Financial Innovation and Borrowers: Evidence from Peer-to-Peer Lending

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ABSTRACT

The impact of technology-enabled (FinTech) lenders on bank credit is theoretically ambiguous. Banks can reduce credit if borrowing from FinTech lenders increases default risk. Alternatively, banks can provide more credit if such borrowing signals creditworthiness. I examine these possibilities using a unique setting of a large peer-to-peer lender. I find that banks increase credit for consumers who obtain peer-to-peer loans, especially consumers with inferior credit histories. Most borrowers use peer-to-peer loans to refinance expensive bank debt. Marginally funded borrowers consume these loans, but their bank credit increases nonetheless. These results are consistent with information spillovers from peer-to-peer lending to banking.

Keywords: access to credit, banking, consumer finance, FinTech, P2P lending

JEL classification: G21, G23, D14, D45, D82

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The consumer credit market is one of the largest and most important credit markets, with outstanding credit of $4 trillion in the United States (U.S.) (FED (2019)). Yet, it is characterized by several imperfections, such as high (and similar) rates on credit cards (Stango and Zinman (2009)) and frequent rejections of credit applications. Sources of imperfections include information asymmetries (Stiglitz and Weiss (1981)), high transaction costs (Brito and Hartley (1995)), and imperfect competition (Parlour and Rajan (2001)). A number of financial technology (FinTech) innovators have entered the consumer credit market, possibly because they can overcome some of these frictions and see profitable opportunities. A major consumer credit bureau, TransUnion, estimates that FinTech lending currently accounts for *one third* of the personal unsecured loan market.

FinTech lenders often position themselves as more convenient, faster, and cheaper alternatives to banks, because of the FinTech lenders' online presence, automation, and favorable regulation. These lenders also claim that their use of superior algorithms (e.g., machine learning, alternative data) offers better screening of borrowers and mitigates information asymmetries. If FinTech entrants can indeed reduce credit market distortions by relieving information frictions, consumers should benefit via expanded access to credit and lower rates. Despite its potential importance, the impact of FinTech lending on borrowers is not yet well understood.

This paper studies how obtaining a loan from a FinTech lender affects the consumer's credit provided by traditional credit intermediaries (e.g., banks) and how it affects the consumer's borrowing patterns. I focus on the most successful FinTech lending model: *Peer-to-peer* (P2P) lending. P2P lending platforms in the U.S. have originated more than $48 billion in consumer loans from 2006 to 2018, and PricewaterhouseCoopers expects P2P lending to grow to $150 billion per year by 2025. The main innovation of P2P lending is the direct matching of borrowers and lenders through two-sided platforms. Borrowers request small unsecured loans online, then multiple investors evaluate and "crowdfund" loan applications. This innovation in lending technology has implications for how borrower information is processed. Another innovative feature is the use of fully automated algorithms to price and underwrite loans in order to lower screening costs. When credit decisions are made sequentially, and when other lenders can observe loans from P2P platforms, these improvements in consumer lending may lead to information spillovers from P2P lenders to banks.

The impact of P2P lending on bank credit is theoretically ambiguous. In a world with complete markets and no frictions, financial innovation is irrelevant: Demand and supply of credit would balance in the equilibrium, and rates would accurately reflect borrower risk. Predictions change, if one allows for imperfections. Asymmetric information and adverse selection (e.g., Dell'Ariccia, Friedman, and Marquez (1999); Marquez (2002)) lead to credit market distortions, such as pooling borrowers of diverse credit quality with similar rates (Akerlof (1970); Leland and Pyle (1977)) and credit rationing when some consumers are denied credit (Stiglitz and Weiss (1981); Bester (1985); Arnold and Riley (2009)). If FinTech innovation does not change the fundamentals of the credit market, then more credit from P2P platforms should be offset by less credit from banks. This credit reduction happens because the take-up of a P2P loan imposes a negative externality on existing lenders, since default risk increases due to higher leverage (i.e., the burden of repayment) or due to actions that lower the probability of earlier loans being repaid (i.e., moral hazard). If the emerging P2P lenders do not screen borrowers carefully, this reduction in bank credit should occur even if sequential borrowing is incorporated ex ante in bank credit decisions (as in Bizer and DeMarzo (1992)). By contrast, if P2P lending contains information, banks may perceive the extension of a P2P loan as a signal of creditworthiness. In this case, banks should update their beliefs about the credit quality of P2P borrowers and change access to credit for these consumers. The tests in this paper seek to differentiate between these opposing views of P2P lending.

Theoretical predictions on how FinTech innovation affects the demand for bank credit are ambiguous, and these predictions depend on whether the innovation mitigates adverse selection (Stiglitz and Weiss (1981)) or the pooling of borrowers (Akerlof (1970); Leland and Pyle (1977)). If P2P lending relieves information frictions, some previously credit-rationed borrowers should be given access to credit, and they should borrow more. However, a reduction in pooling should lead to benefits in the pricing of debt for the highest-quality borrowers, and these borrowers should shift away from bank debt. I expect this repricing effect to be especially strong with respect to revolving accounts because these accounts have the highest interest rates.

It is empirically challenging to identify how obtaining a P2P loan affects a borrower's bank credit because unobservable borrower risk may bias the results if borrowers who obtain P2P loans systematically differ from potential borrowers who are rejected (or do not apply). I use application-level data and a unique setting on Prosper Marketplace (Prosper), one of the largest P2P lenders in the U.S., to overcome this challenge. The main sample period is 2011 to 2015. The median applicant has a strong borrower profile (i.e., a high credit score, high income, and a long credit history), but she may lack collateral and the capacity to take on more debt. A notable characteristic of P2P loan applicants is high credit card utilization. The median loan size is $12,000, with 3- or 5-year maturities. Prosper reports P2P loans to credit bureaus, and banks can observe these loans from credit reports.

Three unique features of Prosper's platform facilitate this study. First, Prosper tracks repeat borrowers (i.e., those who submit applications several times), which allows one to construct a panel of consumers and observe changes in their financials after a P2P loan application. Second, the loan amount and the interest rate are set before funding. Thus, it is funding by investors that determines whether a borrower obtains a loan, and borrowers cannot negotiate a higher rate to get approved. Third, a P2P loan can be extended only if investor commitments surpass 70% of the requested amount. This funding threshold creates a discontinuity in the probability of loan origination that facilitates the identification of the causal effects of P2P lending.

I proceed with the analysis in three steps, which I explain in detail below. First, I demonstrate that obtaining a P2P loan leads to higher access to bank credit, and this effect varies across borrowers. Second, I show that the effect of P2P lending on consumer borrowing patterns is also heterogeneous, but debt refinancing, or the resulting changes in credit scores, cannot explain the increase in bank credit after a P2P loan. Third, I show that the increased access to credit does not result in excess borrowing accompanied by higher delinquencies.

The results can be summarized as follows. First, I find that P2P borrowers not only expand their credit through P2P lending platforms, but banks subsequently increase credit supply to these customers. The key variable of interest is the quantity of revolving credit (e.g., credit cards, lines of credit) provided by banks. I show that P2P lending is associated with an increase in revolver limits of $1,020, or 2.6% relative to the mean, in OLS regressions. The size of this effect is more than half the effect of transition to home ownership on credit limits, and it is a nontrivial increase. I take a step forward in identifying the causal effect of P2P lending on bank credit by focusing on a subsample of marginally funded borrowers. The probability of obtaining a P2P loan for these borrowers "jumps" discretely by 40 percentage points (pp) at the 70% funding threshold. I exploit this fact in my regression discontinuity design (RDD). The RDD results support the hypothesis that P2P lending leads to an increase in credit limits from banks. These results are novel, and they suggest that banks take P2P lending into account when making decisions to increase access to credit. These results are also consistent with banks viewing the extension of a P2P loan as a signal of creditworthiness. I provide additional evidence to support this interpretation by exploring the effects of P2P lending on bank credit for consumers who have different initial likelihoods of being credit constrained (e.g., Jappelli (1990)). I show that the increase in bank credit is larger for P2P borrowers who have shorter credit histories and lower credit scores (5.2% increase). This result implies that P2P lenders produce valuable information about riskier borrowers, which is in line with the information story.

Second, I find that the effect of P2P loan take-up on consumer borrowing patterns is heterogeneous. Some *substitution* of bank debt for P2P debt clearly occurs for many borrowers. I show that a P2P loan take-up is associated with a 7.6% decrease in revolving balances and a 10.4% decrease in revolver utilization for the average borrower. This result is consistent with credit repricing. It suggests that most borrowers take out P2P loans to refinance expensive credit card debt, given that their overall indebtedness does not decrease. If P2P lending indeed uses marginal pricing, the extent of substitution should be related to the pre-existing costs of pooling, and borrowers with the best credit quality should benefit most from refinancing. This is precisely what I find. I document that the decrease in revolving balances is stronger for borrowers with higher credit scores. Marginally funded borrowers, however, do not lower their revolving balances after taking up a P2P loan, which suggests that they consume the loan and bank lending and P2P lending are *complements* for these borrowers. Importantly, banks are willing to provide more credit to marginally funded borrowers nonetheless, which implies that changes in borrowing patterns after taking up a P2P loan cannot explain higher access to bank credit.

Third, I provide additional evidence suggesting that borrowers whom P2P lenders approve are indeed creditworthy. It is possible that banks falsely interpret the extension of a P2P loan as a signal of creditworthiness. If screening in P2P lending is lax, loans can be provided to consumers who are less creditworthy and more prone to biases in decision-making. In this case, consumers should overborrow and default more often (e.g., Laibson (1997); Zhu, Dholakia, Chen, and Algesheimer (2012)). To examine this possibility, I track the total debt and delinquencies on all credit products, including bank debt. I find that the total debt increases by around 4.5% for the average P2P borrower, with no change for marginally funded borrowers. However, I do not find any evidence that increased access to credit leads to higher delinquencies. This result does not support overborrowing. It is consistent with P2P lenders using technology to screen borrowers well, and it is plausible that obtaining a P2P loan may be interpreted by other lenders as a positive signal. Finally, banks may respond to competition from P2P lenders rather than to information spillovers. I provide evidence that this explanation is unlikely. I also evaluate whether the focus on repeat borrowers biases the tests in favor of finding the above results. I show that the opposite is true.

The interpretation of the results that best interconnects the evidence in this paper is that P2P lending gives rise to certification as information spills over through multiple lending relationships. These results may appear surprising, since banks have been active in consumer lending for decades and thus should have better credit models than new entrants. Given the P2P lending process that I describe below, it appears that P2P lenders do not collect any new soft information that is not available to existing lenders. Rather, if they do improve information, this improvement is likely the result of better accuracy of screening due to better processing of hard data with machine learning algorithms or due to information from institutional investors on their platforms (e.g., investment firms, hedge funds). Anecdotal evidence confirms that banks may be willing to outsource screening to proprietary technology developed by non-bank lenders through bank–FinTech partnerships (e.g., Regions Bank with Avant, Beneficial State Bank with LendUp, JP Morgan with OnDeck, ING and Scotiabank with Kabbage). Community banks also invest in loans on major P2P lending platforms (e.g., LendingClub, Prosper), which suggests that these banks trust the screening algorithms of FinTech entrants.

I discuss three possible channels through which banks can find information about a P2P loan valuable for their credit decisions. First, FinTech lenders exist in a different technological space than banks, and they may use algorithmic technology (e.g., machine learning) or alternative data to screen borrowers more precisely (Berg, Burg, Gombović, and Puri (2018)). In contrast to banks, P2P platforms may also obtain feedback from the screening and funding decisions of sophisticated institutional investors in P2P loans (Vallee and Zeng (2018)). Second, banks may regard screening by P2P lenders as complementary to their own screening. Third, P2P lenders may choose to screen borrowers whom banks do not find profitable to screen, and banks may free-ride on these screening efforts.

This paper has important policy implications. The recent regulatory debate calls for stricter regulation of FinTech lenders amid concerns about lax screening and rising delinquencies in P2P lending. My results suggest that P2P lending may facilitate access to credit and generate a feedback effect on the supply of credit by banks without leading to overborrowing.

This paper contributes to the growing literature on FinTech lending (e.g., Berg et al. (2018); Butler, Cornaggia, and Gurun (2016); Fuster, Plosser, Schnabl, and Vickery (2018); Hertzberg, Liberman, and Paravisini (2018); Paravisini, Rappoport, and Ravina (2016); Philippon (2016)). The early literature focused on the determinants of funding on P2P platforms (e.g., Duarte, Siegel, and Young (2012), Ravina (2019)), while recent contributions examine how P2P lending fits into credit markets (e.g., De Roure, Pelizzon, and Thakor (2018); Tang (2018)). This paper explores the effect of P2P innovation on access to bank credit and finds that borrowing from FinTech lenders increases the credit limits provided by banks. In a contemporaneous paper, Chava, Paradkar, and Zhang (2019) use credit bureau data to also study P2P lending outcomes for borrowers. However, these two papers have a very different focus. While I focus on the ability of P2P lending to relieve information frictions and improve access to credit, the other paper's focus is on the financial discipline of P2P borrowers (or lack thereof). It is comforting that Chava et al. (2019) also find that banks increase credit for P2P borrowers, complementing my results with borrowing dynamics.[1] I believe that these two papers together provide compelling evidence that banks

[1]The data used in Chava et al. (2019), however, do not allow for the observation of borrowers who apply

increase access to credit for P2P borrowers.

This paper also contributes to the literature on financial innovation (e.g., Boot and Thakor (1997); Keys, Mukherjee, Seru, and Vig (2010)). Whereas most of the literature focuses on financial innovation in the product space, I examine the technology side of financial innovation. The existing research on technological innovation focuses on information acquisition and the competition effects of improvements in screening (e.g., Gehrig (1998); Mishkin and Strahan (1999); Hauswald and Marquez (2003); Broecker (1990)). However, this research provides little evidence on how FinTech affects borrowers in general and bank credit in particular. To the best of my knowledge, this is the first paper showing that FinTech innovation can alleviate personal financing constraints due to information spillovers.

The paper proceeds as follows. Section I describes P2P lending, Section II describes the data, and Section III presents the methodology. Section IV documents the main findings on the effect of P2P lending on credit provided by banks. Section V examines borrowing patterns and delinquencies after P2P loan origination. Section VI addresses various explanations of the results. Section VII concludes.

