Screening on Loan Terms: Evidence from Maturity Choice in Consumer Credit*

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Abstract

We exploit a natural experiment in the largest online consumer lending platform to provide the first evidence that loan terms, in particular maturity choice, can be used to screen borrowers based on their private information. We compare two groups of observationally equivalent borrowers who took identical unsecured 36-month loans, only one of which had also a higher APR 60-month maturity choice available. When a long maturity option is available, fewer borrowers take the short-term loan, and those that do, default less. Additional findings suggest borrowers self-select on private information about their future ability to repay.

Keywords: Adverse Selection, Loan Maturity, Consumer Credit.

JEL codes: D82, D14.

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Asymmetric information between borrowers and lenders may induce inefficiencies in credit markets (e.g., Jaffee and Russell (1976), Stiglitz and Weiss (1981)). In theory, lenders can partially mitigate these inefficiencies by using contract terms to screen borrowers on their private information. Screening is achieved when lenders offer contracts with features that are relatively more costly to borrowers with a high probability of default, such as high collateral (Bester (1985)), short maturity (Flannery (1986)), or strict covenants (Levine and Hughes (2005)). These contracts attract unobservably safer borrowers and can thus be offered at a lower price.[1]

Although the screening role of contract terms is well established in theory, empirical evidence of its relevance remains elusive. Most stylized facts consistent with screening are derived from the correlation between borrower contract choice and *observable* information (e.g., proxies for creditworthiness or the extent of private information).[2] This evidence is circumstantial at best since, by definition, screening implies that borrowers select loan terms based on information that is *not observable* by the lender (or the econometrician). In an attempt to document selection on unobservables, recent work has turned to the correlation between borrowers' contract choices and ex-post measures of their creditworthiness (e.g., default).[3] However, even ex-ante identical borrowers will exhibit different default probabilities ex-post if they face different contract terms, for example, due to moral hazard. Thus, contract choice and default may be correlated even in the absence of screening.

In this paper we provide the first direct evidence of the screening role of debt contract terms. We first argue that, in order to empirically disentangle screening from the causal effect of contract terms, the econometrician must compare the repayment of screened and non-screened borrower samples who take the *same* loan contract.[4] We then illustrate and apply this approach in the context of consumer credit in the U.S., exploiting the staggered roll-out of long-maturity loans by an online lending platform, Lending Club (hereafter, LC). This allows us to compare the ex-post repayment behavior of ex-ante identical borrowers

[1] Examples of other contractual terms that have been shown in theory to have a screening role are inside ownership (Leland and Pyle (1977)), managerial incentives and capital structure (Ross (1977)), mortgage points (Stanton and Wallace (1998)), and prepayment penalties (Bian and Yavas (2013)).

[2] For the relationship between observable creditworthiness and (1) maturity see Barclay and Smith (1995), Guedes and Opler (1996), Johnson (2003) and (2) collateral choice see Leeth and Scott (1989), Berger and Udell (1990) Booth (1992), Degryse and Van Cayseele (2000), and Jimenez, Salas, and Saurina (2006). For the relationship between observable proxies for the degree of private information and (1) maturity see Berger, Espinosa-Vega, Frame, and Miller (2005) and (2) collateral choice see Berger and Udell (1995) and Berger, Espinosa-Vega, Frame, and Miller (2011).

[3] For examples of this approach see Goyal and Wang (2013) and Gopalan, Song, Yerramilli, et al. (2014). Since contract terms are an endogenous choice (either by the borrower ot the lender), controlling for contract characteristics in a regression estimation relies heavily on functional form assumptions and is likely to yields biased estimates due to reverse causality (for an example of this approach, see Kawai, Onishi, and Uetake (2014)).

[4] Karlan and Zinman (2009) make a parallel argument for the identification conditions to isolate empirically adverse selection on loan prices.

facing the same short-term contract, but who chose their contract facing different menu of options, and who were thus differentially selected on maturity. Maturity serves as a screening device because long maturity reduces the need to roll over debt at a higher price in the future. Higher risk borrowers, with an uncertain future observable creditworthiness, are willing to pay higher interest rates to secure this insurance.[5] Consistent with this intuition, we find that LC borrowers who take a short-term loan when a long-term option is unavailable default substantially more than observationally identical borrowers who take the same short-term loan when LC also offers a higher-priced long-term loan in the menu. Thus, maturity can be effectively used as a screening device in credit markets: offering low-rate/ short-maturity and high-rate/ long-maturity loans induces borrowers of higher unobservable risk to self-select on the high price contract.

In the empirical setting, LC borrowers choose from a menu of loan amount, maturity, and price combinations. LC offers unsecured loans for amounts between \$1,000 and \$35,000 in either short—36 months—or long maturities—60 months. Loan price, set according to a proprietary algorithm, is increasing in amount, borrower risk, and maturity. Before 2013, long maturity loans were 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, during this period LC did not change the terms of any other borrowing option nor the screening criteria to qualify for a loan.

Our empirical strategy compares the default rate of short-term loans for amounts between \$10,000 and \$16,000 issued before and after the availability of the long-maturity option at the corresponding amount (i.e., before and after the borrowers were selected on maturity). By comparing across borrowers that took identical loans (same short maturity, same price) we eliminate, by construction, the possibility that differences in repayment behavior are due to the causal impact of different contractual terms (due, for example, to moral hazard or the burden of repayment). To account for changes over time in the composition of borrowers on the LC platform we estimate a difference-in-differences specification that exploits the staggered roll-out of the long-term menu options, and uses short-term loans of amounts just above and just below the \$10,000 to \$16,000 interval to construct counterfactuals. Intuitively, our main test compares, amongst borrowers who appear ex-ante identical in all observable dimensions and who took the exact same loans, the default rate of loans between \$10,000 and \$16,000 that were issued before and after the long-maturity loan became available

[5]Following the logic in Rothschild and Stiglitz (1976), when borrowers have private information about the value they place on this insurance, the market for loan maturity may not be characterized by a single price at which borrowers can buy all the insurance—maturity—they require.

at these amounts, relative to the same change in the default rate of loans between \$5,000 and \$10,000 or between \$16,000 and \$20,000 issued during the same period. The identification assumption is that any change in the composition of borrowers within a risk category that occurs for reasons other than the menu expansion, for example due to changes in the supply of credit by other lenders, did not affect differentially loans between \$12,000 and \$16,000 in March 2013 and between \$10,000 and \$12,000 in July 2013, relative to other amounts in the analysis sample at those dates. To further ensure that all comparisons are done across observationally equivalent borrowers, we include in our specifications controls for all the borrower characteristics recorded by LC at origination, including month-of-origination, 4-point FICO range, and state fixed-effects, among others.

We begin by documenting that self-selection into long-maturity loans occurs among borrowers who would have borrowed between \$10,000 and \$16,000 had the long-maturity option not been added to the menu. We find that the number of short-maturity loans between \$10,000 and \$16,000 drops by 14.5% 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 on the same month the 60-month loan appeared in the menu for the corresponding amount.

Then we explore how selection on maturity relates to ex-post performance. We find that the average default rate of short-maturity loans decreases by 0.8 percentage points when a long-maturity loan is available at origination relative to when it is not. This implies that borrowers who look identical ex ante from the investors' perspective but who have a higher default risk *self-select* out of short-term loans and into long-term ones. Assuming that the difference in short-term loan performance is due to the 14.5% of borrowers who self-select into long maturity, these self-selected borrowers would have had a default rate 5.5 percentage points higher (0.8/14.5) than the average 36-month borrower in our sample (9.2%). The findings are thus consistent with the joint hypotheses that LC borrowers have private information related to their future repayment probability, and that this private information affects loan maturity choice. Moreover, the large economic magnitude suggests that selection on maturity provides a powerful device for identifying, among a pool of observationally identical borrowers, those with the poorest repayment prospects.

In order for maturity to be an effective screening device, long-term loans must be costlier than short-term loans because they isolate borrowers from repricing risk. Indeed, we find that holding borrower characteristics and loan amount constant, the APR for 60-month LC loans was on average 3.3% higher than the APR for 36-month LC loans during our sample period. This represents a large maturity premium relative to the contemporaneous yield curve (0.2 percentage points) and can fully be explained by the 5.5 percentage point higher default rate of those borrowers who select into the long-maturity option.[6] Consistent with a screening interpretation, only borrowers who are more exposed to repricing risk and value most the insurance provided by the long-maturity loan are willing to pay this higher maturity premium.

Having established that borrowers select maturity based on private information that correlates with their repayment prospects, we turn to understanding the economic nature of this private information. In theory, borrowers who are privately informed about their own high risk aversion will select the higher insurance against repricing risk provided by longer maturity loans (De Meza and Webb (2001)). However, if risk averse borrowers are also expected to default less, self-selection on risk aversion is inconsistent with the higher default rate exhibited by long maturity borrowers. In addition, it is unlikely that borrowers are privately informed (relative to LC's investors) about interest rate risk, the probability of credit supply shocks, or other macro determinants of the future cost of borrowing. It follows that borrowers who select long-maturity loans privately place higher value on the insurance it provides either because, 1) they are more exposed to future shocks to their observable creditworthiness (e.g., the probability of job loss or illness) or, 2) they are more exposed to rollover risk due to privately observed differences in the timing of their income.

The two explanations have different predictions regarding the timing and level of default by borrowers who self-select into long maturity. Regarding the timing of default, borrowers that self-select into long maturity because their income arrives later will tend to default *less* over time, as their income realizes. In contrast, borrowers who self-select into long-maturity loans because they are more exposed to future shocks to their ability to repay default *more* over time, as the negative shocks realize. We find that selection does not significantly affect repayment during the first twelve months after origination, even though, unconditionally, more than a third of the loans that default do so during this period. In other words, we reject the hypothesis that the propensity to default of borrowers who self-select into long maturity loans decreases over time (relative to borrowers who self-select into short maturity loans). Regarding the level of default across maturities, if borrowers prefer a long- over a short-maturity loan because their income arrives in the future, their default probability should be lower under a long-term loan that aligns payments better with the timing of income. In our setting, however, the average default probability of 60-month loans is 3 percentage points higher than that of 36-month loans (conditioning on loan amount, month of origination, and FICO). This evidence is inconsistent with borrowers self-selecting on the basis of the timing of their income, and consistent with them self-selecting on private information about the exposure to shocks to their ability to repay.

[6]The fact that the higher default rate and APR at the long-maturity option are lower than the 5.5 percentage points suggests that the causal effect of longer maturity is to reduce the probability of default.

We find additional evidence in support of the interpretation that borrowers select maturity based on private information about their exposure to shocks to their creditworthiness. We find that, on average, borrowers in the selected group–borrowers who chose the short maturity when the loan maturity was available– have higher future FICO scores and a lower time-series volatility of FICO scores relative to the non-selected group. Thus, borrowers in the selected group are both observably more creditworthy, as measured by their FICO score, and less exposed to shocks to their creditworthiness. Moreover, we find that the propensity for borrowers to prepay the short-term loan is lower in the selected group relative to the unselected group. Although this result is not statistically significant, it is inconsistent with the hypothesis that short-term loans are selected by borrowers based on private information that their income arrives sooner.

