused default. Second, we find that the difference in the default probability of the selected and non-selected borrowers that took the 36-month loan increases with time-since-origination. This implies that borrowers who self-select into the 60-month do not tend to default early in the life of the 36-month loan, as would occur with a borrower that is liquidity constrained today and expecting larger cash flows in the future. On the contrary, this finding is consistent with the predictions of our screening model described above: borrowers that are privately informed about poor future creditworthiness are more likely to self-select into long term loans. Adverse selection on future creditworthiness implies that the default rate of borrowers who self-select into long-maturity loans is increasing with the time since origination. Third, we find that borrowers that self-select

time keeping everything else constant. Third, we test for differences in creditworthiness in the same time period but at other amounts in the borders of the interval of affected amounts. These tests find no evidence of time-varying differences in creditworthiness.

into the 60-month loans would have prepaid their loans more often than those that chose the 36-month loan. Again, this early prepayment behavior is inconsistent with liquidity constrained borrowers. Instead, it suggests that borrowers who self-select into the 60-month loan are less creditworthy because their income is more volatile, and they are thus more likely to experience both negative shocks and default and positive shocks and pre-pay.

Studying the selection response to maturity is important for a number of reasons. First, many common consumer loan products such as mortgages, auto loans, and personal loans offer a selection of loan maturities. Second, debt maturity appears to have a large impact on other dimensions of borrower behavior: existing empirical work has demonstrated that credit demand elasticities with respect to maturity (controlling for selection) are much higher than with respect to interest rates, both for borrowers in developing economies (Karlan and Zinman (2008)) and in the US (Attanasio, Koujianou Goldberg, and Kyriazidou (2008)). Third, screening through maturity choice may be more widely applicable than through other loan contract terms that may require scarce inputs (e.g., screening on collateral), or that may affect consumption and investment decisions (e.g., screening on amounts). Finally, understanding the screening role of maturity provides an insight on the potential consequences of regulation that bans or imposes costs on long term loan contracts, such as Regulation Z in the U.S. mortgage market.

In addition to providing the first evidence that loan maturity can be used to screen borrowers, our results contribute to the literature that documents the importance of adverse selection in consumer credit markets (Ausubel (1999), Adams, Einav, and Levin (2009), Agarwal, Chomsisengphet, and Liu (2010), Dobbie and Skiba (2013), and Zinman (2014)).[5] In particular, our findings show striking evidence of adverse selection among *prime* US borrowers, a novel finding in this literature.[6]

[5]Outside of the US, the seminal methodological contribution of Karlan and Zinman (2009) was to design a field experiment to distinguish adverse selection from moral hazard using random variation in anticipated and realized loan rates. They find little evidence of adverse selection among the working poor in South Africa. Rai and Klonner (2009) use a natural experiment in South India to provide evidence of adverse selection. After the policy change borrowers are more constrained in their ability to bid on loans and they show this lowers the relationship between loan interest rate and default.

[6]Evidence of adverse selection has been provided in other markets. See for example: used cars Genesove (1993), insurance Chiappori and Salanie (2000), real estate Garmaise and Moskowitz (2004),

The rest of the paper proceeds as follows. Section II describes the Lending Club platform and the data, as well as the expansion of the supply of long maturity loans. Section III provides a simple framework to understand how long maturity loans can be used to screen borrowers in terms of their creditworthiness. Section IV describes the empirical strategy. Section V documents that borrowers who self-select into long maturity loans are less creditworthy in terms of the short term loan. Section VI interprets this finding as evidence that fundamentally less creditworthy borrowers select into longer maturity loans. Section VII concludes.

II. Setting

A. Lending Club

LC operates in 45 US states and is the largest online lending platform in the world. In 2014 LC originated $4.4B in consumer loans. By comparison its nearest rival, Prosper Marketplace, originated $1.6B in the same year.[7] LC loans are unsecured amortizing loans for amounts between $1,000 and $35,000 (in $25 intervals). LC loans are available in two maturities: 36 months, which are available for all amounts, and 60 months, which are available for different amounts at different points in time.

When a borrower applies for a loan with LC she enters the following information: a non-binding estimate of the amount to be borrowed, yearly individual income, and sufficent personal information to allow LC to obtain the credit report for the borrower. In most cases (e.g., 71% of all loans issued in 2013) LC verifies the yearly income that a borrower enters using paystubs, W2 tax records or by calling the employer. LC only issues loans to borrowers with a FICO score over 660 and a non-mortgage debt to income ratio below 35%. Using a proprietary credit risk assessment model that uses the information in a borrower's credit report (FICO score, outstanding debt) and income, LC assigns the borrower to one of 25 risk categories. This credit risk

stocks Kelly and Ljungqvist (2012), and the securitized mortgage market Agarwal, Chang, and Yavas (2012).

[7] Figures reported in the firms' 2014 10K reports.

category determines the entire menu of interest rates faced by the borrower for all loan amounts and for the two maturities. At first a borrower only observes the interest rate associated with a 36 month loan of the initial amount they select. Borrowers may accept this offer or choose to observe other borrowing options available to them, which includes loans for amounts 20%, 40%, and 60% larger and smaller than the initial amount, for 36 and available 60 month maturities. Interest rates for each subgrade are weakly increasing in amount and maturity. Apart from the interest rate, amount, and maturity, the terms of all loans are identical. Once a borrower selects a loan, the application is listed on LC's website and investors may choose to fund it.

Investors decide whether or not to fund some fraction of any available loan. Loan terms, including interest rate are fixed by LC's rating algorithm and menu and do not adjust on a loan by loan basis to reflect the supply of funds for a particular loan. Investors can review each borrower's individual information to make this decision or can select to invest in a portfolio of loans that LC selects automatically meeting desired risk and interest rate characteristics. According to LC, over 99% of all approved loans are funded by investors.[8] LC charges an origination fee that varies between 1.1% and 5% depending on credit score, which is subtracted at origination, and a further 1% fee from all loan payments made by investors. Investors receive notes that are registered with the SEC and backed by each loan they invest in. Notes can be bought and sold on an active secondary market trading platform that LC operates, thereby allowing investors to liquidate their position or re-balance their portfolio at any point over the life of the loan.

B. Staggered expansion of 60 month loans

Before March 2013, 60 month loans were only available for loans of $16,000 and above. Figure 2 documents how after March 2013 the minimum threshold for a 60 month loan was lowered, first to $12,000, and later to $10,000. The figure shows the number of 60 month loans for amounts between $10,000 and $12,000 and between $12,000 and

[8]See http://kb.lendingclub.com/borrower/articles/Borrower/What-if-my-loan-isn-t-fully-funded-when-my-listing-ends/?l=

$16,000 (closed left and open right interval in both cases). The graph shows a clear break in the number of 60 month loans between $12,000 and $16,000 on March 2013, and between $10,000 and $12,000 on July 2013.

We found no evidence that this expansion coincided with a marketing campaign or a surge in demand for LC loans. For the months including in our sample, LC's lending policy including the menu of other loans and the rating algorithm was constant. Further, using the Internet Archive website, we verified that LC did not advertise this change or mention it on its own primary web page, which suggests that the pool of applicants right before and after the expansion was fairly constant. We assess this formally by looking at LC's total issuance around the months of the expansions. Figure 3 plots the total dollar amount issued by month. There are no obvious changes in the trend of growth around the dates of the two 60 month loan expansions. Evidence we present in Section VI demonstrates that there is no evidence that prepayment is unusually high among borrowers who took a 36 month loan prior to the expansion to refinance with the newly available 60 month loans. Also, it is not possible for a borrower prior to the expansion to synthetically create a long maturity amount in the affected amounts by borrowing more and repaying the difference. Required monthly payments are unchanged by such a strategy and thus result in a a short maturity loan with a higher APR.

C. Summary statistics

LC's dataset is publicly available on its website. Our main analysis is conducted using data downloaded as of August 2014. We complement this data with an update on loan performance as of April 2015, which we match to to our main analysis dataset with the use of the unique loan ID number. The data is a cross section where the variables are measured either at the time of origination (e.g. date of loan, loan terms, borrower income and credit report data, state of residence) or at the time of the performance data download (e.g. loan status, time of last payment, current FICO score of borrower).

We select our main sample period for two reasons: 1) so that LC's lending policies remain constant during the period, and 2) to allow a reasonable pre- and post- period of time before and after the introduction of the 60 month loan options. Based on an Internet search and on our analysis of the data, we found that LC changed the model it used to assign a borrower's risk category (sub grade) in December 2012. Further, the model remained constant until the end of October 2013.[9] Hence, we limit our sample period to all 36 month loans whose "list date" variable (list_d) is between and including these two months, for amounts between \$6,000 and \$20,000, that is, \$4,000 above and below the limits of the interval of affected amounts.[10] This amount interval includes loan amounts affected by the 60 month threshold reduction (\$10,000 to \$16,000) as well as amounts just above and just below this interval to control for time varying shocks to creditworthiness and credit demand. In robustness tests, we limit the sample to loan amounts between \$8,000 and \$18,000, \$2,000 above and below the limits of the interval of amounts affected by the expansion. We use LC's publicly available information to infer each borrower's initial sub grade by reverse engineering LC's risk model to obtain our final sample of 55,784 loans.[11]

Panel A of Table 1 presents summary statistics for the 11,353 36 month loans issued by LC during the pre-expansion period in our sample, that is, between December 2012 and February 2013. On average, loans for this sub-sample have a 16.2% APR and a monthly installment of \$393. Borrowers self report that 88% of all loans were issued to refinance existing debt (this includes "credit card" and "debt consolidation"). We define a loan to be in default if it is late by more than 120 days. According to this definition, 9.2% of these loans are in default as of April 2015. Figure 7 shows the

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[9]The exact dates correspond to loans listed as of December 4, 2012 and October 25, 2013. Even though we refer to months as the borders of the interval, all our analysis consider these two dates as the starting and end points of the sample period, respectively.

[10]In some placebo tests we shift our sample to loans issued between July 2013 and May 2014. We exclude loans whose "policy code" variable equals 2 , which have no publicly available information and according to the LC Data Dictionary are "new products not publicly available".

[11]In the data, LC reports a borrower's "final" credit risk sub grade, which starts from the initial sub grade (which is unobservable) and is modified to account for a borrower's choice of amount and term. In the Appendix we detail how we reverse engineer the final sub-grade using LC's publicly available info on their credit risk model to infer the initial subgrade. We are able to assign an initial sub grade to 98.6% of all loans in the sample period.

hazard default rate by number of months since origination for loans in our sample issued in the pre-expansion period.[12] The hazard rate exhibits the typical hump shape and peaks abetween 13 and 15 months.

Panel B of Table 1 shows statistics of borrower-level variables of this sample of loans. On average, LC borrowers in our sample have an annual income of $66,470 and pay approximately 17% of their monthly income on other debts excluding mortgages. The average FICO score at origination is 695, and later credit report pulls show that the FICO score has on average decreased to 686 approximately one year later.[13] LC borrowers have access to credit markets: 56% report that they own a house or have an outstanding mortgage. The average borrower has $38,613 in debt excluding mortgage debt and $14,774 in revolving debt, which represents a 61% revolving line utilization rate. LC borrowers have on average 15 years of credit history.

III. Framework

In this section we present a theoretical framework to show how and when maturity choice will be used to screen borrowers in consumer credit markets. The existing literature shows that maturity choice can be used to distinguish corporate borrowers of unobservable creditworthiness. In these papers, screening is achieved because bad credit risks are unwilling to incur the higher transaction costs Flannery (1986) or increased chance of firm liquidation that Diamond (1991) that comes with short-term debt. The first contribution of the model presented here is to show that maturity can be used to screen borrowers when the value of long-term contracts is to provide insurance to risk averse households. Second, unlike the existing literature where the amount of lending is exogenously set, in the model presented here the amount of lending is determined endogenously along with loan maturity. This allows us to highlight when

[12] The date of default is determined by the first month when a borrower failed to make a payment.

[13] This "last FICO score" variable is updated every time LC discloses new information expect for borrowers who have fully paid their loans or who have been charged off.

maturity will be, in equilibrium, the optimal way to screen borrowers as opposed to loan amount (as per Stiglitz and Weiss (1981)).

A. Setup

The timeline of the model is shown in Figure 4. At $t = 1$ there is a continuum of observationally equivalent households. Borrowers wish to consume at $t = 1$ and $t = 3$ but have no income or wealth at $t = 1$.[14] Each household anticipates receiving risky income at $t = 3$ and this creates the desire to borrow in order to smooth consumption over time and between high and low income states. In the interim period $t = 2$, information about a borrower's creditworthiness is released in the form of a signal $S = \{G,\ M,\ B\}$ that indicates the probability a household will generate income at $t = 3$. A borrower for whom good news is released ($S = G$) will earn income of $I = E > 0$ with certainty. A borrower for whom intermediate news is released ($S = M$) has lower expected income –she will generate income of $I = E$ with probability $q \in (0, 1)$ and zero income otherwise. Finally, a borrower for whom bad news is released ($S = B$) will not generate any income at $t = 3$ with certainty. Income is not verifiable in court and therefore contracts cannot be made contingent on the realization of I.

Each borrower can be one of two types, high or low, indexed by $k \in \{H,\ L\}$. Let $\phi \in (0, 1)$ be the fraction of borrowers who are the high type. A borrower's type determines the probability with which each signal is released and hence the expected income and probability with which she will default: a borrower of type i will have intermediate news released at $t = 2$ with probability $p_k \in (0, 1)$ and bad news released at $t = 2$ with probability $x_k \in (0, 1)$ where $p_L \geq p_H$ and $x_L \geq x_H$. [15]

The supply of credit is perfectly competitive, the opportunity cost of funds is normalized to zero, and lenders are risk neutral. Lenders offer non-callable debt contracts for any amount and maturity within the three period model. Specifically, a

[14]For simplicity we abstract from consumption at $t = 2$.

