

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

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ABSTRACT. We provide evidence that borrowers who select into long term debt are unobservably more exposed to shocks to their ability to repay. 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 prepay more conditional on good standing. 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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I. Introduction

A fundamental function of financial markets is to allow households to insure against idiosyncratic risk. Long term financial products, with terms set at the time of signing, provide insurance by insulating the household against shocks over the life of the agreement. For example, relative to a sequence of short-term loans, a thirty year mortgage insures a household against future shocks to its creditworthiness by reducing the need to borrow in the future after such shocks are realized. However, as Rothschild and Stiglitz (1976) first emphasized, the ability of competitive financial markets to provide insurance—in this case, maturity—is limited when households have private information about their exposure to risk. A pooling equilibrium in which all borrowers receive full insurance will break down because unobservably less risky households will opt for reduced insurance—less maturity—to avoid the cost of pooling with riskier households.¹

This 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 higher default risk 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 compare the performance of two observationally equivalent groups of borrowers, A and B, the first with a short term loan and the second with a long term loan. If borrowers in group B default at a rate higher than the borrowers in group A, it may be due to adverse selection on maturity, but also 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 empirical 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 an interest 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 (at rate r_{ST}), 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.

¹The role of asymmetric information in corporate loan maturity choice was first studied theoretically by Flannery (1986) and Diamond (1991).

In particular, $\gamma_B^{ST} - \gamma_A^{ST} < 0$ would indicate that borrowers with a higher privately observed default risk 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. All the borrowers in a risk category are offered the same menu of loan choices, e.g. the same interest rate for every amount and maturity combination. 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 available menu of long term loan options 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 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 that the number of short maturity loans between \$10,000 and \$16,000 dropped by 14% 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

²According 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.

³Our 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 funded. Moreover all terms of the

We find that the average default rate of short maturity loans decreases by 0.7 percentage points and the average future FICO score increases by 2.3 points when a long maturity loan was available at the time of origination. This implies that borrowers with unobservably higher default risk *self-select* out of short-term loans and into long term ones. The increase in performance of 36-month loans is due to the 14% of borrowers that self-select into long maturity, which implies that these borrowers would have had a default rate that is 5 percentage points higher ($0.7/0.14$) 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

Having established that borrowers who select into long term loans are privately informed about their poor repayment prospects with a short term loan, we then attempt to shed light on the underlying private information that drives selection along the maturity dimension. We uncover two facts. First, borrowers who select into 60-month loans do not have a different propensity to default to the average 36-month loans during the first twelve months after origination, even though unconditionally over 40% of the 36-month loans that default do so during this period. The entire difference in repayment behavior of the borrowers that self-select into long maturity occurs beyond a year after origination. Thus, the higher propensity to default of borrowers who select into long term loans increases with time since origination. Second, borrowers who self-select into the 60-month loans, conditional on not defaulting, are more likely to prepay the loans before maturity. Higher default (after a year) and prepayment probabilities combined suggest that borrowers who self-select into long term loans have private information about their higher volatility of future income or consumption shocks. This evidence is consistent with the insurance motivation for self-selection into long maturity contracts.

Alternative interpretations of the results are inconsistent with the evidence. For example, borrowers may self-select into longer maturity loans because they expect their income to increase further in the future. If these borrowers face financial constraints that prevent them from refinancing, they will default more in the short run, before the income arrives, and less in the longer term. Thus, this interpretation requires that borrowers that self-select into 60-month loans exhibit a differential propensity to default that *decreases* with time since origination, as borrowers approach the arrival of their future income streams, the opposite of what we observe in the data. Another possibility is that

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 charges an upfront origination fee between 1.1 and 5% of a borrower's loan amount (subtracted from the amount borrowed). Thus, borrowers who took a short maturity loan prior to the expansion could not costlessly swap them for long maturity ones 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.

⁴We 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 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.

borrowers that care less about their credit scores self-select into long maturity loans because they plan to strategically default in the future (see Liberman (2015) for an example of this behavior). This interpretation is inconsistent with the higher prepayment propensity of borrowers that self-select into the 60-month loans.

We close the paper with a discussion of the conditions under which maturity the *optimal* way to screen borrowers when screening using loan amounts is also an option, as in most unsecured consumer credit applications (see Stiglitz and Weiss (1981), Adams, Einav, and Levin (2009), Dobbie and Skiba (2013)).⁵ We develop a stylized model of consumer credit choice where borrowers have private information about their exposure to adverse shocks in the short and long run. In this setting, long maturity debt provides borrowers with insurance against future shocks to their creditworthiness and income. When borrowers are better informed about their own exposure to adverse shocks, in equilibrium lenders will offer borrowers a menu of contracts so that they self select into based on their unobservable creditworthiness. Better types can credibly separate themselves by either borrowing less or by taking shorter maturity loans. 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 their ability to repay is increasing over time from origination. This condition is confirmed empirically by our finding that borrowers who select into different maturity loans display a differential propensity to default that is increasing in the time since origination.

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 borrowers a choice over loan maturity. 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 (Karlán 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

⁵In general, other contract terms that can be used to screen borrowers are collateral (Bester (1987)) and their willingness to agree to joint liability (Van Tassel (1999)).

Liu (2010), Dobbie and Skiba (2013), and Zinman (2014)).⁶ In particular, our findings show striking evidence of adverse selection among *prime* US borrowers, a novel finding in this literature.⁷

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. In Section III we describe the empirical strategy and document that borrowers who self-select into long maturity loans are less creditworthy in terms of the short term loan. Section IV provides a simple framework to develop a novel testable condition under which it is optimal to screen borrowers using loan maturity. Section V interprets our key empirical finding as evidence that borrowers who are more exposed to shocks to ability to repay select into longer maturity loans and tests the condition from our theoretical framework. Section VI 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.⁸ 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 sufficient 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 pay stubs, 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 category determines the entire menu of interest rates faced by the borrower for all loan amounts and for the two maturities. Interest rates for each subgrade are weakly increasing in amount and maturity. Apart from the interest

⁶Outside 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.

⁷Evidence 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), stocks Kelly and Ljungqvist (2012), and the securitized mortgage market Agarwal, Chang, and Yavas (2012).

⁸Figures reported in the firms' 2014 10K reports.

rate, amount (net of LC's origination fee), and maturity, the terms of all loans are identical. Once a borrower selects a loan and it is approved by LC, the application is listed on LC's website for investors' consideration. According to LC, over 99% of all approved loans are funded by investors and investors do not affect any of the terms of the loans.⁹ Thus, we ignore the fund supply side in the analysis. LC charges an origination fee that varies between 1.1% and 5% of the loan amount depending on credit score, which is subtracted at origination, and a further 1% fee from all loan payments made by investors.

B. Staggered expansion of 60 month loans

Before March 2013, 60-month loans were only available for loans of \$16,000 and above. Since loan prepayment in LC reduces the number of installments and leaves installment amount unchanged, e.g. prepayment reduces the maturity of the loans, it was impossible for a borrower to synthetically create a 60-month loan of an amount smaller than \$16,000 before the menu expansion. After March 2013 the minimum threshold for a 60-month loan was lowered, first to \$12,000, and later to \$10,000. Figure 2 shows the number of 60-month loans for amounts between \$10,000 and \$12,000 and between \$12,000 and \$16,000 (closed left and open right interval in both cases). The graph shows a 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. LC did not change its lending policy, including the menu of other loans and the rating algorithm, during the months included in our sample (described in detail in the next subsection). 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 characteristics of the pool of applicants did not change after the 60-month menu expansion. 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 V 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.

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 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

⁹See http://kb.lendingclub.com/borrower/articles/Borrower/What-if-my-loan-isn-t-fully-funded-when-my-listing-ends/?l=en_US&fs=Re

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 2013.¹⁰ Hence, we limit our sample period to all 36-month loans with a list date (variable `list_d`) is between and including these two months, for amounts between \$5,000 and \$20,000. We choose this interval of control amounts because the interest rate schedule jumps discretely at \$5,000 and \$20,000 for all credit risk categories (sub-grades).¹¹ 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-of-origination shocks to creditworthiness and credit demand. 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 60,514 loans.¹²

Table 1, Panel A, presents summary statistics for the 12,091 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 16.3% APR and a monthly installment of \$380. Borrowers self report that 87% 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 4 shows the hazard default rate by number of months since origination for loans in our sample issued in the pre-expansion period.¹³ The hazard rate exhibits the typical hump shape and peaks between 13 and 15 months.

Table 1, Panel B, shows borrower-level statistics of this sample. On average, LC borrowers in our sample have an annual income of \$65,745 and use 17.4% of their monthly income to pay debts excluding mortgages. The average FICO score at origination is 695, and credit report pulls show that the FICO score has on average decreased to 685 approximately one year later.¹⁴ LC borrowers

¹⁰The 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.

¹¹In 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". In robustness tests, we limit the sample to loan amounts between \$7,000 and \$18,000, a \$2,000 narrower interval.

¹²In 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.

¹³The date of default is determined by the first month when a borrower failed to make a payment.