I. Peer-to-Peer Lending

A. *The Innovation of Peer-to-Peer Lending*

P2P lending, also referred to as "debt crowdfunding" or "marketplace lending," emerged in the United Kingdom in 2005. The first P2P lending platform in the U.S., Prosper, launched in February 2006. P2P lending has grown rapidly, especially after 2013. Currently, the largest segment of the U.S. P2P loan market is personal consumer loans, with over $48 billion in loans originated to 3.2 million borrowers in 2006 to 2018. P2P lending to consumers is dominated by two platforms, LendingClub and Prosper, with market shares of 84.9% and 15.1%, respectively.[2] The recent rapid growth of P2P lending is intriguing, because the disintermediation of lending through P2P transactions seems counterintuitive given the perception that financial intermediation itself emerged in response to credit market imperfections (Diamond (1984); Boyd and Prescott (1986)). To understand the role of FinTech in lending more fully, I describe the innovation of P2P lending and the key differences

for P2P loans but are rejected. This limitation is important, because significant borrower self-selection into P2P markets (e.g., Jagtiani and Lemieux (2017); Balyuk and Davydenko (2019)) may bias the results. By contrast, this paper compares subsequent bank credit between consumers who are successful and consumers who are unsuccessful in obtaining a P2P loan, and I employ a novel identification strategy based on the RDD methodology.

[2]These statistics are taken from company websites and Brismo Analytics: https://brismo.com/market-data.

between P2P lenders and banks in terms of the lending process.[3]

The financial press often describes P2P lending as one of the most prominent innovations in consumer finance.[4] It is noteworthy that P2P lending is not an innovation in the product space, because P2P loans represent unsecured, amortizing loan contracts that are very similar to the personal installment loans provided by banks. Rather, the innovation of P2P lending lies in the lending process. P2P loan markets are two-sided matching markets, where lenders invest directly in consumer loans and assume the default risk. Borrowers request loans in online marketplaces, and investors "crowdfund" these loans by deciding whether to invest and how much to invest. Therefore, one may think about the P2P loan market as a technology-driven public market for consumer debt similar to the corporate bond market, which did not exist before 2006. This model is unlike banks, which pool deposits from investors and then allocate these pooled funds toward loans. The other key difference is that banks perform other functions (e.g., risk sharing, liquidity transformation) that P2P lenders do not typically perform. Whereas banks assume a unique role in monitoring borrowers ex post, P2P lenders have focused on ex ante screening of borrowers (i.e., loan pricing and credit adjudication) using technology.

Another innovative feature of P2P lending is the use of fully automated algorithms throughout the lending process (i.e., application, verification, funding, and repayment). By replacing loan officers with algorithms and conducting business online without branch networks, P2P lending offers a more cost-effective alternative to bank lending. Industry estimates suggest that the operating costs of P2P platforms are around two thirds of those of banks.[5] Therefore, P2P lenders may be in a better position to screen small loans that banks may not find profitable to screen.

These innovative features of the lending process may have implications for how information is processed and for the quality of screening. Bank borrowers are screened only by the bank to which they apply, while P2P borrowers are screened both by the P2P platform and by investors. Although P2P lending started with unsophisticated investors as lenders, the market quickly attracted institutional investors. These sophisticated investors use proprietary algorithms to evaluate borrowers, and they are well positioned to have insights into credit market conditions through leveraging additional local and macroeconomic information that is not available to banks (e.g., WSJ (2016)). P2P platforms have also evolved. Currently,

[3]See Buchak, Matvos, Piskorski, and Seru (2018) for other differences between FinTech lenders and banks in mortgage lending.

[4]*The New York Times*, for example, characterizes P2P lending as a "... rare thing, scarcely seen in the financial world since the debut of the A.T.M. or microfinancing: an innovation to help regular people" (Cortese (2014)). Also, see Jeffery and Arnold (2014) and King (2018) for perspectives on the disruptive potential of P2P lending.

[5]LendingClub CEO's presentation: `http://lendingmemo.com/wp-content/uploads/2013/08/1.pdf`.

they do not serve only as transaction facilitators; they also assume an active role in screening borrowers (Balyuk and Davydenko (2019)). Therefore, improvements in loan evaluation can come from the "wisdom of the crowd," which relies on collective investor screening[6], the improved screening ability of the platform, or interaction between both (Vallee and Zeng (2018)). In fact, feedback from investor screening can produce improvements in information processing that banks cannot replicate without changing their credit intermediation model.

What kind of information could P2P lenders use in their screening process? Several papers report evidence of soft information that was previously available to P2P investors (Duarte et al. (2012); Iyer, Khwaja, Luttmer, and Shue (2016); Larrimore, Jiang, Larrimore, Markowitz, and Gorski (2011); Michels (2012); Ravina (2019)) and evidence of certification mechanisms that arise endogenously in P2P lending (Lin, Prabhala, and Viswanathan (2013); Hildebrand, Puri, and Rocholl (2017)).[7] This additional information is no longer collected in P2P lending. Investors predominantly rely on "hard" information when screening loan applications, and this information is also available to banks. Therefore, any improvements in the quality of screening should come from better processing of hard data (e.g., using machine learning). Overall, the way P2P lending platforms operate suggests that these platforms are very different from banks and specialize in borrower screening.

B. *Prosper's Lending Platform*

Prosper is the second-largest P2P lending platform in the U.S. It provides fixed-interest, fully amortizing consumer loans repaid monthly with 1-, 3-, or 5-year maturities. (The 1-year maturity option was discontinued in 2013.) The loan amounts range from \$2,000 to \$35,000. Interest rates range from 5% to 35%. Prosper also charges loan origination fees, and these fees are higher for lower-quality borrowers. I focus on Prosper's lending platform because its public data are much richer than similar data provided by LendingClub and because the platform's design possesses several institutional features that are attractive from a research standpoint. I describe these features in detail below.

The lending process on Prosper's platform starts with a loan application. Consumers who have a FICO credit score of 640 or above are eligible to apply. A prospective borrower is required to have an official income and a bank account, which means that P2P loans are not provided to the unbanked population. The borrower requests a specific loan amount and submits information about her income and employment. Prosper then checks the borrower's

[6]For example, Allen and Gale (1999) argue that public markets can be superior to financial intermediaries in providing funding, because diversity of opinion is valuable when information is inexpensive.

[7]See Bachmann, Becker, Buerckner, Hilker, Kock, Lehmann, Tiburtius, and Funk (2011) and Morse (2015) for a comprehensive review of the early literature.

credit history and requests her report from a credit bureau without affecting the credit score (i.e., a "soft" credit check). The credit report includes information on the borrower's credit scores, number of accounts, utilization ratios, and balances on revolving, installment, and mortgage accounts.[8] Prosper verifies self-reported data for the majority of borrowers.

Prosper's business model was initially an auction-type model, in which borrowers choose the maximum rate they are willing to pay and investors bid on loan applications. In December 2010, Prosper switched to a model with preset rates, in which investors post their commitments to fund the entire loan, or a fraction of the loan, at an interest rate set by the platform.[9] This model is similar to LendingClub's model. Prosper uses a proprietary algorithm to assign a risk measure to the borrower and automatically generate an application-specific rate at which the loan is provided, if it is originated. This interest rate appears on the application, and it is binding for investors who fund the application, since they cannot change this rate. From a research perspective, this preset rate environment is a useful feature of the institutional setting. This is because the equilibrium in the P2P loan market (in terms of whether a loan is provided) depends directly on funding commitments from P2P investors rather than the outcome of negotiations between borrowers and lenders.

The priced loan application is randomly allocated to one of three funding channels, one for retail investors (the Note Channel) and two for institutions. While I use data on loans in all channels in baseline regressions, I restrict my analysis to the Note Channel in the identification part of the paper. The Note Channel allows high–net worth retail investors to commit to purchasing all or part of the loan listed on the platform with a minimum investment of $25. The listing is "crowdfunded" for 14 days, or until investors commit to funding 100% of the requested amount (there is no oversubscription). Along with the change in its model, Prosper introduced a *partial funding* threshold in the Note Channel, in which a loan may now originate only if investors collectively commit to funding at least 70% of the loan amount. Therefore, the percentage of the requested loan amount funded by investors at the listing expiration date (i.e., the *percent funded*) determines whether the P2P loan can be extended. Loan applications that were subject to partial funding accounted for 93.5% of all Prosper listings in 2011 to 2015. The existence of the funding threshold in the Note Channel is a useful feature of the institutional setting of Prosper's platform, as it facilitates identification of the causal effects of P2P lending.

[8] Consumer credit is divided into two major types: *revolving* and *nonrevolving*. Revolving credit (e.g., credit cards) allows consumers to borrow up to a prearranged limit and repay the debt in one or more installments. Nonrevolving credit consists of installment credit and mortgages. Installment credit (e.g., car loans, education loans, and personal loans) is closed-end credit extended to consumers and repaid on a prearranged repayment schedule. Mortgages are a type of nonrevolving credit secured by real estate. For more details, refer to FED (2019).

[9] See Liskovich and Shaton (2017) and Wei and Lin (2016) for the effects of this model switch.

Not all loans that receive enough funding commitments from investors eventually originate. Some are canceled by Prosper as part of the platform's screening efforts, and some are withdrawn by borrowers. If the funded application passes the platform's screening filters, a P2P loan originates. When an application is only partially funded, the loan originates in the funding amount it received. During 2011 to 2015, 66.2% of applications were converted to loans, 30.0% were canceled by the platform, 1.1% expired unfunded, and 2.7% were withdrawn by the borrower. Prosper restricts repeat borrowing on its platform. Borrowers can have up to two Prosper loans outstanding at any time, and the aggregate outstanding principal must not exceed $35,000. These restrictions likely explain why borrowers who apply more than once submit a median of two applications.

How can banks observe P2P loans? Prosper reports information on its P2P loans to major credit bureaus (through a "hard" credit check). P2P loans are reflected separately in credit reports, although with a lag of 2 to 4 months. The Internet Appendix provides examples of what the reporting of P2P loans looks like and provides evidence on the reporting delay. Loan payment status is also recorded in credit files and reflected in FICO scores. This reporting makes information about a P2P loan available to banks. Thus, banks may infer changes in the credit quality of their clients from observing a P2P loan. If obtaining a P2P loan provides a valuable signal of borrower creditworthiness, banks can use this information in their decision-making. Importantly, banks do not have information on whether a P2P loan is fully or partially funded, and they cannot infer any information from how much the loan receives in funding. Likewise, banks do not observe the purpose of the loan or whether the borrower complies with the stated purpose. Thus, banks are unable to infer borrower credit quality from the borrower's commitment to a certain purpose (e.g., debt refinancing).

II. Data and Sample

A. *Sample Construction*

Prosper provides anonymized data on all loan applications on its P2P platform since inception.[10] The uniqueness of this data set lies in its scope and richness. In addition to retaining both approved and rejected applications, the data set includes key loan-level variables (e.g., loan amount, interest rate) and more than 500 borrower characteristics. These characteristics include self-reported information (e.g., income and employment) and credit bureau data that are supplied at the time of the application.

[10] I used non-authenticated queries to Prosper's public application programming interface (API) to access these data. These data are also available to investors for download at https://www.prosper.com/investor/marketplace#/download.

Prosper provides a unique identifier for each applicant, which allows me to track *repeat borrowers*, that is, consumers who apply for loans several times after loan approval or rejection. I use this structure of the data to construct a panel of observations containing the cross-section of borrowers and the time series of their financials (e.g., credit limits). Figure 1 shows the timeline of credit events and details about how the panel is constructed. The t dimension references the application number. At each application, I observe a snapshot of the borrower's bank loans (e.g., limits, balances, number of loans, delinquencies) drawn from the Experian credit bureau.[11] I also observe the application outcome (i.e., whether the loan is provided) within 14 days of application. The majority of repeat borrowers return for another loan within a year. Each observation in the panel contains borrower financials at each application and a lagged indicator for receiving a P2P loan on the previous application. I assign a lagged P2P loan indicator of zero to the first observation. Therefore, a consumer who has submitted two loan applications results in two observations in the data panel.

[Place Figure 1 about here]

I make three restrictions to the data to obtain the sample for analysis. First, I restrict the sample to the period after the change in Prosper's model in December 2010. Thus, the main sample is from January 2011 to September 2015. I exclude data from the platform's early years to ensure that differences in the platform's funding models are not driving the results and because the funding threshold that I use in my identification strategy was introduced after the change in the business model. This restriction does not significantly affect the number of observations, given the relatively low loan volume in the early years. I extend the sample back to 2006 for placebo tests. Second, since the analysis relies on observing P2P borrowers over time, most of the empirical tests focus on applications by repeat borrowers. I further analyze whether this constraint is biasing the results.[12] Third, there is significant attrition in the number of applications that each borrower submits. In order to mitigate the concern that this sample attrition may introduce selection bias into the estimation because reapplying is a borrower's choice, I restrict the analysis to the first and second applications. For this reason, I retain observations only for those borrowers who submitted their first application after 2010. I relax the restriction on the number of applications in robustness checks and in identification tests where this restriction results in very few observations.

[11] Unfortunately, the data are not detailed enough to observe each individual lender. Likewise, I cannot distinguish between bank and non-bank lenders (e.g., credit unions, credit card companies, other FinTech lenders). Since the data are anonymized, I cannot match them to any other borrower-specific data to explore the effect of P2P lending on borrowers who do not apply for a loan from Prosper.

[12] Refer to Subsection VI.B.2 for this analysis.

B. Descriptive Statistics

I present select descriptive statistics for the sample in Table I. This table summarizes loan-level and borrower-level characteristics for all borrowers (Columns (1)–(5)) and repeat borrowers (Columns (6)–(7)) from Prosper on their first application. This information gives an initial idea of how P2P loans compare to traditional consumer credit products and who borrows from P2P lending platforms.