[15]Note that S is a sufficient statistic for estimating a borrowers expected income and probability of default. This assumption is not necessary for our results but it simplifies the analysis by eliminating the potential for additional screening at $t = 2$.

debt contract at $t = 1$ will specify three quantities: $\{A_1, D_{1,2}, D_{1,3}\}$. $A_1 \geq 0$ is the amount received by the household at $t = 1$. $D_{1,2} \geq 0$ is the face value due at $t = 2$ and $D_{1,3} \geq 0$ is the face value due at $t = 3$. Since households do not have any income at $t = 2$ any amount due at this time must be paid out of savings or through additional borrowing. A loan made at $t = 2$ will specify two quantities: $\{A_2, D_{2,3}\}$ where $A_2 \geq 0$ is the amount received by the household at $t = 2$ and $D_{2,3} \geq 0$ is the face value due at $t = 3$ to repay this loan. The supply of loans at $t = 2$ is also perfectly competitive, and loan terms are set using the information contained in S. However we assume that, while lenders can observe S, the information it contains cannot be verified in court and hence loan contracts offered at $t = 1$ cannot be made continent on the signal.

When a loan payment is due a borrower can either make the payment or default. All loans are uncollateralized so a creditor is unable to seize any household assets upon default. We abstract from ex post moral hazard: in the event of default the household incurs a utility cost of $\Omega > 0$ that captures the inconvenience of being contacted by collection agents and the un-modeled reputation consequences of having default on the borrower's credit history. We assume that Ω is sufficiently high so as to rule out the incentive for strategic default–a borrower will repay or refinance a loan whenever possible–but small enough so as not to deter borrowing.

The objective of each household is to maximize $(1 - \alpha) u (c_1) + \alpha u (c_3)$ where $u (c_t)$ is a strictly increasing and concave utility function and α captures the relative weight that a household places on consumption in each period. Consumption is not contractible or observable by lenders. A household will allocate the funds raised at $t = 1$ between consumption and savings. Because income is risky, households have an incentive to keep a buffer stock of savings to fund consumption when income is zero at $t = 3$. Savings are risk free and earn the opportunity cost of funds (here normalized to zero). If the household does generate income at $t = 3$ they repay all loans and consume their income and savings net of any debt payments. If the face value of debt due at $t = 3$ is greater than household income (which occurs when $I = 0$) then the household will default and consume any savings.

We assume that q is sufficiently high so that in equilibrium all payments due at $t = 2$ are repaid through new borrowing when $S = G, M$. Thus, p_k captures a borrower's private information about their long-term creditworthiness–the propensity with which they can repay a loan due at $t = 3$. Conversely upon receiving the bad signal $S = B$, a household is unable to raise any new debt and hence must default at $t = 2$. Thus x_k captures a borrower's private information about their short-term creditworthiness. The distinction between the two is central to the analysis which follows.

B. Symmetric Information

Consider the benchmark case in which information about a household's type is known by all agents at $t = 1$. Under symmetric information lenders will compete to offer households a loan that maximizes their expected utility subject to breaking even. Since all fairly priced loans will offer a household expected consumption equal to their expect income, the optimal contract will be characterized by a loan that ensures households are fully insured against income risk and consume so that marginal utility is equated across time and states. As we show in Section A of the Appendix, in the unique equilibrium, each household borrows the entire present value of their expected income using a long term-debt contract.[16] Insurance is provided by the default feature of the debt contract, a feature stressed by papers such as Mahoney (2015) and Dobbie and Song (2014). Any debt contract that requires some repayment at $t = 2$ is unable to provide full insurance because the terms at which that payment is refinanced will be contingent upon the uncertain interim news about a borrowers creditworthiness revealed at $t = 2$. The level of consumption in each period is determined privately by the consumer to satisfy the Euler equation $(1 - \alpha) u'(c_1) = \alpha u'(c_3)$ subject to the budget constraint $c_1 + c_3 = A_1^{**k}$ and is invariant to both the realized level of income and the news released about the household's creditworthiness at $t = 2$.

[16]In this equilibrium, each household of type k is offered a contract at $t = 1$ of $\left\{A_1^{**k}, D_{1,2}^{**}, D_{1,3}^{**}\right\}$ where $A_1^{**k} = E\left(1 - p_k\left(1 - q\right) - x_k\right)$, $D_{1,2}^{**} = 0$, and $D_{1,3}^{**} = E$.

C. Asymmetric Information and Adverse Selection

Now suppose that a household's type is private information and so, from a lender's perspective, all borrowers are observationally equivalent at $t = 1$.[17] The credit contract that will arise in a competitive equilibrium will offer the highest expected utility to a high type borrower while ensuring that this loan is not chosen by low creditworthiness borrowers. As is standard in screening models, low type households will be offered the same contract that maximizes their utility under symmetric information. The problem that characterizes the optimal lending contract offered to the high type is shown in Section B of the Appendix. The problem is complicated by the fact that if a low type household deviates from any proposed screening equilibrium then they will privately set a a level of consumption at $t = 1$ (c_1) that is typically different to the level set by a high type contract (i.e., the "hidden savings problem"). As a result, the low type household's expected utility and the truth-telling constraint will themselves be a function of this further optimization problem. The complications of solving for the optimal screening contract in the presence of hidden savings are well known (see Kocherlakota (2004a)) and, as is often the case, make characterizing the optimal contract analytically infeasible. We first solve a special case of the model in which $\alpha = 1$ analytically, and then we study the general version of the model numerically.

First we assume all consumption occurs at $t = 3$ ($\alpha = 1$) so that $c_1 = 0$. In this case, the household's motivation for borrowing is to build a buffer stock of savings to fund consumption in the states where income is zero. The problem characterizing the equilibrium separating contract can be recast as the design of an optimal insurance contract that chooses the level of consumption achieved by the household in each state: $S = G$, M, B. Note that, conditional on reaching the intermediate signal $S = M$, the borrower will always set $D_{2,3} = E - D_{1,3}$ at $t = 2$ to fully insure against income risk . In this formulation there is a one to one correspondence between consumption choices

[17] In the Appendix we show that no pooling equilibrium exists, as per Rothschild and Stiglitz (1976), hence the focus here on the optimal separating contract is without loss of generality.

and contract terms:

$$A_1 = c_3^B \tag{1}$$

$$D_{1,2} = c_3^B + \frac{qc_3^G - c_3^M}{1-q} \tag{2}$$

$$D_{1,3} = E - \frac{c_3^G - c_3^M}{1-q} \tag{3}$$

The Lagrangian for the constrained optimization problem is shown in Section B of the appendix. At an interior solution (i.e., when the non-negativity constraint on $D_{1,2}$ and $D_{1,3}$ is not binding) the first order conditions that characterize the optimal contract are

$$c_3^G : u'\left(c_3^G\right) = \frac{\lambda_1}{1 - \lambda_2\left(\frac{1-p_L-x_L}{1-p_H-x_H}\right)} \tag{4}$$

$$c_3^M : u'\left(c_3^M\right) = \frac{\lambda_1}{1 - \lambda_2\left(\frac{p_L}{p_H}\right)} \tag{5}$$

$$c_3^B : u'\left(c_3^B\right) = \frac{\lambda_1}{1 - \lambda_2\left(\frac{x_L}{x_H}\right)} \tag{6}$$

Note that at any optimal contract, both the zero profit condition and the truth-telling constraint must bind (or else a better contract could be found for high type households) and hence the associated lagrange multipliers λ_1 and λ_2 respectively, must be strictly positive. In the symmetric information case ($p_L = p_H$ and $x_H = x_L$) we are back to the solution discussed in section B.

Now suppose that asymmetric information exists but only with regards to a households long term creditworthiness, i.e., $p_L > p_H$ and $x_L = x_H$. The first order conditions imply $u'\left(c_3^G\right) < u'\left(c_3^B\right) < u'\left(c_3^M\right)$, and since $u()$ is strictly concave, this in turn implies that $c_3^G > c_3^B > c_3^M$. That is, the level of consumption is not constant across states: it is highest in the good news state and lowest in the intermediate news state. Intuitively, following the fundamental insight of Hölmstrom (1979) relative to the full insurance contract, consumption is directed towards the states that are most informative (i.e., highest likelihood ratio) for being the high type. Examining (2) and (3), we see that

that screening is optimally achieved by lowering the duration of the loan ($D_{1,2}$ increased relative to $D_{1,3}$) and not through a variation in the level of borrowing.[18] Intuitively, short-term debt results in low consumption when $S = M$ because the household repays its debt at $t = 2$ and the credit markets price all new lending to take into account the probability of default of $(1 - q)$ at $t = 3$. Conversely since the household defaults on all loans when $S = B$, the only effect of borrowing in these states is to create a buffer stock of savings to fund consumption. Thus if a borrower's private information is increasing in the horizon from origination then debt with lower duration is the optimal screening device. Intuitively, lower duration debt redirects repayment to a horizon where asymmetric information is less severe at the cost of giving up the insurance of long term debt. The central intuition is directly analogous to the seminal paper of Rothschild and Stiglitz (1976), who show that adverse selection will result in inefficient risk sharing in insurance markets. Here, the under provision of insurance comes in the form of an under provision of loan maturity thereby exposing a borrower to shocks to her creditworthiness.

Now suppose asymmetric information exists but only with regards to a household's short term creditworthiness. Then, $p_L = p_H$ and $x_L > x_H$. The first order conditions imply $u'\left(c_3^G\right) < u'\left(c_3^M\right) < u'\left(c_3^B\right)$ and the strict concavity of $u\left(\right)$ implies that $c_3^G > c_3^M > c_3^B$. Now, the state where $S = B$ is most informative for being the low type and hence consumption is optimally driven away from that state. Condition (1) demonstrates that this is achieved by borrowing less. Intuitively,the amount of debt raised is what provides the buffer stock of savings that funds consumption in this state. The effect on debt maturity, given in conditions (2) and (3), is less obvious. Numerical solutions show that it is the effect on loan amount that dominates in this case.

Finally, we solve the model numerically and conduct a comparative static exercise that varies the relative degree information asymmetry about short and long-term

[18] To see this, the one-to-one correspondence between consumption and debt and conditions (2) and (3) imply that raising c_3^G is achieved by raising $D_{1,2}$ and lowering $D_{1,3}$. Similarly, lowering c_3^M is also achieved by raising $D_{1,2}$ and lowering $D_{1,3}$. Finally, since c_3^B is funded only by the borrowing raised at $t = 1$, holding this level of consumption fixed requires keeping A_1 constant.

creditworthiness. In particular let $p_H = \bar{p}_H + \frac{\Delta}{1-q}$ and $x_H = x_L - \Delta$, where $\bar{p}_H < p_L$ and $\Delta \in [0, (1-q)(p_L - \bar{p}_H)]$.[19] Increasing Δ lowers the degree of information asymmetry for long term creditworthiness measured by $\frac{p_L}{p_H}$, and raises the degree of information for short term creditworthiness $\frac{x_L}{x_H}$. By construction, a change in Δ leaves the expected income of a high type household unchanged hence, any change in the amount of borrowing is not mechanically driven by changes in the level of wealth. Figure 5 presents the comparative statics of the equilibrium lending contract varying Δ. The left axis measures the Macaulay duration of the contract offered to high type households at $t = 1$ (the solid line).[20] The right axis measures the total amount borrowed by the high type relative to the low type at the equilibrium contract (the dotted line). These measure the degree to which screening is achieved through maturity and quantity rationing, respectively. Panel A confirms that when the household's private information is larger about their long term creditworthiness (when Δ is low), then the optimal contract will screen borrowers using maturity. For example when $\Delta = 0$, high type borrowers take a loan that is 6.7% *larger* in size than low types, and hence are not quantity rationed at all. Instead they credibly distinguish themselves by accepting a loan with a shorter duration–here with a duration of 1.43, which indicates that 57% of the loans value is repaid at $t = 2$. The equilibrium contract continues to be characterized by maturity rationing, while the extent of information asymmetry in the long term is greater than in the short term as measured by the relative likelihood ratios: $\frac{p_L}{p_H} \leq \frac{x_L}{x_H}$. For the parameters in Figure 5 this is true when $\Delta \leq 0.0286$. When the degree of information asymmetry is higher with regard to short term credit worthiness ($\Delta \geq 0.0286$) then the equilibrium contract offered to the high type household uses loan quantity rather than maturity to screen. In the numerical example, when Δ is large, the high type household accepts a long term loan contract (duration of 2) for an amount that is 19% below the amount taken by the low type household.

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[19] These inequalities ensure that $p_H \leq p_L$ and $x_H \leq x_L$. We also only consider parameters for which $x_L \geq (1-q)(p_L - \bar{p}_H)$ to ensure $x_H \geq 0$ for all Δ.

[20] Formally: $Duration = 1 \times \frac{D_{1,2}^{*H}(1-x_H)}{A_1^{*H}} + 2 \times \frac{D_{1,3}^{*H}(1-p_H(1-q)-x_H)}{A_1^{*H}}$.

The comparative statistics presented in Figure 5 are not driven by the simplifying assumption that the household consumes only at $t = 3$ ($\alpha = 1$) and hence only has an insurance motive to borrow. The contracting problem where $\alpha < 1$, which includes the hidden savings problem, is characterized in the Appendix the numerical solutions are presented in Figure 12. In Panel A the parameters of the model are identical to Figure 5, with the only change being that households value consumption at $t = 1$ and $t = 3$ equally ($\alpha = 0.5$). Strikingly, the comparative statics with regards to maturity and quantity rationing are almost identical to those in Figure 5. Figure 12 also shows that the results are robust to changes in the parameter used for q (Panel B) and to the choice of utility function (Panel C).