¹⁴This "last FICO score" variable is updated every time LC discloses new information except for borrowers who have fully paid their loans or who have been charged off.

have access to credit markets: 56% report that they own a house or have an outstanding mortgage. The average borrower has \$38,153 in debt excluding mortgage debt and \$14,549 in revolving debt, which represents a 61% revolving line utilization rate. LC borrowers have on average approximately 15 years of credit history.

III. Measuring Selection On Loan Maturity

We exploit the staggered menu expansion of 60-month loans during 2013 to identify selection of borrowers into long maturity loans. As prescribed in the ideal experiment, LC offered new loan options 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.

Since the expansion in the menu of options occurred for all borrowers at the same time, time-of-origination-varying differences in creditworthiness or credit demand would make borrowers who were exposed to the menu expansion different from borrowers who were not. We address this potential problem using a difference-in-differences approach: we compare the change in the default rate of borrowers in the affected ("treated") amounts, \$10,000 to \$16,000, before and after the expansion, to the same difference for borrowers at "control" loan amounts, which are amounts just above and just below the affected range: \$5,000 to \$10,000 and \$16,000 to \$20,000. Since the menu expansion was staggered, we can also use only eventually-treated amounts to build a counterfactual, e.g. use loan amounts between \$10,000 and \$12,000 before July 2013 as a control for loan amounts between \$12,000 and \$16,000. Our identifying assumption is that time varying shocks to observationally equivalent borrowers are the same for control and treated amounts.

A second identifying assumption behind this approach is that the loan amount choice is inelastic to loan maturity. If this is not the case, the difference-in-difference estimates will be biased towards zero since some of the "control" borrowers are in fact "treated," either before or after the menu expansion. Let's consider the first case, where control amounts are treated before the menu expansion. Take the example borrowers that would like to take a \$10,000 60-month loan. Before the menu expansion this option is not available, and the closest alternatives are: 1) a \$10,000 36-month loan, and 2) a \$16,000 60-month loan. If borrowers choose the first option, e.g. take a loan for the amount they prefer at a 36-month maturity, then our empirical strategy will estimate the effect of maturity on selection. The reason is that these borrowers will self-select out of the 36-month once the longer term option becomes available. If, on the contrary, borrowers choose the second option, e.g. take a loan on the 60-month maturity but for a larger amount, then our difference-in-differences estimate will find a zero. The reason is that these borrowers will not be either in the treatment or in the control

group of loans in our estimation (our estimation is based exclusively on the outcomes of 36-month loans). If there is a mixture of the two types of behavior, then the difference-in-differences estimate will be somewhere in between zero and the true selection effect.

Now consider the second case: where the control amounts are treated after the menu expansion. Take for example borrowers that would like to take a \$5,000 60-month loan, but since this option is not available before the menu expansion, they take a \$5,000 36-month loan instead. Although these borrowers are in the control group in our estimation, it is possible that they choose a \$10,000 60-month loan when this option becomes available in the menu. If this is the case, then the menu expansion will also cause self-selection into long maturity among the control group of loans, and the comparison between treatment and control loans will be biased towards zero.

We explore formally whether the expansion induced selection at the treated amounts. To do so we collapse the data and count the number of loans $N_{j,t,amount1000}$ at the month of origination (t) \times sub-grade (j) \times \$1,000 loan amount bin (k) level for all loans issued during our sample period (amount bins measured starting from \$10,000, e.g. \$10,000 to \$11,000, \$11,000 to \$12,000, etc)..¹⁵ We define a “treatment” dummy variable D_{kt} equal to one for those loan amount bin - month pairs where a 60-month option was available, and zero otherwise. That is:

$$D_{kt} = \begin{cases} 1 & \text{if } \$16,000 > \text{Loan Amounts of bin } k \geq \$12,000 \text{ \& } t \geq \text{March 2013} \\ 1 & \text{if } \$12,000 > \text{Loan Amounts of bin } k > \$10,000 \text{ \& } t \geq \text{July 2013} \\ 0 & \text{otherwise} \end{cases}$$

Then we estimate the following difference-in-differences regression:

$$(1) \quad \log(N_{jkt}) = \beta_k + \delta_{jt} + \gamma \times D_{kt} + \varepsilon_{jkt}.$$

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) relative to control amounts. We include amount bin fixed effects β_k , which control for level differences in the number of loans in each \$1,000 bin. In turn, sub-grade \times month fixed effects, δ_{jt} , control for the terms of the contract offers.¹⁶

Table 2, column 1, shows the results of regression (1), estimated on the full sample of borrowers who took a 36-month loan between \$5,000 and \$20,000 during the sample period (December 2012 to October 2013). The point estimate of γ is negative and significant, and the magnitude implies that the number of borrowers who took a short term loan is 14% lower once the new long term loan option for the same amount becomes available. This estimate provides us with a magnitude for the number of borrowers who would have taken a short term loan instead borrow long term when that option becomes available. And for the reasons explained above, it represents a lower bound on the

¹⁵Results are insensitive to using actual loan amount instead.

¹⁶Results are qualitatively and quantitatively unchanged by collapsing the data at the month of origination $t \times k$ level instead and not including δ_{jt} fixed effects.

selection effect on the number of 36-month loans induced by the introduction of the 60-month loan option.

A. Pre-trends and robustness

Our identification strategy rests on the assumption that in the absence of the menu expansion 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 pre-trends by running an amended version of (1) using a series of dummies that become active τ months after a 60-month loan is offered at each amount. Formally, we define:

$$D(\tau)_{kt} = \begin{cases} 1 & \text{if } \$16,000 > \text{Loan Amounts of bin } k \geq \$12,000 \text{ \& } t = \text{March 2013} + \tau \\ 1 & \text{if } \$12,000 > \text{Loan Amounts of bin } k \geq \$10,000 \text{ \& } t = \text{July 2013} + \tau \\ 0 & \text{otherwise} \end{cases},$$

and we run the following regression:¹⁷

$$(2) \quad \log(N_{jkt}) = \beta_k + \delta_{jt} + \sum_{\tau=-3}^3 \gamma_{\tau} \times D(\tau)_{kt} + \varepsilon_{jkt}.$$

Figure 5 shows the results of regression 2. The results show no differential pre-trends 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 driven by differential trends in the demand for loans of varying amounts, we run regression (1) on a sample shifted forward to start when the 60-month loan option is available for any amount above \$10,000 (after the expansion in menus is complete). That is, we shift the definition of D_{kt} forward by 7 months and run the regression on the sample of loans originated between July 2013 and May 2014. Column 2 of Table 2 shows the results. The coefficient on D_{kt} 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 (e.g., we measure whether the control loans were treated after the menu expansion). To

¹⁷The 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).

do this we modify the definition of the treatment dummy D_{kt} to equal 1 one after March 2013 or July 2013 for different loan amounts as explained below.

First, using the subsample of amounts between \$16,000 and \$24,000, we define D_{kt} to be equal to one after March 2013 for all amounts between \$16,000 and \$20,000. The coefficient on this dummy tells us whether the number of loans of amounts close to the \$16,000 expansion threshold declined relative to those farther from the threshold. If so, it would be an indication that loans in the control group were affected by the expansion. The coefficient on the interaction term is 5.9% and not significantly different from zero (Table 2, column 3). 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.

We repeat the exercise at the \$10,000 amount threshold. We restrict the analysis to the sample of loan amounts between \$1,000 and \$10,000, and define D_{kt} equal one after July 2013 for amounts between \$5,000 and \$10,000 and zero otherwise. The coefficient on the interaction term is -3.9% 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 larger long maturity loan above the \$10,000 threshold when they became available in July. In other words, we find no evidence that the control group of loans in our main empirical design were affected by the menu expansion. Taken together the results in Column 1, 3 and 4 confirm our conjecture that the bulk of any selection to longer maturity loans induced by the expansion of the menu was in the treated amounts.

B. Regression framework to measure selection

We implement our empirical design to test how borrowers with unobservable differences in creditworthiness choose loans of different maturity. To do this we estimate the following difference-in-differences specification on the sample of 36-month loans:

$$(3) \quad \text{outcome}_i = \beta_i^{1000bin} + \delta_{jlt} + \gamma \times D_{it} + X_{it} + \varepsilon_{it},$$

where data is at the loan level i . The dummy, D_{it} , is equal to one if loan i was issued at a time when the 60-month was also available at the same amount (similar to the first definition in this section). and zero otherwise. The coefficient of interest, γ , measures 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 (t) \times sub-grade (j) \times 4-FICO score at origination bin (l) fixed effects, δ_{jlt} , which ensure we compare borrowers who took a loan on the same month, with the same contract terms (same sub-grade), and with similar observed creditworthiness. 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 fixed effects.