[**Place Table I about here**]

Panel A of Table I shows that P2P loans are small loans with a median size of $12,000. The vast majority of these loans (74%) are taken to refinance existing debt, predominantly credit card debt. Therefore, one can think of P2P loans as a substitute for credit card borrowing, perhaps because these consumers cannot obtain personal loans from banks or because it is costly to take out small bank loans for refinancing. It is noteworthy that during my sample period, P2P platforms did not enforce the use of loan proceeds for their stated purpose, which makes it unclear whether borrowers indeed used P2P loans for debt repayment. The median loan maturity is three years. While credit card issuers generally charge similar rates to borrowers who have diverse credit quality, P2P lending platforms use marginal pricing, that is, the rate is determined based on the credit quality of the borrower (see the Internet Appendix). P2P loan rates inclusive of fees (APRs) range from 6.4% to 36.0%, with a median of 17.1%. These rates compare favorably to the credit card rates. A significant portion of P2P borrowers, however, pay rates well above 20%, which are presumably higher than the rates they could receive on personal bank loans. This analysis suggests that while some consumers use P2P lending because it is a less expensive alternative to bank credit, the platform also attracts borrowers who are credit constrained. The funding rate of loan applications is 96.6%, but only 64% of applicants obtain P2P loans, because the platform screens out many applicants. I classify 18.1% of borrowers as *repeat borrowers.*

Panels B and C of Table I present borrower characteristics. P2P borrowers have strong borrower profiles and are prime or near-prime borrowers (Panel B). The median binned FICO (ScoreX) score is 690 (713).[13] The FICO distribution is skewed toward riskier borrowers compared to the U.S. population (Figure 2). This suggests that the pool of P2P borrowers is dominated by consumers who have a higher likelihood to be credit rationed by banks (i.e., denied credit or given lower access to credit). The median (mean) annual income of Prosper

[13]The FICO score is the most widely used and most reliable measure of borrower credit quality, although it is a coarse measure. For this reason, I focus on FICO in this paper. FICO scores are not available in the data throughout the entire sample period. Therefore, for comparison, I also report specifications in baseline tests that include a borrower's ScoreX score, another aggregate measure of borrower creditworthiness designed for apartment renters.

borrowers is \$63,000 (\$73,889), which is more than twice the U.S. per capita annual income of \$30,176 (BLS (2014)). The median P2P borrower has been employed with her current employer for 6.5 years and has 17 years of credit history. This evidence is broadly consistent with the findings in Di Maggio and Yao (2018) and suggests that Prosper borrowers are high-income, but young, individuals. If most of their income comes from self-employment, they may lack the documentation necessary to obtain a bank loan.

[Place Figure 2 about here]

Despite high credit scores and employment, P2P borrowers seem to lack collateral and the capacity to take on more debt (Panel C of Table I). The median borrower does not own a home that she can post as collateral for a home equity line of credit frequently used for debt consolidation. The median mortgage balance among homeowners is \$18,200, much lower than the median of \$116,000 in the U.S. (FED (2013)), suggesting smaller home values for P2P borrowers. Nonetheless, P2P borrowers have a high overall debt burden. The median total debt is \$93,400, which is 1.5 times the median *household* debt in the U.S. (FED (2013)). The debt structure of P2P borrowers contains a large proportion of revolving debt (\$11,000 or 11.8% of total debt, on the median), which is usually unsecured debt with high rates, such as credit card debt. For every nine open accounts, seven are revolving accounts, and these consumers utilize their accounts aggressively. The median revolver utilization, which is the ratio of credit balances to limits, is 48%, and the median credit card utilization is 56%, much higher than the U.S. average of 30% reported in Chava et al. (2019). This evidence is in line with the conjecture that P2P borrowers may be credit rationed by banks, although it may also indicate that P2P borrowers are present-biased (also see Di Maggio and Yao (2018), Meier and Sprenger (2010)).[14]

The last two columns of Table I report statistics for the sample of repeat borrowers. While most differences between one-time borrowers and repeat borrowers are economically very small or statistically insignificant, a few dissimilarities are noteworthy. Repeat borrowers appear to be riskier from the lending platform's standpoint, as the platform assigns a higher rate to first-time applications by repeat borrowers. The platform also screens out more of these applications, so the probability of loan origination for repeat borrowers is less than half the probability for all borrowers (i.e., 35% vs. 64%, respectively). Together, this evidence suggests that riskier borrowers are more likely to return for another P2P loan. (I discuss this issue, along with a possible selection issue, in Subsection VI.B.2.)

[14] One may argue that utilization ratios far below 100% indicate that credit limits are not binding for P2P borrowers. However, it is important to keep in mind that consumers value having a liquidity buffer to insure against adverse shocks to consumption, and they can optimally underutilize their credit lines but still be financially constrained, as in precautionary saving models (e.g., see Gross and Souleles (2002) for evidence).

III. Empirical Methodology

The methodological approach in this paper utilizes the panel structure of the data, constructed from application-level observations. This section describes the design of the empirical tests and the steps I take to facilitate identification of the causal effects of obtaining a P2P loan on bank credit. I use two main types of tests: Ordinary least squares (OLS) with borrower fixed effects and the regression discontinuity design (RDD) analysis. I outline the general methodology and the additional tests I use to support the validity of the approach in this section but reserve the discussion of any additional robustness checks for the subsequent sections, in which I describe the results.

A. OLS with Fixed Effects

The first set of empirical tests makes use of within-borrower variation in credit outcomes, depending on whether borrowers receive P2P loans or are rejected. To this end, I estimate OLS regressions with fixed effects. This approach allows for measuring the effects of P2P lending in the entire sample of repeat borrowers.

I focus on revolving accounts when measuring the credit provided by banks. Revolving accounts are associated with two metrics: *revolver limits* and *revolving balances*. The revolver limit is the maximum amount that consumers can borrow on their revolving account. These limits are set by banks. Therefore, revolver limits proxy for access to bank credit (i.e., supply of bank credit, possibly conditional on requesting a limit increase). The revolver balance is the amount that consumers draw from their revolving account. It measures the actual usage of revolving credit (i.e., demand for bank credit). Other types of credit accounts, such as car loans or mortgages, do not allow one to distinguish between the credit supply and credit demand. The observed balances on these accounts are the outcomes of the partial equilibria between the supply and demand in the markets for these credit products. Another advantage of focusing on revolving accounts is that these accounts exclude P2P loans, which are treated as installment debt. I measure the *intensive* margin of the effect of obtaining a P2P loan on bank credit with the total dollar value of revolver limits for each P2P borrower. I measure the *extensive* margin with the total number of revolving accounts for each borrower.

The OLS regression with borrower fixed effects measures the marginal impact of obtaining a P2P loan on revolver limits provided by banks after each loan application. The baseline specification is

$$Y_{ist} = \beta \mathit{P2P\ loan}_{is,t-1} + \mathbf{X}_{ist}\zeta + \alpha_i + \gamma_{st} + u_{ist}, \tag{1}$$

where Y_{ist} represents revolver limits, $\mathit{P2P\ loan}_{is,t-1}$ is the lagged indicator for a P2P loan,

$\mathbf{X}_{ist}$ is a matrix of controls, α_i is borrower fixed effects, and γ_{st} is state–year fixed effects. The regressor of interest, $P2P\ loan_{is,t-1}$, is an indicator variable that takes the value of 1 if a P2P loan is originated on the preceding loan application and 0 otherwise. This variable is set to zero for the first application. Refer to Internet Appendix for definitions of variables.

I control for income, length of employment, home ownership, debt-to-income ratio, and the credit score (FICO or ScoreX) bins. I chose the former variables as controls in addition to the credit score because credit bureaus do not typically update these data as frequently as the loan data in their files, thus these controls are likely not part of the credit score. However, they can be complementary to the credit score when assessing borrower risk. Since I restrict the estimation to the first two applications, and one cannot construct lagged controls for the first application, all controls are contemporaneous. These contemporaneous controls are better than lagged controls when capturing the credit quality of borrowers at a given point in time. I use borrower fixed effects to absorb time-invariant unobservable borrower characteristics. State–year fixed effects remove the effect of local economic conditions in a given year on the results. I include states in the definition of fixed effects because some P2P borrowers move from one state to another, and there may be differences in consumer credit markets across states because of local supply and demand factors or state-level regulation.

The parameter β captures the effect of P2P lending on credit provided by banks. I assume that P2P lenders provide loans to more creditworthy borrowers and reject less creditworthy ones (additional analysis confirms that this is the case, on average). If P2P loan take-up increases the default risk of borrowers, one should expect $\beta \leq 0$, but if obtaining a P2P loan signals creditworthiness, one should expect $\beta > 0$. This effect is causal only if $P2P\ loan_{is,t-1}$ is uncorrelated with the error term (u_{ist}).

I also use OLS to test the heterogenous effects of the extension of a P2P loan on borrowers who have different likelihoods of being credit rationed by banks. The literature has shown that credit constraints can be binding for borrowers who have shorter credit histories (Jappelli (1990)) or lower credit scores (Agarwal, Chomsisengphet, and Liu (2010), Agarwal, Chomsisengphet, Mahoney, and Stroebel (2018)). I measure borrower credit history and credit scores at the time of the first loan application before funding, that is, they are measured *ex ante*. To test for the heterogeneous effects for borrowers who have different lengths of credit histories, I add to the OLS regressions an interaction term between the P2P loan indicator and the length of the borrower's credit history. The coefficient of the interaction term captures the sensitivity of the effect of P2P lending on bank credit to the length of the credit history. One should expect this coefficient to have an opposite sign from the parameter β, because a longer credit history should attenuate this effect.

I then split the sample into subsamples of borrowers, based on their ex ante FICO score, and I rerun the baseline OLS regressions on these subsamples. The regression specification is the same as in Equation (1). I prefer examining the sample splits rather than using an interaction term, as in the case with credit history, because the FICO score variable is not continuous in the data and is only provided in bins. Similar to my expectations about consumers who have shorter credit histories, I expect the results on access to bank credit to be stronger for borrowers who have lower FICO scores.

A major concern with using OLS regressions to examine the effect of P2P lending on bank credit is that the inference is likely *not* causal, because parameter β may capture a trend in a borrower's creditworthiness or because omitted unobservable variables may affect both bank credit and P2P lending. Including borrower fixed effects in panel regressions removes time-invariant unobservable differences in borrower risk, but this specification also implies that the effect is identified on the basis of borrowers who have non-constant $\textit{P2P loan}_{is,t-1}$. These are the borrowers whose application status changes from the previous application, that is, borrowers who move from being rejected to being granted a P2P loan or vice versa. This argument is pertinent to my analysis because the focus on the first two applications and the assignment of zero to $\textit{P2P loan}_{is,t-1}$ for the first observation imply that borrowers who move from $\textit{P2P loan}_{is,t-1} = 0$ to $\textit{P2P loan}_{is,t-1} = 1$ may be driving the results. In other words, the above specification may not adequately control for borrowers who do not obtain P2P loans. Also, although the zero for $\textit{P2P loan}_{is,t-1}$ in the first observation is an assigned zero, which is not equivalent to the borrower being denied a P2P loan, it may still be possible that borrowers who obtain P2P loans are in a positive trend in terms of their creditworthiness. If this trend is correlated with their future ability to receive credit from banks, the coefficient of interest β may become positively biased.

I conduct four additional tests to mitigate the above concerns. First, I use simulations to show that the assignment of the initial zero to $\textit{P2P loan}_{is,t-1}$ does not qualitatively affect the results. I randomly assign 0 and 1 to $\textit{P2P loan}_{is,t-1}$ for the first observation in a manner that keeps the assumed probability of loan origination before the first application at 35%. I then re-estimate the OLS regression. I repeat this procedure 1,000 times to obtain a distribution of the coefficients and analyze this distribution. While this procedure allows for a better way to control for rejected borrowers, it likely biases the coefficient downward. Second, I estimate a cross-sectional regression with the *change* in revolver limits as the dependent variable, with only the P2P loan indicator on the first application as the regressor of interest. All controls and fixed effects are maintained, except for borrower fixed effects. Third, I directly address the concern about the trend in creditworthiness. I identify borrowers who are likely in a positive trend as borrowers whom P2P lenders reject on their first application but approve on their second application. Likewise, I identify borrowers who are likely in a negative trend as borrowers whom P2P lenders approve on their first application but reject on their second application. I omit the borrowers who show a non-constant trend in creditworthiness from the estimation. This restriction does not change the fact that some borrowers still have a *lagged* P2P loan indicator that changes from 0 to 1, so the effect can be identified, while mitigating the concern about the trend. Fourth, I relax the restriction about measuring the outcome only after the first application by including all applications in the sample.

Another problem with the OLS methodology is the potential endogeneity bias that arises due to unobservable borrower risk. This risk may be correlated both with consumer credit variables (e.g., credit limits) and with loan origination in P2P lending markets. Banks infer the credit quality of borrowers based on "hard" and "soft" information, and they determine the level of credit supply to consumers based on borrower risk. The P2P lending platform and P2P investors also use borrowers' characteristics when assessing the credit risk of consumers and making funding decisions. The unobservable borrower risk creates a potential omitted variable bias, because fixed effects cannot capture any time-varying differences in unobservables, and borrower risk determines whether a consumer receives a P2P loan *and* access to credit from banks. This endogeneity may impair the ability of an econometrician to establish causality between P2P lending and credit provided by banks. I attempt to overcome this issue with the approach that I describe next.

B. Regression Discontinuity Design

The purpose of the second set of empirical tests is to identify the *causal* effect of obtaining a P2P loan on the credit provided by banks. I utilize the RDD approach to facilitate identification (Imbens and Lemieux (2008)).[15] To this end, I focus on the subsample of marginally funded borrowers in the Note Channel.