In theory, our results could also be driven by borrowers who have a preference for long-term loans for behavioral reasons (e.g., borrowers may evaluate the price of a loan by the installment amount instead of by the interest rate and fees) and who, at the same time, are more likely to default. However, 87% of LC borrowers claim to use the LC loan proceeds to repay credit card debt. Since credit card debt is essentially very long-term debt, most borrowers in our sample are actively choosing to lower the maturity profile of their debt and to *increase*, not decrease, the monthly installment amounts.[7] Thus, LC borrowers seem to be unconstrained enough to commit to increase their minimum monthly payments relative to those imposed by their existing credit card debt and sufficiently sophisticated to understand the difference between price and monthly payment amounts. Moreover, it is important to note that, for unconstrained sophisticated borrowers, loan maturity (a contractual feature of the loan) is distinct from the actual timing of loan repayments (a choice variable). An impatient borrower that has a short-term loan can lower the effective out-of-pocket payments by undertaking additional borrowing each period.

Our paper is related to but distinct from the theory of Diamond (1991), who uses a framework with asymmetric information to predict a link between *observable* creditworthiness and the type of maturity that all borrowers will pool on in equilibrium. By isolating selection on private information, our paper is also distinct to theories of maturity choice that are unrelated to ex-ante asymmetric information such as: asset maturity matching (e.g., Myers (1977), Hart and Moore (1994)), agency problems (e.g., Hart and Moore (1995)), market conditions (e.g., Barry Bosworth (1971), Taggart (1977)), minimize rollover risk (e.g., Graham and Harvey (2001)), predictable violations of the expectations hypothesis (e.g., Baker, Greenwood, and Wurgler

[7] For comparison, the monthly installments of a $10,000 5-year 10% APR LC loan would be $210, while the minimum repayment per month in a credit card with the same balance and APR would be $93. If the credit card APR were 20%, the minimum monthly payments would be $157, still lower than the monthly installments in the LC loan.

(2003)), and government behavior (e.g., Greenwood, Hanson, and Stein (2010)). Our paper contributes to this literature by relating maturity choice to a borrower's private, i.e. *unobservable*, information.

Our paper also contributes to a relatively small empirical literature that has measured adverse selection in credit markets. Karlan and Zinman (2009) use an experiment in South Africa that isolates adverse selection on loan interest rates by randomizing the offered loan interest rate but resetting all loan terms after selection occurs. A different approach is taken by Adams, Einav, and Levin (2009) and Dobbie and Skiba (2013) estimate adverse selection on loan amount among subprime borrowers as a residual, given by the correlation between default and loan size that cannot be explained by the direct effect of loan size on default. Our results not only constitute evidence of adverse selection on a novel contract term (maturity), but also demonstrate that selection on maturity allows the lender to charge prices that are commensurate with borrowers' unobserved default risk. Finally, our results suggest that the screening role of maturity may extend to other settings where long-term contracts provide insurance against repricing risk, such as labor (Holmstrom (1983)) and health insurance markets (Cochrane (1995), Finkelstein, McGarry, and Sufi (2005)).

The rest of this paper proceeds as follows. Section I describes the LC platform and the data, as well as the expansion of the supply of long-maturity loans. In Section II we describe our empirical strategy and document that borrowers who self-select into long-maturity loans exhibit a higher propensity to default on the short-term loan. In Section III we evaluate what is the specific private information that is driving selection. Section IV concludes.

I. Setting

A. Lending Club

LC is the largest online lending platform in the U.S. In 2014 alone, LC originated \$4.4B in consumer loans across 45 states. By comparison, Prosper Marketplace, its nearest rival, 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. Loans are funded directly by institutional and retail investors (LC holds no financial stake in the loans), and 80% of the total funds are provided by institutional investors (Morse (2015)). Since each loan is considered an individual security by the Securities

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

and Exchange Commission, the agency that regulates online loan marketplaces in the U.S., LC is required to reveal publicly all the information used to evaluate the risk of each loan. This is an ideal institutional setting for the purposes of studying screening borrowers based on their private information, since we have all the borrower information that the lenders and investors observe at the time of origination.

When a borrower applies for a loan with LC, she first enters her yearly individual income and sufficient personal information to allow LC to obtain the borrower's credit report. 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. Every loan application is processed in two steps. First, LC decides whether or not a borrower is eligible for a loan on the platform. The eligibility decision is made mechanically based purely on hard borrower information observable at the time of origination. For example, during 2013 LC only issued loans to borrowers with a FICO score over 660, non-mortgage debt payments to income ratio below 35%, and credit history of at least 36 months. If LC determines that a borrower is eligible for a loan in the first step, she is then assigned to one of 25 risk categories (labeled by LC as risk "subgrades"). This assignment is made using a proprietary credit risk assessment algorithm that uses the hard information in a borrower's credit report (e.g., FICO score, outstanding debt, repayment status) and income. The assignment to risk category is made *prior* to the borrower selecting a loan amount or maturity and is therefore independent of both choices. The risk category determines the entire menu of interest rates faced by the borrower, for all loan amounts and for the two available maturities. That is, two borrowers assigned to the same risk category at the same time will face the same menu of interest rates for all amounts and for the two maturities. Interest rates for each subgrade are weakly increasing in amount and strictly increasing in maturity (ceteris paribus). The terms of all loans, other than interest rate, amount, and maturity, are identical. Once a borrower selects a loan from the menu it is listed on LC's website for investors' consideration. Investors cannot affect any of the terms of the loan: they only decide whether or not to fund it. According to LC, over 99% of all listed loans are funded.[9] Thus, we ignore the supply side of funds in the analysis. As of 2013, 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 to investors.

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

B. Staggered expansion of 60 month loans

Before March 2013, 60-month loans were only available for loans of \$16,000 and above. A borrower could not synthetically create a 60-month loan for an amount less than \$10,000 using prepayment, because prepayment reduces the number of installments without changing their amount, effectively reducing the maturity of the loan. In March 2013 LC introduced to the menu 60-month loans between \$12,000 and \$16,000. And in July 2013, it further expanded the available 60-month loans to include amounts between \$10,000 and \$12,000. The consequences of the menu expansion can be seen in Figure 1, where we plot the fraction of loans originated every month that have a 60-month maturity, in groups of loan amount. On December 2012, the first month of the analysis sample period, around 40% of loans between \$16,000 and \$20,000 are 60-month loans. This fraction remains relatively constant throughout the sample period, until October 2013. The fraction of 60-month loans is zero for loan amounts below \$16,000 in December 2012, and jumps up for \$12,000 to \$16,000 loans in March 2013, and then for \$10,000 to \$12,000 loans on July 2013. By the end of the sample the fraction of 60-month loans stabilizes at around 30% for \$12,000 to \$16,000 loans and around 25% for \$10,000 to \$12,000 loans. The fraction of 60-month \$5,000 to \$10,000 loans remains at zero throughout the sample period. As we discuss in detail in Section II, our empirical strategy exploits the fact that loan amounts between \$10,000 and \$16,000 were affected by the expansion of a long maturity option, and that loan amounts outside this range were not.

C. Summary statistics

LC makes publicly available in its website all the information used to assign borrowers to risk categories, the assigned risk category, and the loan performance of all funded loans. Our main analysis is conducted using data downloaded from LC's website as of April 2015. The data is a cross section of all loans originated at LC. Variables are measured either at the time of origination (e.g. date of loan, loan terms, borrower income and credit report data, state of residence) or at the time of the performance data download (e.g. loan status, time of last payment, current FICO score of borrower). We complement our main outcomes, which are measured as of April 2015, with measures of FICO score obtained from two previous loan performance updates, August 2014 and December 2014.[10]

We use the origination date of each loan to restrict the sample period of the analysis to meet two criteria: 1) that it contains the dates in which the 60-month loan menu was expanded (March 2013 and June 2013)

[10] This allows us to estimate a measure of time-series volatility of FICO score for each individual.

and that are the basis of our empirical analysis, and 2) that the interest rate assigned to each amount-maturity combination remained constant within each risk category (in other words, that all menu options other than the added long-term option remained constant). Thus, the beginning and ending months of our analysis sample are determined by two dates, surrounding the menu expansion events, on which we observe that LC repriced menu options (December 2012 and October 2013). We verify empirically that the interest rates of all risk category-amount pairs for 36-month loans are unchanged between these dates.[11]

We further limit the sample of loans to include those for amounts between $5,000 (closed) and $20,000 (open) because the interest rate schedule jumps discretely at $5,000 and $20,000 for all credit risk categories.[12] This interval includes all 36-month loans issued at amounts affected by the 60-month borrowing threshold reduction ($10,000 to $16,000), as well as amounts above and below this interval that allow us to control for any time-of-origination changes in unobserved borrower creditworthiness or credit demand. Finally, we further limit our sample to those loans where we can uniquely match the loan that a borrower chose to the menu associated with the risk category she was assigned to based on her publicly available data. We obtain this unique match for 98.6% of all loans in the sample period (we drop observations for which this matching does not yield a unique value). Our final sample has 60,514 loans.[13]

Table 1, Panel A, presents summary statistics for the subset of our sample corresponding to the 12,091 36-month loans with amounts between $5,000 and $20,000 issued between December 2012 and February 2013 (prior to the menu expansion). 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.[14] According to this definition, 9.2% of the loans in the sub-sample are in default as of April 2015. Figure

[11]The exact dates correspond to loans listed as of December 4, 2012 and October 25, 2013. Even though we refer to months as the borders of the interval, all our analysis consider these two dates as the starting and end points of the sample period, respectively. We verify empirically that the interest rates of all risk category-loan quantities pairs are unchanged over this period. For example, Figure 10 in the Internet Appendix shows supply schedules (rate versus amount) before and after the expansion of the menu of borrowing options for borrowers assigned to risk categories B1 through B5: the graphs are identical. We establish the same point in general in a tractable way in Appendix F by regressing the interest rate of all 36 month loans in our sample on fixed effects for loan amount by risk category. The regression yields an R^2 of 99.7%, which confirms that the pricing of each menu was constant throughout the sample period for all 25 risk categories.

[12]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 $6,000 and $19,000, a $1,000 narrower interval. Also, in some placebo tests we shift our sample to loans issued between July 2013 and May 2014.

[13]See Appendix E for details on this reverse-engineering procedure. The error in matching loans to their subgrade does not vary systematically over the same period or by loan amount.

[14]We also define a borrower to be in default if she is reported in a "payment plan". Our results are robust to not including these borrowers as in default.

2 shows the default hazard rate by months-since-origination for loans issued before the menu expansion.[15] 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. LC borrowers 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 (the average revolving credit limit is $27,464). LC borrowers have on average approximately 15 years of credit history.

We compare our summary statistics to the credit card user statistics from Agarwal, Chomsisengphet, Mahoney, and Strobel (2015) to obtain a sense on how representative LC borrowers are of the average US consumer credit user within the same FICO range. Using the average credit card limit in the subsample of borrowers with FICO scores between 660 and 719 ($7,781) and assuming the average number of credit cards held by the average card-holder is 3.7 (according to Gallup 2014 survey) implies that the representative U.S. user of consumer credit has a revolving credit limit of $28,789, very close to the $27,464 average revolving credit limit of the LC borrowers in our sample. Thus, LC's selection criteria imply that the analysis sample is drawn exclusively from prime U.S. consumer credit users (as measured by FICO scores), but LC borrowers do not seem to be different in their revolving credit availability to the average U.S. consumer credit user in the same FICO range.