We also report results including as controls the *full* set of variables that LC reports and that investors observe at origination. These variables include, for example, annual income, 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). The outcome measures 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 2015.¹⁸ 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 other intermediaries. As one example, Prosper Marketplace, LC's largest rival, offered loans of three and five year maturity to borrowers of similar creditworthiness during our sample period. 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 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 fully account for the influence of the competitive environment. We are not aware of any such change elsewhere in the consumer credit market during our sample period. 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.¹⁹

C. Selection on 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 (3). Columns 1 through 4 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 γ equals -0.7% and is significant at the 5% 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 14%. Thus, the average default rate on the 36-month loan of the 14% of borrowers who chose to take a

¹⁸Our 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.

¹⁹For evidence of search frictions in consumer credit markets see Stango and Zinman (2013).

60-month loan when it became available must be $0.7\%/14\% = 5\%$ higher than those who chose instead to take the 36-month loan.²⁰ 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 -0.71% to -0.66%, but estimated with more noise, as statistical significance drops to 10%. 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 0.7% 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. Column 3 of Table 2 shows the results of regression (3) 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 FICO score that is 2.3 points higher. In economic terms this means that the average FICO score of the 14% of the total pool of pre-expansion short term applicants who became long term borrowers is $2.3/14\% = 16.4$ points higher. This higher FICO score may, for example, result in better access to credit and labor markets. Finally in Column 4 we repeat the regression model of Column 2, where we include every single observable variable at origination, using FICO score as the outcome. The

²⁰This ratio is significant at a 10% level, based on bootstrapping with 1,000 repetitions.

coefficient drops to 2.1 and remains statistically significant, which confirms that our measure of selection on maturity is based on variables that are unobservable at the time of origination.

D. Robustness

We present in Table 4 several tests that demonstrate the robustness of our results. First, Columns 1 and 2 of Table 4 present counterparts to our main results in Table 2, Columns 1 and 2, limiting the sample to loan amounts between \$7,000 and \$18,000 (a \$2,000 narrower window than our main sample, which uses loans from \$5,000 to \$20,000). The results are qualitatively similar, although estimates using default as the outcome are noisier and significant only at a 10% level.

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 3 of Table 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 loan 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:

$$D_{amount1000,t} = \begin{cases} 1 & \text{if } \$16,000 > amount_{1000} \geq \$12,000 \text{ \& } t \geq \text{October}2013 \\ 1 & \text{if } \$12,000 > amount_{1000} > \$10,000 \text{ \& } t \geq \text{February}2014 . \\ 0 & \text{in other cases} \end{cases}$$

The results show that the coefficient on $D_{amount1000,t}$ is positive, small, and insignificant, which contrasts sharply with to Column 1 of Table 3, where the coefficient is negative and strongly significant. We interpret these results with caution, as the entire sample of placebo loans has had less time to default than loans issued in our main sample. However, 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. Column 2 repeats this exercise for FICO score as the outcome, again showing a small and negative coefficient, which contrasts with the relatively large and positive coefficient of Column 3 in Table 3.

As we mentioned above when describing our empirical strategy, the expansion in the menu of borrowing options may have induced selection in the unaffected or control group of amounts, above and below the \$10,000 to \$16,000 interval. In Table 2 above we show that the number of loans issued at the control amounts did not change, which suggests that no such selection occurred. However, it is important to independently verify that there is no change in the credit quality of loans issued at control amounts induced by the menu expansion. Here we test for this possibility. 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 coefficient is positive and insignificant. Column 6 of Table

4 repeats the exercise for loans between \$1,000 and \$10,000 issued between December 2012 and October 2013. Here, the independent variable of interest equals one for loans between \$5,000 and \$10,000 issued after July 2013. Here, the coefficient is negative and insignificant. In both cases, we find no significant differences in the default rate of loans issued at amounts bordering the interval of treated amounts. 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. 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.

IV. Framework

Our results imply that maturity choice can be used to screen borrowers in consumer credit markets. The existing theoretical literature on corporate debt maturity choice under asymmetric information can rationalize this finding. 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 (Diamond (1991)) that comes with short-term debt. The model presented here applies the same fundamental logic to consumer finance 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. The primary contribution of the model is to allow lenders to screen borrowers using both loan maturity (as per Flannery (1986) and Diamond (1991)) and loan amount (as per Stiglitz and Weiss (1981)).²¹ In unsecured consumer credit markets these are the two primary contract dimensions available to creditors. This allows us to derive a testable condition for when maturity will be, in equilibrium, the optimal way to screen borrowers as opposed to loan amount. We provide evidence that supports this condition in the next section of the paper.

A. Setup

The timeline of the model is shown in Figure 6. 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 $t = 1$.²² 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 $t = 2$, public information about a borrower's ability to repay 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

²¹In Flannery (1986) and Diamond (1991) loan size is fixed exogenously and so maturity is the only dimension along which screening can occur. Conversely loan maturity is fixed in Stiglitz and Weiss (1981).

²²For simplicity we abstract from consumption at $t = 2$.

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 her exposure to adverse shocks to her ability to repay loans in the future: 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 $x_L \geq x_H$.²³

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 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 contingent 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 using income or by taking on additional debt 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

²³Note 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 $t = 2$.

their exposure to long-term shocks to their ability repay 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 exposure to short term shocks to their ability to repay. The distinction between the two is central to the analysis which follows.

B. Symmetric Information

Consider the benchmark case in which a household's type is known by all agents at $t = 1$. 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 that requires no repayment at $t = 2$.²⁴ All households receive full insurance that is provided by the default feature of the debt contract.²⁵ 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 ability to repay that is revealed at $t = 2$.

C. Asymmetric Information and Optimal Screening

Now suppose that a household's type is private information and so, from a lender's perspective, all borrowers are observationally equivalent at $t = 1$.²⁶ As is standard in screening models, low type households will be offered the same full insurance contract that maximizes their utility under symmetric information. The contract offered to high types will offer them the highest expected utility possible while ensuring that this loan is not chosen by low creditworthiness borrowers. The problem that characterizes the optimal lending contract offered to the high type is shown in Section B of the Appendix. Characterizing the optimal contract analytically is in general infeasible due to the well known "hidden savings problem" (see Kocherlakota (2004a)). To deal with this, we first analytically solve a special case of the model in which all consumption occurs at $t = 3$ ($\alpha = 1$), and then we use numerical solutions to show that the findings are robust to a wider set of cases. Applying the logic of Rothschild and Stiglitz (1976) we know that high types will be screened by choosing a contract that gives up some of the full insurance provided under symmetric information. Our focus is to show under what condition this is optimally achieved by rationing loan maturity as opposed to loan amount.

²⁴In this equilibrium, each household of type k is offered a contract at $t = 1$ of $\{A_1^{**k}, D_{1,2}^{**}, D_{1,3}^{**}\}$ where $A_1^{**k} = E(1 - p_k(1 - q) - x_k)$, $D_{1,2}^{**} = 0$, and $D_{1,3}^{**} = E$.

²⁵The insurance provided by defaultable debt is stressed in theory papers such as Zame (1993) and Dubey, Geanakoplos, and Shubik (2005) and empirical papers such as Mahoney (2015) and Dobbie and Song (2014).

²⁶In 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.

C.1. Analytical Solutions with $\alpha = 1$

We compare two extreme cases of the model analytically (all derivations in Appendix B). First, consider the case where there is only asymmetric information about a borrower's exposure to shocks to her ability to repay at $t = 3$. This will be the case when $x_H = x_L$ and $p_L > p_H$. For tractability we normalize $p_H = 0$. Under these conditions there is a set of optimal contracts that can be offered to the high type borrower. These have the following characteristics: $A_1^{*H} = E(1 - x_H)$, $D_{1,2}^{*H} + D_{1,3}^{*H} = E$, $D_{1,2}^{*H} \geq \bar{D}_{1,2} \in (0, E)$ and $D_{1,3}^{*H} \geq 0$. In words, optimal screening is achieved by any contract that has a sufficiently high repayment at $t = 2$ when there is asymmetric information about a borrowers ability to repay. There is no quantity rationing at all as the high type household borrows the fully present value of her expected income.

Next consider the opposite scenario where there is only asymmetric information about a borrowers ability to repay at $t = 2$. This will be the case when $x_L > x_H$ and $p_L = p_H$. For tractability we normalize $x_H = 0$. Under these conditions the optimal contract that is offered to the high type borrower is a long term loan with no repayment at $t = 2$: $D_{1,2}^{*H} = 0$, $D_{1,3}^{*H} \in (0, E)$, and $A_1^{*H} = (1 - (1 - q)p)D_{1,3}^{*H} < (1 - (1 - q)p)E$. In this case screening is achieved by through quantity rationing - high type borrowers chose a long maturity loan that has no repayment die at $t = 2$ for an amount that is lower than the present value of their expected income. Contrasting both cases, we see that screening is optimally achieved by targeting repayment towards the horizon from origination when the degree of asymmetric information about repayment is lowest. When the asymmetry about a borrowers ability to repay close to origination is low then short maturity debt targets repayments to this time and optimally screens borrowers. Conversely when a borrowers private information has more power to predict the ability to repay close to origination then short term debt does not achieve screening. Screening must then be achieved in another way and this is when loan amount will be used.