The main conclusion of our theoretical framework is that loan maturity can be used to screen borrowers: we expect borrowers with lower creditworthiness to self select into long maturity loans. Further, it shows that maturity rather than quantity will be the optimal screening device when the power of their private information to predict default is increasing in the time from origination. We test this condition in our empirical setting in Section VI.

The model also implicitly demonstrates that default is not driven by a mismatch between the maturity of a loan and the timing of income. In the model, income is received only at $t = 3$, but the ability to roll over short term debt at $t = 2$ means that the riskiness of short term debt is not affected by the timing of cash-flows. Instead, the riskiness of short term debt is driven by the expected creditworthiness of a borrower at $t = 2$. Hence, the roll over risk is driven by a borrower's fundamental creditworthiness: good borrowers can refinance short term loans even if their income has not yet arrived.

IV. Empirical strategy

We exploit the staggered reduction of the amount threshold for 60 month loans during 2013 to identify selection of borrowers into long maturity loans. As prescribed in the ideal experiment, the expansion offered new menu items at longer maturities for amounts already offered on short term contracts prior to the expansion. Also, crucially, LC's risk algorithm did not change over our sample and so none of the loan terms for the menu items that existed prior to the expansion, in particular interest rate, were altered. Thus, our empirical strategy compares the outcomes of borrowers who took the short term loan before and after the menu expanded to proxy groups A and B in our idealized experiment, respectively. However, there are three important differences between our empirical setting and the idealized setting.

The first challenge we face is that the expansion occurred for all borrowers at the same point in time. If there are time-of-origination-varying differences in creditworthiness or credit demand, then borrowers who were exposed to the 60 month threshold reduction may be unobservably differentfrom borrowers who were not exposed to the reduction. We solve this problem by comparing the default rate of borrowers in the affected ("treated") amounts, \$10,000 to \$16,000, before and and after the expansion, to the same difference but for borrowers at "control" loan amounts, which are amounts just above and just below the affected range: \$6,000 to \$10,000 and \$16,000 to \$20,000. Our identifying assumption is that time varying shocks to observationally equivalent borrowers are the same for control and treated amounts.

This comparison is the basis for the second empirical challenge we face. It is possible that offering 60 month loans in amounts between \$10,000 and \$16,000 will induce selection from borrowers who in the absence of the 60 month threshold reduction would have taken a 36 month loan outside of this range. Hence, it is not immediately obvious which loans are more treated by the expansion. If selection operates in the same way at these control amounts than at the treated amounts, then this will lead us to underestimate the degree of selection. To show formally that the expansion induced selection primarily at treated amounts, we collapse the data and count the number of loans $N_{j,t,amount1000}$ at the month of origination $t\times$subgrade $j\times amount1000$ level for all loans issued during our sample period as defined above (December 2012 to October 2013).[21] Note that $amount1000$ includes all loan amounts in the same \$1,000

[21]Recall that subgrades vary from 1 through 25 and reflect LC's assessment of each borrower's creditworthiness taking into consideration observable variables. All borrowers with the same subgrade

increment (e.g., \$10,000 to \$11,000, \$11,000 to \$12,000, etc).[22] We define a dummy variable $D_{amount1000,t}$ for each loan issued in our sample period that takes the following values:

$$D_{amount1000,t} = \begin{cases} 1 & if\ \$16,000 > amount_{1000} \geq \$12,000\ \&\ t \geq March2013 \\ 1 & if\ \$12,000 > amount_{1000} > \$10,000\ \&\ t \geq July2013 \\ 0 & in\,other\,cases \end{cases},$$

and run the regression:

$$(7) \qquad log(N_{j,t,amount1000}) = \beta_{amount1000} + \delta_{j,t} + \gamma \times D_{amount1000,t} + \epsilon_{i,t}.$$

The coefficient of interest is γ, the average percent change in the number of short maturity loans originated for affected amounts (i.e., amounts in which a long maturity loan was introduced as a option). We include amount level fixed effects $\beta_{amount1000}$, which control for level differences in the number of loans for each \$1,000 amount increment. In turn, sub-grade$\times$month fixed effects $\delta_{j,t}$ control for the terms of the contract offers.[23] Results of this regression are presented in the next section.

The third challenge we face stems from the fact that the change in the 60 month loan threshold was not publicized. Therefore, borrowers are only aware of the expansion if they chose to view the full menu of loan offers instead of accepting the first 36 month loan offered to them at the initial amount they inputted. In effect, borrowers at affected amounts in the post-period who chose not to look at the full set of choices will incorrectly be labeled as treated in our analysis, thereby reducing the power of results. We do not have data on which borrowers chose to view the expanded menu. We can however find a proxy for the likelihood that a borrower viewed the expanded menu by using the fact that borrowers have a preference for using "round numbers",

face the same supply schedule–the same menu of borrowing options from LC. Our specific sample corresponds to loans whose lista date (list_d variable) is between December 4, 2012 and October 25, 2013.

[22]Results are insensitive to using actual loan amount instead.

[23]Results are qualitatively and quantitatively unchanged by collapsing the data at the month of origination t $\times amount1000$ level instead and not including $\delta_{j,,t}$ fixed effects.

i.e., multiples of $1,000 . This preference can be seen in the histogram in Figure 6 showing the number of loans by amount (i.e., in $25 increments) for all 36 month loans that are issued in the pre-expansion period of our sample (December 2012 to February 2013). Of the 11,353 loans issued during this period, roughly 60% are round numbers. This indicates that most borrowers start by entering a round number as their initial non-binding loan amount. We also know that borrowers who chose to view the menu of options see the amount they entered, as well as amounts in 20% increments higher and lower. Therefore we can infer which loan amounts are more likely to be chosen by a borrower who did or did not see the full menu. As an example, a $13,000 loan only appears on the menu for borrowers who initially asked for that amount. By contrast a $12,000 loan appears on the menu for borrowers who initially asked for loans of $30,000, $20,000, $15,000, $12,000, and $10,000. Therefore, it is more likely that borrowers who ended up selecting a $12,000 loan (as compared to a $13,000 loan) did so after seeing the expanded menu of loans available to them, and hence were more likely to be aware of which 60 month loans were available to them. Using this insight, we create a dummy variable for the following round number amounts: $10,000, $11,000, $13,000, $17,000, and $19,000 that we refer to as the "no menu" amounts. These amounts either do not appear on menus for other initial round number amounts or appear more than 60% above or below the initial amount requested, and are therefore unlikely to have been arrived at in that way.[24] Thus, we address this challenge by interacting our other control variables (in particular, the fixed effects) with this dummy. This allows for the possibility that the effect of the menu expansion was lower at these specific amounts because they are likely to contain a higher fraction of borrowers who were unaware of the expansion. All of our specifications below include these interactions, while our results measuring the number of loans are estimated on a sample that drops these "no menu" amounts.[25]

[24]Results are insensitive to modifying the definition of this dummy to include loans that appear as the second most removed borrowing option, i.e., 40% lower or higher amount.

[25]For both regression tests, our results are qualitatively unchanged if we include these amounts and if we drop the interactions.

A. Pre-trends

Our identification strategy rests on the assumption that in the absence of the reduction in the 60 month threshold there would be no difference in the change in origination of 36 month loans between treated and control amounts after March 2013 and July 2013. We test for differences in pretends by running an amended version of (7) using a series of dummies that become active τ months after a 60 month loan is offered at each amount. Formally, we define:

$$D\left(\tau\right)_{amount1000,t} = \begin{cases} 1 & if\ \$16,000 > amount_{1000} \geq \$12,000\ \&\ t = March2013 + \tau \\ 1 & if\ \$12,000 > amount_{1000} \geq \$10,000\ \&\ t = July2013 + \tau \\ 0 & in\, other\, cases \end{cases},$$

and we run the following regression:[26]

$$\text{(8)} \qquad log(N_{j,t,amount1000}) = \beta_{amount1000} + \delta_{j,t} + \sum_{\tau=-3}^{3} \gamma_{\tau} \times D\left(\tau\right)_{amount1000,t} + \epsilon_{i,t}.$$

We discuss results of this regression in Section V.

B. Regression framework to measure selection

We implement our methodology to test how borrowers with unobservable differences in creditworthiness choose loans of different maturity. To do this we implement a formal regression test on our sample of 36 month loan borrowers:

$$\text{(9)} \qquad outcome_i = \beta_{amount1000} + \delta_{j,\overline{FICO},t} + \gamma \times D_{amount100,t} + X_{i,t} + \epsilon_i,$$

where data is at the loan level i and the coefficient of interest is again γ, the change in the outcome variable for short maturity loans originated for affected amounts before and after the expansion of the menu options. We include granular month of origination

[26]The final 60 month threshold reduction takes place in July 2013, which leaves three more months in our sample period up to October 2013. Similarly, the first 60 month threshold reduction occurs in March 2013, which leaves three months in the preperiod (from December 2012).

$t \times$sub-grade $j \times$4-FICO score at origination ($\overline{FICO}$) bin $\delta_{j,FICO,t}$ fixed effects, which ensure we compare borrowers who took a loan on the same month, with the same contract terms (same sub-grade), and with very similar level of observed creditworthiness.[27] We also include a vector of control variables observable at origination, $X_{i,t,}$. In our baseline specification, $X_{i,t}$ includes US state address $\times$ month$\times$round number amount fixed effects, and annual income.[28] We also report results including the *full* set of variables that LC reports and that investors observe at origination. These variables include, for example, a dummy for home ownership, stated purpose of the loan, length of employment, length of credit history, total debt balance excluding mortgage, revolving balance, and monthly debt payments to income, among others (more than 58 variables). This regression allows us to interpret our results as demonstrating selection on unobservables at origination.

Our outcomes include $default_i$, a dummy that equals one if the loan is late by more than 120 days, and $FICO_i$, the high end of borrower's FICO score 4 point bin, both variables measured as of April 2015.[29] We also report regression results for $fullypaid_i$, a dummy that equals one if the loan has been prepaid as of April 2015. In all our regression, standard errors are clustered at the sub grade level (25 clusters).

While our analysis focuses on the expansion in the menu of loans that occurred at LC it is important to note that borrowers potentially had access to consumer credit loans with many other intermediaries. As one example, over our sample period LC's largest rival Prosper Marketplace offered loans of three and five year maturity to borrowers of similar creditworthiness. To the extent that credit markets are perfectly competitive, the availability of other five year loans will bias us towards finding no effect of the expansion. That is, borrowers who wish to select long term loans would already be

[27]These fixed effects are also interacted with the "no menu" amounts dummy defined above.

[28]We use the methodology in Guimaraes and Portugal (2010) for regressions with two high-dimensional fixed effects implemented through the REG2HDFE Stata command (as suggested in Gormley and Matsa (2014)).

[29]Our main dataset corresponds to the LC update as of September 2014. We merge these data with the latest default information using the unique ID variable. We also define a borrower to be in default if she is reported as in a "payment plan". Our results are robust to not including these borrowers as in default.

taking them elsewhere. We cannot explicitly control for the entire set of loan options available to households elsewhere. However, as long as those options did not change at the same time as the LC menu expansion and did not target the treated loan amounts ($10,000 to $16,000) relative to the control amounts then our empirical strategy will absorb these effects. We are aware of no such change elsewhere in the consumer credit market. In effect, any impact of the menu expansion at LC can also be interpreted as evidence that consumer credit markets are imperfectly competitive. This might be true because some intermediaries have a technology advantage over others which generates some market power or because there are search frictions in the market.[30]

V. Results

We first show formally that the number of 36 month loans issued at affected amounts falls relative to the number of loans at unaffected loan amounts after the expansion.. We then show that the borrowers who selected the longer maturity loans were those with a lower propensity to repay the 36 month loan. We defer an interpretation of why these borrowers are less likely to repay these loans until Section VI.

A. Selection between short and long maturity loans

Table 2 shows the results of regression (7), which tests whether borrowers who would have taken a short term loan instead borrow long term when that option becomes available. Column 1 is estimated on the full sample of borrowers who took a 36 month loan between $6,000 and $20,000 during the sample period (December 2012 to October 2013). The coefficient γ estimates the selection at the intensive margin induced by the existence of another loan option and implies that the number of borrowers who took a short term loan is 17% lower once the new long term loan option for the same amount becomes available.

[30]For evidence of search frictions in consumer credit markets see Stango and Zinman (2013).

Figure 8 shows the results of regression 8, which tests whether the decrease in the number of borrowers occurs directly at the time of the menu expansion. The results show no differential pretends in the three months leading up to the expansion and then show a discontinuous fall in the number of loans made in these amounts exactly at the time of the expansion. This rules out that our results are coming from pre-existing trends in borrower demand or composition unrelated to the menu expansion.

To further ensure that our results are not simply driven by differential trends in the demand for loans of varying amounts, we run regression (7) on a sample shifted forward by 7 months to the period of time exactly after the expansion of 60 month loans to lower amounts is concluded. That is, we shift the definition of $D_{amount1000,t}$ forward by 7 months and run the regression on the sample of loans originated between July 2013 and May 2014. Column 2 of Table2 shows the results. The coefficient on $D_{amount1000,t}$ equals -3.0% and is insignificant, and given the confidence interval we can reject the null that this coefficient equals our main estimate.

The result in Column 1 indicates that the bulk of selection induced by the menu expansion occurred at affected loan amounts (between \$10,000 and \$16,000) because this estimate measures changes relative to the control amounts. We investigate further whether the expansion induced selection away from 36 month loans in amounts above or below this range by comparing the number of loans at these unaffected amounts relative to amounts further removed from the impact of the expansion. To do this we modify our regression slightly. We modify the staggered introduction dummy $D_{amount1000,t}$ to equal 1 one after March 2013 or July 2013, the expansion months, for specific loan amounts defined appropriately for each of the following regressions.