II. Measuring Selection On Maturity

A. Empirical Strategy

We exploit the staggered menu expansion of 60-month loans during 2013 to identify screening on maturity. As described above, LC offered new loan options at longer maturities for amounts already offered on short-term contracts prior to the expansion. Crucially, the pricing of all loan options available prior to the expansion was unchanged after the expansion for all 25 risk categories during our sample period. This ensures that the only difference in the menu of borrowing options offered to borrowers assigned to the same risk category before

[15] The date of default is determined by the last payment date, a variable that is available in the LC data.

and after the expansion is the availability of 60-month loans in lower amounts.[16]

We compare the outcomes of borrowers who took the short-term loan before the menu expanded with those who were assigned to the same risk category and took it after the expansion. We develop a research design that accounts for any other changes over time in the composition of borrowers within a risk category that are not driven by the menu expansion. The LC setting provides two sources of variation that allow us to construct a counterfactual using a difference-in-differences approach: 1) the menu expansion was staggered over time for different loan amounts (*eventually-selected* amounts), 2) some loan amounts were never affected by the menu expansion (*never-selected* and *always-selected* amounts).

The three groups of loans defined this way by the loan amount and the time of origination are represented in Figure 3. Loans of amounts between \$10,000 and \$16,000 are eventually-selected, in the sense that they are unselected at the beginning of the sample (no long-term option available at the time of origination) and selected (long-term option available) at the end of the sample. Since the menu expansion was staggered, loan amounts between \$10,000 and \$12,000 serve as a control group for loan amounts between \$12,000 and \$16,000 that were affected by the March expansion and the reverse applies for the expansion in July. We build two additional control groups with loan amounts whose selection status was not affected by the menu expansion. The always-selected, for which the long-term loan was always available at the time of origination during the sample period (\$16,000 to \$20,000), and the never-selected, for which the long-term option never became available (\$5,000 to \$10,000). Our identification assumption is that any change in the composition of borrowers within a risk category, for example, due to changes in the economic environment, changes in the borrowing options outside of LC, or changes in how LC assigns borrowers to risk categories, does not affect differentially borrowers opting to take loans between \$12,000 and \$16,000 in March and borrowers opting to take loans between \$10,000 and \$12,000 in July relative to loans issued at control amounts. Under this assumption, comparing the change in performance of eventually-selected amounts before and after the menu expansion at those amounts with the change in performance of the control amounts in the same risk category isolates the effect of maturity selection induced by the menu expansion. We further include a comprehensive set of granular borrower controls, which ensures that the estimations come from comparing borrowers who took loans at selected amounts to *observationally equivalent* borrowers taking loans at non-selected amounts.

Before providing evidence to support the identification assumption (see section II.E below), we discuss

[16]Note that due to the upfront origination fee, 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 changed ex post by the expansion itself.

here its plausibility. First, even though it is unlikely that changes in economic conditions may have affected the demand for loans between \$10,000 and \$16,000 exactly at the same month of the menu expansion, to check whether there were any aggregate changes in the demand for LC loans we plot in figure 4 the total dollar amount of LC loans issued by month. There is no indication that the growth rate of LC lending changed around the dates of the two 60-month loan expansions. Second, in web searches we found no evidence of a change in the outside borrowing options that *exclusively* targeted the eventually-selected loan amounts (\$10,000 to \$16,000) in a manner that corresponds with the staggered expansion of the menu. Third, we found no evidence that LC released advertisement targeted at 60-month loans between \$10,000 to \$16,000 during the analysis sample. On the contrary, according to the information reported in the website Internet Archive, LC continued to advertise that 60-months loans were available only for amounts above \$16,000 until November 2013, after our analysis period ends.[17] Fourth, any change in LC's screening process or assignment to risk categories cannot, by construction, affect borrower selection across different amounts within a risk category. The reason is that both eligibility for an LC loan and the assignment to risk categories are determined using borrowers' observable information *before* the borrower selects a loan amount from the menu. Nevertheless, we verify that the criteria used to determine eligibility to a LC loan (the minimum FICO score of 660, minimum credit history length of 36 months, and maximum non-mortgage debt to income threshold of 35%) remain constant over the sample period.

It is important to emphasize why our estimates rely exclusively on a comparison of 36-month loans taken before and after the expansion, and ignore any changes in the composition of borrowers who take 60-month loans. There is no appropriate counterfactual for borrower selection on the 60-month loans. The mix of borrowers taking a 60-month loan could have changed, for example, because some borrowers who take the 60-month loan would have not borrowed at all before this option became available in the menu. Since we are unable to account for such selection on the extensive margin for 60-month loans, we are limited in how much we can infer about the determinants of the performance of the 60-month loans. The focus on 36-month loans also implies that our approach for measuring the effect of selection is based on a revealed-preference argument, which relies on the axiom of independence of irrelevant alternatives. Specifically, we assume that a borrower who prefers not to borrow from LC over taking a 36-month loan when there is no 60-month option available will not prefer to take the 36-month loan once the 60-month loan becomes available.

Finally, we note that the empirical approach is aimed at estimating the effect of selection on maturity in

[17] Indeed, we found no evidence of any change in outside borrowing options or advertisement campaigns at all.

LC loans. If LC borrowers have access to 60-month loans between \$10,000 and \$16,000 at a similar price elsewhere during the analysis period, we should fail to reject the null hypothesis and conclude that there is no screening on maturity in LC (since borrowers who wish to select long-term loans would already be taking them elsewhere). In effect, any impact of the menu expansion at LC can also be interpreted as indirect evidence that consumer credit markets are imperfectly competitive. This might be true because some intermediaries have a technology advantage over others that generates some market power or because there are search frictions in the market.[18]

B. Evidence of Selection

We start by measuring the amount of selection induced by the menu expansion: how does the number of borrowers who take the short-term loan at any given amount change after the long-term option becomes available at that amount. To do so we collapse the data and count the number of loans N_{jkt} at the month of origination $(t)\times$risk category $(j)\times$\$1,000 loan amount bin (k) level for all 36-month 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). We define a "selected" 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:

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

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

$$log(N_{jkt}) = \beta'_k + \delta'_{jt} + \gamma' \times D_{kt} + \varepsilon_{jkt}. \qquad (1)$$

The coefficient of interest is γ', the average percent change in the number of short-maturity loans originated for eventually-selected amounts (i.e., amounts in which a long-maturity loan was not available at the beginning of the sample and became available due to the menu expansion) 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, risk category$\times$month fixed effects δ'_{jt} control for any changes over time in the number of borrowers who are

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

approved at each of the 25 different risk categories.

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 implies that the number of borrowers who took a short-term loan is 14.5% 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 if the long term option had not been available.[19]

In the Internet Appendix Table 6 we conduct robustness tests where we vary the dimensions along which we collapse the loan-level data to count the number of loans. There we show that the selection result is slightly smaller in magnitude, ranging from 6.3% to 10%, but statistically significant across all specifications when we collapse the data in month of origination$\times$risk category$\times$\$1,000 loan amount$\times$ 4-point FICO score bins (Column 1 in Panel A), month of origination$\times$risk category$\times$\$100 loan amount $\times$ 4-point FICO score bins (Column 1 in Panel B), and month of origination$\times$ 4-point FICO score$\times$5-point debt-to-income bins (Column 1 in Panel C). We emphazise the estimated coefficient of 14.5% shown in column 1 of Table 2 as our baseline result because it implies the smallest difference in default rates for individuals who choose the short term loan. We note that, qualitatively or quantitatively, none of our results, except the magnitude of the average difference in default rates, depend on this choice.

C. Selection and Repayment

Having shown that the expansion of the menu of borrowing options induced a significant amount of self-selection from short-term to long-term loans, we run our main test to uncover the unobserved quality of the borrowers who selected into the new long term contract. We estimate the following difference-in-differences specification on the sample of 36-month loans:

$$Default_i = \beta_i^{1000bin} + \delta_i^{jt} + \gamma \times D_i + X_i + \varepsilon_i, \tag{2}$$

where data is at the loan level i. The outcome variable, $Default_i$, is defined as a dummy that equals one if the loan is late by more than 120 days measured as of April 2015. Standard errors are clustered at the state level (45 clusters).

[19] Standard errors for estimates of equation (1) are robust to heteroskedasticity, but other alternatives, e.g., clustering in any dimension, are irrelevant in terms of statistical significance. For example, when clustering at the risk category level (25 clusters), the standard error of the coefficient γ' in Column 1 of Table 2 is 0.028.

The main explanatory variable of interest, D_i, is a dummy equal to one if the 36-month loan i is issued at a time when a 60-month loan of the same amount is also available, and zero otherwise:

$$D_i = \begin{cases} 1 & if\ \$16,000 > LoanAmount_i \geq \$12,000\ \&\ t \geq March\,2013 \\ 1 & if\ \$12,000 > LoanAmount_i \geq \$10,000\ \&\ t \geq July\,2013 \\ 0 & otherwise \end{cases}$$

The coefficient of interest, γ, measures the change in the default rate of 36-month loans for eventually-selected amounts before and after the expansion of the menu options, relative to the change of the default rate for never-selected and always-selected amounts, which were not affected by the menu expansion. We include granular month of origination $t\times$risk category j fixed effects, δ_i^{jt}, which ensure we compare borrowers who took a loan on the same month with the same contract terms and with similar observed measures of credit risk (same risk category). We also include a vector of control variables observable at origination, X_i. In our baseline specification, X_i includes 4-FICO score-at-origination bin and state fixed effects. The rich set of fixed effects ensures that we perform the difference-in-differences estimation by comparing borrowers that are observationally equivalent. We also report results including as controls the *full* set of variables that LC reports and that investors observe at origination. These variables (61 in total) include, 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.

Table 3, columns 1 and 2, reports results of regression (2). The negative point estimate for γ indicates that borrowers who take a 36-month loan once a 60-month option is available are significantly less likely to default than borrowers who take the same 36-month loan when the long term option is not available. The point estimate of -0.0081 means that the default rate of the borrowers that are selected on maturity is 0.8 percentage points lower than the default rate of the non-selected borrowers (column 1), and the magnitude is unchanged when we include as additional controls every single variable observable at origination in LC's dataset (column 2). The fact that our estimate is virtually unaffected by including this full suite of additional controls demonstrates that the granular fixed-effect structure in our baseline regression is sufficiently comprehensive to absorb any changes in the composition of observed borrower characteristics.

This decline in the default probability is due to the borrowers that self-select into the long-term loan, which we estimated to be 14.5% of the borrowers in the not-selected sample (Table 2, column 1). Combining the two results allows us to obtain an estimate of the default probability of the borrowers that self-selected into the 60-month loan: it is $^{0.8\%}/_{14.5\%} = 5.5\%$ higher than those who self-select into the 36-month loan when the long-term loan is available (significant at a 10% level, based on bootstrapping with 1,000 repetitions). This is an estimate of the counterfactual probability we are after: it is the default rate that borrowers who self-selected into the 60-month loan would have had if they had taken the 36-month loan. The economic magnitude of this difference is large compared to the average default rate of 36-month loans issued before the menu expansion, 9.2% (Table 1). The comparison implies that amongst observationally equivalent borrowers, those who self-select into a long maturity contract are 59% more likely to default than those borrowers who self-select into the short term contract, ceteris paribus (e.g., holding constant the contract characteristics).