C.2. Numerical Solution

Next, we solve the model numerically and conduct a comparative static exercise that varies the relative degree information asymmetry about short and long-term creditworthiness. To do this let $p_H = \bar{p}_H + \frac{\Delta}{1-q}$ and $x_H = x_L - \Delta$, where $\bar{p}_H < p_L$ and $\Delta \in [0, (1 - q)(p_L - \bar{p}_H)]$.²⁷ Our comparative static exercise will show how the optimal contract offered to the high type varies with Δ . Increasing Δ lowers the degree of information asymmetry about a borrowers ability to repay at $t = 3$ ($p_L - p_H$), and raises the degree of information asymmetry about a borrowers ability to repay at $t = 2$ ($x_L - x_H$). 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 expected income.

²⁷These 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 .

Figure 7 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).²⁸ 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. The comparative statics show that when the household's private information has more power to predict their ability to repay far from origination (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 $t = 2$. When the degree of information asymmetry is higher with regard to the ability to repay in the short term credit 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.

The comparative statics presented in Figure 7 are robust to the parameter assumptions used to derive those numerical solutions. We demonstrate this in the Appendix in Figure 10. We solve the model allowing consumption at $t = 1$ by setting $\alpha < 1$ (Panel A), by varying the choice of q (Panel B) and to the choice of utility function (Panel C). The comparative statics are qualitatively the same in each case.

Our theoretical framework demonstrates that loan maturity can be used to screen borrowers thereby rationalizing our finding that borrowers who are more exposed to shocks to their creditworthiness will self select into long maturity loans. The novel insight of the model is to show that maturity rather than quantity will be the optimal screening device when the power of their private information to predict their ability to repay is increasing in the time from origination. We now use our empirical strategy to test this condition.

V. Interpretation of Results

We now exploit our empirical strategy to interpret our results. First, we test the condition implied by our framework that maturity would be the optimal way to screen borrowers is present. Second, we provide evidence to isolate what is the private information that is driving selection.

A. Is Screening on Maturity Optimal?

Our theoretical framework demonstrates that maturity is the optimal way to screen borrowers when the power of their private information to predict their ability to repay is increasing in the time from

²⁸Formally: $Duration = 1 \times \frac{D_{1,2}^{*H}(1-x_H)}{A_1^{*H}} + 2 \times \frac{D_{1,3}^{*H}(1-p_H(1-q)-x_H)}{A_1^{*H}}$.

origination. We exploit the information we have about when borrowers in our setting enter default to test this condition. 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 4 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 8 plots the coefficient from a sequence regressions run in the same manner using default from the first to the 24th month from origination.²⁹ 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.

The pattern in Figure 8 confirms that the condition for maturity to be the optimal way to screen 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.³⁰

B. Isolating the Source of Private Information

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: what is the private information that is driving the selection decision? We argue that this difference stems from borrowers who privately observe that they are more exposed to shocks to their ability to repay self select into longer maturity loans. This could stem from greater risk of income fluctuations or shocks to unavoidable expenses such as medical bills. It is important to note that since we are trying to understand the fundamental source of selection among observationally equivalent borrowers, we must focus on differences in their ex-post

²⁹At 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.

³⁰The finding that information asymmetries grow with the horizon from origination is itself new and potentially important in its own right. For example this supports the assumed time structure of information asymmetry in Milbradt and Oehmke (2014).

behavior using the measures available to us in our setting. Indeed, this analysis cannot focus on differences in variables that are observable at the time of origination, such as type of job.

Our first evidence in support of this interpretation is in Columns 3 and 4 of Table 5, where we run our main regression using a dummy for full loan pre-payment as of April 2015 as the outcome variable. The coefficient in column 3 equals -0.7% but is not statistically significant, which suggests that there is no evidence that borrowers who select into long term loans prepay less, as would be expected given that their default rate is higher. In fact, the point estimate provides weak evidence that they prepay at a higher rate. The point estimate for the full sample, -0.7% , suggests that borrowers who select into long term loans prepay at a rate that is $(-0.7\%/14\%)$ 5% higher relative to a 38% baseline as per Table 1. Further, in column 4 we show that, conditional on not defaulting, the coefficient is negative and significant at the 10%. Thus, borrowers who self-select to borrow long term loan prepay their short maturity debt at the same or slightly higher rate than those who chose instead to take the 36-month loan, and significantly so when the sample is conditional on no default. Our interpretation is that borrowers who select long maturity loans are more exposed to shocks—they prepay when the shocks are positive and default when they are negative.

Our findings allow us to rule out several alternate hypothesis. First, one possibility is that borrowers select into loans based on private information about when in the future they will have income available to repay. By this argument, a household that expects to have income further into the future may self select into long maturity loans. If borrowers are exposed to liquidity constraints then such a borrower may have an increased propensity to default on short term loans as they struggle to refinance their repayments. Our finding in Figure 8 that the differential propensity to default for borrowers who select into long maturity loans *increases* in the time from origination is the opposite of what this alternate interpretation would predict. Default should become less likely as these borrowers move further from origination and get closer to the income they are expecting in the future. By the same argument, the (weakly) higher propensity to prepay among the borrowers who select into the 60-month loan contradicts this hypothesis as well.

Another potential interpretation of our selection result is that households who borrow with the intention of strategically defaulting select into long maturity loans. This choice might minimize total repayment prior to defaulting. The fact that these borrowers exhibit an identical propensity to default over the first 12 months of a 36-month loan rejects this hypothesis. Moreover, the increased propensity for repayment cannot be explained by this account.

C. Equilibrium implications

A more direct test that selection is based on a borrower's exposure to shocks to their ability to repay 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. However, we cannot produce an independent measure of such a measure is 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. Notwithstanding this problem we can provide suggestive evidence by computing 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%.³¹ Commensurate with this increased risk, LC charges observationally equivalent borrowers who select a 60-month loan an APR that 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 borrowers who are more exposed to default risk were selected away from the short maturity loan by the menu expansion, our theory predicts that competitive pressures would eventually drive the interest rate on the short maturity loan down to reflect this. Indeed, 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 conjecture. We show this in Figure 9 which plots the average APR charged to borrowers on 36-month loans in each month controlling for loan amount and borrower characteristics.³² Consistent with our interpretation, we see that, after controlling for these observable characteristics, the APR fell, as theory suggests, by roughly 0.8%. This number is in the same order of magnitude to our estimate in Column 1 of Table 2 that showed the expected default rate of the 36-month loans fell by 0.7% as a result of the selection into long maturity loans.

VI. 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 more exposed to shocks to their ability to repay 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 as the primary cost of adverse selection. Our results indicate that maturity rationing is empirically important. More broadly, our results show that information asymmetry limits the ability of financial markets to provide insurance through the provision of long term contracts that protect borrowers from future shocks to their creditworthiness.

³¹This 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.

³²These 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.

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Appendix

Appendix A. Figures and Tables

FIGURE 1. Description of variation

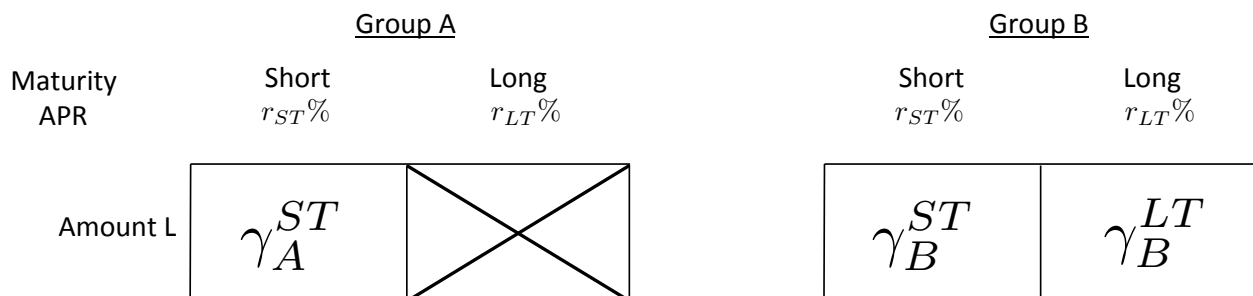


FIGURE 2. Staggered expansion of 60-month loans

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.