First, in Column 3 of Table 2, the sample corresponds to all loans originated during our main sample period (December 2012 to October 2013) for amounts between \$16,000 and \$24,000. In this regression, $D_{amount1000,t}$ equals one after March 2013 for all amounts between \$16,000 and \$20,000. The coefficient on the interaction term is 5.9% and not significantly different from zero. This result confirms that the expansion of the menu did not induce selection away from short term loans above \$16,000. Given that long maturity loans were always available for these amounts, this is not a surprising result. In Column 4, we shift the sample to all loans originated during our main sample period for amounts between \$2,000 and \$10,000, and we define $D_{amount1000,t}$ for this specific regression to equal one after July 2013 for amounts between \$6,000 and \$10,000 and zero otherwise. The coefficient on the interaction term is -7% and, again, not significantly different from zero. Thus, there is no evidence that borrowers who in the pre-period selected a short maturity loan below \$10,000 would have taken a large long maturity loan above the \$10,000 threshold when they became available in July. Taken together the results in Column 1, 3 and 4 confirm our conjecture that the bulk of any selection induced by the expansion of the menu was the draw borrowers in the treated amounts to longer maturity loans.

B. Screening using maturity choice

We now measure whether borrowers who selected into the long term loans when they were available are systematically different in their propensity to repay the 36 month loan from those who chose the short term contract. Table 3 reports results of regression (9). Columns 1 through 3 show that borrowers who took a 36 month loan after the 60 month loan option was available for the same amount are significantly less likely to default and have significantly higher FICO scores in the future relative to before the option was available and relative to borrowers of slightly larger and smaller amounts. Column 1 reports the result of our main outcome variable, $default_i$: the coefficient of interest equals -1.16% and is significant at the 1% level. Note that as per Column 1 in Table 2 the expansion of the 60 month loan option reduces demand for the short term loan by roughly 17%. Thus, the average default rate on the 36 month loan of the 17% of borrowers who chose to take a 60 month loan when it became available must be $^{1.16\%}/_{17\%} = 6.8\%$ higher than those who chose instead to take the 36 month loan. To get an idea of the economic magnitude of this effect, note that as per the summary statistics shown in Table 1, the average default rate of 36 month loans issued between December 2012 and February 2013 is 9.2%.

We study whether the lower default rate of borrowers who selected a short maturity loan could be predicted by variables available to investors at the time of origination. This allows us to confirm that our results are indeed capturing screening of borrowers based on unobservable creditworthiness. Note that our main result already controls in a very granular manner for month of origination by sub grade by 4-point FICO score bin fixed effects, as well as by state of residence by month fixed effects. In Column 2 of Table 1 we run the same regression as in Column 1 but adding every single variable known at origination that is available in LC's dataset as a control in the right hand side. The results are striking: the coefficient only goes down from -1.16% to -1.03%, and remains statistically significant at the 5%. This suggests that maturity choice reveals truly unobserved heterogeneity that cannot be priced in by the lender.

We do not measure the borrower's exact FICO score at origination. Instead, LC provides in its data a 4-point range–all our regressions and fixed effects are calculated using the high end of each range bin. This is potentially a problem if our default regression simply captures selection along FICO scores *within* each 4-point FICO bin. In that case, we could not interpret our results as evidence of self selection by FICO score not unobserved creditworthiness. While this is a theoretical possibility, we find that the effect of FICO on default in our sample is too small to account for our results. Indeed, a regression of $default$ on the high end of the FICO range at origination within each sub grade by $1,000 amount range by month gives a coefficient of -0.0000362 (i.e., a 1 point increase in FICO score at origination is correlated with a 0.004% decline in default rate, not statistically significant). Thus, variation in default rates within FICO score bins can at most account for a 0.012% difference in default rates (0.004% $\times 3$), quantitatively irrelevant next to our estimated effect of 1.2% reduction in default.

Next we use a borrower's future FICO score as outcome. Note that a borrower's FICO score aggregates repayment on a borrower's entire set of liabilities, including LC. Thus, if borrowers prioritize the repayment of some debts over others, lower default rates on LC debt would not necessarily translate into higher FICO scores. E.g., if long and short maturity borrowers prioritize repayment of different debts, the negative effect of defaults at LC on FICO scores would cancel out. Column 3 of Table 11 shows the results of regression (9) when the outcome is a borrower's FICO score pulled at the time we downloaded our dataset (April 2015). The coefficient implies that on average, borrowers who chose the short term loan have a 2.5 higher FICO score in subsequent data updates. In economic terms this means that the average FICO score of the 17% of the total pool of pre-expansion short term applicants who became long term borrowers is $^{2.5}/_{17\%} = 14.7$ points higher. This higher FICO score may, for example, result in better access to credit and labor markets. This result is consistent with our framework and with theories that emphasize borrowers own assessment of their ability to refinance short term debt in the future.

C. Robustness

We present in Table 4 several tests that demonstrate the robustness of our results. First, Columns 1 through 3 of Table 4 present counterparts to our main results (Table 2 Columns 1, 2, and 3) limiting the sample to loan amounts between \$8,000 and \$18,000 (our main sample uses loans from \$6,000 to \$20,000). The results are qualitatively and quantitatively unchanged.

Second, we study whether our results could be driven by differential changes in creditworthiness of borrowers at different amounts that are unrelated to the reduction of the long maturity threshold. Column 4 runs our main test in a placebo sample of 36 month loans for amounts between \$6,000 and \$20,000, just as in our main results, but listed between July 2013 and May 2014 (7 months after our main sample period), after the 60 month expansion has been completed. For these regressions, we also shift forward the definition of our variable of interest by 7 months in the following manner:

$$D_{amount1000,t} = \begin{cases} 1 & if\ \$16,000 > amount_{1000} \geq \$12,000\ \&\ t \geq October2013 \\ 1 & if\ \$12,000 > amount_{1000} > \$10,000\ \&\ t \geq February2014 \\ 0 & in\, other\, cases \end{cases} \cdot$$

The results show that the coefficient on $D_{amount1000,t}$ is positive, small, and insignificant, which contrasts sharply with to columns 1 Table 3, where the coefficient is negative and strongly significant. Even though the entire sample of placebo loans has had less time to default than our main sample, the magnitude and sign of the effect are completely different from our main result. This suggests that our results are not driven by time-of-origination secular trends in creditworthiness for different loan amounts.

Next we return to the concern that the menu expansion may have induced selection in the unaffected or control group of amounts, above and below the \$10,000 to \$16,000 interval. Table 2 showed that the number of loans in the control amounts did not change but it is important to ensure that there is no change in the credit quality in the control group induced by the menu expansion. Column 5 of Table 4 restricts the sample to loans issued between December 2012 and October 2013, between \$16,000 and \$24,000. The independent variable of interest equals one for loans between \$16,000 and \$20,000 after March 2013. The estimated coefficient equals 1.4% and is not statistically significant. Even though the point estimate is not significant, its magnitude is large, and suggests an increase, albeit not significantly different from zero, of the default rate of loan amounts slightly higher than \$16,000. One concern is that the sample of borrowers just above the affected amounts became unobservably less creditworthy at the time of the 60 month expansion for reasons unrelated to the expansion, which would confound our main results. However, in unreported results available on request, we find that the coefficient γ of regression (9) is negative and significant even when we restrict the sample to loans between \$6,000 and \$16,000. This confirms our results are not driven by this potentially confounding fact.

Finally, column 6 of Table 4 repeats the exercise for loans between \$2,000 and \$6,000 issued between December 2012 and October 2013. Here, the independent variable of interest equals one for loans between \$6,000 and \$10,000 issued after July 2013. The coefficient is negative but insignificant. If anything this can be taken as weak evidence that some selection occurred away from these loans to long maturity loan in larger amounts when the menu expanded and that as in the group of affected amounts it was unobservably worse borrowers who self-selected into these loans. This implies that our main estimates, which compare affected amounts to amounts that are slightly lower, may underestimate the extent of selection induced by the long maturity. Overall, these tests point to a robust conclusion: borrowers who choose a long maturity loan when it is available are unobservably more likely to default on a 36 month loan. The results in column 5 and 6 of Table 4 also serve as placebo tests and confirm that our results are not spuriously driven by shifting creditworthiness at different loan amounts.

A potential alternate explanation for our results is the possibility that less creditworthy borrowers are more likely to choose to see all their borrowing options and hence be aware of the long maturity loan. We test for this possibility by regressing default on a dummy that equals one for the "no menu" amounts as defined on section II, on the sample of 36 month loans for multiples of \$1,000–e.g., \$6,000, \$7,000, \$8,000, etc— issued between December 2012 and February 2013, and controlling for loan amount, APR, and $t\times$sub-grade $\times$4-FICO score at origination bin fixed effects. Recall that the "no menu" amounts correspond to loan amounts where borrowers are less likely to have seen the menu of options available to them. The results, presented in the Supplemental Appendix Table 6, show that the coefficient on the initial menu amounts dummy is positive but small (equal to 0.005) and insignificant. That is, on average, borrowers who are more likely to have seen the full menu of borrowing options (i.e., borrowers for whom the value of the initial menu amount dummy is zero) are equally or more creditworthy than those that did not. This suggests that our results are not driven by the propensity of borrowers of different creditworthiness to see the full menu of borrowing options available to them.

VI. Interpretation of Results

In order to isolate the effect of selection our analysis has focused on the propensity to default holding the terms of the contract constant. Therefore our results so far have shown that borrowers who selected the long term loan are systematically more likely to default *on a 36 month loan* than those borrowers who chose the short term loan when both were available. We now turn to the question of how this result should be interpreted. We argue that this difference stems from fundamentally less creditworthy borrowers selecting into longer maturity loans.[31] An alternate hypothesis is that borrowers select into loans that better suit their income profile over time. Borrowers who expect to receive income further into the future may select longer maturity loans. This alternate hypothesis can only explain an increased propensity to default on short term lending if borrowers are unable to rollover their short term loans with new short term lending in the future for reasons that are unrelated to their creditworthiness. We now present several pieces of evidence, all of which contradict this alternate hypothesis and lead us to conclude that our results indicate that maturity can be used to screen borrowers based on fundamental creditworthiness.

First, as indicated in Table 1 the borrowers in our sample have substantial access to credit. The median borrower in the sample has total non-mortgage debt of \$29,904, debt repayments to income ratio of 17%, and has 37.1% of the their revolving credit balances unutilized at the time of origination. This indicates that the borrowers in our sample are likely to have ample access to credit in the future if they need to roll over short term lending until income arrives. In addition, as long as they remain in good standing, borrowers could access an additional loan at LC in the future if needed. A sequence of short term loans leaves a creditworthy borrower with long duration income subject to rollover risk if the market price (or credit supply) is adversely shocked in the future. Figure 10 shows that consumer credit interest rates in the US were essentially unchanged in the years that followed our sample period and hence rollover risk cannot account for our results.

The alternate hypothesis suggests that borrowers select long term loans do so because their ability to make loan repayments is low in the short term and increasing over time as their income gets closer. We can test this by looking at how selection into long maturity loans induces a propensity to default at different horizons from origination.

[31] We use the term "creditworthiness" to denote the ability of a borrower to repay a loan, not the utility they would derive from a loan.

We redefine our baseline measure of default and create two variables for default at different horizons for loans that are 120 days past due in April 2015 and who missed their first payment within the first 12 and 18 months of loan origination, respectively. We label these variables $default12m$ and $default18m$ respectively and use them as dependent variables in regressions that are otherwise identical to the one we ran in Column 1 of Table 3. The results are presented in Columns 1 and 2 of Table 5. Column 1 shows that borrowers who self-select into long term loans have no differential propensity to default within the first year of the loan. Recall that Figure 7 showed that the hazard rate of default in our sample peaks at 13 months, so this cannot be mechanically driven by a lack of statistical power due to a low frequency of default early in the life of the average loan. Column 2 shows that the differential propensity to default is present at the 18 month horizon from origination. Figure 9 plots the coefficient from a sequence regressions run in the same manner using default from the first to the 24th month from origination.[32] The figure shows a striking pattern: borrowers who select into the 60 month loan have a propensity to default on the 36 month loan that is *increasing* in the time since origination of their loan. This is exactly the opposite of what the alternate hypothesis would predict.

We add to this evidence in Column 3 of Table 5, where we run our main regression using a dummy for loan pre-payment as of April 2015 as the outcome variable. The coefficient is -1.4% and is statistically significant: thus, borrowers who self-select to borrow long term have a higher propensity to prepay their short maturity debt that is ${}^{1.41\%}/_{17\%} = 8.3\%$ higher than those who chose instead to take the 36 month loan. Table 1 shows that 38% of all loans have been pre-paid as of April 2015 and hence an increase of 8.3% is economically large relative to the average.[33] Combining the higher propensity to repay and the increased propensity to default over time strongly contradicts an explanation of our results stemming from borrowers selecting long maturity loans to

[32]At horizons of 19 months and further the sample used to run the regression is right censored because loans issued late in our sample do not have sufficient time to enter default at these horizons. This affects loans in the treatment and control amounts in the same way and does not affect the identification strategy.

[33]Further, in unreported results, the pre-payment hazard rate peaks around 20 months.

suit longer duration income profiles. Instead, it suggests that borrowers who select long maturity loans are less creditworthy because their income profile is more volatile: they have a higher probability of defaulting, but they may also experience a positive shock that allows them to pre-pay early.