The results suggest that maturity choice reveals unobserved heterogeneity among borrowers. The lower default rate of borrowers who self-select into a short-maturity loan cannot be predicted by variables available to investors at the time of origination, as attested by the comparison between the estimates with and without controls for observables. Although we do not control for the exact FICO score but for scores *within* each 4-point FICO bin, the predictive power of FICO on default in our sample is too small for selection within 4-point FICO bins to account for our results. Indeed, a regression of $Default_i$ on the high end of the FICO 4-point range at origination, including risk category by $1,000 amount bin by month fixed effects, gives a coefficient of -0.0000362. That is, 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 0.8% reduction in default.

D. Screening and the APR Premium for 60-month Loans

In order for maturity to operate as a screening device, low-risk borrowers must be rewarded with a lower APR for self-selecting into 36-month loans. We estimate the difference in the long and short-term loan by running a regression of *APR* on *Long*, a dummy that equals one for long term loans, controlling by credit risk grade by month by $1,000 amount by 4-point FICO range fixed effects. As Internet Appendix Table 8 shows, LC charges a 3.3% higher APR for 60-month loans, holding all borrower and loan characteristics constant.[20]

[20]The distribution of the long-short spread is shown in the Internet Appendix Figure 9, where we plot the median long- and short-term APRs for loans issued between $15,000 and $20,000 in the post period by subgrade (Panel A) and initial 4-point FICO range (Panel B).

Importantly, this APR differential cannot be explained by the upward sloping yield curve during our sample period. In Internet Appendix Section C we show that the upward sloping treasury yield curve on March 1, 2013 would imply an APR premium between the 36 and 60-month loan of 0.2 percentage points. This implies that 3.1% of the 3.3% differential in our sample (94%) cannot be explained by the yield curve. Thus, borrowers who selected into the 60-month loan option were required to pay a premium for the insurance provided by this contract, consistent with any screening mechanism. Further, the APR difference between short and long-maturity loans can be more than fully accounted for by the 5.5% higher expected default rate of those borrowers who self-select into the long-maturity option.[21]

E. Identification Tests

E.1. Evidence to Support the Identifying Assumption

Our empirical strategy rests on the identifying assumption that there were no changes in unobserved borrower creditworthiness that differentially impacted borrowers taking loans between \$12,000 and \$16,000 in March and borrowers taking loans between \$10,000 and \$12,000 in July. One potential threat to this assumption is the possibility that there is a gradual shift in the composition of borrowers over 2013 that approximately matched the pattern of the staggered expansion. We test for this possibility 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:

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

and we run the following regression:[22]

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

Figure 6 shows the results of regression 3. The results show no differential pre-trends in the three months

[21] As we discuss below, the fact that the APR gap is less than 5.5% suggests that longer maturity may causally lower the expected default rate on a loan.

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

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 5 of Table 2 shows the results. The coefficient on D_{kt} equals -4.4% and is insignificant, and given the confidence interval we can reject the null that this coefficient equals our main estimate.

E.2. Simultaneous Choice of Maturity and Loan Amount

A second identifying assumption behind our empirical approach is that the choice of loan amount is sufficiently inelastic to loan maturity. If this is not the case, the difference-in-difference estimate will be biased towards zero. This could be the case either because borrowers in what we classify as eventually-selected amounts may be already selected on maturity before the menu expansion (e.g. if some borrowers who wanted to take a long-term loan at a treated amount before the expansion took a long-maturity loan at larger amount instead) or because borrowers in what we classify as never-selected amounts may be a selected group after the menu expansion (e.g., because some borrowers who wanted to take a long-term loan at a control amount after the menu expansion took a long-maturity loan at a treated amount instead).

We first consider the possibility that eventually-selected amounts are selected before the menu expansion. As an example, consider borrowers who would like to take a \$10,000 60-month loan, which is not available before the menu expansion. The closest feasible alternatives are: 1) a \$10,000 36-month loan, and 2) a \$16,000 60-month loan.[23] Our empirical strategy will estimate the effect of maturity on selection if borrowers choose the first option, e.g. take a loan for the amount they prefer at a shorter maturity—36-months— when the 60-month option is not available. The reason is that these borrowers select out of the 36-month loan when the 60-month option is available, after the menu expansion.[24] If, on the contrary, borrowers choose the second

[23]These borrowers may also choose not to borrow at all when their preferred option is not available in the menu, and take the 60-month loans when it becomes available. This extensive margin will not affect our estimates, since our results are based exclusively on the behavior of 36-month loans before and after the menu expansion.

[24]One way to test for whether borrowers at control amounts are selected before the menu expansion is to look for evidence of bunching at the borders of the treated interval. The top panel in Figure 5 presents the pre-period loan amount histogram at the short maturity. The histogram suggests that borrowers choose "round" numbers like \$10,000 and \$12,000 much more frequently than other intermediate amounts. In turn, this makes it very hard to find evidence of bunching at specific amounts.

option, e.g. take a 60-month maturity loan but for a larger amount, then our difference-in-differences estimate will be zero. Indeed, these borrowers will not be in the eventually-selected group of loans before or after the expansion because our estimation is based exclusively on the outcomes of 36-month loans. Thus, selection from one long-term loan to another will not affect our estimates.[25]

Now consider the second case, where the never-selected amounts are treated after the menu expansion. Take for example borrowers who 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 eventually-selected and control loans will be biased towards zero.

We investigate formally whether eventually-selected loans amounts were affected prior to the expansion or if control loan amounts were impacted after the expansion. To do this we exploit the same set-up as regression 1, which measures the change in the number of short-term loans issued at eventually-selected and control amounts, before and after the menu expansion, and compare the evolution of the number of 60-month (36-month) loans in the \$16,000 to \$20,000 (\$5,000 and \$10,000) range relative to the evolution of the number of loans in the \$20,000 to \$24,000 (\$1,000 and \$5,000) range around the menu expansion. We estimate the same difference-in-differences regressions with a modified definition of the "selected" dummy D_{kt} to equal 1 one after March 2013 or July 2013 for different loan amounts according to the timing of the menu expansion, as explained below.

First, using the sub-sample of 60-month loans for 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 eventually-selected loan amounts experienced selection to 60 month loans prior to the expansion. The coefficient on the interaction term is -8.25% and is not significantly different from zero (Table 2, column 2). This suggests weak evidence that our estimates may understate the degree of selection because some borrowers in eventually-selected amounts may have opted for 60-month loans above \$16,000 prior to the expansion.[26]

[25]The bottom panel in Figure 5 presents the pre-period loan amount histogram at the long maturity. The histogram has the same pattern as the top panel. Evidence of bunching is, again, very hard to establish because of borrower's preference for round numbers.

[26]In an analogous test we also check whether borrowers of 36-month loans in control amounts above the \$16,000 threshold were affected by the expansion. The coefficient on the interaction term is 5.9% and is not significantly different from zero (Table 2,

We repeat the exercise at the \$10,000 amount threshold using 36-month loans. 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 -3.6% and, again, not significantly different from zero (column 4). 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 Table 2 confirm our conjecture that the bulk of any selection to longer maturity loans induced by the expansion of the menu was in the eventually-selected amounts.[27]

F. Robustness

We present in Table 4 several tests that demonstrate the robustness of our results. First, column 1 of Table 4 presents a counterpart to our main result in column 1 of Table 2 but limiting the sample to loan amounts between \$6,000 and \$19,000 (a \$1,000 narrower window than our main sample, which uses loans from \$5,000 to \$20,000). The results are qualitatively similar, although the estimate is noisier and significant only at a 10% level.

As we mention 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 3 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 4 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

column 3) indicating 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.

[27] In the Internet Appendix Table 6 we conduct robustness tests that mimic the results in Table 6 where we vary the dimensions along which we collapse the loan-level data and count the number of loans. These robustness tests consistently show that selection along the margins of the interval of Treated amounts is not significantly different from zero, aside from one case in which it is significant at the 10% level.

cases, we find no significant differences in the default rate of loans issued at amounts bordering the interval of eventually-selected amounts. The results in column 2 and 3 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 self-select into long-maturity loans are unobservably more likely to default, holding the loan contract characteristics constant.

III. Interpretation: Private Information About What?

A. Time Structure of Private Information

So far, our empirical results show that borrowers who select into longer maturity loans are privately informed about their increased propensity to default on a short-term loan. We turn to understanding what is the specific private information that borrowers are selecting on. Since maturity provides insurance against future changes in the price of credit, then the private information must relate to how borrowers value this protection. It is theoretically possible that borrowers who are privately informed about their own high degree of risk aversion select into longer maturity loans (De Meza and Webb (2001)). If more risk averse individuals also default less (e.g., because they endogenously select less risky income streams), selection on risk aversion is inconsistent with the higher default rate that these borrowers exhibit.

It follows that borrowers who select long-maturity loans privately place higher value on the insurance it provides either because: 1) they have a high exposure to future shocks to their observable creditworthiness (e.g., probability of a job loss or illness), or 2) they have a higher exposure to rollover risk due to privately observed differences in the timing of their income (the cash flow timing hypothesis). These two explanations differ in the horizon after origination at which borrowers become risky. Borrowers who self-select into long maturity because they are more exposed to shocks to their observable ability to repay will tend to default more the longer the horizon after origination, as the negative shocks realize. In contrast, borrowers who self-select into long maturity because their income arrives later will tend to default less with time after origination, as their income realizes. Therefore, the two selection mechanisms can be distinguished by their predictions of the time structure of default implied by the private information.

We exploit the fact that we observe when a borrower in our sample enters default to differentiate between these two accounts. To do this, we redefine our baseline measure of default and create two variables for default at different horizons: borrowers who missed their first payment within the first 12 and 24 months of loan origination (for loans that are 120 days past due in April 2015). We label these variables $Default12m$ and $Default24m$ respectively and use them as dependent variables in regressions that are otherwise identical to the one we estimated 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. Since the hazard rate of default in our sample peaks at 13 months (Figure 2), this result is not mechanically driven by lack of statistical power due to a low frequency of default early in the life of the average loan (unconditionally, loans are as likely to default in the first 12 months after origination than later). Column 2 shows that the differential propensity to default is present at the 24 month horizon from origination.

We present in Figure 7 the coefficients from estimating our main specification using as the dependent variable an indicator for whether the first missed payment occurred before 1, 2, and so on, up to 24, months after origination.[28] The figure indicates that the cumulative default probability differential between the two groups of borrowers increases linearly with the months after origination. That is, 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.[29] This evidence indicates that the source of private information that is driving maturity selection is a borrower's exposure to shocks to their own future observable creditworthiness. Note that Figure 2 demonstrates that the hazard rate of default for 36 month loans peaks at 16 months.[30] This indicates that the bulk of default at either maturity occurs well before the 24 month horizon possible in our analysis, thereby ruling out the concern that our results are too near to origination to account for default behavior for either type of loan.

B. Private Information About Future Observable Creditworthiness

We provide additional evidence in support of our preferred interpretation. Specifically, we observe the realized observable creditworthiness measured by each borrower's credit score (FICO score) as of April 2015, roughly two years after origination. Table 5, columns 3 and 4 run our main regression model but replace the main outcome $Default_i$ with $FICO_i$, the borrower's FICO score as reported in the latest LC data pull. In column 2

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

[29]The 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).