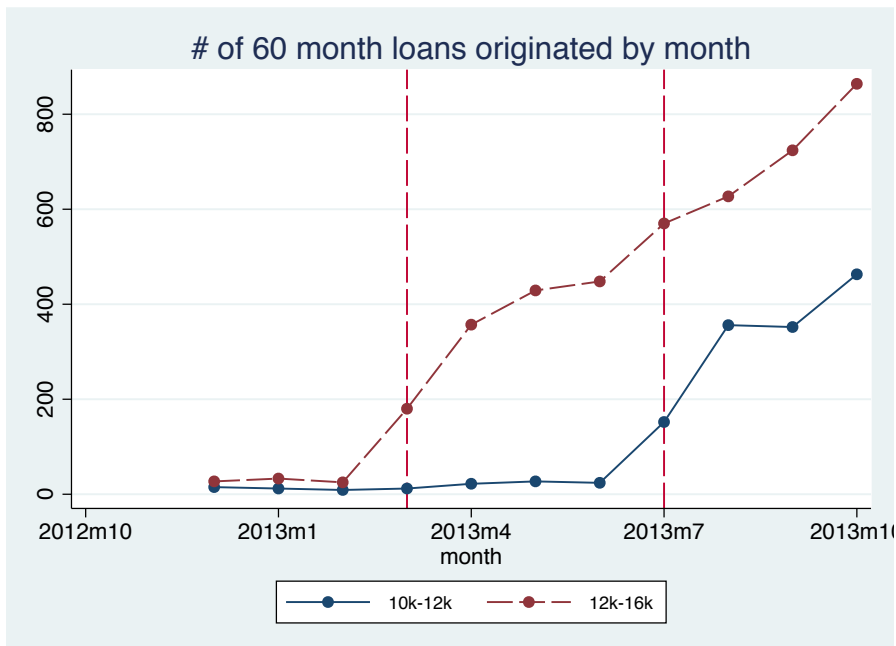


FIGURE 3. Total \$ amount issued by LC by month of listing

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.

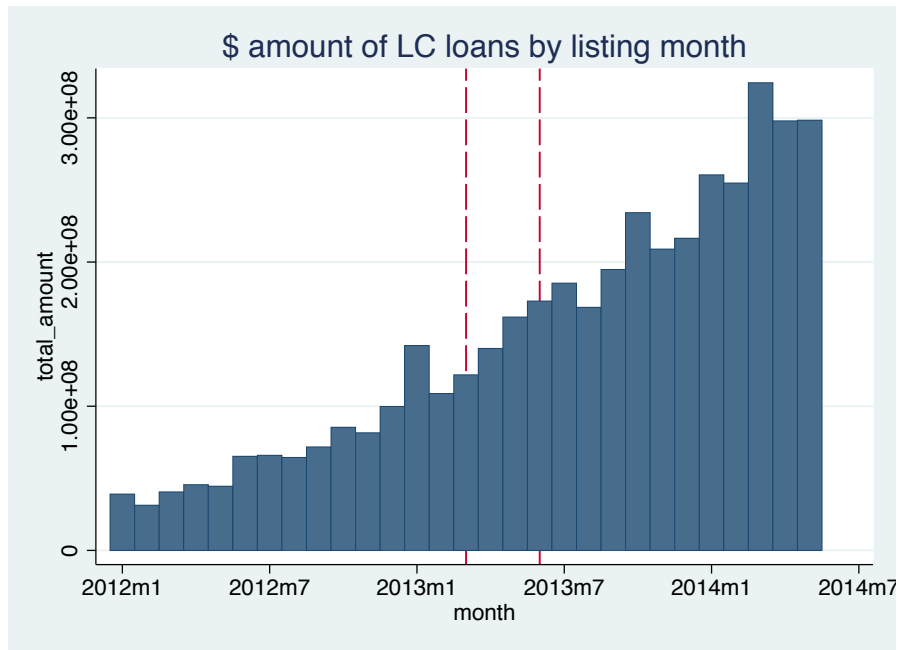


FIGURE 4. Hazard rate of default

This figure shows the hazard rate of default by month since origination for 36-month loans issued by LC in amounts between \$5,000 and \$20,000, between December 2012 and February 2013 (pre-period). 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 $t - 1$.

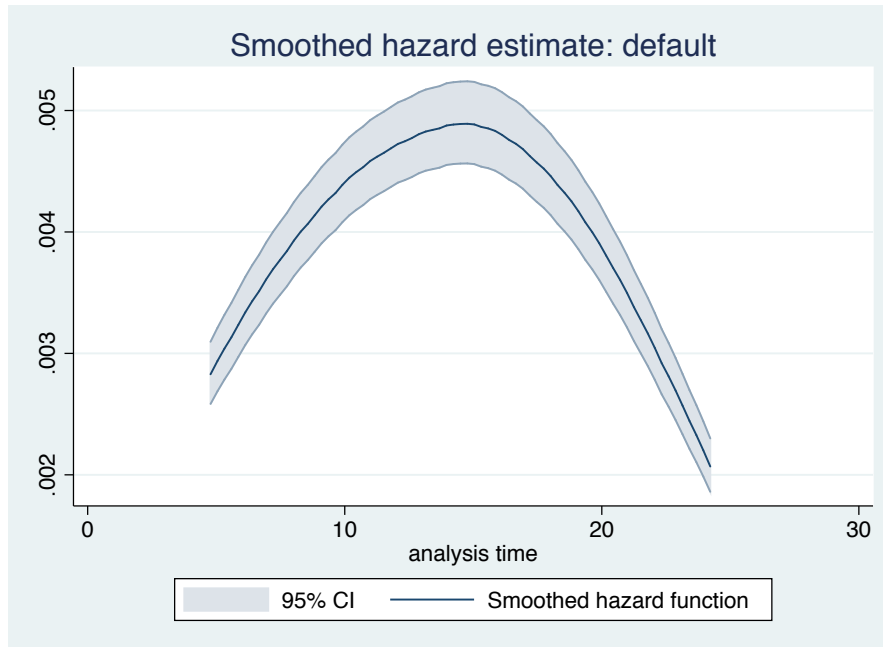


FIGURE 5. Pre-trends on number of loans originated

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

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

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.

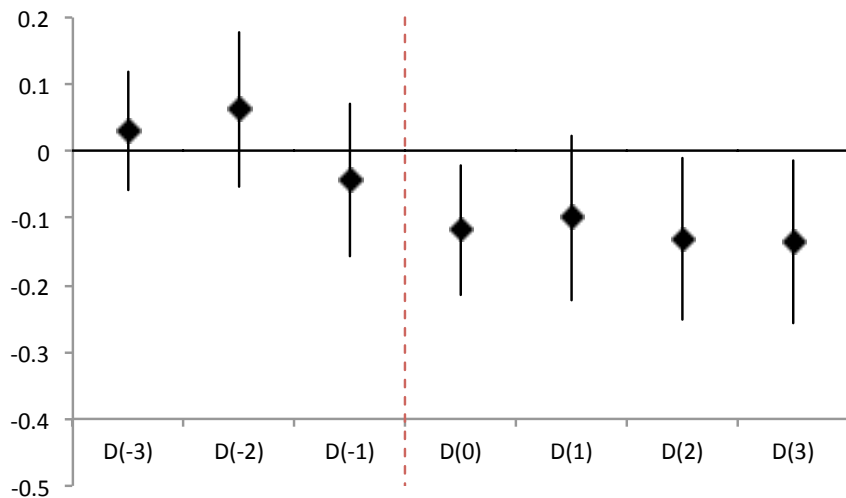


FIGURE 6. Model Time-line

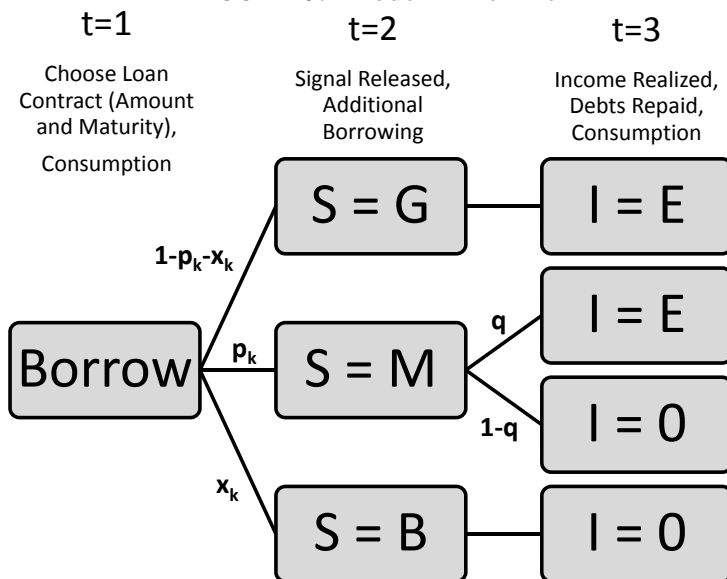


FIGURE 7. Model Comparative Statics

This figure shows comparative statics from numerical solutions of the theoretical framework presented in Section IV. 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 $t = 1$: $\frac{A_1^{*H}}{A_1^{*L}}$.