This idea relies on exploiting the way applications are funded in this channel: P2P loan applications are "crowdfunded" by retail investors, and the probability of loan origination rises discontinuously around the funding commitments comprising 70% of the loan amount. The RDD approach identifies on the basis of borrowers in the near vicinity of the discontinuity threshold. If the assignment of applications to just above and just below the threshold is close to random, the revolver limits of borrowers who are denied P2P loans can serve as a counterfactual for limits of successful P2P borrowers. In this case, the RDD will produce estimates of the causal effect of receiving the loan. To illustrate why this may be the case

[15]RDD design was also used to study the outcomes of a loan accept/reject decisions in Keys et al. (2010) and Berg (2018), among others. My approach is different in terms of the setting and the methodology. I apply the fussy version of the RDD to P2P lending, and I use a novel cut-off that leads to an accept/reject decision.

in P2P lending, consider a P2P investor who observes two similar loan applications with commitments just below the 70% threshold when the listing is about to expire. Let us assume that the investor has the time, or the funds, to review and invest in *only one* of these applications, contributing a small fraction of the total. Funding from this "pivotal" investor brings one application across the threshold, but not the other one. As a result, only the application from this "lucky" borrower is converted to a loan. If many investors contribute to funding loans in the Note Channel, and the randomness is satisfied, the RDD estimates should be unbiased.

The RDD specification is described by the following two-equation system:

$$\begin{aligned} \textit{P2P loan}_{is,t-1} = \phi_1 Above_{is,t-1} + \phi_2 f(D_{is,t-1}) + \phi_3 f(D_{is,t-1}) Above_{is,t-1} \\ + \phi_4 Y_{is,t-1} + \gamma_s + \delta_t + \omega_{ist} \end{aligned} \tag{2a}$$

$$\begin{aligned} Y_{ist} = \beta_1 \widehat{\textit{P2P loan}}_{is,t-1} + \beta_2 f(D_{is,t-1}) + \beta_3 f(D_{is,t-1}) Above_{is,t-1} \\ + \beta_4 Y_{is,t-1} + \gamma_s + \delta_t + \epsilon_{ist}, \end{aligned} \tag{2b}$$

where Y_{ist} represents the revolver limits, $\textit{P2P loan}_{is,t-1}$ is the lagged indicator of a P2P loan, $Above_{is,t-1}$ is an (excluded) instrument equal to 1 if $D_{is,t-1} \geq 0$ and $-h \leq D_{is,t-1} \leq h$, and equal to 0 otherwise. The variable $D_{is,t-1}$ is the distance between the forcing variable (*Percent funded*) and the discontinuity threshold (70%), and h is the bandwidth. The expression $f(.)$ is a function that is either linear or quadratic, $Y_{is,t-1}$ is the lagged revolver limit, γ_s represents state fixed effects, and δ_t represents year fixed effects. Equation (2a) is the first stage of the RDD equation system, and Equation (2b) is the second stage. The estimates from this regression system derive from the cross section of borrowers.[16]

I use the local fuzzy version of RDD (e.g., Porter (2003)) for two reasons. First, not all applications that cross the 70% funding threshold lead to loan origination. Some applications are canceled by the platform or withdrawn by borrowers. Thus, in the sample of borrowers above the threshold, some treated observations are *compliers* and some are *non-compliers*. To account for this institutional feature, I instrument P2P loan origination with an indicator variable for the loan application crossing the funding threshold ($Above_{is,t-1}$). Second, I use the local RDD approach rather than the global approach because I expect significant heterogeneity in the characteristics of borrowers along the spectrum of funding rates, given that borrower risk is correlated with funding. This heterogeneity means that borrowers at the extremes of the forcing variable are likely not comparable and the global RDD may produce biased estimates. I use 10% for the bandwidth. This choice seems to be a reasonable

[16] I do not include borrower fixed effects in the estimation, because there are very few cases in which the same borrowers repeatedly fall into the narrow bandwidth around the funding threshold.

compromise in the trade-off between the noise and the bias, since around 67.3% of treated compliers fall within the 5% bandwidth, and 98.8% fall within the 10% bandwidth.[17] I rely on Gelman and Imbens (2018) in the choice of the polynomial order. I follow Lee and Lemieux (2010) and include the lagged outcome variable (e.g., revolver limits) as a control. Such specification is warranted if the outcome variable is persistent, which seems to be the case for revolver limits. Controlling for lagged revolver limits also allows me to utilize the panel structure of the data more fully. I leave out other controls from the baseline RDD specification in order to limit the influence of the correlations between these regressors and the forcing variable on the estimation results.

The parameter β_1 captures the *causal* effect of P2P lending on credit provided by banks if the RDD is valid. The estimate of this parameter gives the local average treatment effect (LATE). The internal validity of this approach relies on satisfying the relevance condition and the exclusion restriction for the instrument. Figure 3 illustrates that the relevance condition likely holds. The graph exhibits a clear discontinuity around the funding threshold of 70%. The probability of receiving a P2P loan is zero below the threshold, rises discretely by approximately 40 pp at the threshold, and remains positive above the threshold. This evidence suggests that only borrowers whose applications cross the funding threshold can receive P2P loans. I estimate the first stage of the RDD system (Equation (2a)) to support this conclusion. I also mitigate the weak instrument concern by designing the RDD system in a way that the loan origination is just identified.

[**Place Figure 3 about here**]

The exclusion restriction is satisfied if the outcome variable (e.g., revolver limits) is uncorrelated with the instrument. This correlation, however, may be present if banks take the loan funding rate into account in their credit decisions or if borrowers are not randomly allocated to below and above the threshold. The institutional setting in P2P lending is unique, in that the former is unlikely because of the way information on P2P loans is reported to credit bureaus (see Internet Appendix). Whereas banks can observe which borrowers obtain P2P loans from credit bureau reports, banks can neither observe the funding rate of applications nor match Prosper's data to their internal documents. Thus, there is discontinuity in banks' learning about the borrower from observing a P2P loan: Banks can infer that a borrower is of a certain credit quality from the *fact* of her obtaining the loan, but they

[17] I do not use optimal bandwidth estimators because of the sample size and the shape of distribution of borrowers around the threshold. Optimal bandwidth estimators (see, e.g., Imbens and Kalyanaraman (2012)) give a very narrow bandwidth due to clustering of observations around the funding threshold. Since the resulting samples are relatively small, I lack the statistical power to obtain meaningful estimates. In robustness tests, I check for the sensitivity of these results to the selection of bandwidth.

cannot infer borrower credit quality from the funding rate of her application. Although the exclusion restriction is untestable, I conduct additional analyses of the distribution of borrower characteristics around the threshold to determine whether it is reasonable to assume that this restriction is satisfied.

I examine whether borrowers on opposite sides of the discontinuity threshold are different. These marginally funded borrowers do not appear substantially different, on average, in terms of their FICO scores or borrower profiles (see Internet Appendix). What matters, however, is the "jump" in characteristics at the threshold rather than average levels. I observe that the density of the forcing variable is clustered just above the funding threshold (see Internet Appendix). A closer look at this clustering reveals a heaping of observations on a very narrow section of the distribution between 70% and 70.1% funding. This heaping may be due to rounding by the platform or by the marginal investor who may be willing to fund just enough to bring the loan application across the threshold. Alternatively, it may suggest non-randomness due to manipulation by borrowers, increased due diligence by investors, or the strategic choice of the funding threshold by the platform.

Manipulation of the funding threshold can occur if borrowers take strategic actions to increase the likelihood of obtaining the loan after they apply. To examine the possibility that borrowers manipulate funding by asking relatives or friends to invest in their loan when it is close to the threshold, I manually collect data on investments in P2P loans from family and friends.[18] The data show that a mere 0.68% of P2P loans have investments from friends and family; and, if they do have them, adjusting for these investments keeps most of these loans above the bandwidth chosen for the RDD analysis (see Internet Appendix). This test allows me to comfortably argue that manipulation through friends and family, if it occurs at all, cannot explain the heaping.

Non-randomness may also occur if *investors* are strategic. It may be the case that the number of investors per loan is small and each investor has substantial power over the outcome of the application. I hand-collect data on the number of investors per loan to verify my assumption about loan "crowdfunding" by many investors.[19] I find that the mean number of investors per $12,000 lent (equal to the median loan size) is 138 and the median is 150, and each investor does usually contribute a small fraction of the total. Alternatively, investors may be exerting extra diligence in assessing the risk of these marginal borrowers and only funding the most creditworthy ones. To check whether key borrower characteristics "jump"

[18]These data are available from Prosper's sales reports for the period starting from December 2010, when Prosper changed to preset interest rates, to April 2013, when the whole loan program was introduced and Prosper stopped disclosing this information.

[19]As in the case with data on investments by family and friends, these data are available from Prosper's sales reports for the period between late December 2010 and April 2013.

at the 70% funding threshold, I estimate separate RDD equations with these characteristics as dependent variables. Finally, I check whether this specific funding threshold was set by the platform because of some prior belief about the funding probability. I do not find such evidence. The choice of the threshold seems unrelated to any pre-existing discontinuity in borrower access to credit or lack of comparability above and below the threshold.

I conduct three additional tests to mitigate any remaining concerns about the validity of the RDD analysis. First, to ensure that the heaping does not drive the RDD results, I estimate a "donut" RDD regression, in which I omit observations at the data heap (as in Almond and Doyle (2011); Barreca, Guldi, Lindo, and Waddell (2011). This approach ensures that estimates of the treatment effect are unbiased (Barreca, Lindo, and Waddell (2016)). Second, I add all controls that I use in OLS regressions to the RDD system to account for the key borrower characteristics. Adding these controls should also increase the precision of the estimates by absorbing residual variation. Third, I conduct a placebo RDD analysis by examining the outcomes for loan applications in the Note Channel that are 60% to 80% funded but are not subject to the 70% funding threshold (i.e., they are converted to P2P loans only if funded in full). This sample consists mainly of applications submitted before December 2010 (92.7%), although some of these applications are by borrowers who do not qualify for, or do not choose, the partial funding option. To match the local fuzzy RDD specification in the main tests, I set placebo loan origination to zero if the application is canceled or withdrawn. The placebo analysis allows me to ascertain that the RDD results indeed capture the effect of receiving a P2P loan rather than capturing some spurious correlation between the forcing variable and credit limits. If the RDD is valid, there is no reason to expect any effect of crossing the placebo funding threshold. The placebo analysis also provides an indirect test for whether this specific funding threshold was set by Prosper because of pre-existing discontinuity in borrower characteristics around the 70% funding rate.

IV. Empirical Results: Access to Bank Credit

A. OLS with Fixed Effects

The first set of results establishes the relationship between the extension of a P2P loan and subsequent access to bank credit. I first present the estimates from OLS regressions in Table II. I use the total dollar value of revolver limits (i.e., the intensive margin) as the dependent variable in Columns (1)–(4) and the number of revolving accounts (i.e., the extensive margin) in Columns (5)–(8).

[Place Table II about here]

I find that P2P lending is associated with an increase in revolver limits for borrowers who receive P2P loans compared to those who do not. The coefficient of the P2P loan indicator is positive and significant in all specifications. The point estimate of the limit increase is between $780 and $1,020, or 2.0% to 2.6% relative to the mean initial revolver limit (i.e., the revolver limit on the first application). The effect is meaningful: It is half the effect of the transition to home ownership on credit limits. When I restrict the analysis to borrowers who return for another loan within a year, the coefficient almost doubles, suggesting an increase in revolver limits of $1,650, or 4.1% relative to the mean initial limit. This increase is intuitive, as the inclusion of borrowers who return to obtain another P2P loan after a long time likely introduces noise to the data. Borrower credit quality and credit choices change significantly in the long run, so any effects on credit limits persisting beyond the first year after receiving a P2P loan would be difficult to explain by P2P lending itself. I further examine how the number of revolving accounts changes after the extension of a P2P loan. I find that the number of accounts also increases for consumers who obtain P2P loans (Columns (5)–(8) of Table II), although this increase is smaller than the increase in revolver limits, namely, a 1.3% to 1.5% increase relative to the mean initial number of accounts. Thus, it appears that a large part of the limit increase comes from existing lenders (i.e., the intensive margin).

These initial results imply that P2P borrowers have more revolving credit available from banks in the form of either higher limits on existing accounts or additional credit offers after P2P loan take-up. Although the OLS estimates are not causal, these findings are consistent with banks improving access to credit for clients who borrow from P2P platforms, because they regard the loan as a signal of creditworthiness. The lower magnitude of the effect on the number of accounts may appear surprising, since one would expect that an additional informative signal should play a more important role for transactional loans rather than for relationship loans. Recall, however, that this analysis focuses on revolving accounts, such as credit cards, which can be thought of as recurring lending, whereas borrowers must apply for new accounts. P2P borrowers in my sample already have many revolving accounts, so they are less likely to apply for one more account. For this reason, revolver limits may also be a cleaner measure of credit supply by banks than the number of accounts.[20]

The effect of obtaining a P2P loan on access to bank credit is heterogeneous. Table III shows that the effect is sensitive to the length of the borrower's credit history. Consistent with the hypothesis that P2P lending should lead to more credit expansion for borrowers who are more likely to be credit constrained, borrowers who have *shorter* credit histories enjoy

[20]Borrowers can also ask banks for a limit increase. However, regardless of the reason for a review of credit limits, banks adjust these limits only if their beliefs about borrower creditworthiness have changed.

a *larger* increase in revolver limits and in the number of revolving accounts. Importantly, the coefficient of the P2P loan dummy remains positive and significant. The point estimates suggest that obtaining a P2P loan is associated with a $3,370 increase in revolver limits for borrowers who have one year of credit history, but only a $900 increase in revolver limits for borrowers with 20 years of credit history.

[Place Table III about here]

I report the results of OLS regressions of revolver limits and the number of revolving accounts on obtaining a P2P loan for each FICO bin in Figure 4, for ease of exposition. The gray bars on the graph represent the economic magnitudes of the P2P loan coefficients, and the data labels show the coefficients and their statistical significance. I focus on the specification with FICO score and state–year fixed effects, which I consider the most stringent specification because the FICO score is the most widely recognized measure of creditworthiness, and state–year fixed effects remove the impact of any time-varying local economic conditions. (The results, however, are not sensitive to this specification.) Consistently, I find that the increase in revolver limits and the number of revolving accounts comes from borrowers who have lower FICO scores.

The coefficients suggest that revolver limits for borrowers with FICO scores below 700 are 2.6% to 5.2% higher after a P2P loan than their respective mean levels at the first application, when controlling for unsuccessful borrowers. It is noteworthy that the average revolver limits for each FICO bin appear to increase monotonically from the mean limit of $21,700 for the lowest FICO bin to $64,000 for the highest one, whereas the number of accounts does not exhibit this pattern (see Internet Appendix). Thus, the total revolver limits are positively correlated with borrower creditworthiness, and they may be a better measure of the degree of a consumer's financial constraints than the number of accounts.[21] Overall, these cross-sectional results suggest that P2P lending elevates credit constraints for consumers who are more likely to be credit rationed by banks. These findings are also more consistent with the improved accuracy of processing hard information rather than with lower screening costs, which should not have differential effects on access to bank credit for borrowers.