[30]The hazard rate of default on 60-month loans issued at the same time is similarly shaped and peaks at 17 months. This indicates that repayment over the first 24 months of a loan is the crucial determinant of default at either maturity.

we include as controls all variables that are observable by investors at origination, as in column 2 of Table 3. The results imply a statistically significant increase of future FICO scores of approximately 2.7 points among selected short term borrowers relative to unselected ones. In economic terms this means that the average future FICO score of the 14.5% of borrowers who self-select into the long-maturity loans is $^{2.7}/_{14.5\%} = 18.6$ points lower than the average borrower that selects the 36-month loan.

To further demonstrate that borrowers are selecting maturity based on private information about their exposure to shocks to their future observable creditworthiness, we use the volatility of a borrower's future credit rating as a measure of the reclassification risk she is exposed to. If borrowers have private information about this reclassification risk, we expect borrowers who self-select into the 36-month loan to have less volatile future FICO scores. To test this hypothesis we present in column 1 of Table 5 the results of estimating our main specification using the within-individual standard deviation of the FICO score as the outcome variable, using FICO scores obtained from 4 different pulls of the LC loan performance data: at origination, as of August 2014, as of December 2014, and as of April 2015, which is the same outcome variable used in Table 2.[31] The cross sectional average and standard deviation of this measure for loans in our sample that were issued in the three preperiod months are 24.5 and 19.1, respectively.

The point estimate in column 5 of Table 5 is -0.57 and statistically significant at the 5% level. This implies that borrowers who select the 36-month loan have a future FICO score that is 2.3% (equal to 0.57/24.5) less volatile when the 60-month loan is available than when it is not. This pattern is strongly consistent with the insurance rationale for the screening mechanism: borrowers who select long-maturity loans are (unobservably) more exposed to reclassification risk.

Note that $FICO_i$ measures a borrower's repayment status in *all* of their debts. In particular, it considers a borrower's performance not only on the 36-month loan with LC, but on loans of different maturities as well. Thus it is unlikely that this result is driven by the incompatibility between the short-term LC loan and the time profile of borrower's future income. Instead, this shows directly that borrowers who select long-maturity loans have private information that directly relates to shocks to their observed creditworthiness and the impact that this will have on the price at which future lending will occur.

Finally, we study how maturity choice relates to a borrower's unobserved propensity to prepay her loan prior to maturity. The LC data record loans that have been fully prepaid as of April 2015, which we code in *Prepayment*, a dummy variable. If borrowers select maturity based on private information about the timing

[31] The standard deviation is calculated as $sd(FICO_i) = \sqrt{\frac{1}{4} \times \sum_{t=1}^{4} \left(FICO_{i,t} - \overline{FICO_i}\right)^2}$.

of their income, we would expect that those borrowers who select into a short-term loan would prepay at a higher rate than borrowers in an unselected group. If this were the case, the main coefficient in regression model (2) where we replace the outcome variable $Default$ with $Prepayment$ should be positive. We document the output of this regression in column 6 of Table 5. The point estimate is negative but insignificant (p-value is 0.44). Although this result is not conclusive, it does suggest that maturity choice does not seem to be driven by private information about the timing of borrowers' income shocks. It is difficult to believe that selection based on private information about the timing of income would simultaneously generate a statistically significant reduction in default but would produce a change in loan prepayment that is statistically undetectable, when the prediction about the *timing* of payment is most directly tied to the hypothesis itself.[32] On the other hand, this finding is fully consistent with the interpretation that borrowers who are privately informed of their increased exposure to shocks to their ability to repay select into long-maturity loans: positive realized shocks lead to early prepayment, while negative shocks lead to default.

C. 60-month Loan Performance

Further evidence about the underlying private information that is driving maturity choice can be provided by looking at the default rate of borrowers who took 60-month loans. If, as we hypothesize, these borrowers are more exposed to shocks to their ability to repay then, after controlling for observables, the default rate should be higher at the longer maturity loans. In contrast, if borrowers are selecting to match the privately observed horizon of their income then the default rate should be no higher.

Before presenting this evidence, an important caveat that stems from our core empirical challenge is required. Our analysis has so far focused on the propensity to default holding the terms of the contract constant, that is, focusing exclusively on a sample of 36-month loans. Thus, our analysis tells us what the default probability of borrowers who self-select into 60-month loans *would have been* had they selected a 36-month loan. We cannot empirically identify what their default probability is for a a 60-month loan. This is because the default rate of 60-month loans is also driven by selection in the *extensive* margin: there are some borrowers who would have chosen not to take a loan at all in the absence of a 60-month option, but do so when it becomes available, and we cannot independently isolate the repayment propensity of these extensive margin borrowers.

Notwithstanding this problem, we can provide suggestive evidence by comparing the average default rate

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of 36-month and 60-month loans that have the same measured expected default risk (initial risk category and 4-point FICO score bin), issued the same month, and of the same size ($1,000 amount bin). The propensity to enter default by April 2015, which holds the repayment horizon equal across the two loan contracts, is 3% higher for the 60-month than for the 36-month loans. This is commensurate with the 3.1% APR risk premium for the 60-month loan that we documented in Section D. This provides further evidence that selection is based on private information about exposure to shocks to creditworthiness. If, alternatively, borrowers were selecting maturity based on the time horizon of their income rather than their future creditworthiness, then we should not expect to see higher default or interest rates at the longer maturity loan.

A different but related question is whether increased maturity impacts a borrower's propensity to repay a loan. The answer also hinges on the average ability to repay of borrowers who select to take 60-month loans on the extensive margin, which we cannot measure in our setting. If we make the stark assumption that their ability to repay is the same as borrowers who are selected away from the 36-month loan, then our results suggest that 2 more years of maturity reduces the propensity to default by 2.5% over the horizon for which we observe these loans.[33] If borrowers who take the 60-month loan on the extensive margin have a lower (higher) ability to repay then this will under (over) state the effect. This unmeasured margin could reconcile our results with 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 or with Field, Pande, Papp, and Rigol (2013) who find that increased maturity induces entrepreneurs to undertake risky projects and leads to higher default.

D. Price Reaction to Selection

Our empirical analysis benefits from the natural experiment created by LC's decision to expand the availability of long-term loan contracts without changing any of the characteristics or terms of the short maturity contract. This implies that, within the window of the natural experiment, the default probability of 36-month loans between $10,000 and $16,000 dropped while the interest rate did not change. If LC was earning a competitive return on these loans before the menu expansion, then it must have been earning rents after the expansion. In theory, competitive pressures should eventually drive the interest rate on the short-maturity loan down to reflect the lower risk of the borrowers that self-select into short maturity.

[33] We obtain this number as 5.5%-3%=2.5%, where 5.5% is the excess default probability at the short-term loan of the 14.5% of borrowers who chose the long-term loan when it became available in the menu expansion, measured in Section II) and 3% is the average excess default rate of long-term loans over short-term loans.

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 8 which plots the average APR charged to borrowers on 36-month loans in each month controlling for loan amount and borrower characteristics.[34] Consistent with our conjecture, we see that the APR fell by roughly 0.8% for short-term loans after long-term loans were added to the menu. 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 0.8% as a result of the selection into long-maturity loans.

IV. Conclusion

We document that loan terms, in particular maturity, can 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 with higher APRs. Extrapolating from the results in our paper may help understand the broader unsecured consumer credit market that platforms like LC and Prosper operate in. Relative to their main competition, credit-card debt, these platforms offer significantly shorter-maturity loans which thereby, through the mechanism we document in this paper, allows these platforms to cream-skim low-risk borrowers from the credit-card market. This may explain how these platforms offer investors competitive returns while offering APRs to borrowers below that offered on credit-card debt. As these options grow in size, it is possible that the will eventually impact the credit-card market, which will be left with an increasingly screened pool of high-risk borrowers. It also remains an open question, both from an empirical and a theoretical perspective, whether screening on maturity is also a first order determinant of equilibrium loan prices in consumer credit markets where lenders may screen on other dimensions of the contract, such as collateral in mortgage markets. Providing concrete evidence of these broader implications is left for future work.

[34] These characteristics are FICO score bin, annual income, and address state. Note that variation in APR before November 2013 in this graph is entirely accounted for by the fact that we do not control for the borrower initial risk category, 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

A. Figures and Tables

Figure 1: 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.

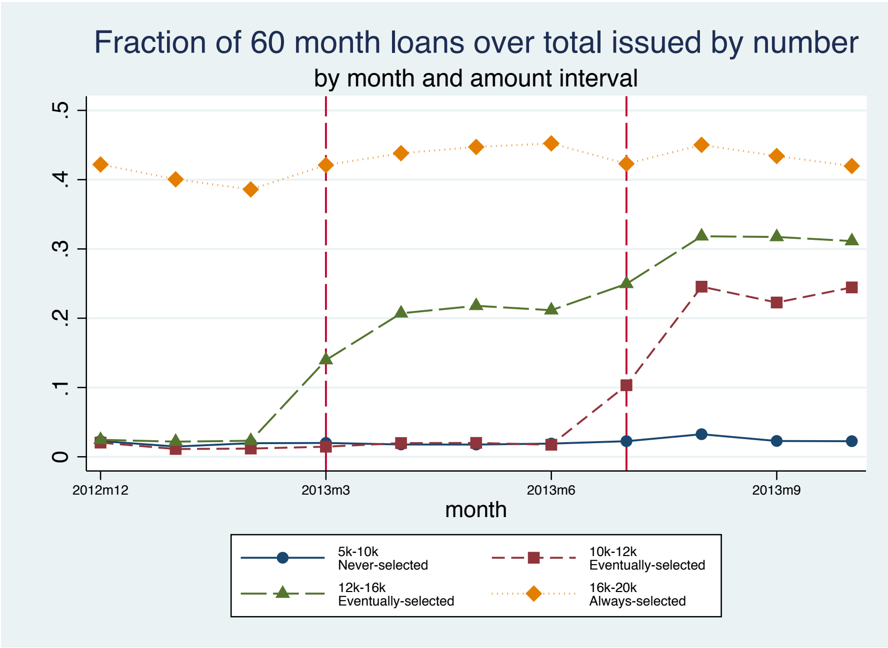


Figure 2: Hazard rate of default

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 $t-1$.

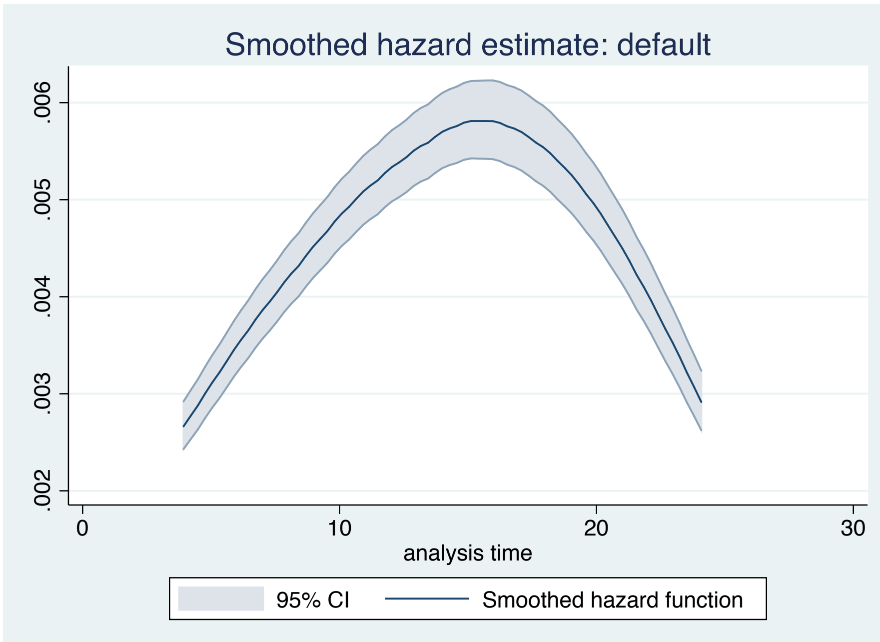


Figure 3: Stylized depiction of identification strategy

This figure shows a stylized depiction of our difference-in-differences strategy using the expansion of the menu of borrowing options.