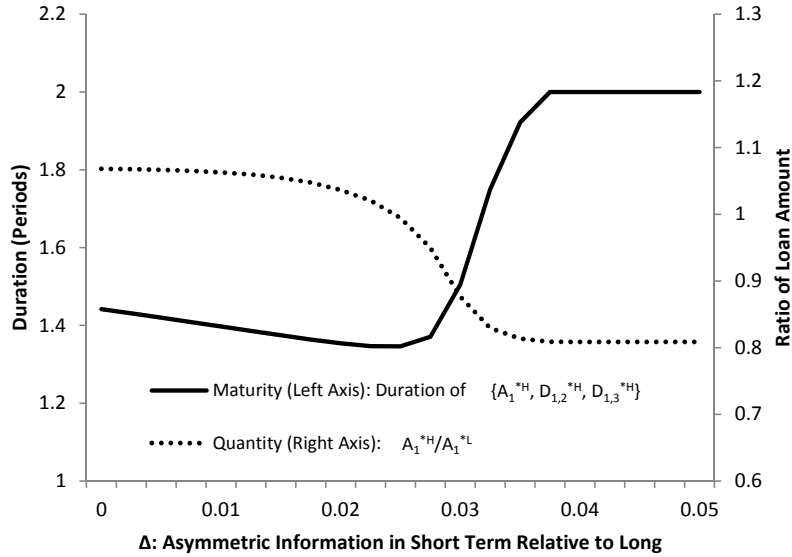


FIGURE 8. Default rate coefficient by number of months since origination

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

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

where the outcome is $default(\Delta t)$, 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 occurred Δt months after origination, on $D_{amount100,t}$, 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 \$5,000 and \$20,000.

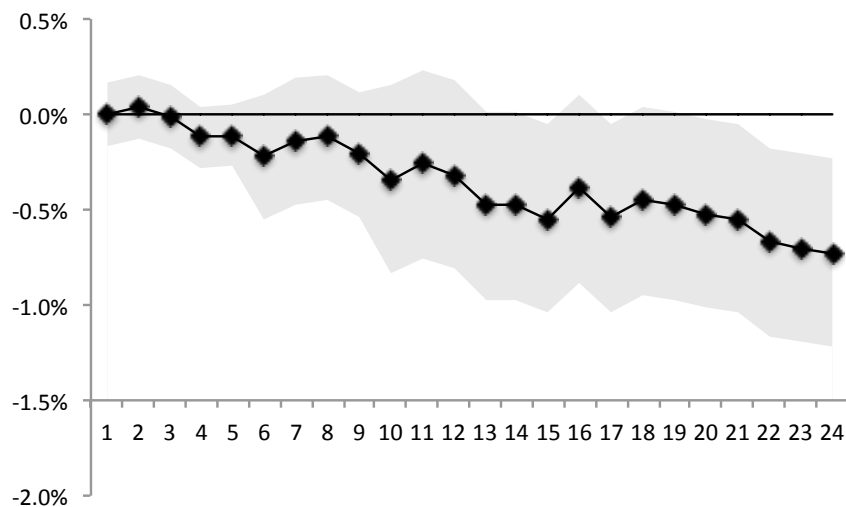


FIGURE 9. Reduction in APR

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, for 36-month loans issued between \$10,000 and \$16,000.

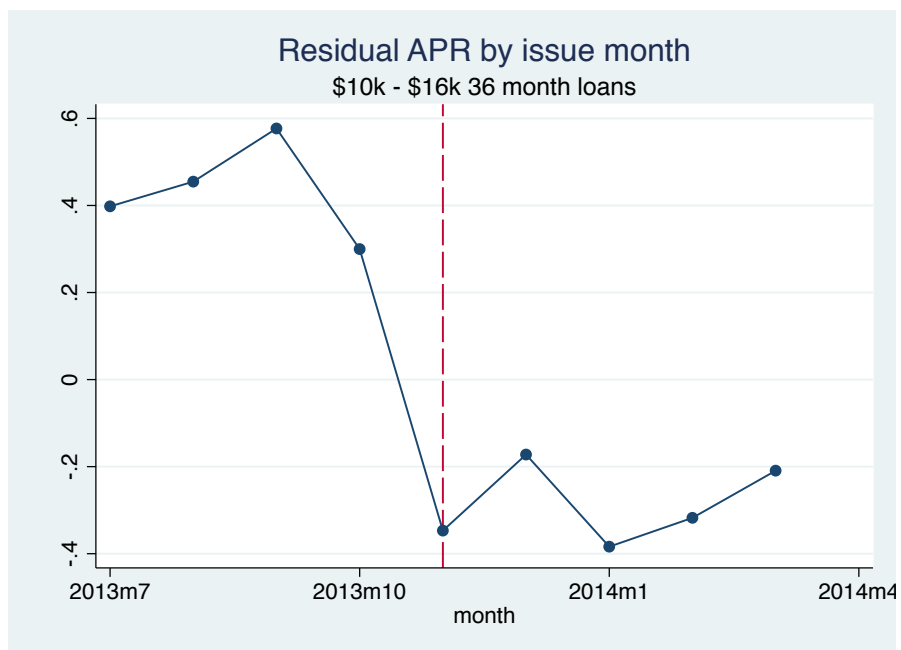


TABLE 1. Pre-period summary statistics

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 \$5,000 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 (%)	16.3	16.0	4.1
Installment (\$)	379.9	360.9	125.1
For refinancing (%)	87.0	100	33.6
Default 120 days (%)	9.2	0.0	28.9
Fully paid (%)	37.6	0.0	48.4
Panel B: borrower characteristics			
Annual income (\$)	65,745	57,500	74,401
Debt payments / Income (%)	17.4	16.9	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)	685	699	70
Home ownership (%)	55.5	100	49.7
Total debt excl mortgage (\$)	38,153	29,507	33,805
Revolving balance (\$)	14,549	11,592	12,719
Revolving utilization (%)	60.7	62.7	21.9
Months of credit history	182	164	84
N	12,091		

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 \$5,000 and \$20,000 whose list date is between December 2012 and October 2013. 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 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 \$5,000 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 \$1,000 and \$10,000; $D_{amount1000,t}$ is defined as one loan amounts between \$5,000 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)
	$\log(\#loans)$	$\log(\#loans)$	$\log(\#loans)$	$\log(\#loans)$
$D_{amount1000,t}$	-0.1451*** (0.028)	-0.0441 (0.031)	0.0586 (0.060)	-0.0355 (0.056)
Sample	Main	Placebo	36m, 16k - 24k	36m, 1k - 10k
Observations	3,663	3,861	1,637	2,374
R^2	0.817	0.862	0.802	0.761
# 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. Both outcome variables are measured as of April 2015. The sample corresponds to loan amounts between \$5,000 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. Columns 2 and 4 include 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)	(4)
	<i>default</i>	<i>default</i>	<i>FICO</i>	<i>FICO</i>
$D_{i,t}$	-0.0071** (0.003)	-0.0066* (0.003)	2.26** (1.1)	2.05* (1.0)
Sample	MAIN	MAIN	MAIN	MAIN
Observations	60,514	57,263	60,514	57,263
R^2	0.1110	0.125	0.259	0.283
# clusters	25	25	25	25

TABLE 4. Robustness

The table shows the output of several robustness tests. Columns 1 and 2 replicate columns 1 and 2 in Table 3 on a sample of loans listed between December 2012 and October 2013 and issued for amounts between \$7,000 and \$18,000 (\$2,000 narrower interval than main sample). Columns 3 and 4 show the output of the regression when the outcomes are *default* and *FICO*, respectively, for a placebo sample of loan amounts between \$5,000 and \$20,000, same as in our main sample, but listed between July 2003 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 using *default* as outcome. 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 \$1,000 and \$10,000; $D_{i,t}$ is equal to one for loan amounts between \$5,000 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. 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)
	<i>default</i>	<i>FICO</i>	<i>default</i>	<i>FICO</i>	<i>default</i>	<i>default</i>
$D_{i,t}$	-0.0058* (0.003)	2.66** (1.1)	0.0024 (0.004)	-0.433 (0.98)	0.0093 (0.013)	-0.0103 (0.008)
Sample	7k - 18k	7k - 18k	Placebo	Placebo	36m, 16k - 24k	36m, 1k - 10k
Observations	47,072	47,072	80,996	80,996	14,667	33,495
R^2	0.132	0.271	0.097	0.287	0.308	0.158
# 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 occurred 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 \$5,000 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)	(4)
	<i>default12m</i>	<i>default18m</i>	<i>prepaid</i>	<i>prepaid default = 0</i>
$D_{i,t}$	-0.0033 (0.003)	-0.0047* (0.003)	-0.0068 (0.006)	-0.0120* (0.006)
Sample	MAIN	MAIN	MAIN	MAIN
Observations	60,514	60,514	60,514	56,220
R^2	0.100	0.104	0.105	0.115
# clusters	25	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

$$(4) \quad \max_{c_1, c_3^G, c_3^M, c_3^B} (1 - \alpha) u(c_1) + \alpha \left[(1 - p_k - x_k) u(c_3^G) + p_k u(c_3^M) + x_k u(c_3^B) \right]$$

subject to

$$(5) \quad c_1 + (1 - p_k - x_k) \times c_3^G + p_k \times c_3^M + x_k \times c_3^B \leq E(1 - p_k(1 - q) - x_k)$$