A more direct test that selection is based on fundamental creditworthiness would be to measure the default rate of the borrowers who selected the long maturity loan and compare this to their default rate at the 36 month loan. Such a measure is however not possible in our setting because the default rate at the long maturity loan is also driven by selection at the *extensive* margin, from borrowers who selected to take no loan prior to the expansion. In our setting, we cannot produce an independent measure of the propensity to default of these borrowers. Notwithstanding this problem we can compute the average default rate for borrowers in the same risk category at LC at the 60 month loan. The propensity to enter default by April 2015, which holds the repayment horizon equal across loans, is higher for the 60 month loans by 3%.[34] Commensurate with this increased risk, LC charges observationally equivalent borrowers who select a 60 month loan an APR that is is on average 3.3% higher. These stylized facts are consistent with the findings of Dobbie and Song (2015) who use a randomized experiment on US household credit card borrowers to show that increased maturity does not causally change a borrowers propensity to default. Since this experiments explicitly removes the effect of selection, it provides further evidence against the alternative hypothesis that long maturity loans help borrowers with more distant income avoid default.

Taken together the evidence points strongly towards an interpretation of the results that coincides with our theoretical framework: loan maturity can be used to screen borrowers based on their fundamental creditworthiness. Our framework also showed that maturity, as opposed to loan quantity, is the optimal way to screen borrowers if the power of the private information they possess about their ability to repay is increasing in the horizon from origination. The pattern in Figure 9 serves as a direct test of

[34] This number is estimated as the coefficient on a regression of $default$ on a dummy for 60 month loans, controlling for subgrade $\times$month$\times$FICO bin, and by $1,000 amount bin.

this condition and confirms that it is indeed satisfied in our setting. Shorter maturity loans screen borrowers effectively by concentrating a greater share of repayment in the months closer to origination, when the information asymmetry between lender and borrower is far lower. In fact our results indicate that there is no information asymmetry between borrower and lender over their ability to repay in the first year of the loan. Since worse borrowers were selected away from the short maturity loan by the menu expansion, our theory would predict that competitive pressures would eventually drive the interest rate on the short maturity loan down to reflect this. After our analysis sample period (during which all lending terms were held constant), LC adjusted the APR of the 36 month loan in a way that is consistent with this. We show this in Figure 11 which shows the average APR charged to borrowers on 36 month loans in each month controlling for loan amount and borrower characteristics.[35]After controlling for these observable characteristics we see that the APR fell, as theory suggests, by roughly 0.8%. This number is similar to our estimate in Column 1 of Table 2 that showed the expected default rate of the 36 month loans fell by 1% as a result of the selection into long maturity loans.

VII. Discussion

We have documented that loan maturity may be used to screen borrowers based on unobserved creditworthiness in US consumer credit markets. Borrowers who are unobservably less creditworthy self-select into longer maturity loans. We provide a framework that rationalizes this finding and demonstrates that screening borrowers using maturity as opposed to loan quantity is optimal when the power of their private information to predict default is increasing over time from origination. We confirm that this condition is indeed true in our empirical setting. Our analysis contrasts with the bulk of work since Stiglitz and Weiss (1981) that has focused on quantity rationing

[35]These characteristics are FICO score bin, annual income, and address state. Note that variation in APR before November 2013 in this graph is entirely accounted for by the fact that we do not control for the borrower initial subgrade, which we cannot estimate after October 2013. This also implies that we are unable to simply compare the APR for the 36 month loan at each menu.

as the primary cost of adverse selection. Our results indicate that maturity rationing may be at least as empirically important and may may account for the welfare costs of asymmetric information in consumer credit markets.

Our results provide one rationale for upward sloping rates along the maturity dimension. Further, they may help explain the success of online lending platforms such as LC or Prosper, which derive most of their business from making three to five year loans to prime borrowers who want to refinance their longer term consumer debt. Indeed, these sites may be using shorter maturity loans to cream skimming good borrowers who do not want to pay high interest rates on their credit card debt which is, effectively, a very long maturity zero collateral loan.

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Appendix

Appendix A. Figures and Tables

Figure 1. Description of variation

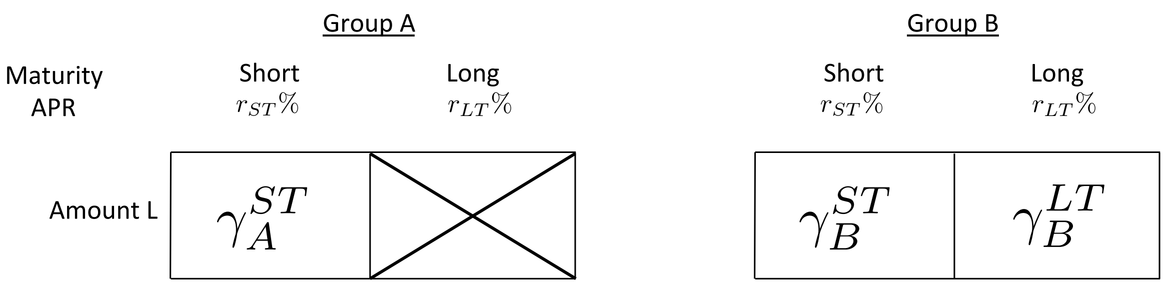


Figure 2. Staggered expansion of 60 month loans

This figure shows the time series of the number of 60 month loans by listing month for \$10,000 to \$12,000 and \$12,000 to \$16,000.

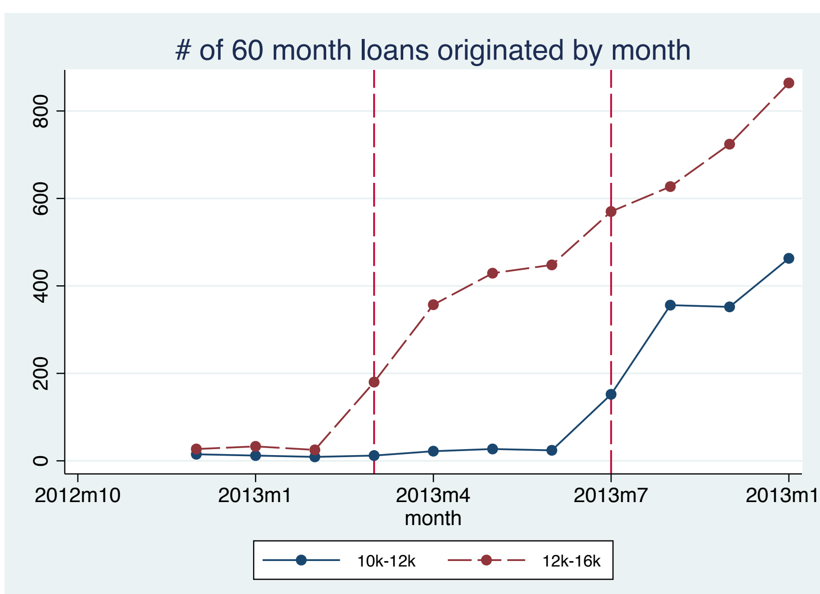


FIGURE 3. Total $ amount issued by LC by month of listing

This figure shows the time series of total $ amount of LC loans (of both maturities) by listing month since 2012. The vertical dashed lines show the two months in which the 60 month loan minimum amount was reduced.

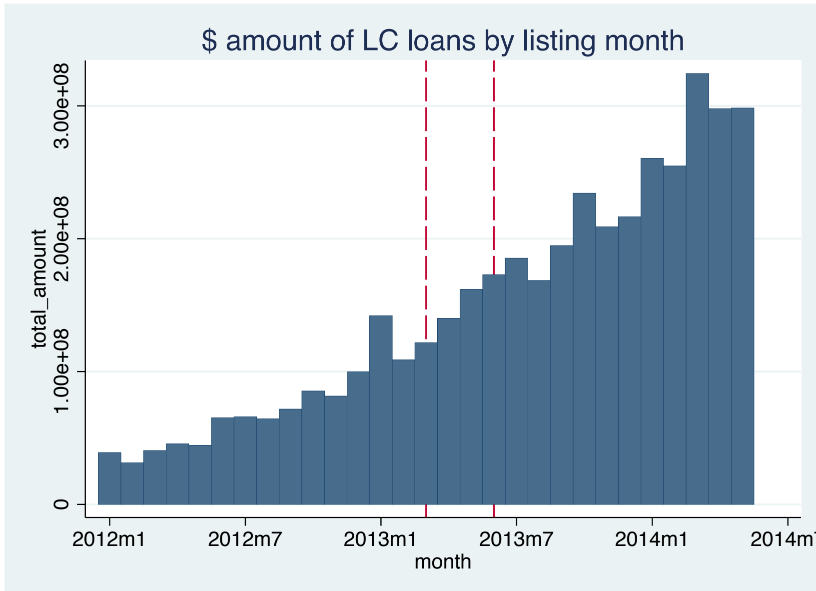


FIGURE 4. Model Time-line

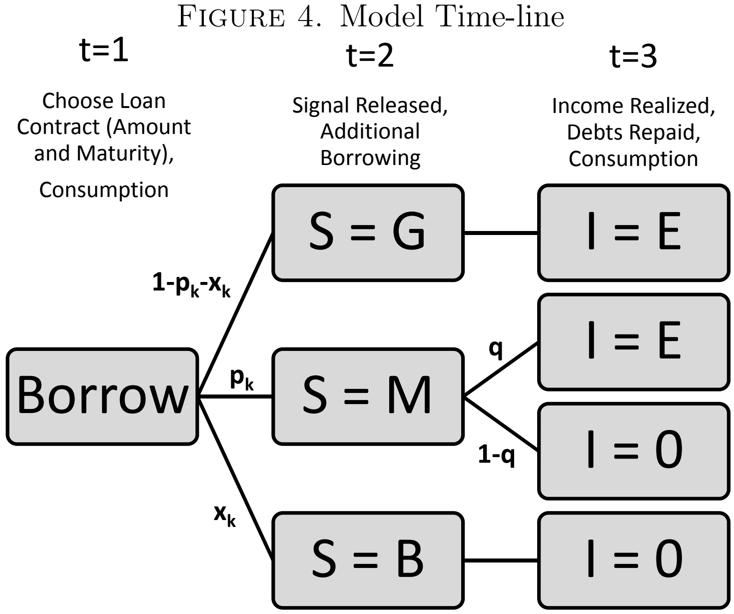


FIGURE 5. Model Comparative Statics

This figure shows comparative statics from numerical solutions of the theoretical framework presented in Section III. The household utility function is assumed to be CARA: $u(c) = 1 - \frac{1}{\eta}e^{-\eta c}$. The following parameter assumptions are used: $E = 100$, $p_L = 0.3$, $x_L = 0.1$, $q = 0.75$, $\bar{p}_H = 0.1$, and $\eta = 0.1$. The model is solved for the special case where$\alpha = 1$ and hence the household consumes only at $t = 3$. The left axis in each panel shows the degree of maturity rationing as captured by the Macaulay duration of the equilibrium loan offered to the high type:
$Duration = 1 \times \frac{D_{1,2}^{*H}(1-x_H)}{A_1^{*H}} + 2 \times \frac{D_{1,3}^{*H}(1-p_H(1-q)-x_H)}{A_1^{*H}}$. The right axis in each panel shows the degree of quantity rationing as captured by the ratio of the amount lent to high and low type borrowers at $t = 1$: $\frac{A_1^{*H}}{A_1^{*L}}$.

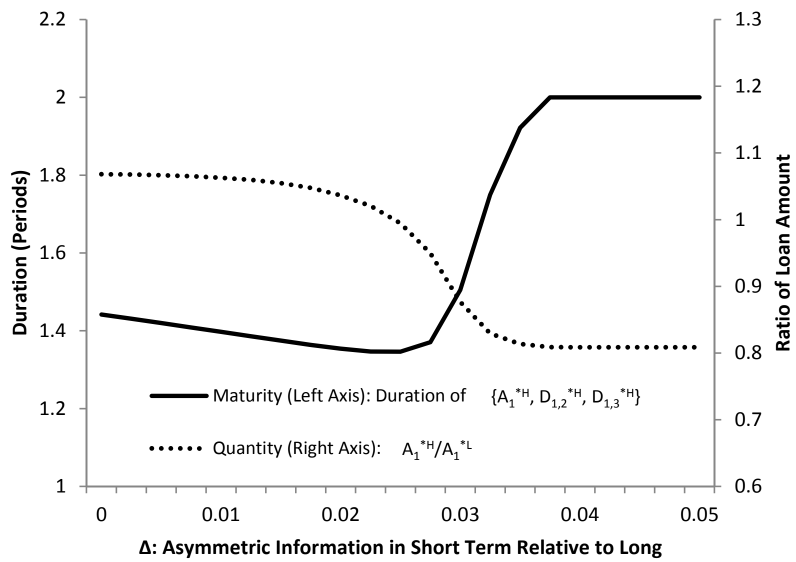


FIGURE 6. Preperiod loan amount histogram

This figure shows the number of 36 month loans issued by LC in $25 increments, between $6,000 and $25,000 between December 2012 and February 2013.