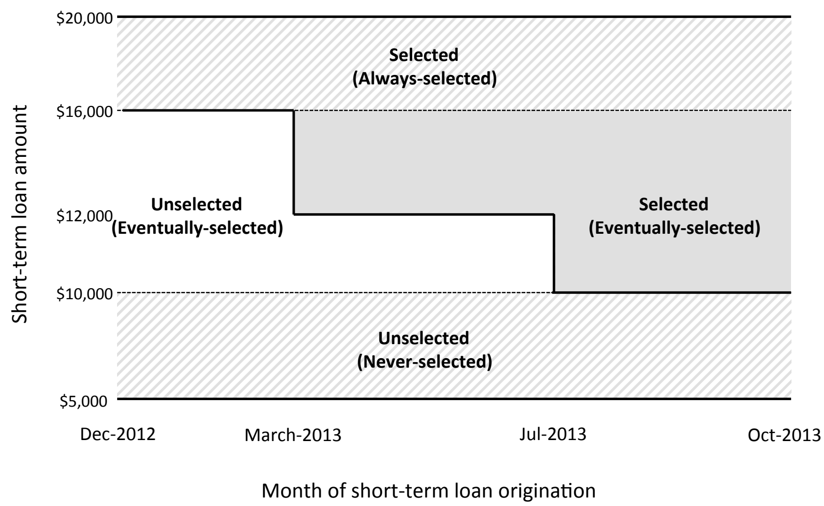


Figure 4: Total $ amount issued by LC by month of listing

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.

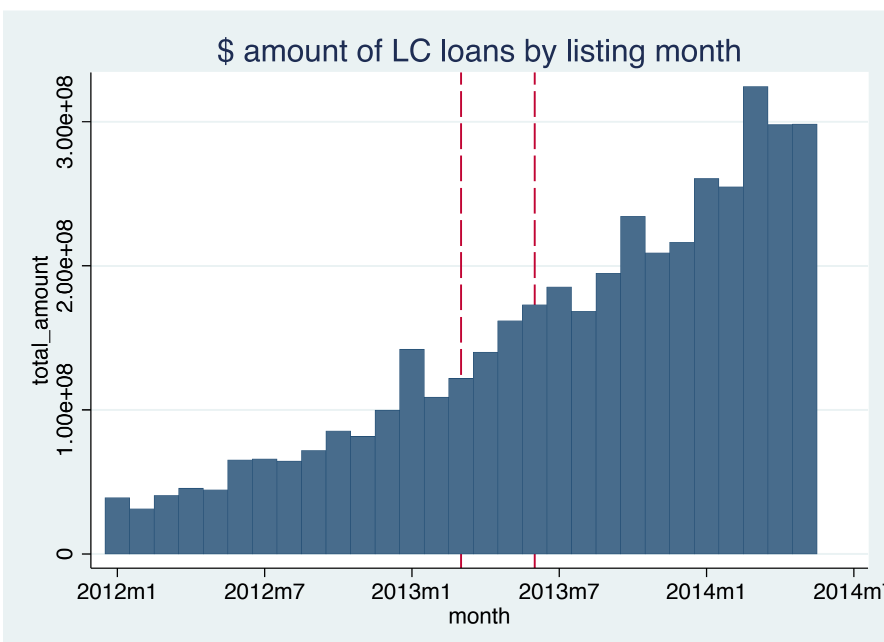


Figure 5: Pre-period loan amount histogram

This top panel shows the number of 36-month loans issued by LC by loan amount in $25 increments, between $5,000 and $25,000 between December 2012 and February 2013. The bottom panel shows the same histogram for the same period of time but for 60-month loans.

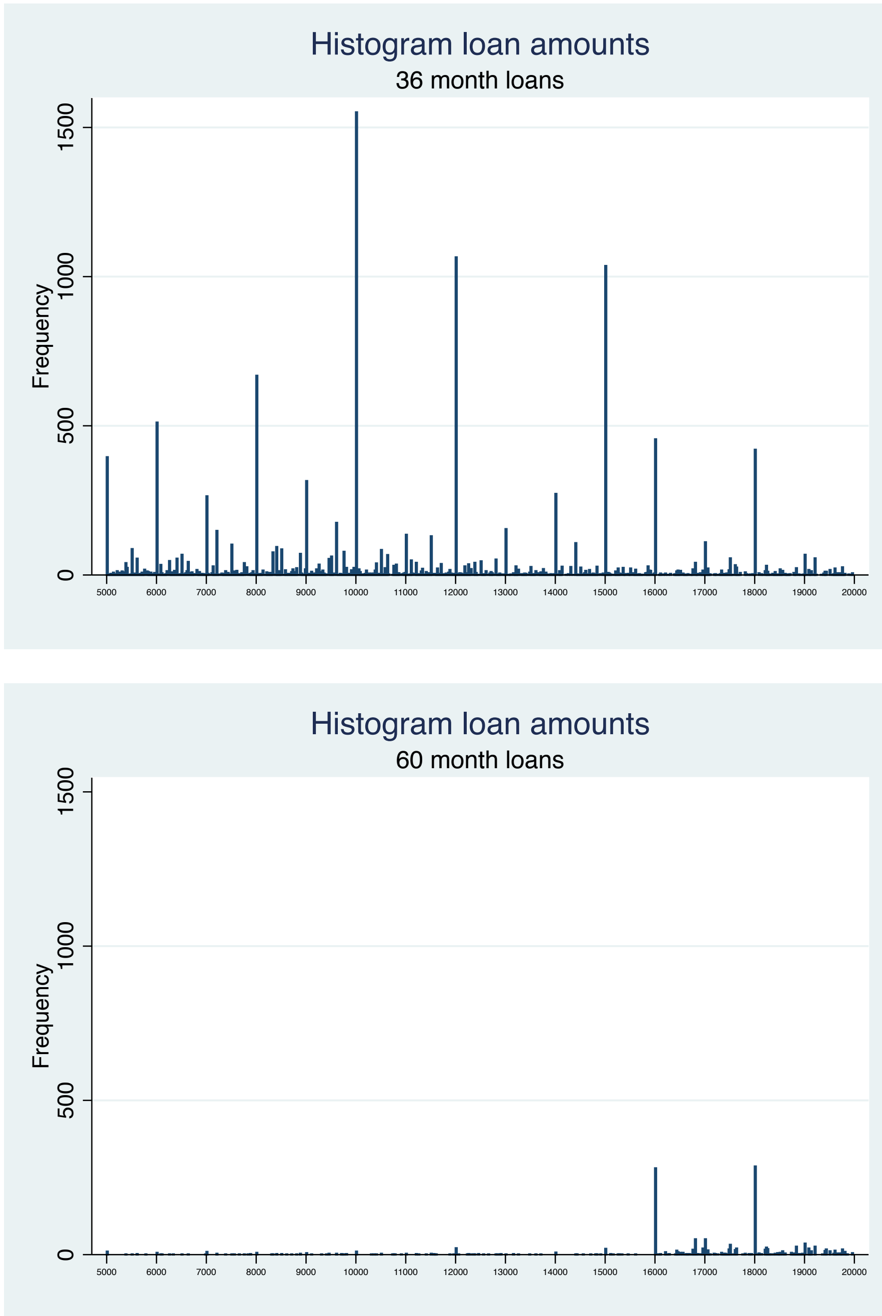


Figure 6: Pre-trends on number of loans originated

This figure shows the regression coefficients (γ_τ) and 90% 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 eventually-selected and control amounts τ months after the threshold expansion. Standard errors are robust to heteroskedasticity.

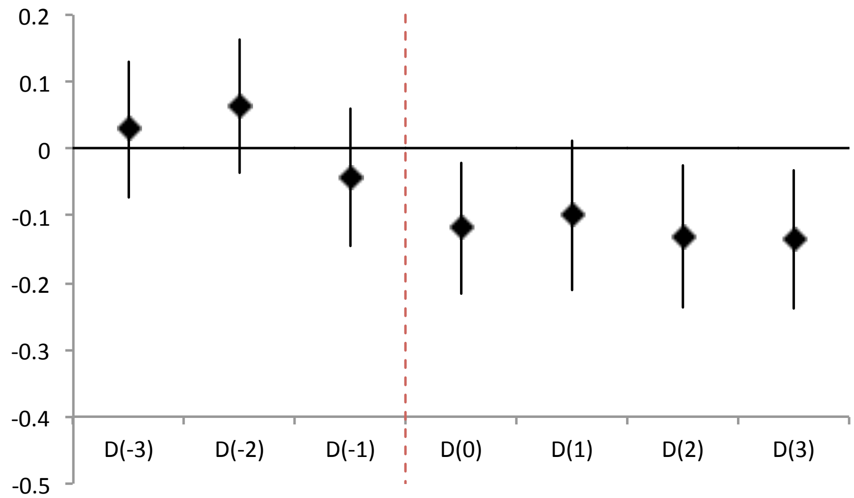


Figure 7: Default rate coefficient by number of months since origination

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 late by more than 120 days as of April 2015 and if the last payment on these loan occurred Δt months after origination, on $D_{amount1000,t}$, a dummy that captures the staggered expansion of the 60-month loans for amounts above \$12,000 and \$10,000 on March and July 2013, respectively. Standard errors are clustered at the state level. Sample includes loans issued between December 2012 and October 2013, for loan amounts between \$5,000 and \$20,000.

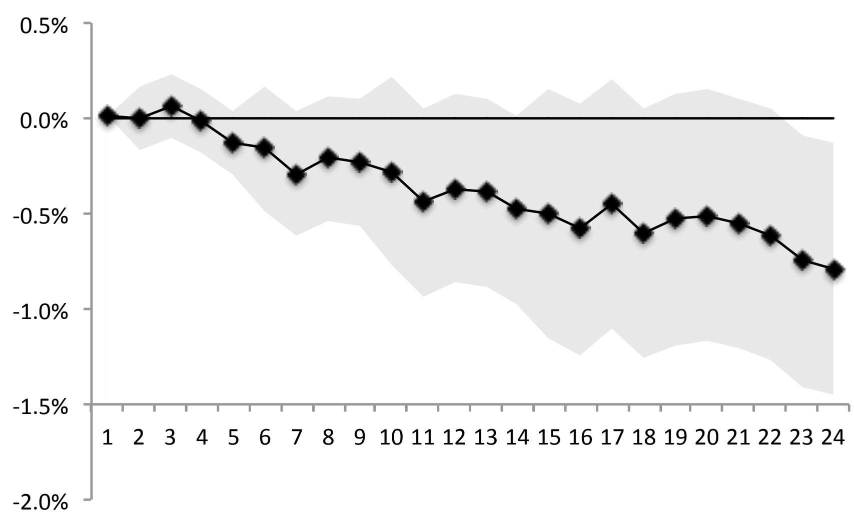


Figure 8: Reduction in APR

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.