[Place Figure 4 about here]

I conduct several robustness checks of the OLS results (see Internet Appendix). First, in order to assess whether the assignment of the initial lagged P2P loan indicator of zero is driv-

[21] This result suggests that the definition of credit rationing that is appropriate in the context of revolving accounts should be similar to the one in Kirschenmann (2016), where rationing is defined as providing loans with a lower amount than requested.

ing the results, I compute 1,000 estimates of the P2P loan coefficient from simulated data, where I randomly assign an initial lagged P2P loan indicator of 1 to a subset of observations (see Section III). The distribution of obtained estimates shows that even the 5th percentile of this distribution is positive, which suggests that the positive coefficient of the P2P loan is not a result of the way I construct the panel. Second, to strengthen the credibility of the OLS estimates, I explore coefficients from cross-sectional regressions without borrower fixed effects, where I contrast the change in revolver limits on the second application between borrowers who are successful and borrowers who are unsuccessful in obtaining P2P loans on their first application. The coefficient of the P2P loan remains positive and statistically significant. Third, I exclude from the estimation borrowers who are in the positive or negative trend in terms of creditworthiness. The results are comparable both qualitatively and quantitatively to the results obtained in the main specification. Finally, I extend the sample beyond the first two applications and re-estimate the OLS regressions using two alternative definitions of the regressor of interest. In addition to the indicator of P2P loan origination on the previous application, I use the number of prior Prosper loans, which takes the initial value of 0 and increases by 1 after each P2P loan is obtained, as an alternative. The results remain qualitatively similar.

B. Regression Discontinuity Design

The next set of tests focuses on the RDD analysis based on a subsample of marginally funded borrowers. To set the stage for the RDD, I plot the conditional expectation of revolver limits against the *lagged* percentage of the P2P loan amount funded by investors in the vicinity of the 70% funding threshold. Figure 5 shows a clear discontinuity in subsequent access to bank credit around the 70% threshold. This discontinuity is in line with banks increasing revolver limits for marginally funded borrowers who obtain P2P loans. I note that the confidence intervals overlap in the graph that plots the number of revolving accounts. This suggests that the increase in the number of accounts, if any, should be less pronounced. Two other observations should be made about these graphs. First, all borrowers below the threshold do not receive P2P loans, while some, but not all, borrowers above the threshold do receive loans. Thus, the graphical analysis likely underestimates the effect of P2P lending on borrowers who do receive P2P loans. Second, these graphs use raw revolver limits and do not condition on any other variables, such as state and year fixed effects or lagged limits.

[Place Figure 5 about here]

Further, I test for a jump in ex ante borrower characteristics around the funding threshold by applying the RDD analysis to borrower credit scores, income, employment, home ownership, and the debt-to-income ratio. Importantly, I find no evidence of a "jump" in ex ante aggregate measures of borrower creditworthiness, such as FICO and ScoreX scores, around the threshold (see Table IV). The results also indicate that employment, home ownership, and debt-to-income ratio do not exhibit a discontinuity at the threshold. While there is some evidence of a discontinuous increase in income around the threshold, the respective coefficient is significant only at the 10% level. I conduct multiple robustness checks, which I describe below, to mitigate any concerns about non-randomness.

[Place Table IV about here]

The first stage of the local fuzzy RDD analysis reported in Table V supports the relevance condition: The instrument is strong and relevant for P2P loan origination. The indicator for crossing the funding threshold positively and significantly affects the probability of loan origination. When a borrower crosses the 70% funding rate, the probability of receiving a P2P loan rises discretely by 39%. Standard errors in the first-stage estimates are small, the F-statistic is above 10, and the Cragg–Donald Wald F statistic of 32.4 to 56.7 across the specifications is above the critical value of 16.38, so the weak instrument hypothesis can be rejected (see Staiger and Stock (1997); Stock, Wright, and Yogo (2002)).

[Place Table V about here]

The results from the second stage of the RDD analysis corroborate the findings from the OLS tests (Table V). I find that banks respond to P2P lending by increasing revolver limits to marginally funded borrowers who receive P2P loans. As before, the results on the number of accounts are smaller in magnitude but insignificant, implying that this increase likely comes from existing lenders. These results are not driven by the functional form of RDD regressions (i.e., the quadratic function or splines), and they are robust to variation in the bandwidth around the discontinuity threshold.[22] These findings suggest that banks extract information from P2P lending and incorporate this information into their credit decisions. It is possible that the commitment to borrow from P2P lending platforms that offer fixed-term debt rather than borrow on credit cards results in this positive updating and increased access to credit. It is also possible that banks perceive the screening algorithms of P2P lenders as complementary to their own screening, which results in a positive adjustment of beliefs about borrower quality. I discuss this latter possibility in greater detail in Section VI.

[22]The estimates become more significant with wider bandwidths, but the economic magnitude changes somewhat with the bandwidth change (see Internet Appendix). This evidence is consistent with estimates based on smaller samples having lower precision, but biases being larger when I include observations farther from the threshold.

The RDD results can be interpreted as the *causal* evidence that obtaining a P2P loan leads to increased access to credit from banks only if P2P borrowers are quasi-randomly assigned to below and above the discontinuity threshold. Recall, however, that the density of borrowers around the funding threshold exhibits a clustering of observations at the threshold. Therefore, I take additional steps to ensure that the possible non-random assignment of borrowers to below and above the threshold does not significantly bias the results. I perform three tests that support the validity of the causal inference. First, I check whether the RDD results are robust to using the "donut" RDD strategy. Table VI shows that the results remain qualitatively unchanged. Second, I check whether the RDD results hold when I include key borrower characteristics as controls into the RDD analysis. The results are robust (see Internet Appendix). Controlling solely for income also does not change the results. Third, I conduct the placebo RDD analysis, and I see no effect of crossing the 70% funding rate on credit provided by banks, as expected (see Internet Appendix).

[Place Table VI about here]

The RDD results are economically stronger than the results of the OLS regressions. The RDD coefficients suggest that obtaining a P2P loan leads to an increase in revolver limits of at least $14,100, which is a 32.4% increase relative to the mean initial limit, for marginally funded borrowers. A much larger increase in this category of borrowers can be explained by the local average treatment effect (LATE), since I identify this effect based on a small sample of borrowers who are likely not representative of all P2P borrowers. When the treatment is heterogeneous, which is likely the case in my analysis, differences in magnitudes may result from compositional differences between the samples.

A closer examination of the characteristics of marginally funded borrowers suggests that these borrowers are indeed different from the entire sample in several ways. Marginally funded borrowers have somewhat higher FICO scores and higher income, but they are more likely to be self-employed and have shorter employment histories. These borrowers have higher debt across all credit categories (i.e., revolving, installment, and mortgage debt). One striking difference is that marginally funded borrowers are significantly less likely to take out a P2P loan for debt consolidation compared to one-time borrowers (58% vs. 74%); they request many more P2P loans for consumption (e.g., home improvement, large purchases) or for small business purposes. Importantly, credit limits represent the maximum allowed bank credit on which the bank and the borrower agree. Therefore, revolver limits may increase more for borrowers who ask for a limit increase. It may be the case that borrowers who use P2P loans for consumption or for small business purposes are more likely to ask for such an increase. While the results from the RDD analysis are cleaner and provide for identification, the magnitude of the coefficients should be interpreted with caution (see Jiang (2017)). Nonetheless, the fact that I find the same sign and significance of the coefficient as in the OLS regressions can be interpreted as the effect of obtaining a P2P loan on revolver limits moving in the same direction. Thus, the results of the RDD analysis allow me to assign a causal interpretation to my findings.

Overall, my findings so far suggest that P2P lending may give rise to certification when information spills over through multiple lending relationships, consistent with the literature on signaling when screening is costly and imprecise (e.g., Welch (1992); Banerjee (1992); Bikhchandani, Hirshleifer, and Welch (1992)). These results are in line with banks rationing credit to some consumers because of information frictions (Stiglitz and Weiss (1981); Berger and Udell (1992)), but then updating their beliefs about the borrowers' credit quality after observing P2P loans and adjusting credit upward for P2P borrowers. Indeed, banks periodically review client profiles and unilaterally offer limit increases to borrowers whom banks perceive to be more creditworthy than before. This positive effect of obtaining a P2P loan on bank credit is also reminiscent of the certification effect of credit ratings (Sufi (2009)) and trade credit (Biais and Gollier (1997)).

The increase in bank credit is difficult to reconcile with theoretical models that show that sequential or instantaneous lending leads to higher default risk, which implies an opposite effect on the bank supply of credit (e.g., Bizer and DeMarzo (1992); Laibson (1997)). These results are also in contrast to empirical evidence in Liberman, Paravisini, and Pathania (2018) and Degryse, Ioannidou, and von Schedvin (2016), who find that loan take-up has a negative effect on bank credit. Liberman et al. (2018), however, examine the consequences of high-cost loans for creditworthy borrowers, while I study the effect of loans provided by P2P lending platforms, which offer *low-cost* loans to consumers. These two different types of credit may have very different implications for subsequent access to bank credit. Degryse et al. (2016) test for the effect of breaking an exclusive lending relationship with banks by borrowing from other lenders. The borrowers in my sample have many different accounts, presumably with many lenders, so the effects I document are not directly comparable to Degryse et al. (2016). My results suggest that P2P loans may be different from loans from other banks or high-cost sources of credit, in that P2P lending relieves, rather than exacerbates, financing frictions for borrowers.

But, do banks indeed learn from the fact that consumers obtain, and are repaying, P2P loans? It may be the case that banks increase access to credit for consumers who receive P2P loans because these consumers become lower risks after repaying their credit card debt or improving their credit scores. I turn to this question next.

V. Empirical Results: Borrowing Patterns

A. Demand for Bank Credit and Creditworthiness

The second set of results focuses on the demand for bank credit and consumers' creditworthiness after P2P loan take-up. In addition to documenting how P2P lending changes the borrowing patterns of consumers, I use the findings in this section to rule out several alternative explanations of the above results on access to bank credit. It is possible that banks increase credit for P2P borrowers because these consumers improve their creditworthiness by shifting away from revolving debt. If this is the case, the above results should capture the effect of debt refinancing and should not differ from the effect of using bank loans to refinance revolving debt. It is also possible that borrowers do not become lower risks, but banks mistake debt refinancing for debt repayment because of a lag in reporting P2P loans to credit bureaus. Last, it is possible that banks excessively rely on FICO scores, which may or may not correctly incorporate changes in borrower creditworthiness after a P2P loan.

Table VII reports the estimates from the OLS regressions for credit balances on revolving accounts, revolver utilization ratios, and credit scores. Recall that P2P borrowers are, on average, prime creditworthy borrowers. Therefore, one can expect that these borrowers refinance their other debt with P2P loans, thus decreasing the demand for bank debt and lowering credit utilization ratios. In contrast, marginally funded borrowers are more likely to request a P2P loan for consumption or for small business purposes. Thus, one can expect that these borrowers should increase their borrowing (from P2P lenders, banks, or both). The findings are consistent with these predictions.

[Place Table VII about here]

I find that P2P loan take-up is associated with the average P2P borrower having lower revolving balances and lower utilization of bank credit. Revolving balances decrease by at least 7.6% for borrowers who receive P2P loans compared to borrowers whose applications are rejected.[23] Revolver utilization decreases by at least 10.4%. Although not reported, the effect on credit card utilization is comparable in magnitude. This evidence suggests a *substitution* of P2P lending for bank lending: The average P2P borrower uses the proceeds from P2P loans to consolidate her high-interest credit card debt and to increase her short-term liquidity. This repayment behavior may improve her creditworthiness and trigger an increase in access to credit from banks. These findings also indicate that banks and P2P lenders may be servicing the same borrower clientele, in line with the substitution found in

[23] As with revolver limits, I measure the decrease relative to the sample mean on the first application.

Tang (2018) and De Roure et al. (2018).[24]

I do not find the same pattern for marginally funded borrowers in the RDD analysis. Table VIII shows that these borrowers do not decrease their revolving balances after a P2P loan. The coefficient of the P2P loan is positive and insignificant in the most conservative specification that I report, it is always non-negative, and it is often significant in other specifications as well (e.g., when I do not include lagged outcome variables as controls).[25] The result that revolver utilization does not decrease despite higher revolver limits for these borrowers also suggests that they likely borrow more through revolving debt. Thus, P2P lending *complements* bank lending for marginally funded borrowers. These marginal borrowers do not refinance their bank debt with P2P loans and, if anything, they only increase consumption via P2P loans. Recall, however, that banks still increase credit limits for marginally funded borrowers, so debt consolidation alone cannot explain the above finding on higher access to bank credit.

[Place Table VIII about here]

To provide additional evidence that debt refinancing is not a driver of higher access to bank credit after P2P loan take-up, I examine the effect of obtaining a P2P loan on revolving balances and utilization ratios for each ex ante FICO score bin. If P2P lending mitigates information frictions in the consumer credit market by providing pricing benefits for high-quality borrowers, one should expect refinancing to be stronger for borrowers who have higher credit scores. The results speak in support of this explanation. Figure 6 reports the estimates of subsample OLS regressions for each FICO score bin. Similar to Figure 4, the gray bars represent the economic magnitudes of the P2P loan coefficient, and the data labels show the coefficients and their statistical significance. This figure indicates that borrowers of higher credit quality appear to be paying down more of their expensive revolving debt with proceeds from P2P loans. This result confirms that lower credit utilization due to debt refinancing probably does not drive banks' decisions to increase access to credit for P2P borrowers: Revolver balances decrease more for borrowers who have higher FICO scores, but revolver limits increase more for borrowers with lower scores.