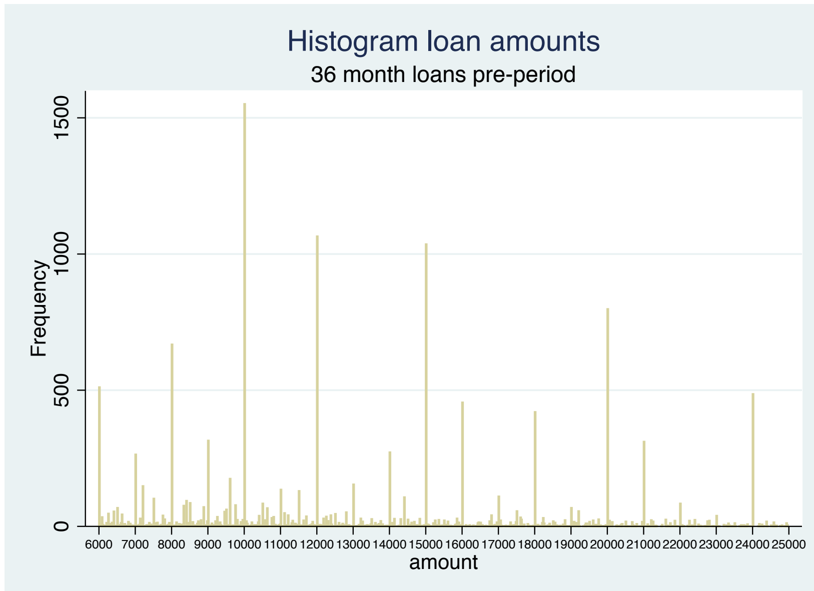


FIGURE 7. Hazard rate of default

This figure shows the hazard rate of default by month since origination for 36 month loans issued by LC in amounts between $6,000 and $20,000, between December 2012 and February 2013. A loan is in default if payments are 120 or more late on April 2015. The timing of default is the month, measured as time since origination in which payments were first missed. The hazard rate at horizon t is the number of loans that enter default at that horizon as a fraction of the number of loans that are in good standing at $t-1$.

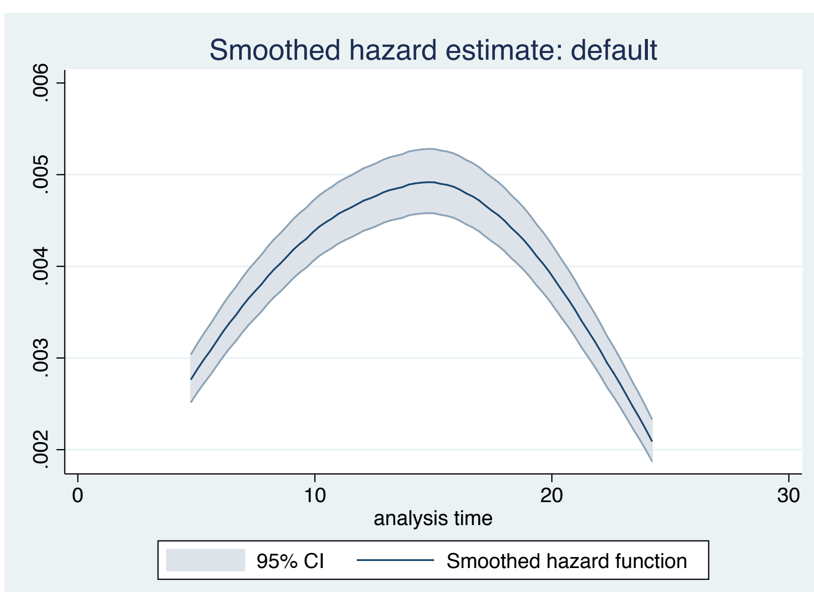


FIGURE 8. Pre-trends on number of loans originated

This figure shows the regression coefficients (γ_τ) and 95% confidence interval of regression:

$$log(N_{j,t,amount1000}) = \beta_{amount1000} + \delta_{j,t} + \sum_{\tau=-3}^{3} \gamma_\tau \times D\left(\tau\right)_{amount1000,t} + \epsilon_{i,t},$$

which measures the difference in the number of loans issued between treated and control amounts τ months after the threshold expansion. Standard errors are clustered at the subgrade level.

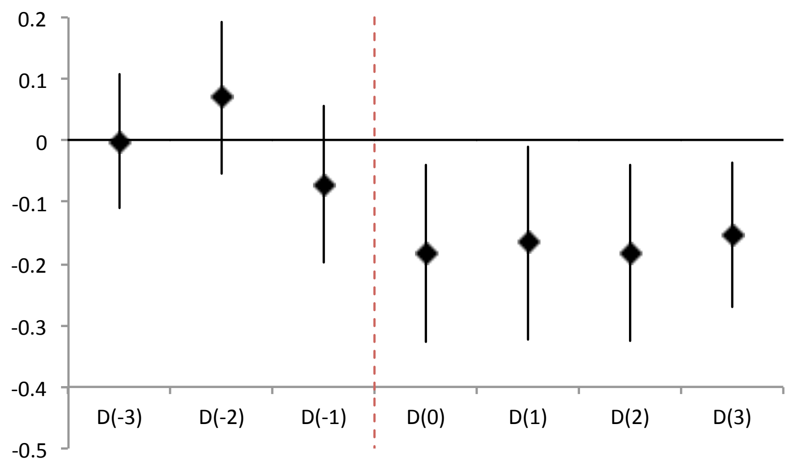


FIGURE 9. Default rate coefficient by number of months since origination

This figure shows the estimated coefficient and 90% confidence interval of the regression:

$$default\left(\Delta t\right) = \beta_{amount1000} + \delta_{j,\overline{FICO},t} + \gamma \times D_{amount100,t} + X_{i,t} + \epsilon_i,$$

where the outcome is $default\left(\Delta t\right)$, a dummy that equals one if a loan is not current by more than 30 days as of April 2015 and if the last payment on these loan occured Δt months after origination, on $D_{amount1000,t}$, a dummy that captures the staggered expansion of the 60 month loans for amounts above \$12,000 and \$10,000 on March and July 2013, respectively. Standard errors are clustered at the subgrade level. Sample includes loans issued between December 2012 and October 2013, for loan amounts between \$6,000 and \$20,000.

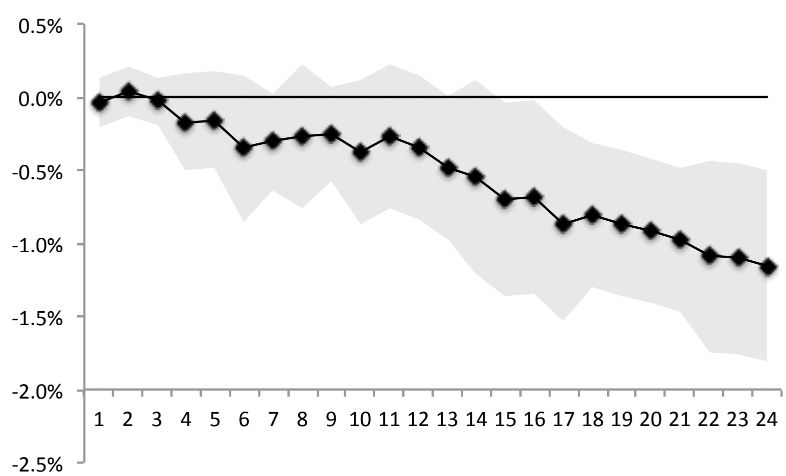


FIGURE 10. Consumer credit interest rates

This figure shows the time series of the commercial bank interest rate on credit card plans, all accounts; not seasonally adjusted. Data source: Board of Governors of the Federal Reserve System. Data series G.19 Consumer Credit, Terms of Credit.

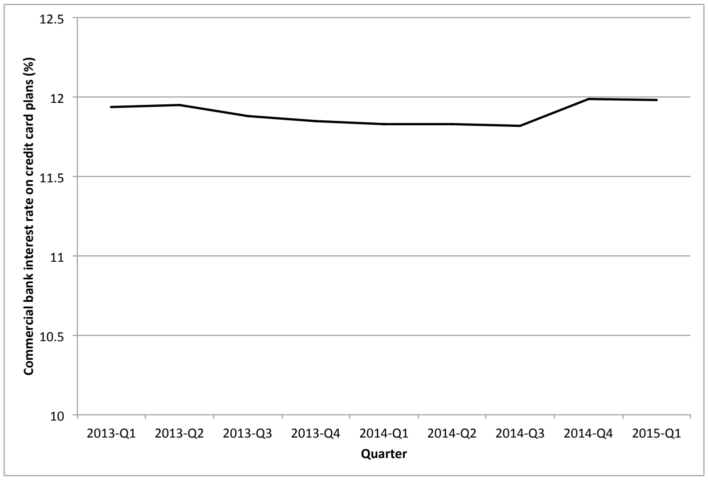


FIGURE 11. Reduction in APR

This figure shows the time series of the predicted residual of a regression of loan APR on $1,000 amount dummies, FICO score bin dummies, annual income, and address state dummies, by month of origination.

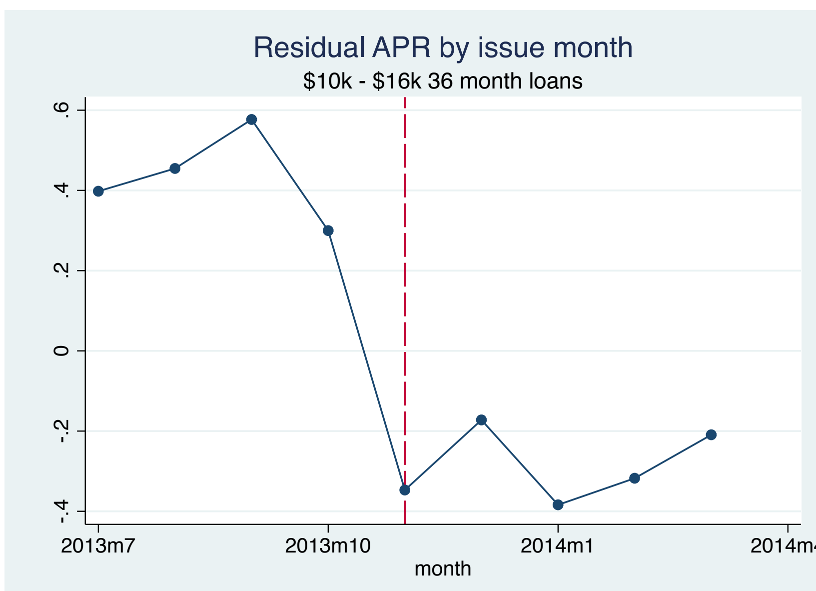


Table 1. Summary statistics

This table shows summary statistics of the main sample of Lending Club borrowers for pre-expansion months, which includes all 36 month loans whose listing date is between December 2012 and March 2013, for an amount between $6,000 and $20,000, and for which we estimate an initial subgrade based on LC's publicly available information.

	mean	p50	sd
Panel A: loan characteristics			
APR (%)	16.2	16.0	4.0
Installment ($)	392.8	373.9	118.0
For refinancing (%)	87.8		
Default 120 days (%)	9.2		
Fully paid (%)	37.5		
Panel B: borrower characteristics			
Annual income ($)	66,475	58,000	76,267
Debt payments / Income (%)	17.4	17.0	7.7
FICO at origination (high range of 4 point bin)	695	689	26
FICO at latest data pull (high range of 4 point bin)	686	699	70
Home ownership (%)	56.2		
Total debt excl mortgage ($)	38,613	29,904	33,978
Revolving balance ($)	14,774	11,848	12,614
Revolving utilization (%)	60.9	62.9	21.8
Months of credit history	182	165	84
N		11,353	

Table 2. Regression results: selection into long maturity loans

This table shows that selection into the new 60 month options was higher among borrowers who would have selected a 36 month loan of the same range of amounts as the new 60 month options. The sample corresponds to loan amounts between \$6,000 and \$20,000 whose list date is between December 2012 and October 2013, dropping “no menu” amounts as defined in the text. Column 1 shows the coefficient of the regression of the logarithm of the number of loans at each month, credit risk sub-grade, and \$1,000 amount interval level, on a dummy that equals one for loan amounts at which the 60 month maturity loan was first not available and then made available, and zero otherwise. Column 2 reports the tests of a Placebo sample, which includes loan amounts between \$6,000 and \$20,000 issued between July 2013 and May 2014. Columns 3 and 4 show the regression results on different samples where we re-define $D_{amount1000,t}$ in an ad-hoc manner for each column. Column 3 restricts the sample to 36 month loans issued in the main sample period for amounts between \$16,000 and \$24,000; $D_{amount1000,t}$ is defined as one for loan amounts between \$16,000 and \$20,000 on and after March 2013, and zero in other cases. Column 4 restricts the sample to 36 month loans issued in the main sample period for amounts between \$2,000 and \$10,000; $D_{amount1000,t}$ is defined as one loan amounts between \$6,000 and \$10,000 on and after July 2013 and zero in other cases. Standard errors are clustered at the initial credit risk sub grade (25 clusters). *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
	$log\,(\#loans)$	$log\,(\#loans)$	$log\,(\#loans)$	$log\,(\#loans)$
$D_{amount1000,t}$	-0.1707***	-0.0304	0.0586	-0.0696
	(0.035)	(0.032)	(0.060)	(0.056)
Sample	Main	Placebo	36m, 16k - 24k	36m, 2k - 10k
Observations	3,300	3,531	1,637	2,140
R^2	0.813	0.842	0.802	0.782
# clusters	25	25	25	25

TABLE 3. Regression results: screening with maturity

This table shows that the default rate of borrowers who selected into a short term loan when they could take a long term loan is higher than borrowers who could not take a long term loan. The table shows the output of the regression of each outcome on a dummy for the staggered reduction of the minimum amount threshold for long maturity loans on March 2013 (to $12,000) and July 2013 (to $10,000). Outcomes include $default$, a dummy that equals one if a borrower is late by more than 120 days;$FICO$, which measures a borrower's FICO score. All variables are measured as of the April 2015 LC data update. The sample corresponds to loan amounts between $6,000 and $20,000 whose listing date is between December 2012 and October 2013. All regressions include sub grade $\times$4-point FICO bin $\times$ month $\times$round number dummy, and US state $\times$ month dummy fixed effects. Column 2 includes all borrower level variables observed by investors at the time of origination as controls. Standard errors are clustered at the initial credit risk sub grade (25 clusters). *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)
	$default$	$default$	$FICO$
$D_{i,t}$	-0.0116***	-0.0103**	2.5122*
	(0.004)	(0.004)	(1.360)
Sample	MAIN	MAIN	MAIN
Observations	55,784	52,949	55,784
R^2	0.167	0.175	0.308
# clusters	25	25	25