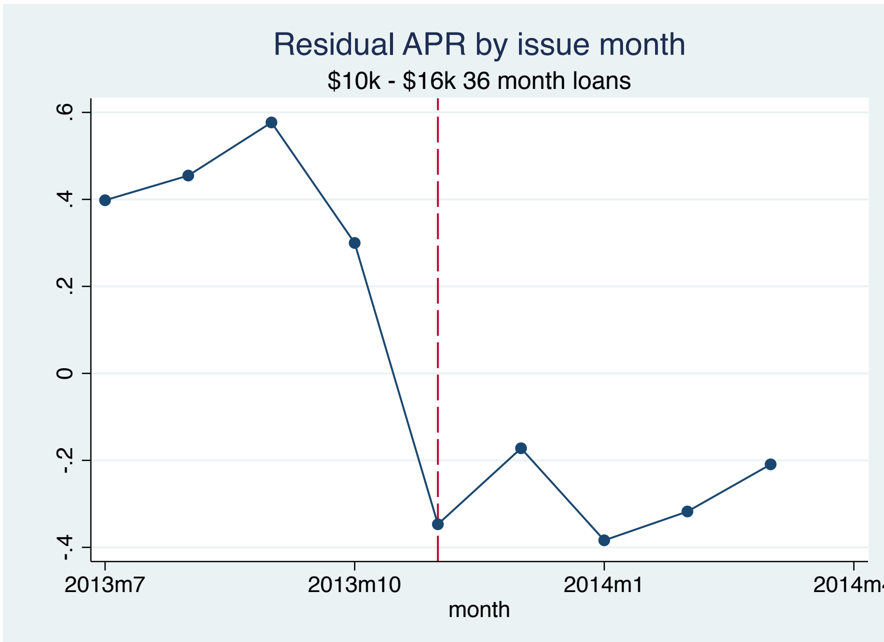


Table 1: Pre-period summary statistics

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 risk category based on LC's publicly available information.

	mean	p50	sd
Panel A: loan characteristics			
APR (%)	16.3	16.0	4.1
Installment ($)	379.9	360.9	125.1
For refinancing (%)	87.0		
Default (%)	9.2		
Fully paid (%)	37.6		
Panel B: borrower characteristics			
Annual income ($)	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		
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 *log(N)*, the logarithm of the number of loans at each month, credit risk risk category, 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. Columns 2, 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 2 restricts the sample to 60-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 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. Column 5 reports the tests of a Placebo sample, which includes loan amounts between $5,000 and $20,000 issued between July 2013 and May 2014. Standard errors are robust to heteroskedasticity. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)
	$log(N)$	$log(N)$	$log(N)$	$log(N)$	$log(N)$
$D_{amount1000,t}$	-0.1451***	-0.0825	0.0586	-0.0355	-0.0441
	(0.033)	(0.065)	(0.064)	(0.048)	(0.028)
Sample	Main	60m, 16k - 24k	36m, 16k - 24k	36m, 1k - 10k	Placebo
Observations	3,663	1,738	1,637	2,374	3,861
R^2	0.817	0.724	0.802	0.761	0.862

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). The outcome is $Default$, a dummy that equals one if a borrower is late by more than 120 days, 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 risk category $\times$month, and 4-point FICO score bin, state, and $1,000 amount-bin fixed effects. Column 2 includes all borrower level variables observed by investors at the time of origination as controls. Standard errors are clustered at the state level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)
	$Default$	$Default$
$D_{i,t}$	-0.0081**	-0.0080**
	(0.004)	(0.004)
Sample	MAIN	MAIN
Observations	60,511	57,263
R^2	0.035	0.047
# clusters	45	45

Table 4: Robustness

The table shows the output of several robustness tests. Column 1 replicates column 1 in Table 3 on a sample of loans listed between December 2012 and October 2013 and issued for amounts between $6,000 and $19,000 ($1,000 narrower interval than main sample). Columns 2 and 3 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 2 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 3 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 $10,000 listed on or after July 2013, and zero otherwise. All regressions include risk category$\times$month, and 4-point FICO score bin, state, and $1,000 amount-bin fixed effects. Standard errors are clustered at the state level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)
	$Default$	$Default$	$Default$
$D_{i,t}$	-0.0066*	0.0018	-0.0106
	(0.004)	(0.010)	(0.007)
Sample	6k - 19k	36m, 16k - 24k	36m, 1k - 10k
Observations	54,689	14,652	33,493
R^2	0.037	0.061	0.035
# clusters	45	45	46

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 $Default24m$, 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 24 months after origination, respectively*;* *FICO*, the (the high end of the 4-point bin) FICO score measured as of April 2015; *sd(FICO)* the time series standard deviation of (the high end of the 4-point bin) FICO scores within an individual, using four observations per individual: at origination, as of August 2014, as of December 2014, and as of April 2015; and $Prepayment$, a dummy that equals one if the loan is fully paid as of April 2015. In column 4 we include as controls all variables that are observable by investors at origination, as in column 2 of Table 3. 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 risk category×month, and 4-point FICO score bin, state, and \$1,000 amount-bin fixed effects. Standard errors are clustered at the state level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	$Default12m$	$Default24m$	$FICO$	$FICO$	$sd(FICO)$	$Prepayment$
$D_{i,t}$	-0.0039	-0.0082*	2.7464**	2.6705**	-0.5764**	-0.0063
	(0.003)	(0.004)	(1.032)	(0.999)	(0.266)	(0.008)
Sample	MAIN	MAIN	MAIN	MAIN	MAIN	MAIN
Observations	60,511	60,511	60,511	57,263	60,511	60,511
R^2	0.024	0.032	0.192	0.215	0.027	0.023
# clusters	45	45	45	45	45	45

B. Additional Tables

Table 6: Selection into long-maturity loans: robustness tests

This table shows a robustness test of the results shown in Table 2 that show 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. In Panel A we collapse the loan-level data into month by credit risk risk category by $1,000 amount interval level by 4-point FICO score bins (at origination). The regressions include credit risk category by month of origination by 4-point FICO score bin fixed effects. In Panel B, we collapse the loan-level date into month by credit risk risk category by $100 amount interval level. In Panel C, we collapse the loan-level data at the month of origination by 4-point FICO score at origination by 5 point debt-to-income bins. The regressions include month of origination by 4-point FICO score at origination by 5 point debt-to-income bins. Standard errors are robust to heteroskedasticity. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

Panel A: count number of loans issued each month by credit risk category by $1,000 amount and by 4-point FICO score at origination bins

	(1)	(2)	(3)	(4)	(5)
	$log(N)$	$log(N)$	$log(N)$	$log(N)$	$log(N)$
$D_{amount1000,t}$	-0.0625***	-0.0487	0.0492	0.0217	-0.0046
	(0.014)	(0.031)	(0.031)	(0.019)	(0.013)
Sample	Main	60m, 16k - 24k	36m, 16k - 24k	36m, 1k - 10k	Placebo
Observations	27,178	7,590	8,645	16,671	33,282
R^2	0.521	0.448	0.508	0.523	0.551

Panel B: count number of loans issued each month by credit risk category by $100 amount bins

	(1)	(2)	(3)	(4)	(5)
	$log(N)$	$log(N)$	$log(N)$	$log(N)$	$log(N)$
$D_{amount1000,t}$	-0.0916***	-0.0540	0.0705*	0.0090	-0.0153
	(0.018)	(0.035)	(0.036)	(0.021)	(0.017)
Sample	Main	60m, 16k - 24k	36m, 16k - 24k	36m, 1k - 10k	Placebo
Observations	15,561	4,793	4,669	11,056	18,203
R^2	0.688	0.608	0.689	0.600	0.684

Panel C: count number of loans issued each month by credit risk category by 5 point debt-to-income category.

	(1)	(2)	(3)	(4)	(5)
	$log(N)$	$log(N)$	$log(N)$	$log(N)$	$log(N)$
$D_{amount1000,t}$	-0.0974***	-0.0348	0.0421	0.0036	-0.0006
	(0.017)	(0.034)	(0.032)	(0.023)	(0.016)
Sample	Main	60m, 16k - 24k	36m, 16k - 24k	36m, 1k - 10k	Placebo
Observations	17,581	6,109	7,009	10,353	19,710
R^2	0.678	0.494	0.559	0.706	0.721

Table 7: Robustness

The table repeats the output of columns 2 and 3 in Table 4 but estimates standard errors that are robust to heterokedasticity, without clustering. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)
	Default	*Default*
$D_{i,t}$	0.0018	-0.0106
	(0.010)	(0.007)
Sample	36m, 16k - 24k	36m, 1k - 10k
Observations	14,652	33,493
R^2	0.061	0.035
# clusters	45	46

C. Yield Curve and the APR Premium

In this section we evaluate how much of the APR premium for 60-month loans over 36 month loans can be explained by the upward slope of the yield curve during the time of our sample period. To do this, we use the risk-free treasury yield curve (as recorded at https://www.treasury.gov). We use the market yields from March 1 2013 to coincide with the date of the first menu expansion at LC. The yield curve did not vary appreciably over our sample period and so the conclusion we reach is not sensitive to this choice of date. On this date the five-year yield curve was[35]:

Year (t)	1	2	3	4	5
Yield (Y_t)	0.16%	0.25%	0.35%	0.55%	0.75%
Implied Future 1-Year Annual Yield (F_t)	0.16%	0.34%	0.55%	1.15%	1.55%
Implied Future Monthly Yield (M_t)	0.013%	0.028%	0.046%	0.096%	0.129%

The Implied Future 1-Year Annual Yield is calculated as

$$F_t = \frac{(1+Y_t)^t}{(1+Y_{t-1})^{t-1}} - 1, \tag{4}$$

and measures the future risk-free one-year rate of interest that is implied by the yield curve at each horizon. The implied future monthly yield for any year is calculated as:

$$M_t = (1+F_t)^{\frac{1}{12}} - 1, \tag{5}$$

and measures the monthly risk free rate of return that will produce, with monthly compounding an effective annual yield of F_t.

To calculate the APR maturity premium implied solely by the yield curve, consider a 36-month and 60-month risk free loan for \$15,000 with equal monthly payments.[36] We look for the APR on each loan that would make them zero NPV given the yield curve. For a loan of maturity T the monthly payment P_T implied by a loan APR of R_T is

$$P_T = 15,000 \times \left[\frac{\frac{R_T}{12}}{1 - \left(1 + \frac{R_T}{12}\right)^{-T}} \right]. \tag{6}$$

The present value of any monthly payment is determined by the risk-free discount factor at each month DF_t.

[35]The 4-year yield is not recorded on www.treasury.gov. We are using a yield that is interoperated as the geometric average of the 3-year and 5-year yield.

[36]The APR wedge we find is the same for any loan amount.