[24] These findings are tangential to the credit card literature (e.g., Brito and Hartley (1995)). High balances on credit cards with high rates are a puzzling phenomenon. Researchers have proposed both rational and behavioral explanations for this puzzle. The evidence in this paper is consistent with the rational explanation, that is, high credit card balances are caused by search frictions or switching frictions that result from transaction costs or asymmetric information. This result suggests that once the information asymmetry is reduced, consumers refinance their expensive credit card debt.

[25] I include lagged controls for revolving balances and revolver utilization, since they appear to be persistent, similar to revolver limits. The results are not changed by including lagged outcomes as controls for credit scores.

[Place Figure 6 about here]

The effect of P2P lending on credit scores is ambiguous: Obtaining a P2P loan is associated with a slight increase in FICO but a slight *decrease* in ScoreX in OLS regressions (Table VII). In the RDD analysis, I do not find that borrower credit scores increase as a result of obtaining a P2P loan (Table VIII). These results suggest that banks likely do not react to higher credit scores when deciding to increase access to credit for P2P borrowers. These scores, however, appear as binned variables in the data set, which means that my empirical tests may not be able to capture very small increases in credit scores, even if these increases indeed result from obtaining a P2P loan.

B. *Delinquencies and Overborrowing*

Next, I examine the total debt and delinquencies to show that there is no overborrowing (i.e., higher debt accompanied by higher delinquencies) and borrowers who pass screening by P2P lenders are likely creditworthy. Given the evidence shown so far, I expect the total debt to increase as a result of P2P lending. This prediction relies on the argument that there may exist a category of highly creditworthy borrowers who are unwilling to take credit under high rates (e.g., because of a free cash flow constraint) but are willing to take loans when credit is repriced (and rates become lower). If higher debt leads to overborrowing, then delinquencies should rise for borrowers who obtain P2P loans, and delinquencies should remain unchanged or become less frequent if there is no overborrowing. Since one cannot observe P2P loan defaults for borrowers who do not receive P2P loans, I examine the probability of P2P borrowers becoming delinquent on any debt, including P2P loans and bank debt. The defaults are effectively measured at a 1-year horizon because most repeat borrowers return for another loan within a year.

Table IX presents the results of OLS regressions that use the dollar value of total debt, the debt-to-income ratio (i.e., consumer leverage), and the probability of delinquency as dependent variables. I find that the total debt of borrowers who receive P2P loans becomes, on average, 3.6% to 4.5% higher after P2P loan take-up relative to the total debt of borrowers whose applications are rejected. The results for debt-to-income are qualitatively similar. Using non-mortgage debt instead of the total debt produces similar results. The increase in the total debt despite the decrease in the revolving balances suggests that P2P borrowers were indeed credit constrained before, as they do not use all proceeds from P2P loans for debt refinancing but rather consume (or invest) the remainder. These results may also imply that some creditworthy borrowers find it too expensive to take out a loan at the high interest rates that prevailed before P2P lending. These creditworthy borrowers now choose to take on more debt because the benefits outweigh the costs.

I also find that the probability of delinquency *decreases* for the average P2P borrower, which is consistent with findings in Fuster et al. (2018) for mortgages extended by FinTech lenders. The results for the number of delinquent accounts are similar. These findings suggest that although most P2P borrowers have higher debt after P2P loan take-up, their overall creditworthiness is not significantly affected. In fact, their overall creditworthiness may even improve due to P2P lending. One could also expect the effect of P2P loan take-up on delinquencies to be heterogeneous, as the negative effect of debt on defaults may dominate for some borrowers (especially low-quality borrowers) because of the burden of repayment or moral hazard. However, I do not find any systematic evidence of this heterogeneous effect in the data, presumably because the sample in this paper consists of prime and near-price borrowers.

[Place Table IX about here]

I also do not find any evidence of overborrowing for marginally funded borrowers (Table X). The RDD results show that P2P lending does not lead to higher delinquency rates, and total debt is only marginally higher. These results are inconsistent with overborrowing, and they suggest that borrowers who pass screening by P2P lenders are indeed of high credit quality. One can also attribute the result of P2P borrowers not becoming delinquent more often despite higher overall debt to the "seasoning" or "vintage" effect documented in the banking literature: Borrowers who take new loans benefit from the liquidity buffer in the short-term, as cash inflow from loan proceeds offsets higher leverage. Unfortunately, I cannot explore the default rates at longer horizons because of data limitations, thus I cannot distinguish between these possibilities. Another caveat to the analysis of delinquencies is that the sample in this paper covers a period of credit expansion in the economy when delinquencies were relatively infrequent; most delinquencies materialized in the contraction period of the credit cycle. Therefore, my empirical analysis may not adequately capture true changes in delinquency probabilities after P2P loan take-up, so these results on delinquencies should be interpreted with caution.

[Place Table X about here]

In summary, having examined consumer borrowing patterns and creditworthiness after obtaining a P2P loan, I find evidence that the average P2P borrower exhibits lower demand for bank debt, likely because of credit repricing. I also find that the demand for revolving credit by marginally funded borrowers remains unchanged, or increases somewhat, presumably because of reduced credit rationing for these borrowers. Marginally funded borrowers do not reduce their credit utilization and do not improve their credit scores after P2P loan take-up. Instead, they consume the loan proceeds. However, banks still increase access to credit for these borrowers. Therefore, banks likely react to consumers obtaining or repaying a P2P loan as a signal of credit quality[26] rather than to debt consolidation. I do not find convincing evidence of an increase in delinquencies, despite somewhat higher debt, which rules out overborrowing and lax screening by P2P lenders.

VI. Discussion

A. Interpretation of Findings

How does one interpret the results in this paper? Taken as a whole, the findings are consistent with P2P lending mitigating information frictions for borrowers and increasing access to credit even from existing lenders. It may appear surprising that banks, which have been in the consumer lending market for decades, could learn anything from emerging FinTech lenders. However, this interpretation is facilitated by reiterating several important differences between banks and FinTech lenders.

Banks' lending model historically relied on screening borrowers based on soft and private information collected by loan officers. By contrast, FinTech lenders operate exclusively online, they use algorithms to collect and evaluate borrower data, and they rely predominantly on hard data in their screening process. While many banks have, over time, shifted toward the use of credit scoring techniques, especially for credit cards, the use of these techniques in banking has been a relatively recent phenomenon.[27] The new FinTech lenders exist in a different technological space than banks and place technological improvements at the core of their business. While banks' IT investments have been significant, their legacy IT infrastructure is largely fragmented and outdated. Another difference is that FinTech lenders tend to specialize, in contrast to banks, which are more universal. Banks typically offer many products to many customer segments (e.g., consumers, small businesses, corporations). Banks play a special role in monitoring borrowers in addition to many other functions such as risk-sharing and liquidity transformation. FinTech lenders tend to specialize in the screening function and in providing small loans to one customer segment (e.g., consumers). Last, while banks fund loans themselves (although they may further sell some of these loans), FinTech lenders separate funding from screening. Specifically, the lending process of P2P

[26] Although the data set contains the amount of outstanding debt due to prior Prosper loans at each application, this variable appears noisy and cannot be used to differentiate between these possibilities.

[27] This shift may also imply that using hard data may be sufficient to screen consumers accurately enough to make a credit decision.

lenders benefits from the feedback effect of investor screening (see Vallee and Zeng (2018)).

Anecdotal evidence on bank–FinTech partnerships suggests that banks may be willing to outsource screening to the proprietary technology developed by FinTech lenders. A noteworthy example is a partnership between Regions Bank, a top-20 bank, and Avant, a P2P lending platform that uses algorithms, machine learning, and analytical tools for borrower screening. Under this partnership, Regions Bank entrusts Avant with vetting consumer loan applications between $1,000 and $35,000, which Regions Bank subsequently originates. JP-Morgan Chase has established a similar partnership with OnDeck in small business lending. The partnerships of Beneficial State Bank with LendUp and the partnership between Scotiabank and Kabbage are other examples of bank–FinTech partnerships in which banks license or otherwise receive access to the screening technology of FinTech lenders. Independent and community banks (bank consortia as well as several independent banks) also invest in loans on major P2P lending platforms, suggesting that these banks trust the screening algorithms of P2P lenders. I provide evidence of this in the Internet Appendix.

I discuss three possible channels through which banks can regard information about extension of a P2P loan valuable when making credit decisions. These channels are (1) the superior screening ability of FinTech, (2) the complementarities between the screening algorithms of banks and P2P lenders, and (3) banks free-riding on screening by FinTech when it is not profitable for banks to screen certain borrowers.

First, it may be the case that P2P lending platforms have superior algorithms because of their specialization (i.e., better information *processing* rather than additional information). Another possibility is that the separation between screening and funding in P2P lending produces positive incentives to screen borrowers responsibly. P2P platforms attract funding from institutional investors, and P2P platforms obtain their revenues from fees tied to loan originations, so screening must be accurate to maximize loan volume. Although it is widely recognized that securitization produces adverse incentives in terms of screening (e.g., Keys et al. (2010)), P2P platforms are different in several respects: P2P platforms issue loans with shorter maturities, for which defaults materialize faster than defaults on mortgage-backed securities. These platforms also effectively retain some "skin in the game" in their loans because of investor servicing fees that these platforms can collect only in the absence of defaults. In addition, these platforms are more transparent than banks in providing information to investors.[28] Therefore, institutional investors can monitor the platform and withdraw new funding immediately if they observe evidence of inadequate loan screening by

[28]This lack of transparency may alter banks' incentives to screen borrowers accurately, as in Rajan, Seru, and Vig (2015). By contrast, the disclosure of detailed information on borrowers to investors by P2P lending platforms may serve as a governance mechanism that enforces more accurate screening.

the platform (Balyuk and Davydenko (2019)).

Second, banks may regard screening by P2P lenders as complementary. In other words, it should be sufficient that P2P lending provides incremental information about borrower creditworthiness for banks to update their beliefs about borrower credit quality after observing a P2P loan. In this case, increased access to credit from a P2P lender may serve as a useful additional signal that complements the banking model.

Third, it is possible that P2P lenders choose to screen borrowers whom banks do not find profitable to screen. The set of issues that banks face in their profit-maximization activities (e.g., agency issues, regulatory requirements, incentives to conceal screening algorithms to extract rents) are different from those of P2P lending platforms. These issues may result in an equilibrium in which banks decide not to screen certain borrowers, or not to screen them accurately. Recall that P2P loans are small consumer loans. If the screening costs are larger for banks than for P2P platforms (which is a plausible situation given the full automation of the lending process in P2P lending), then banks may be willing to free ride on FinTech's screening efforts if the FinTech algorithms can screen borrowers reasonably well, even if these algorithms are not superior. Moreover, the increase in access to credit from banks may be accompanied by an increase in interest rates that banks charge P2P borrowers to compensate for the possibly higher risk of lending to these clients. I do not observe the interest rates that banks charge, so I cannot test this conjecture.

Overall, it appears plausible that banks can gain additional insights about borrower credit quality from observing a P2P loan. Precisely identifying the mechanism behind this finding is not possible, because such precise identification requires data on banks' screening models, and these data are, unfortunately, unavailable because they constitute commercially sensitive information. However, I am able to rule out some alternative interpretations of the results. Below, I discuss these alternative interpretations.

B. Alternative Interpretations

B.1. Competition

One possible alternative interpretation of the above findings is that banks respond to P2P lending because of competition rather than by treating P2P loans as a signal of credit quality. If high-quality borrowers substitute bank credit for P2P loans, P2P lending platforms may be imposing adverse selection on banks (as in Broecker (1990)), so banks may be interested in retaining these consumers. Also, banks may be cross-selling other products to these borrowers, such as mortgages, and they will likely lose revenue from these other products if consumers leave. Therefore, banks may be increasing credit limits to borrowers who obtain

P2P loans to compete with P2P lenders, even though the riskiness of these borrowers does not decrease, and their debt increases after a P2P loan. The cross-sectional results in this paper may also be interpreted as competition in segments where switching costs are low, if credit histories capture the length of bank relationships and if credit scores capture information asymmetry about these borrowers. In this case, it is possible that banks compete more for these borrowers, since these borrowers are more likely to leave because of low switching costs.

I present evidence that seems inconsistent with this interpretation. Specifically, I use bank competition in cities where borrowers reside, which I measure with the Herfindahl–Hirschman index (HHI) of bank deposits, and I include an interaction term between the P2P loan origination and the HHI of deposits in OLS regressions.[29] If banks indeed respond to competition from P2P lending platforms, one should expect the results to be stronger in local markets that have less bank competition, because the entry of a new competitor erodes more bank revenue in less competitive markets. I do not find this to be the case, since the interaction term appears to be insignificant in all regression specifications. These results are available in the Internet Appendix.

B.2. Selection on Repeat Borrowers

Recall that the identification of the effect of P2P lending on credit provided by banks relies on observing P2P borrowers over time. Since I cannot observe the outcomes of receiving a P2P loan unless borrowers apply for at least one more loan, this paper focuses on repeat borrowers. One of the potential concerns of the above analysis is that the results may be driven by self-selection of P2P borrowers into repeat borrowers (as opposed to one-time borrowers) because returning for another loan is a matter of choice. If the propensity to borrow repeatedly is higher for more creditworthy borrowers, then OLS regressions may overestimate the effect of P2P lending on access to bank credit. I show that this is unlikely because the bias, if any, works in the opposite direction.

Comparison of borrower characteristics in Table I shows that one-time borrowers appear more creditworthy than repeat borrowers. One-time borrowers have higher FICO scores and appear less credit constrained. Consistently, one-time borrowers receive a lower interest rate from the lending platform. I then verify that propensity to borrow repeatedly from P2P lending platforms is higher for *lower* quality borrowers in regression analysis (refer to Internet Appendix for these results). The evidence suggests that the self-selection bias, if any, works against finding the results in OLS regressions and may lead to underestimation of the results on access to bank credit. This may also explain the significant difference in

[29] I consider the borrower's city to be a more relevant local market for consumer debt than the borrower's state. However, when I rerun the analysis using the state-level HHI of deposits, the results are very similar.

coefficients between OLS regressions and the RDD analysis.