Let λ^{Symm} be the Lagrange multiplier on the break even constraint (5). The first order conditions for each choice variable are

$$(6) \quad c_1 : (1 - \alpha) u'(c_1) - \lambda^{Symm} = 0$$

$$(7) \quad c_3^G : \alpha(1 - p_k - x_k) u'(c_3^G) - (1 - p_k - x_k) \lambda^{Symm} = 0$$

$$(8) \quad c_3^M : \alpha p_k u'(c_3^M) - p_k \lambda^{Symm} = 0$$

$$(9) \quad c_3^B : \alpha x_k u'(c_3^B) - x_k \lambda^{Symm} = 0$$

The first order conditions for consumption at $t = 3$, (7) (8) and (9), are satisfied if and only if $u'(c_3^G) = u'(c_3^M) = u'(c_3^B)$. 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 $\{A_1^{**k}, D_{1,2}^{**k}, D_{1,3}^{**k}\}$ is

$$(10) \quad c_3^G = A_1^{**k} - c_1 + E - D_{1,2}^{**k} - D_{1,3}^{**k}$$

$$(11) \quad c_3^M = A_1^{**k} - c_1 - D_{1,2}^{**k} + q(E - D_{1,3}^{**k})$$

$$(12) \quad c_3^B = A_1^{**k} - c_1$$

recalling that the household defaults when it is unable to repay the loan: $S = M, I = 0$ and $S = B$. Using (10) and (11) we have that $c_3^G = c_3^M$ if and only if $E - D_{1,2}^{**k} = q(E - D_{1,3}^{**k})$ which can only hold if $E = D_{1,3}^{**k}$. Using (11) and (12) we have that $c_3^M = c_3^B$ if and only if $D_{1,2}^{**k} = q(E - D_{1,3}^{**k})$ and since $E = D_{1,3}^{**k}$ this implies $D_{1,2}^{**k} = 0$. Competition ensures that the the breakeven condition (5) must hold and so $A_1^{**k} = E(1 - p_k(1 - q) - x_k)$. Using (6) and (6) the choice of c_1 will be determined by the Euler equation:

$$(13) \quad (1 - \alpha) u'(c_1) = \alpha u'(E(1 - p_k(1 - q) - x_k) - c_1).$$

B. Asymmetric Information

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

$$(14) \quad \max_{c_1, A_1, D_{1,2}, D_{1,3}} (1 - \alpha)u(c_1) + \alpha \left[(1 - p_H - x_H)u(c_3^G) + p_H u(c_3^M) + x_H u(c_3^B) \right]$$

subject to

$$(15) \quad c_3^G = A_1 + E - D_{1,2} - D_{1,3} - c_1$$

$$(16) \quad c_3^M = A_1 + qE - D_{1,2} - qD_{1,3} - c_1$$

$$(17) \quad c_3^B = A_1 - c_1$$

$$(18) \quad A_1 \leq (1 - x_H)D_{1,2} + (1 - (1 - q)p_H - x_H)D_{1,3}$$

$$(19) \quad D_{1,2} \leq A_1 - c_1 + q(E - D_{1,3})$$

$$(20) \quad D_{1,2} \geq 0$$

$$(21) \quad D_{1,3} \geq 0$$

$$(22) \quad U^{*L} \geq U^{*L'}(A_1, D_{1,2}, D_{1,3})$$

where c_3^S is the consumption that will be achieved at $t = 3$ for each possible realization of the interim signal.³³ The conditions 15, 16, 17 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 18 ensures that a lender will break even in expectation. Condition 19 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 20 and 21 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 22 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 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

³³Note 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 .

the following problem:

$$(23) \quad \max_{c'_1} (1 - \alpha) u(c'_1) + \alpha \left[(1 - p_L - x_L) u(c'_3{}^G) + p_L u(c'_3{}^M) + x_L u(c'_3{}^B) \right]$$

subject to

$$(24) \quad c'_3{}^G = A_1 + E - D_{1,2} - D_{1,3} - c'_1$$

$$(25) \quad c'_3{}^M = A_1 + qE - D_{1,2} - qD_{1,3} - c'_1$$

$$(26) \quad c'_3{}^B = A_1 - c'_1$$

where (24), (25), (26), are the counterparts to (15), (16), (17) in the problem above. Substituting (24), (25), (26) into (23) and taking the first derivative with respect to c'_1 gives the following first order condition:

$$(27) \quad \begin{aligned} (1 - \alpha) u'(c'_1) &= \alpha (1 - p_k - x_k) u'(A_1 + E - D_{1,2} - D_{1,3} - c'_1) \\ &\quad + \alpha p_k u'(A_1 + qE - D_{1,2} - qD_{1,3} - c'_1) \\ &\quad + \alpha x_k u'(A_1 - c'_1) \end{aligned}$$

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 analytically in two special cases as well as providing a range of numerical solutions 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. To simplify the problem we make use of the fact that, as is standard, the zero profit condition 18 will bind at the optimal contract. Combining this with 15, 16, 17 allows us to express consumption in each state as a function of $D_{1,2}$ and $D_{1,3}$. The Lagrangian for the constrained optimization problem is:

$$(28) \quad \mathcal{L} = \max_{D_{1,2}, D_{1,3}} (1 - p_H - x_H) u(E - x_H D_{1,2} - (x_H + (1 - q) p_H) D_{1,3})$$

$$(29) \quad + p_H u(qE - x_H D_{1,2} + ((1 - q)(1 - p_H) - x_H) D_{1,3})$$

$$(30) \quad + x_H u((1 - x_H) D_{1,2} + (1 - x_H - (1 - q) p_H) D_{1,3})$$

$$(31) \quad + \lambda_1 u((1 - p_L(1 - q) - x_L) E)$$

$$(32) \quad - \lambda_1 (1 - p_L - x_L) u(E - x_H D_{1,2} - (x_H + (1 - q) p_H) D_{1,3})$$

$$(33) \quad - \lambda_1 p_L u(qE - x_H D_{1,2} + ((1 - q)(1 - p_H) - x_H) D_{1,3})$$

$$(34) \quad - \lambda_1 x_L u((1 - x_H) D_{1,2} + (1 - x_H - (1 - q) p_H) D_{1,3})$$

$$(35) \quad + \lambda_2 D_{1,2} + \lambda_3 D_{1,3}$$

where (34) is the truth telling condition ensuring low types do not accept the contract designed to the high type while (35) ensures $D_{1,2}$ and $D_{1,3}$ are non-negative. The associated Lagrange multipliers, $\lambda_1, \lambda_2, \lambda_3$ are non-negative and obey the standard Kuhn-Tucker conditions. Observe that the truth-telling condition must bind for the optimal contract or else the high type agent would be given the full insurance contract but this is strictly preferred by the low type household. The first order conditions with respect to two choice variables are

$$D_{1,2} : \quad - (1 - p_H - x_H) x_H u'(c_3^G) - p_H x_H u'(c_3^M) + x_H (1 - x_H) u'(c_3^B) \\ + \lambda_1 [(1 - p_L - x_L) x_H u'(c_3^G) + p_L x_H u'(c_3^M) - x_L (1 - x_H) u'(c_3^B)] + \lambda_2 = 0$$

$$D_{1,3} : \quad - (1 - p_H - x_H) (x_H + (1 - q) p_H) u'(c_3^G) + p_H ((1 - q)(1 - p_H) - x_H) u'(c_3^M) + x_H (1 - x_H - (1 - q) p_H) \\ + \lambda_1 [(1 - p_L - x_L) (x_H + (1 - q) p_H) u'(c_3^G) - p_L ((1 - q)(1 - p_H) - x_H) u'(c_3^M) - x_L (1 - x_H - (1 - q) p_H) u'(c_3^B)] + \lambda_3 = 0$$

We now use (36) and (37) to characterize the optimal contract under the two scenarios considered in the paper.

B.2. Information Asymmetry Only About Ability to Repay at $t = 3$

Consider the case where $x_L = x_H = x \geq 0$ and $p_L > p_H = 0$. We conjecture that at the optimal contract $\lambda_1 = 0$. We will verify this conjecture below. Now (36) and (37) can be re-written as

$$(38) \quad D_{1,2} : x(1 - x) \left[u'(c_3^G) - u'(c_3^B) \right] = \lambda_2$$

$$(39) \quad D_{1,3} : x(1 - x) \left[u'(c_3^G) - u'(c_3^B) \right] = \lambda_3$$

Together (38) and (39) imply that $\lambda_2 = \lambda_3$. Suppose first that $\lambda_2 = \lambda_3 > 0$. By complementary slackness this would imply $D_{1,2} = D_{1,3} = 0$. In that case $c_3^G = E > c_3^B = 0$ which implies $u'(c_3^G) < u'(c_3^B)$ and hence creates a contraction. It therefore must be that $\lambda_2 = \lambda_3 = 0$. Using (38) and (39) this implies that $c_3^G = c_3^B$ at the optimal contract. Using this together with (15) and (17) we get that

the unique solution to the optimal contract problem is to set $D_{1,2}$ and $D_{1,3}$ such that

$$(40) \quad D_{1,2} + D_{1,3} = E$$

The range of allowable values of $D_{1,2}$ and $D_{1,3}$ is determined by checking that the truth-telling constraint binds. Letting $D_{1,3} = E - D_{1,2}$ the truth-telling constraint can be written as:

$$(41) \quad \begin{aligned} & u((1 - p_L(1 - q) - x)E) \geq \\ & (1 - p_L)u(E(1 - x)) \\ & + p_L u(E(1 - x) - D_{1,2}(1 - q)) \end{aligned}$$

Observe that the right hand side of (41) is strictly decreasing in $D_{1,2}$. If $D_{1,2} = 0$ then (41) is not satisfied because the right hand side becomes $u(E(1 - x))$. If $D_{1,2} = E$ then (41) must be strictly satisfied because in this case the expected consumption is the same under either contract and is risky under the high type contract. It follows immediately that there exists a $\bar{D}_{1,2} \in (0, E)$ which (41) is satisfied with equality and thus (41) is ensured to hold as long as $D_{1,2} \geq \bar{D}_{1,2}$. By complementary slackness this confirms our conjecture that $\lambda_1 = 0$. The total amount of debt raised by the high type at $t = 1$ is $A_1 = E(1 - x)$ as determined by the break even condition.