Table 4. Robustness

The table shows the output of several robustness tests. Columns 1 through 3 replicate columns 1 through 3 in Table 3 on a sample of loans listed between December 2012 and October 2013 and issued for amounts between $8,000 and $18,000. Column 4 shows the output of the regression when the outcome is $default$ for a placebo sample of loan amounts between $6,000 and $20,000, same as in our main sample, but listed between July 203 and May 2014. Columns 5 and 6 report the output for regressions ran on a sample of loans listed between December 2012 and October 2013 for different loan amounts, where the independent variable is defined in an ad-hoc manner. Column 5 restricts the sample to 36 month loans issued in the main sample period, for amounts between $16,000 and $24,000; $D_{i,t}$ is equal to one for loan amounts between $16,000 and $20,000 listed on or after March 2013, and zero otherwise. Column 6 restricts the sample to 36 month loans issued in the main sample period for amounts between $2,000 and $10,000; $D_{i,t}$ is equal to one for loan amounts between $4,000 and $10,000 listed on or after July 2013, and zero otherwise. All regressions include sub grade $\times$4-point FICO bin $\times$ month, and US state $\times$ month fixed effects. Column 2 includes all borrower level variables observed by investors at the time of origination. Standard errors are clustered at the initial credit risk sub grade (25 clusters). *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	$default$	$default$	$FICO$	$default$	$default$	$default$
$D_{i,t}$	-0.0127***	-0.0111***	3.3020**	0.0009	0.0139	-0.0076
	(0.004)	(0.004)	(1.346)	(0.006)	(0.014)	(0.007)
Sample	8k - 18k	8k - 18k	8k - 18k	Placebo	36m, 16k - 24k	36m, 2k - 10k
Observations	42,867	40,761	42,867	73,870	14,667	32,499
R^2	0.207	0.214	0.334	0.146	0.371	0.217
# clusters	25	25	25	25	25	25

Table 5. Interpretation of results

This table shows the output of the regression of each outcome on a dummy for the staggered reduction of the minimum amount threshold for long maturity loans on March 2013 (to $12,000) and July 2013 (to $10,000). Outcomes include $default12m$ and $default18m$, dummies that equal one if a borrower is late by more than 120 days as of April 2015 and whose last payment occured within 12 and 18 months after origination, respectively; prepaid, measures if a loan has been fully prepaid as of April 2015. The sample corresponds to loan amounts between $6,000 and $20,000 whose listing date is between December 2012 and October 2013. All regressions include sub grade $\times$4-point FICO bin $\times$ month, and US state $\times$ month fixed effects. Standard errors are clustered at the initial credit risk sub grade (25 clusters). *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)
	$default12m$	$default18m$	$prepaid$
$D_{i,t}$	-0.0038	-0.0085***	-0.0141**
	(0.003)	(0.003)	(0.007)
Sample	MAIN	MAIN	MAIN
Observations	55,784	55,784	55,784
R^2	0.155	0.161	0.164
# clusters	25	25	25

Appendix B. Mathematical Appendix for Framework

A. Symmetric Information

Here we solve the optimal lending contract when lenders and borrowers are symmetrically informed about borrower type. Consider the following optimal insurance problem

$$\max_{c_1,\, c_3^G,\, c_3^M,\, c_3^B} (1-\alpha)\, u(c_1) + \alpha \left[(1-p_k-x_k)\, u\left(c_3^G\right) + p_k u\left(c_3^M\right) + x_k u\left(c_3^B\right)\right] \tag{10}$$

subject to

$$c_1 + (1-p_k-x_k) \times c_3^G + p_k \times c_3^M + x_k \times c_3^B \leq E\,(1-p_k(1-q)-x_k) \tag{11}$$

Let λ^{Symm} be the Lagrange multiplier on the break even constraint (11). The first order conditions for each choice variable are

$$c_1 : (1-\alpha)\, u'(c_1) - \lambda^{Symm} = 0 \tag{12}$$

$$c_3^G : \alpha\,(1-p_k-x_k)\, u'\left(c_3^G\right) - (1-p_k-x_k)\, \lambda^{Symm} = 0 \tag{13}$$

$$c_3^M : \alpha p_k u'\left(c_3^M\right) - p_k \lambda^{Symm} = 0 \tag{14}$$

$$c_3^B : \alpha x_k u'\left(c_3^M\right) - x_k \lambda^{Symm} = 0 \tag{15}$$

The first order conditions for consumption at $t=3$, (13) (14) and (15), are satisfied if and only if $u'\left(c_3^G\right) = u'\left(c_3^M\right) = u'\left(c_3^B\right)$. Given the strict concavity of $u()$ this requires $c_3^G = c_3^M = c_3^B$. Let c_3 denote this state independent level of consumption at $t=3$. Consumption at $t=3$ in each state as a function of the loan contract $\left\{A_1^{**k},\, D_{1,2}^{**k},\, D_{1,3}^{**k}\right\}$ is

$$c_3^G = A_1^{**k} - c_1 + E - D_{1,2}^{**k} - D_{1,3}^{**k} \tag{16}$$

$$c_3^M = A_1^{**k} - c_1 - D_{1,2}^{**k} + q\left(E - D_{1,3}^{**k}\right) \tag{17}$$

$$c_3^M = A_1^{**k} - c_1 \tag{18}$$

recalling that the household defaults when it is unable to repay the loan: $S = M$, $I = 0$ and $S = B$. Using (16) and (17) we have that $c_3^G = c_3^M$ if and only if $E - D_{1,3}^{**k} = q\left(E - D_{1,3}^{**k}\right)$ which can only hold if $E = D_{1,3}^{**k}$. Using (17) and (18) we have that $c_3^M = c_3^B$ if and only if $D_{1,2}^{**k} = q\left(E - D_{1,3}^{**k}\right)$ and since $E = D_{1,3}^{**k}$ this implies $D_{1,2}^{**k} = 0$. Competition ensures that the the breakeven condition (11) must hold and so $A_1^{**k} = E\left(1 - p_k\left(1 - q\right) - x_k\right)$. Using (12) and (12) the choice of c_1 will be determined by the Euler equation:

$$(1-\alpha)\,u'\left(c_1\right) = \alpha u'\left(E\left(1 - p_k\left(1-q\right) - x_k\right) - c_1\right). \tag{19}$$

B. Asymmetric Information

We start by studying the general case where household value consumption at both $t = 1$ and $t = 3$: $\alpha \in [0, 1]$. Formally, the contract offered to high creditworthy households $\left\{A_1^{*H},\, D_{1,2}^{*H},\, D_{1,3}^{*H}\right\}$ will be the solution to:

$$\max_{c_1,\, A_1,\, D_{1,2},\, D_{1,3}} (1-\alpha)\,u\left(c_1\right) + \alpha\left[\left(1 - p_H - x_H\right)u\left(c_3^G\right) + p_H u\left(c_3^M\right) + x_H u\left(c_3^B\right)\right] \tag{20}$$

subject to

$$c_3^G = A_1 + E - D_{1,2} - D_{1,3} - c_1 \tag{21}$$

$$c_3^M = A_1 + qE - D_{1,2} - qD_{1,3} - c_1 \tag{22}$$

$$c_3^B = A_1 - c_1 \tag{23}$$

$$A_1 \leq \left(1 - x_H\right)D_{1,2} + \left(1 - \left(1-q\right)p_H - x_H\right)D_{1,3} \tag{24}$$

$$D_{1,2} \leq A_1 - c_1 + q\left(E - D_{1,3}\right) \tag{25}$$

$$D_{1,2} \geq 0 \tag{26}$$

$$D_{1,3} \geq 0 \tag{27}$$

$$U^{*L} \geq U^{*L'}\left(A_1,\, D_{1,2},\, D_{1,3}\right) \tag{28}$$

where c_3^S is the consumption that will be achieved at $t = 3$ for each possible realization of the interim signal.[36] The conditions 21, 22, 23 give the level of consumption that the household will have at $t = 3$ in each state given the debt contract and the choice of c_1. Condition 24 ensures that a lender will break even in expectation. Condition 25 ensures that the household is able to repay the payment due at $t = 2$ whenever $S = G, M$. We assume that q is sufficiently large so that this constraint does not bind. Condition 26 and 27 ensures that the contracted repayments at $t = 2$ and $t = 3$ are non-negative. Crucially, since the debt is defaulted on in certain states this ensures that the lender is unable to sign a contract to make payments to the borrower at $t = 2$ or $t = 3$ that is conditional on the signal S or the realized amount of income I. Condition 28 is the truth telling constraint that ensures low type households do not choose the loan designed for the high type. $U^{*L'}(A_1, D_{1,2}, D_{1,3})$ is the expected utility that a low type will achieve if she deviates and takes the contract designed for the high type: $\{A_1, D_{1,2}, D_{1,3}\}$. The function $U^{*L'}(A_1, D_{1,2}, D_{1,3})$ is is defined by finding the level of consumption at $t = 1$, c_1', that a low type will choose if they deviate and take the contract designed for this high type household. Formally $U^{*L'}(A_1, D_{1,2}, D_{1,3})$ is the maximized objective of the following problem:

$$\max_{c_1'} (1-\alpha) u(c_1') + \alpha \left[(1 - p_L - x_L) u\left(c_3'^G\right) + p_L u\left(c_3'^M\right) + x_L u\left(c_3'^B\right) \right] \tag{29}$$

subject to

$$c_3'^G = A_1 + E - D_{1,2} - D_{1,3} - c_1' \tag{30}$$

$$c_3'^M = A_1 + qE - D_{1,2} - qD_{1,3} - c_1' \tag{31}$$

$$c_3'^B = A_1 - c_1' \tag{32}$$

where (30), (31), (32), are the counterparts to (21), (22), (23) in the problem above. Substituting (30), (31), (32)into (29) and taking the first derivative with respect to c_1'

[36]Note that additional borrowing at $t = 2$ will ensure that conditional on reaching $S = B$ all remaining income risk is insured at $t = 2$ and hence independent of the realization of I.

gives the following first order condition:

$$(1-\alpha)\,u'\left(c_1'\right) = \alpha\left(1-p_k-x_k\right)u'\left(A_1+E-D_{1,2}-D_{1,3}-c_1'\right) \quad (33)$$
$$+\,\alpha p_k u'\left(A_1+qE-D_{1,2}-qD_{1,3}-c_1'\right)$$
$$+\,\alpha x_k u'\left(A_1-c_1'\right)$$

As argued by Kocherlakota (2004b) this first order condition cannot in general be simply used as an additional constraint in the first problem. Also, doing so renders the problem such that analytical solutions (and often numerical solutions) are unworkable. We avoid this problem by solving the model in two special cases below.

B.1. Consumption only at $t = 3$ ($\alpha = 1$)

We now consider the contracting problem in the case where the household only consumes at $t = 3$. This eliminates the possibility of hidden savings since all debt raised at $t = 1$will be saved. The Lagrangian for the constrained optimization problem in this case is:

$$\mathcal{L} = \max_{c_3^G, c_3^M, c_3^B} \left(1-p_H-x_H\right)u\left(c_3^G\right)+p_H u\left(c_3^M\right)+x_H u\left(c_3^B\right) \quad (34)$$

$$+\lambda_1\left[\left(1-p_H\left(1-q\right)-x_H\right)E-\left(1-p_H-x_H\right)c_3^G-p_H c_3^M-x_H c_3^B\right] \quad (35)$$

(36)

$$+\lambda_2\left[u\left(\left(1-p_L\left(1-q\right)-x_L\right)E\right)-\left(1-p_L-x_L\right)u\left(c_3^G\right)+p_L u\left(c_3^M\right)+x_L u\left(c_3^B\right)\right]$$

$$+\lambda_3\left[c_3^B+\frac{qc_3^G-c_3^M}{1-q}\right] \quad (37)$$

$$+\lambda_4\left[E-\frac{c_3^G-c_3^M}{1-q}\right] \quad (38)$$

$$+\lambda_5 c_3^G+\lambda_6 c_3^M+\lambda_7 c_3^B \quad (39)$$

where (35) is the break even condition that must hold under perfect competition, (36) is the truth telling condition ensuring low types do not accept the contract designed to the high type, (37) and (38) ensure $D_{1,2}$ and $D_{1,3}$ are non-negative, and (39) ensure consumption is non-negative in each state. The associated Lagrange multipliers, $\lambda_1, \lambda_2, ..., \lambda_7$ are non-negative and obey the standard Kuhn-Tucker conditions. The first order conditions with respect to three choice variables are

(40)

$$c_3^G : (1-p_H-x_H)\, u'\left(c_3^G\right) - \lambda_1 (1-p_H-x_H) - \lambda_2 (1-p_L-x_L)\, u'\left(c_3^G\right) + \lambda_3 \frac{q}{1-q} - \lambda_4 \frac{1}{1-q} + \lambda_5 = 0$$

(41)

$$c_3^M : p_H u'\left(c_3^M\right) - \lambda_1 p_H - \lambda_2 p_L u'\left(c_3^M\right) - \lambda_3 \frac{1}{1-q} + \lambda_4 \frac{1}{1-q} + \lambda_6 = 0$$

(42)

$$c_3^B : x_H u'\left(c_3^B\right) - \lambda_1 x_H - \lambda_2 x_L u'\left(c_3^B\right) + \lambda_3 + \lambda_7 = 0$$

The first order conditions that are examined in the paper consider an interior solution where (37), (38) , and (39) do not bind and hence $\lambda_3 = \lambda_4 = \lambda_5 = \lambda_6 = \lambda_7 = 0$.