B.3. Improvements in Credit Scores

Last, it may be the case that this certification effect comes from the credit bureau incorporating information about P2P loans into the borrowers' credit scores and banks increasing access to credit for P2P borrowers because of improvements in FICO. Whereas I do not find strong evidence of an increase in borrowers' credit scores, it is useful to remind the reader that the FICO score in the data is binned into FICO buckets. Although these buckets are rather narrow, I may be unable to discern any small improvements in FICO, even if these improvements were caused by P2P lending.

In fact, Chava et al. (2019) explain this increase in bank credit limits for P2P borrowers with improvements in FICO due to lower credit utilization, since borrowers in their sample use P2P loans for debt refinancing. The results in this paper are different, in that borrowers who do not reduce their credit utilization still enjoy higher credit limits from banks after P2P loan take-up. Moreover, I find that borrowers with the lowest FICO scores receive larger increases in credit limits, but these borrowers do not lower their utilization ratios as much as borrowers with higher FICO scores. These differences in findings are difficult to reconcile, given differences in samples and empirical approaches.[30] Data on bank screening algorithms could clarify which of these is the first-order mechanism, but such data are not widely available.

VII. Conclusions

This paper studies how P2P lending, one of the major FinTech innovations in the consumer credit market, impacts the credit provided by traditional credit intermediaries such as banks. The main contribution of this paper is to show that P2P lending relieves information frictions in the consumer credit market and increases access to credit for borrowers even from existing lenders.

The key findings can be summarized as follows. First, I find that banks increase credit for consumers who obtain loans from P2P lending platforms. This increase is larger for borrowers who are ex ante more likely to be credit rationed by banks, such as borrowers who have shorter credit histories or lower credit scores. Second, I document changes in the consumers' borrowing patterns from banks after P2P loan take-up. I find that the average

[30] An important distinction between these two studies is that I compare borrowers who choose to apply for P2P loans but either obtain a loan or are rejected, whereas Chava et al. (2019) compare borrowers who obtain P2P loans with borrowers who are rejected by banks.

P2P borrower substitutes some of her bank debt with P2P loans. By contrast, marginally funded borrowers do not change their use of bank debt after additional borrowing from P2P lenders. Nonetheless, banks increase access to credit for both groups of borrowers. This suggests it is unlikely that banks are willing to provide more credit to P2P borrowers solely due to the refinancing of bank debt. Third, I do not find any evidence that P2P lending leads to more delinquencies, although P2P lending results in somewhat higher total debt.

Collectively, the evidence presented in this paper suggests that financial innovation can play an important role in reducing imperfections in the consumer credit market. This paper focuses on the improvements derived from FinTech's ability to relieve information frictions, while also recognizing the many potential ways in which FinTech can improve outcomes for borrowers, such as reducing bank rents, increasing the speed and convenience of obtaining credit, and promoting financial inclusion. I provide evidence consistent with banks relying on certification by FinTech lenders when deciding whether to increase access to credit for consumers. My findings suggest that FinTech innovations in lending can mitigate credit market imperfections by relieving information frictions for consumers and generating information spillovers to traditional credit intermediaries such as banks.

The results in this paper imply that some important changes in the traditional banking sector may be underway with the rise of FinTech lenders. How is the structure of bank revenues affected by FinTech? What are the implications of FinTech for the risk in the banking sector? How should banks respond to FinTech? I consider these questions interesting avenues for future research.

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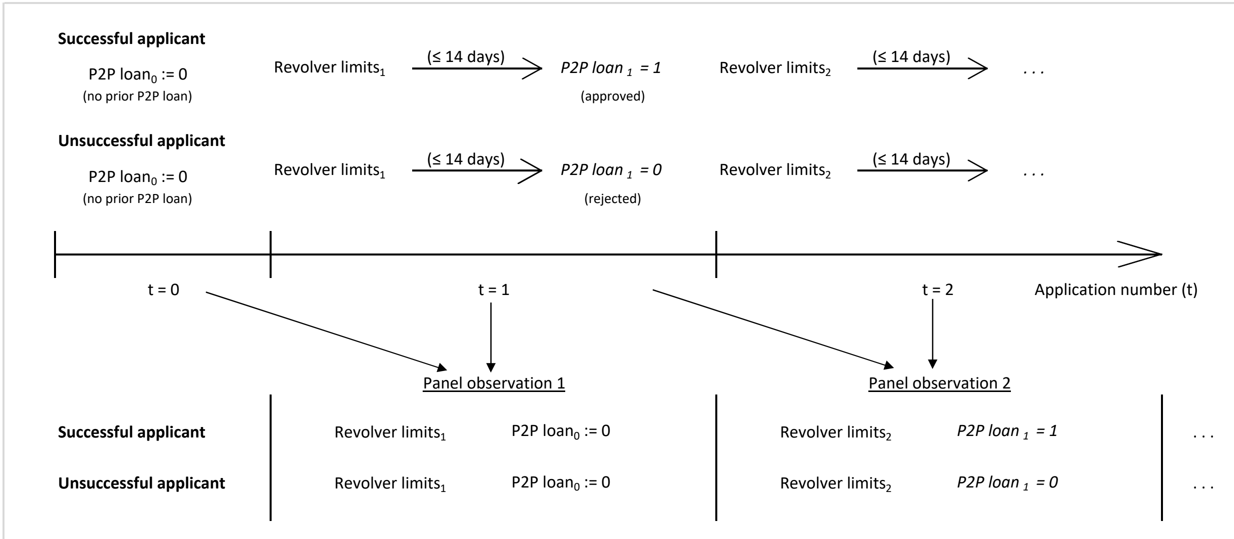

Figure 1. Timeline and construction of data panel. This figure provides the timeline of credit events, and it details the construction of the data panel using revolver limits as an example of borrower financials. The t dimension references the application number. Borrower financials are observed at application, and whether a P2P loan is provided is observed within around 14 days of application. Panel observations are constructed by lagging an indicator for receiving a P2P loan. The first observation is assigned a lagged P2P loan indicator of zero.

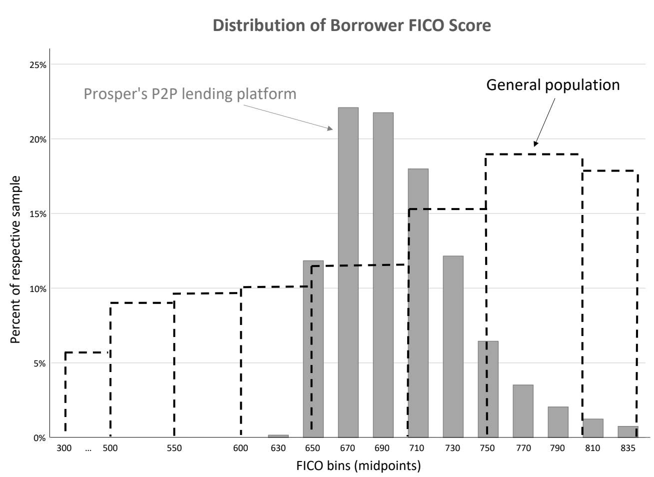

Figure 2. Credit score of P2P borrowers and all consumers. This figure compares the distribution of the FICO score for borrowers from Prosper's P2P lending platform (gray solid bars) with the FICO distribution in the general population of consumers in the U.S. (dashed transparent bars). The bars for the two distributions do not overlap, since the FICO score data comes in bins and the bins are defined differently for P2P borrowers and for the general population.

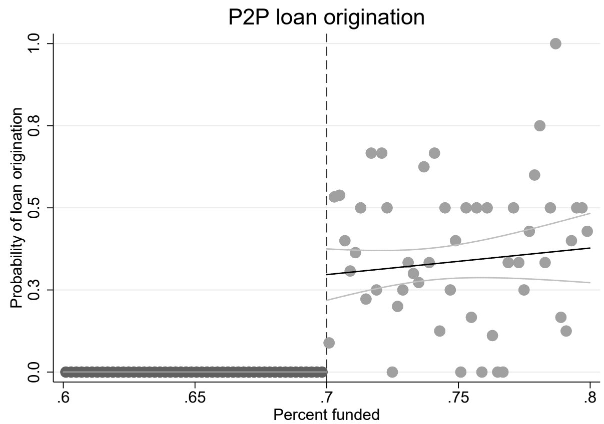

Figure 3. Discontinuity in probability of P2P loan origination. This figure reports the probability of P2P loan origination as a function of the percentage of the loan amount funded by P2P investors. The binned averages represent the conditional expectation of P2P loan origination. *Percent funded* is the percentage of the listing funded by Prosper investors. The straight lines are fitted values of the probability of loan origination, allowing for different linear models to be estimated below and above the threshold. The light gray lines are 99% confidence intervals.

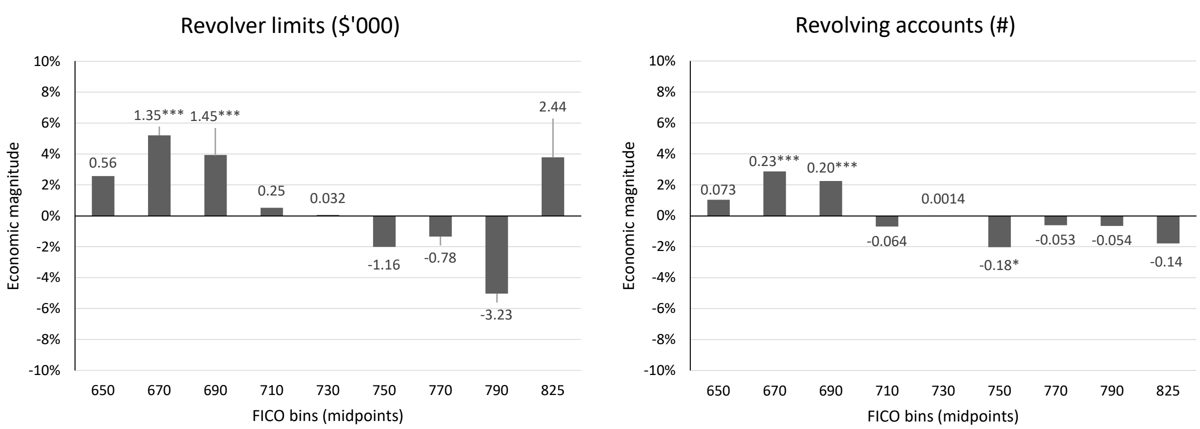

Figure 4. Effect of P2P borrowing on access to bank credit by FICO bin. This figure reports estimates of the effect of obtaining a P2P loan on revolver limits (left) and the number of revolving accounts (right) based on OLS regressions with borrower fixed effects for subsamples split by the ex ante FICO score bin of the borrower. The regression specification is the same as in Equation (1). The gray bars represent economic magnitudes, and the data labels show the coefficients of P2P loan$_{t-1}$. The economic magnitude of the coefficient is measured relative to the mean level of the respective dependent variable on the first P2P loan application. The analysis is restricted to outcomes after the first application.

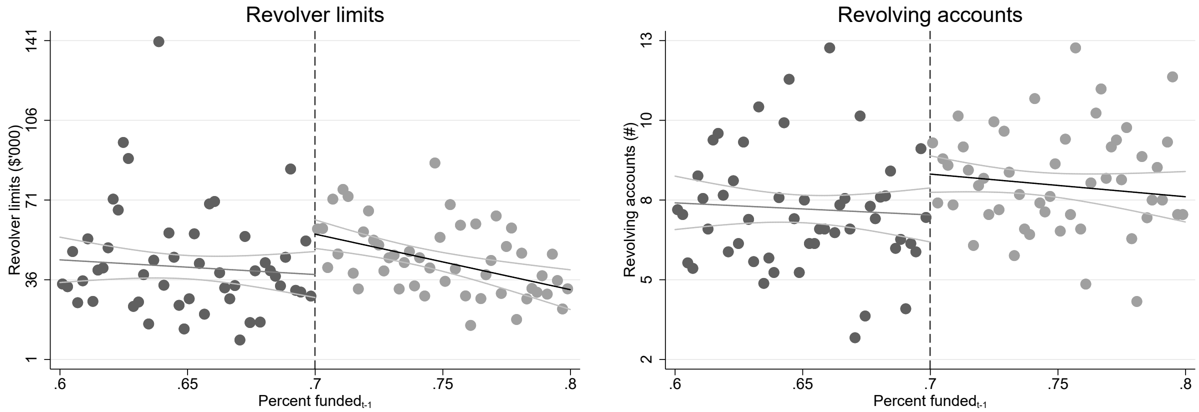

Figure 5. Access to bank credit after a P2P loan application. This figure illustrates subsequent access to bank credit for P2P borrowers whose applications were at least 70% funded by P2P investors and for borrowers whose applications received less in funding commitments. The dashed vertical line is the funding threshold for the loans to originate on the platform. The binned averages represent the conditional expectation of revolver limits and the number of revolving accounts, respectively. *Percent funded* is the percentage of the listing funded by Prosper investors. *Revolver limits* is the total credit limit on revolving accounts. *Revolving accounts* is the number of open revolving accounts. All outcome variables are winsorized at the 1st and the 99th percentiles. The straight lines are fitted values. The light gray lines are 99% confidence intervals.

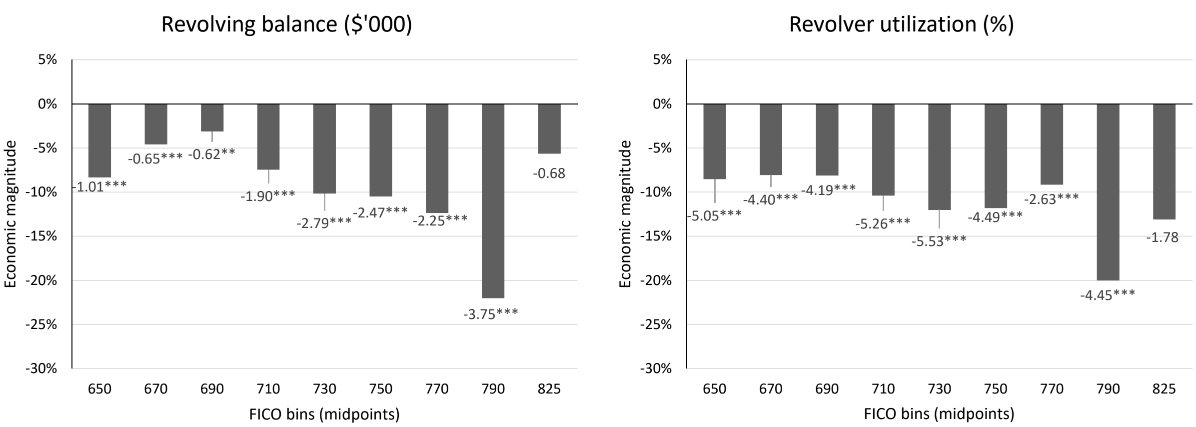

Figure 6. Effect of P2P borrowing on demand for bank credit by FICO bin. This figure reports estimates of the effect of obtaining a P2P loan on revolving balances (left) and revolver utilization (right) based on OLS regressions with borrower fixed effects for subsamples split by the ex ante FICO score bin of the borrower. The regression specification is the same as in Equation (1). The gray bars represent economic magnitudes, and the data labels show the coefficients of P2P loan$_{t-1}$. The economic magnitude of the coefficient is measured relative to the mean level of the respective dependent variable on the first P2P loan application. The analysis is restricted to outcomes after the first application.