For the payment in the first month, $DF_1 = (1+M_1)^{-1}$. The discount factor for each subsequent month is $DF_t = DF_{t-1} \times (1+M_t)^{-1}$. For each maturity we look for the R_T that sets

$$15,000 = \sum_{t=1}^{t=T} \frac{P_T}{DF_t}. \tag{7}$$

For a 36-month risk-free loan, this occurs at a monthly payment of \$418.37 which implies a loan APR of 0.265%. For the 60-month risk-free loan, this occurs at a monthly payment of \$252.96 which implies a loan APR of 0.465%. Thus the upward sloping yield curve at March 01 2013 can only explain 0.2 percentage points of the APR difference between the 36-month and 60-month loan.

D. Interest rate differential across short and long-term loan

Table 8: Average APR difference between short and long-term loan

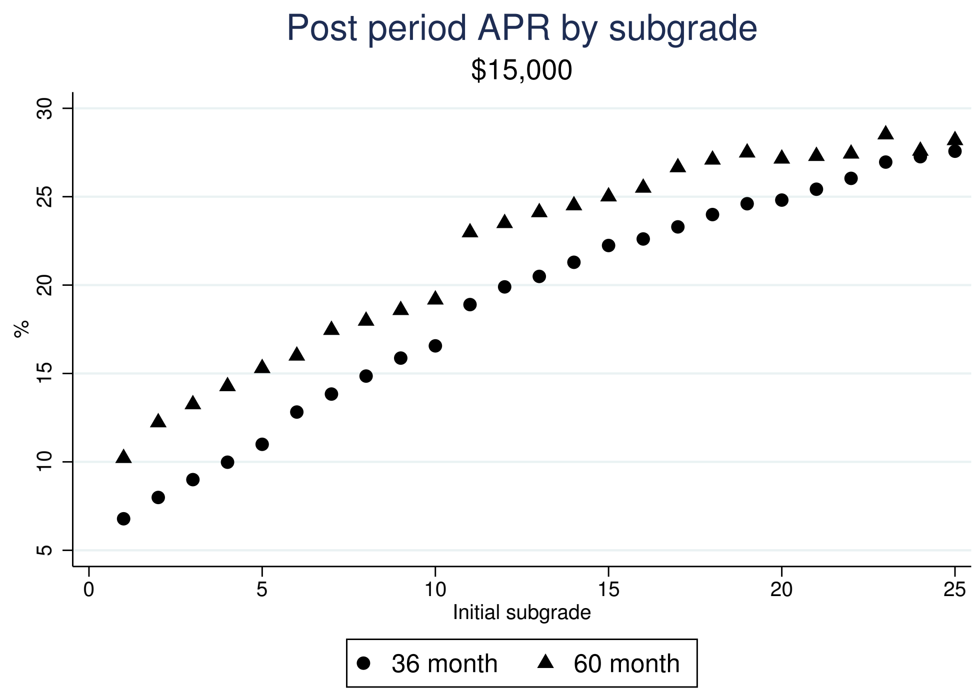
This table documents that long-term loans are 3.3% more expensive that short-term loans, controlling for observables. The table shows the output of a regression of *APR* on *Long*, a dummy for long maturity loans, controlling for credit risk category by month by $1,000 amount bin by 4-point FICO range. Sample corresponds to all loans issued by LC between December 2012 and October 2013. Standard errors are clustered at the state level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)
	APR
Long	3.3013***
	(0.008)
Observations	57,012
R^2	0.998
# clusters	46

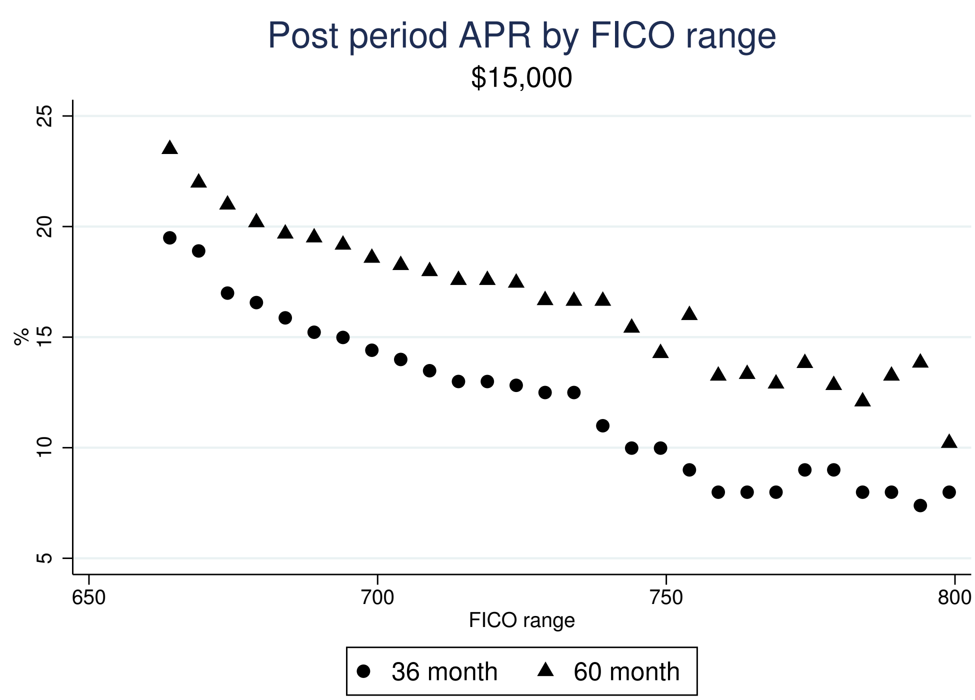
Figure 9: Long- and short-term loan APR

This figure shows the distribution of long- and short-term loan APRs issued by LC between $15,000 and $20,000 between March and October 2013 by initial credit risk (Panel A) and by initial 4-point FICO range.

Panel A: APR by credit risk grade



Panel B: APR by 4-point FICO range



E. Inferring initial credit risk category (sub-grade) from data

LC assigns each loan's interest rate depending on the credit risk sub-grade. In the data, the variable sub-grade 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 sub-grade is then assigned an interest rate. For example, as of December 2012, A1 loans had an interest rate of 6.03%, while A2 loans had a rate of 6.62%. We take a snapshot of LC's "Interest Rates and How We Set Them" page as of December 31, 2012 from the Internet Archive.[37] According to this page, the borrower's credit risk grade is calculated in the following manner. First, "*the applicant is assessed by Lending Club's proprietary scoring models which can either decline or approve the applicant.*" If an applicant is approved by the model, she receives a Model Rank (an "initial sub-grade"), 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 sub-grade is then modified depending on the requested loan amount and maturity. For example, the initial sub-grade of 36-month loans was not modified, while the initial sub-grade of 60-month loans was modified by 4 grades for A borrowers (initial sub-grades 1 to 5), 5 grades for B borrowers (initial sub-grades 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 sub-grade		
	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

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

According to this table, the initial sub-grade of a borrower who requests a loan for $10,000 is the same as her final sub-grade 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 sub-grade will have very similar risk characteristics as assessed by LC's lending model, while their interest rate will only vary according to their choice of amount and maturity. Thus, our analysis above uses the initial sub-grade before amount and maturity modifications to construct fixed effects. This variable–initial sub-grade– is not observable in the data. Instead, LC only provides the credit risk sub-grade after all modifications have been made. To re-construct a borrower's initial sub-grade, 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 sub-grade that we cannot assign an initial sub-grade 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 sub-grade					
Final sub-grade	%	Total loans	Final sub-grade	%	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 sub-grade 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 sub-grade in our regression model with the observed final sub-grade.

F. Pricing of existing loans constant during expansion - evidence

A. Evidence for all 25 risk categories

We verify that interest rates by subgrade and loan amount remain constant during our sample period. As an expample, Figure 10 shows the schedule of APRs (by loan amount) for sub-grades B1 through B5 measured before and after the menu expansion, for amounts in \$5,000 bins between \$5,000 and \$20,000, for 36 month loans. The figure shows that interest rates remain unchanged before and after the menu expansion. Formally, we regress APR on sub-grade by \$5,000 loan amount bins (rates remain fixed within \$5,000 bins) for all loans issued during our sample period,

$$APR_i = \delta_{\$5,000 amount \times subgrade} + \varepsilon_i.$$

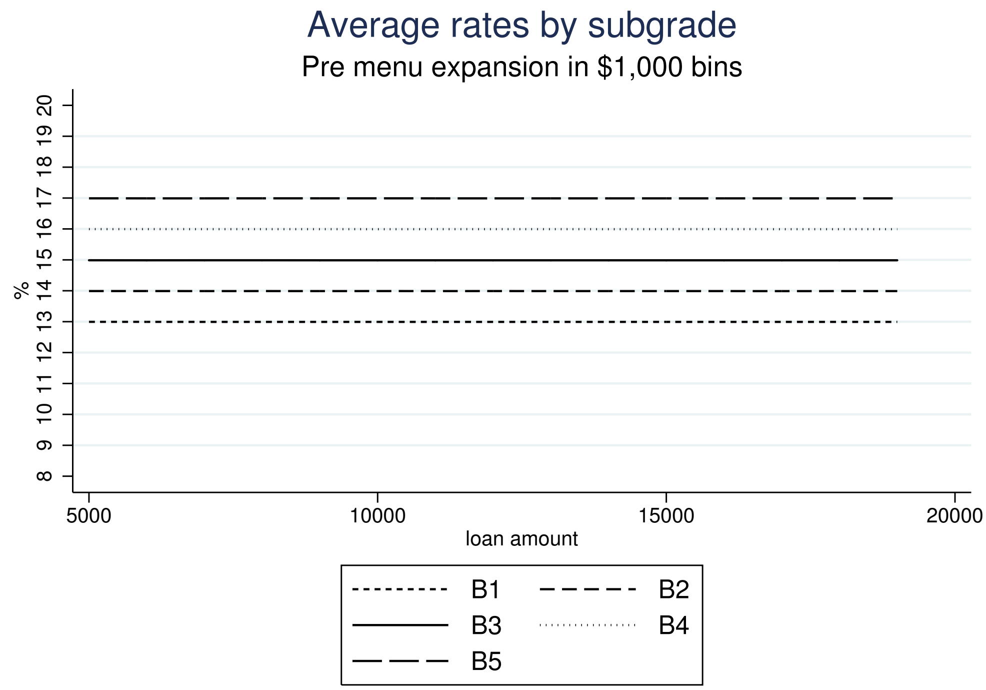
The R^2 of this regression is 99.7%, i.e., only 0.3% of the variation in APRs is explained by other variables that include month of issuance. This implies that the contract terms offered to each borrower remained constant during our sample period.

B. Example - The pricing of B grade 36-month loans

Figure 10: Interest rate schedule for B sub-grades

This figure shows that interest rates, measured as APRs, remain constant by sub-grade and loan amount among B group borrowers for 36 month loans before and after the expansion of the menu of borrowing options. The top panel shows the average APR by initial sub-grade for B grade borrowers (B1 through B5) for 36 month loans for loan amounts between $5,000 and $20,000 (labeled up to $15,000, as loans between $15,000 and $20,000 have the same terms) issued on May 2013. The bottom panel shows the same plot for loans issued on November 2013.

Panel A: Interest rate schedule before menu expansion



Panel B: Interest rate schedule after menu expansion