B.2. CARA Utility and $\alpha \in [0,1]$

Assume that the household utility function exhibits constant absolute risk aversion:

$$u(c) = 1 - \frac{1}{\eta} e^{-\eta c} \tag{43}$$

where $\eta > 0$ is the coefficient of absolute risk aversion. With this assumption (33) simplifies to give the level of consumption at $t = 1$ that a household of type k will select conditional on accepting a contract of $\{A_1, D_{1,2}, D_{1,3}\}$ as

$$c_1' = \frac{-1}{2\eta} \ln \left[\frac{\alpha}{1-\alpha} \left\{ (1-p_k-x_k)\, e^{-\eta A_G} + p_k e^{-\eta A_M} + x_k e^{-\eta A_B} \right\} \right] \tag{44}$$

where

$$A_G \equiv A_1 + E - D_{1,2} - D_{1,3}$$

$$A_M \equiv A_1 + qE - D_{1,2} - qD_{1,3}$$

$$A_B \equiv A_1$$

Substituting (44) into (29) and using this in (28) defines the optimal contracting problem under CARA utility. Numerical solutions to this problem are provided in Figure 5.

C. Pooling Equilibrium

The analysis in the paper has focused on characterizing the debt contracts that will arise in a separating equilibrium. The goal of this sub-section is to briefly argue that this focus has been without loss of generality because pooling equilibrium do not exist as long as out of equilibrium beliefs are reasonable in the sense of the intuitive criteria of Cho and Kreps (1987). Under this criteria if a high type household deviates from a proposed pooling equilibrium to accept a contract that a low type does not prefer to the pooling contract then they will be believed to be high type.

Competition ensures that a pooling equilibrium, if it exists, will only occur at the contract that maximized the expected utility of both types subject to the break even constraint. Thus a pooling equilibrium, if it were to exist would have all household accepting the following contract:

$$A_1^{Pool} = \left[\phi \left(1 - p_H \left(1 - q\right) - x_H\right) + \left(1 - \phi\right) \left(1 - p_H \left(1 - q\right) - x_H\right)\right] E$$

$$D_{1,2}^{Pool} = 0$$

$$D_{1,3}^{Pool} = E$$

This pooling equilibrium can only survive if there is no other contract $\{A_1,\, D_{1,2},\, D_{1,3}\}$ that (i) would be preferred by high type household and not by a low type and (ii) would allow the lender offering the contract to high type households to at least break even. In fact competition would ensure that the contract which maximized the expected utility of high type households were offered if any such contract exists and thus we can characterize this deviating contract in exactly the same way as the separating contract with the only difference being that the truth telling constraint for the low type (28) is now

$$U^{Pool} \geq U^{*L'}\left(A_1,\ D_{1,2},\ D_{1,3}\right) \tag{45}$$

where

$$U^{Pool} \equiv u([\phi\left(1-p_H\left(1-q\right)-x_H\right)+\left(1-\phi\right)\left(1-p_H\left(1-q\right)-x_H\right)]\,E)$$

It must be that the truth-telling constraint (45) will bind at this contract because the optimal contract where this doesn't bind is simply the full insurance contract that arises under asymmetric information and the low type would always strictly prefer this contract. But if the low type is indifferent between both contracts then the high type must strictly prefer this new contract if it is set optimally. To avoid the hidden savings problem and thus allow an analytical characterization of the optimal contract suppose $\alpha = 1$. Take the first example we considered in the paper where $p_L > p_H$ and $x_L = x_H$. As we argued in the paper the first order conditions imply that $c_3^G > c_3^B > c_3^M$. So if (45) binds for the low type then

$$U^{Pool} = \left(1-p_L-x_L\right)u\left(c_3^G\right)+p_L u\left(c_3^M\right)+x_L u\left(c_3^B\right) \tag{46}$$

but then it must be that expected utility of the high type is strictly higher than this since $p_L > p_H$ and $c_3^G > c_3^M$. So this contract will break the pooling equilibrium. A similar argument applies the more general case where $p_L \geq p_H$ and $x_L \geq x_H$. For each of the numerical solutions for the general case of $\alpha \in (0,1)$ presented in 5 it is also verified that no pooling equilibrium exists by a similar argument - a deviating contract can always be found that the high type strictly prefers.

Appendix C. inferring initial credit risk subgrade from data

LC assigns each loan's interest rate depending on the credit risk sub grade. In the data, the variable subgrade takes one of 35 possible values for each loan: A1, A2, ... A5, B1, ... B5, ... G5. Each grade is assigned a number: A1 = 1, A2 = 2, ... G5 = 35 ranging from least risky to most risky. Each subgrade is then assigned an interest rate. For example, as of December 2012, A1 loans had an interest rate of 6.03%, while A2 loans had a rate of 6.62%. We take a snapshot of LC's "Interest Rates and How We Set Them" page as of December 31, 2012 from the Internet Archive.[37] According to this page, the borrower's credit risk grade is calculated in the following manner. First, "*the applicant is assessed by Lending Club's proprietary scoring models which can either decline or approve the applicant.*" If an applicant is approved by the model, she receives a Model Rank (an "initial subgrade"), which can range from A1 (1) through E5 (25). According to the website, *"The Model Rank is based upon an internally developed algorithm which analyzes the performance of Borrower Members and takes into account the applicant's FICO score, credit attributes, and other application data."* The initial subgrade is then modified depending on the rquested loan amount and maturity. For example, the initial subgrade of 36 month loans was not modified, while the initial subgrade of 60 month loans was modified by 4 grades for A borrowers (initial subgrades 1 to 5), 5 grades for B borrowers (initial subgrades 6 to 10) and 8 grades for all other grades. The amount modifications are publicly available for each period on LC's website, and vary over time. We choose our main sample period between December 2012 and October 2013 so that these modfications stay constant. For example, between December 2012 and October 2013, the amount modifications for each grade were as follows:

[37]See https://web.archive.org/web/20121204222850/http://www.lendingclub.com/public/how-we-set-interest-rates.actio

	Initial subgrade		
	A	B	C-E
<$5,000	1	1	1
$5,000 - $15,000	0	0	0
$15,000 - $20,000	0	0	2
$20,000 - $25,000	0	1	3
$25,000 - $30,000	2	3	4
$30,000 - $35,000	4	4	5
$35,000	6	6	6

According to this table, the initial subgrade of a borrower who requests a loan for $10,000 is the same as her final subgrade before the modification for maturity. Instead, a borrower who was ranked initially as C1 (equivalento an 11) who requests a $16,000 loan will see her grade modified two steps to a C3 (13).

Borrowers who share the same initial subgrade will have very similar risk characteristics as assessed by LC's lending model, while their interest rate will only vary according to their choice of amount and maturity. Thus, our analysis above uses the initial subgrade before amount and maturity modifications to construct fixed effects. This variable–initial subgrade– is not observable in the data. Instead, LC only provides the credit risk subgrade after all modifications have been made. To re-construct a borrower's initial subgrade, we reverse engineer LC's credit risk process for every loan in our sample using their publicly available information. For example, a 36 month loan issued on January 2013 for $16,000 that appears in the data as a C4 borrower must have been assigned an initial grade of C2 (2 modifications for the loan amount, no modifications for maturity). The table below documents the fraction of loans on each final subgrade that we cannot assign an initial subgrade from our reverse engineering procedure for loans issued between December 2012 and October 2013, for amounts between $6,000 and $20,000:

% of loans in main sample period not assigned an initial subgrade					
Final subgrade	%	Total loans	Final subgrade	%	Number of loans
A1	0.7	1,535	D1	0.7	2,200
A2	0.2	1,222	D2	0.1	2,019
A3	0.4	1,321	D3	0.8	1,588
A4	0.8	1,541	D4	0.4	1,306
A5	1.5	2,257	D5	0.4	783
B1	0.3	5,334	E1	0.4	284
B2	0.8	5,772	E2	0.0	363
B3	0.6	5,992	E3	0.0	172
B4	0.6	5,828	E4	0.7	143
B5	0.7	2,899	E5	0.0	95
C1	12.8	3,578	F1	81.5	27
C2	2.2	2,710	F2	88.2	34
C3	0.5	2,970	F3	100.0	8
C4	0.5	2,606	F4	100.0	8
C5	0.5	2,072	G1	100.0	1

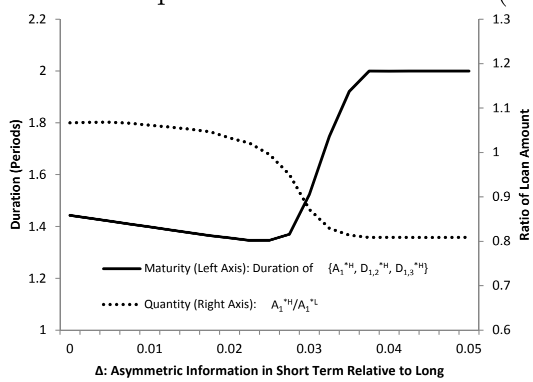
By construction, almost all loans below an F1 rating (26) will not have an initial subgrade because LC's model states that only 25 initial grades are issued. Second, we succeed in matching a borrower's initial subgrade for more than 97.8% of the loans of each final subgrade in 24 out of the 25 top subgrades. Grade C1 (grade 11) is slightly problematic as the success rate drops to 87.2%. The reason is that, given the algorithm presented above, we should not observe C1 loans between $15,000 and $20,000, but LC categorizes 441 of these loans during our sample period. All our results are robust to eliminating loans issued in final grade C1 and to replacing the initial subgrade in our regression model with the observed final subgrade.

Appendix D. Supplementary Tables and Figures

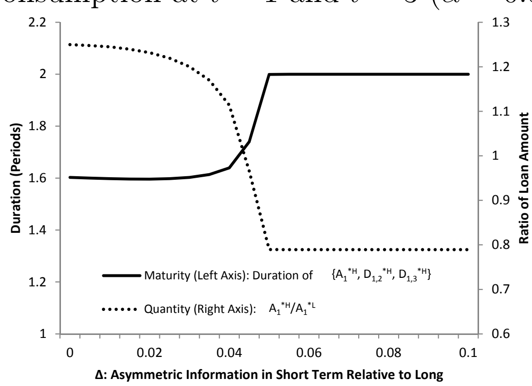
Figure 12. Model Comparative Statics - Robustness

This figure shows additional comparative statics from numerical solutions of the theoretical framework presented in SectionIII in order to demonstrate the robustness of the results in Figure 5. The following parameters are used (identical to Figure 5): $E = 100$, $p_L = 0.3$, $x_L = 0.1$, $\bar{p}_H = 0.1$. Panel A and B continue to use a CARA utility function: $u(c) = 1 - \frac{1}{\eta}e^{-\eta c}$ with $\eta = 0.1$. In Panel A and B the household values consumption at both dates equally: $\alpha = 0.5$. In Panel A $q = 0.75$ and (i.e. the same as in Figure 5) and in Panel B this is lowered to $q = 0.25$. For Panel C the CARA utility function is replaced with a CRRA utility function of $u(c) = \frac{c^{1-\eta}-1}{1-\eta}$ with $\eta = 2$. Otherwise the parameters in Figure C are identical to those in Figure 5: $q = 0.75$ $\alpha = 1$. Thus Panel A varies the concern for consumption at $t = 1$, and Panel B the probability of repayment conditional on $S = M$., and Panel C varies the utility function. The left axis in each panel shows the degree of maturity rationing as captured by the Macaulay duration of the equilibrium loan offered to the high type: $Duration = 1 \times \frac{D_{1,2}^{*H} \times (1-x_H)}{A_1^{*H}} + 2 \times \frac{D_{1,3}^{*H} \times (1-p_H(1-q)-x_H)}{A_1^{*H}}$. The right axis in each panel shows the degree of quantity rationing as captured by the ratio of the amount lent to high and low type borrowers at $t = 1$: $\frac{A_1^{*H}}{A_1^{*L}}$.

Panel A: Consumption at $t = 1$ and $t = 3$ ($\alpha = 0.5$)



Panel B: Consumption at $t = 1$ and $t = 3$ ($\alpha = 0.5$), $q = 0.25$



Panel C: Consumption only at $t = 3$ ($\alpha = 1$), CRRA Utility

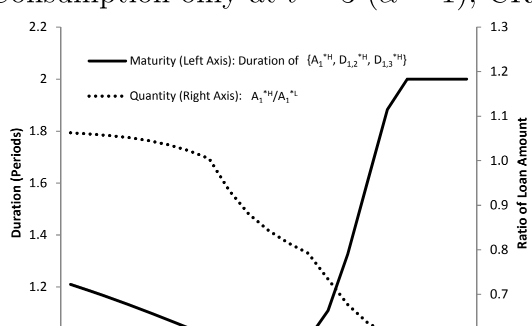


Table 6. Robustness: potential correlation between seeing a menu and creditworthiness

This table shows that there is no correlation between the propensity to see the menu of borrowing options made available by LC and a borrower's creditworthiness. The table reports the coefficient on "no menu" of the regression of default on no menu, controlling for loan amount, APR, and month×subgrade×FICO score bin at origination fixed effects. The sample correspond to 36 month loans issued in multiples of \$1,000 between December 2012 and February 2013, for amounts between \$6,000 and \$20,000. Standard errors are clustered at the initial credit risk sub grade (25 clusters). *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)
VARIABLES	default
no menu	0.0049
	(0.008)
Constant	-0.5076**
	(0.213)
Observations	6,778
R^2	0.147
# clusters	25