B.3. Information Asymmetry Only About Ability to Repay at $t = 2$

Consider the case where $p_L = p_H = p \geq 0$ and $x_L > x_H = 0$. Now (36) and (42) can be re-written as

$$(42) \quad D_{1,2} : \lambda_1 x_L u'(c_3^B) = \lambda_2$$

$$(43) \quad \begin{aligned} & D_{1,3} : p(1 - p)(1 - q) [u'(c_3^M) - u'(c_3^G)] \\ & + \lambda_1 [p(1 - p - x_L)(1 - q)u'(c_3^G) - p(1 - q)(1 - p)u'(c_3^M) - x_L(1 - (1 - q)p)u'(c_3^B)] \\ & + \lambda_3 = 0 \end{aligned}$$

To start we prove $\lambda_2 > 0$ by contradiction. Suppose instead that $\lambda_2 = 0$. By (42) this would imply $\lambda_1 = 0$. This implies that (43) becomes

$$(44) \quad \lambda_3 = -p(1 - p)(1 - q) [u'(c_3^M) - u'(c_3^G)]$$

Note that $c_3^G \geq c_3^M \iff D_{1,3} \leq E$ and that if one of these inequalities is strict then the other must be as well. If $D_{1,3} = E$ this would recreate the symmetric information contract and the truth-telling constraint must be violated. It follows that $D_{1,3} < E$ which implies $c_3^G > c_3^M$ but this, combined with the strict concavity of $u(\cdot)$, means that (44) requires $\lambda_3 < 0$ which cannot hold. Therefore by argument on contradiction it must be that $\lambda_2 > 0$ and by complementary slackness this requires $D_{1,2} = 0$ at the optimal contract. Further (42) implies that $\lambda_1 > 0$ at the optimal contract and hence the optimal choice of $D_{1,3}$ must be such that the truth-telling constraint binds with equality. With

$D_{1,2} = 0$ the truth-telling constraint is

$$\begin{aligned}
& u((1-p(1-q)-x_L)E) \\
& \geq (1-p-x_L)u(E-(1-q)pD_{1,3}) \\
& \quad + pu(qE+(1-q)(1-p)D_{1,3}) \\
(45) \quad & \quad + x_L u((1-(1-q)p)D_{1,3})
\end{aligned}$$

Notice that (45) is not satisfied when $D_{1,3} = E$ since this would guarantee the low type agent a higher level of consumption with certainty. Also, (45) is slack when $D_{1,3} = 0$ since this provides the same level of expected consumption and is risky. If we label the the right hand side of (45) as $\Upsilon(D_{1,3})$ then

$$\begin{aligned}
(46) \quad \frac{\partial^2 \Upsilon(D_{1,3})}{\partial D_{1,3}^2} &= p^2(1-q)^2(1-p-x_L)u''(c_3^G) \\
& \quad + [p(1-q)(1-p)]^2 u''(c_3^M) \\
& \quad + x_L [1-(1-q)p]^2 u''(c_3^B) < 0
\end{aligned}$$

where the strict inequality in (46) follows directly from the strict concavity of $u(\cdot)$. Since $\Upsilon(D_{1,3})$ is strictly concave, and feasibility requires $D_{1,3} \leq E$, then there must be a unique value of $D_{1,3} \in (0, E)$ for which (45) is satisfied with equality.

B.4. CARA Utility and $\alpha \in [0, 1]$

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

$$(47) \quad u(c) = 1 - \frac{1}{\eta} e^{-\eta c}$$

where $\eta > 0$ is the coefficient of absolute risk aversion. With this assumption (27) 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

$$(48) \quad c'_1 = \frac{-1}{2\eta} \ln \left[\frac{\alpha}{1-\alpha} \left\{ (1-p_k-x_k)e^{-\eta A_G} + p_k e^{-\eta A_M} + x_k e^{-\eta A_B} \right\} \right]$$

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 (48) into (23) and using this in (22) defines the optimal contracting problem under CARA utility. Numerical solutions to this problem are provided in Figure 7.

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:

$$\begin{aligned} A_1^{Pool} &= [\phi(1 - p_H(1 - q) - x_H) + (1 - \phi)(1 - p_H(1 - q) - x_H)]E \\ D_{1,2}^{Pool} &= 0 \\ D_{1,3}^{Pool} &= E \end{aligned}$$

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 (22) is now

$$(49) \quad U^{Pool} \geq U^{*L'}(A_1, D_{1,2}, D_{1,3})$$

where

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

It must be that the truth-telling constraint (49) 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 (49) binds for the low type then

$$(50) \quad U^{Pool} = (1 - p_L - x_L)u(c_3^G) + p_L u(c_3^M) + x_L u(c_3^B)$$

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 7 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.³⁴ 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 requested 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 modifications stay constant. For example, between December 2012 and October 2013, the amount modifications for each grade were as follows:

	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 (equivalent to 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

³⁴See <https://web.archive.org/web/20121204222850/http://www.lendingclub.com/public/how-we-set-interest-rates.action>

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 \$5,000 and \$20,000:

% of loans in main sample period not assigned an initial subgrade					
Final subgrade	%	Total loans	Final subgrade	%	Number of loans
A1	0.66	1,658	D1	0.73	2,465
A2	0.23	1,292	D2	0.22	2,291
A3	0.36	1,389	D3	0.83	1,814
A4	0.74	1,624	D4	0.33	1,508
A5	1.59	2,383	D5	0.32	927
B1	0.34	5,623	E1	0.30	328
B2	0.78	6,120	E2	0.00	437
B3	0.55	6,399	E3	0.00	208
B4	0.64	6,283	E4	0.60	166
B5	0.65	3,080	E5	0.00	120
C1	11.79	3,884	F1	85.29	34
C2	2.03	2,957	F2	92.31	52
C3	0.56	3,236	F3	100.00	9
C4	0.49	2,830	F4	100.00	9
C5	0.56	2,325	G1	100.00	1

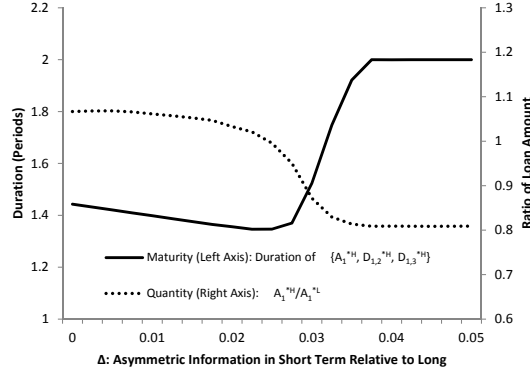
First, by construction, almost all loans below an F1 rating (26) will not have an initial subgrade because LC’s model states that only 25 initial grades are issued. Second, we succeed in matching a borrower’s initial subgrade for more than 98% of the loans of each final subgrade in 24 out of the 25 top subgrades. Grade C1 (grade 11) is slightly problematic as the success rate drops to 88.2%. The reason for this drop is that, given the algorithm presented above, we should not observe C1 loans between \$15,000 and \$20,000, but LC categorizes 458 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

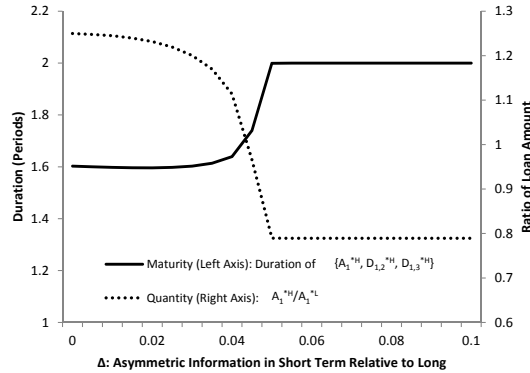
FIGURE 10. Model Comparative Statics - Robustness

This figure shows additional comparative statics from numerical solutions of the theoretical framework presented in Section IV in order to demonstrate the robustness of the results in Figure 7. The following parameters are used (identical to Figure 7): $E = 100$, $p_L = 0.3$, $x_L = 0.1$, $\bar{p}_H = 0.1$. 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 7) 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 7: $q = 0.75$ $\alpha = 1$. 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