Table I
Summary Statistics

This table provides the descriptive statistics of borrower characteristics on the first P2P loan application. *Listing amount* is the requested P2P loan amount. *Interest rate (APR)* is the interest rate on the loan, including origination fees. *Percent funded* is the percentage of the requested P2P loan amount funded by investors. *P2P loan* is an indicator of the originated P2P loan. *Repeat borrower* is an indicator of a repeat P2P loan applicant. *FICO score* is the midpoint value of the FICO credit score binned by Prosper. *ScoreX score* is the midpoint value of the ScoreX score binned by Prosper. *Debt-to-income* is the ratio of monthly debt to monthly income. *Monthly income* is the monthly income. *Employment* is the length of employment. *Credit history* is the length of the credit history. *Homeowner* is an indicator of home ownership. *Open accounts* is the number of current accounts that are still open. *Revolving accounts* is the number of open revolving accounts. *Credit card utilization* is the ratio of balances to limits on bank card accounts. *Revolver utilization* is the ratio of balances to limits on revolving accounts. *Revolver limits* is the total credit limit on revolving accounts. *Revolving balance* is the total balance on revolving accounts. *Installment balance* is the total balance on installment accounts. *Mortgage balance* is the total balance on real estate accounts. *Total debt* is the total balance on all credit accounts. *Delinquency* is an indicator of a delinquent borrower. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles.

	All borrowers (N=445,502)					Repeat borrowers (N=80,656)	
	Mean	SD	Median	Min	Max	Mean	Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: P2P loan application							
Listing amount ($'000)	13.2	8.0	12.0	2	35	13.3	12.0
Loan maturity (months)	43.7	11.4	36.0	12	60	43.7	36.0
Refinancing purpose (1/0)	0.74	0.44	1	0	1	0.75	1
Interest rate (APR)	18.3%	6.6%	17.1%	6.4%	36.0%	19.0%	17.8%
Percent funded	96.6%	16.6%	100%	0%	100%	93.7%	100%
P2P loan (1/0)	0.64	0.48	1	0	1	0.35	0
Panel B: Borrower risk profile							
FICO score	700	37	690	650	810	698	690
ScoreX score	708	51	713	590	789	708	713
Debt-to-income	0.181	0.127	0.164	0	2.793	0.168	0.152
Monthly income ($'000)	6.2	3.7	5.3	0	21.8	6.6	5.6
Employment (years)	8.9	8.4	6.5	0	35.9	8.5	6.0
Credit history (years)	18.1	8.3	17.0	1.8	41.2	18.2	17.1
Homeowner (1/0)	0.48	0.50	0	0	1	0.48	0
Panel C: Selected credit variables							
Open accounts (#)	10.1	4.7	9	1	25	10.0	9
Revolving accounts (#)	8.3	4.4	7	0	23	8.2	7
Credit card utilization	54.5%	27.0%	56.0%	0%	122%	54.9%	57.0%
Revolver utilization	48.6%	23.9%	48.0%	0%	100%	49.2%	49.0%
Revolver limits ($'000)	39.1	40.1	26.1	0	249	39.4	25.6
Revolving balance ($'000)	18.7	24.0	11.0	0	159	19.4	10.8
Installment balance ($'000)	26.7	32.4	17.0	0	179	26.9	17.0
Mortgage balance ($'000)	103.7	142.3	18.2	0	714	105.5	0
Total debt ($'000)	151.3	162.4	93.4	0.8	851	153.7	89.3
Delinquency (1/0)	0.16	0.37	0	0	1	0.16	0

Table II
P2P Lending and Access to Bank Credit

This table reports the results of the OLS regressions with the total dollar amount of revolver limits (Columns (1)–(4)) and the number of revolving accounts (Columns (5)–(8)) as the dependent variables and the P2P loan indicator as the regressor of interest. The analysis is restricted to outcomes after the first application. The regression specification is given in Equation (1). The regressor of interest *P2P loan*$_{t-1}$ is the lagged indicator of an originated P2P loan. *Log (income)* is the natural logarithm of monthly income, measured in thousands. *Log (employment)* is the natural logarithm of length of employment, measured in years. *FICO bins* are indicators for FICO score bins. *ScoreX bins* are indicators for ScoreX score bins. All other variables are defined in Table I. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. The estimates of the intercept and fixed effects are omitted for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent variable:	Revolver limits ($'000)				Revolving accounts (#)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
P2P loan$_{t-1}$	0.98***	1.02***	0.78***	0.82***	0.11***	0.11***	0.12***	0.12***
	(4.65)	(4.81)	(3.74)	(3.91)	(4.85)	(4.83)	(6.13)	(6.19)
Log (income)	3.73***	3.72***	3.85***	3.81***	0.53***	0.53***	0.48***	0.48***
	(10.91)	(10.90)	(12.88)	(12.80)	(13.64)	(13.58)	(15.60)	(15.68)
Log (employment)	0.12	0.13	0.19	0.21	0.022*	0.021*	0.015	0.015
	(1.00)	(1.09)	(1.46)	(1.55)	(1.68)	(1.67)	(1.14)	(1.12)
Homeowner	1.73***	1.74***	1.99***	1.98***	0.20***	0.20***	0.32***	0.32***
	(2.80)	(2.80)	(3.55)	(3.51)	(3.99)	(4.02)	(6.50)	(6.50)
Debt-to-income	20.6***	20.6***	16.1***	16.1***	2.86***	2.85***	2.00***	1.99***
	(9.80)	(9.80)	(10.22)	(10.23)	(10.94)	(10.89)	(11.70)	(11.75)
FICO bins	Yes	Yes	No	No	Yes	Yes	No	No
ScoreX bins	No	No	Yes	Yes	No	No	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State & year FE	Yes	No	Yes	No	Yes	No	Yes	No
State-year FE	No	Yes	No	Yes	No	Yes	No	Yes
Clustered SE	Borrower	Borrower	Borrower	Borrower	Borrower	Borrower	Borrower	Borrower
Observations	126,816	126,804	150,644	150,616	127,464	127,452	152,184	152,156
Adj. within R^2	0.013	0.013	0.011	0.011	0.024	0.024	0.022	0.021
Adj. R^2	0.96	0.96	0.96	0.96	0.97	0.97	0.97	0.97

Table III
P2P Lending and Access to Bank Credit: Sensitivity to Credit History

This table reports the results of the OLS regressions with the total dollar amount of revolver limits (Columns (1)–(4)) and the number of revolving accounts (Columns (5)–(8)) as the dependent variables and the interaction term between the P2P loan indicator and the ex ante length of credit history as the regressor of interest. The analysis is restricted to outcomes after the first application. All variables are defined in Tables I and II. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. The estimates of the intercept and fixed effects are omitted for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent variable:	Revolver limits ($'000)				Revolving accounts (#)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Credit history $\times$ P2P loan$_{t-1}$	-0.13***	-0.13***	-0.14***	-0.14***	-0.0059***	-0.0057***	-0.0068***	-0.0067***
	(-7.10)	(-6.94)	(-7.98)	(-7.95)	(-3.63)	(-3.51)	(-4.67)	(-4.59)
P2P loan$_{t-1}$	3.50***	3.50***	3.36***	3.39***	0.22***	0.22***	0.25***	0.25***
	(9.15)	(9.09)	(9.14)	(9.20)	(5.86)	(5.74)	(7.36)	(7.32)
Log (income)	3.61***	3.60***	3.69***	3.65***	0.53***	0.53***	0.47***	0.47***
	(10.61)	(10.60)	(12.37)	(12.30)	(13.51)	(13.46)	(15.38)	(15.46)
Log (employment)	0.083	0.094	0.14	0.16	0.020	0.020	0.012	0.012
	(0.67)	(0.77)	(1.08)	(1.17)	(1.55)	(1.54)	(0.94)	(0.93)
Homeowner	1.67***	1.68***	1.92***	1.91***	0.20***	0.20***	0.32***	0.31***
	(2.71)	(2.71)	(3.44)	(3.40)	(3.94)	(3.96)	(6.43)	(6.43)
Debt-to-income	20.2***	20.2***	15.7***	15.7***	2.84***	2.84***	1.98***	1.97***
	(9.66)	(9.66)	(10.00)	(10.01)	(10.88)	(10.84)	(11.63)	(11.68)
FICO bins	Yes	Yes	No	No	Yes	Yes	No	No
ScoreX bins	No	No	Yes	Yes	No	No	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State & year FE	Yes	No	Yes	No	Yes	No	Yes	No
State-year FE	No	Yes	No	Yes	No	Yes	No	Yes
Clustered SE	Borrower	Borrower	Borrower	Borrower	Borrower	Borrower	Borrower	Borrower
Observations	126,816	126,804	150,644	150,616	127,464	127,452	152,184	152,156
Adj. within R^2	0.015	0.015	0.014	0.014	0.025	0.025	0.022	0.022
Adj. R^2	0.96	0.96	0.96	0.96	0.97	0.97	0.97	0.97

Table IV
P2P Lending and *Ex Ante* Borrower Characteristics: RDD

This table reports the results of the local fuzzy RDD regression of key borrower characteristics measured on the first P2P loan application. The indicator for crossing the funding threshold of 70% is the (excluded) instrument for the lagged P2P loan origination. The baseline bandwidth is 10%. Column (1) reports the first-stage estimates, and Columns (2)–(7) report the second-stage estimates. The regression specification is a modification of Equations (2a) and (2b), respectively. The instrument $Above_{t-1}$ is an indicator of the funding commitments being at least 70% of the requested loan amount. *Distance* is the difference between the forcing variable (*Percent funded*) and the discontinuity threshold, measured in decimals. *RDD model* is the RDD model specification. All other variables are defined in Tables I and II. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. The estimates of the intercept and fixed effects are omitted for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. *t*-statistics are presented in parentheses.

Dependent variable:	P2P loan_{t-1}	Log (income)	Log (employment)	Home-owner	Debt-to-income	FICO score	ScoreX score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\widehat{\text{P2P loan}}_{t-1}$		0.54*	1.19	0.18	-0.038	6,442	11.9
		(1.84)	(1.58)	(0.72)	(-0.52)	(0.06)	(0.56)
Above_{t-1}	0.39***						
	(7.97)						
Distance	-0.86	-1.30	-13.8	2.23	0.67	-4,632	346
	(-0.79)	(-0.31)	(-1.29)	(0.61)	(0.64)	(-0.06)	(1.07)
Distance × Above_{t-1}	-0.77	0.50	16.5	-5.29	0.25	-28,566	-1,162***
	(-0.30)	(0.09)	(1.23)	(-1.12)	(0.19)	(-0.07)	(-2.77)
Distance^2	-9.17	-0.13	-101	11.8	7.22	-50,964	2,525
	(-0.87)	(-0.00)	(-1.02)	(0.34)	(0.74)	(-0.06)	(0.80)
Distance^2 × Above_{t-1}	23.7	-2.34	45.2	10.8	-19.7	409,414	5,256
	(0.88)	(-0.04)	(0.35)	(0.25)	(-1.51)	(0.06)	(1.36)
RDD model	Quadratic + splines	Quadratic + splines	Quadratic + splines	Quadratic + splines	Quadratic + splines	Quadratic + splines	Quadratic + splines
State & year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Robust SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	820	807	765	820	807	353	818
Adj. R^2	0.22	-0.04	-0.09	0.003	-0.01	-1556	0.03
R^2	0.27	0.03	-0.02	0.07	0.05	-1361	0.09

Table V
P2P Lending and Access to Bank Credit: RDD

This table reports the results of the local fuzzy RDD regression of variables related to access to bank credit on receiving a P2P loan. The indicator for crossing the funding threshold of 70% is the (excluded) instrument for the lagged P2P loan origination. The baseline bandwidth is 10%. Column (1) reports the first-stage estimates, and Columns (2)–(9) report the second-stage estimates. The basic regression specification is the same as in Equations (2a) and (2b), respectively. All variables are defined in Tables I, II, and IV. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. The estimates of the intercept and fixed effects are omitted for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent variable:	P2P loan$_{t-1}$	Revolver limits ($'000)				Revolving accounts (#)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\widehat{\text{P2P loan}}_{t-1}$		51.2***	46.6***	46.0***	38.4**	-0.44	-0.35	-0.30	-0.16
		(3.28)	(3.14)	(3.08)	(2.00)	(-0.89)	(-0.80)	(-0.71)	(-0.33)
Above$_{t-1}$	0.39***								
	(8.01)								
Distance	-0.71	-163***	-152***	-82.3	-69.3	2.83	2.60	0.51	-4.58
	(-0.64)	(-3.26)	(-3.16)	(-1.19)	(-0.25)	(1.61)	(1.62)	(0.36)	(-0.69)
Distance × Above$_{t-1}$	-1.25			-135	55.9			3.89	11.4
	(-0.48)			(-1.40)	(0.15)			(1.25)	(1.12)
Distance^2	-8.36		-762		123		16.2		-50.6
	(-0.76)		(-1.61)		(0.05)		(1.03)		(-0.79)
Distance^2 × Above$_{t-1}$	26.9				-2,389				23.9
	(0.98)				(-0.84)				(0.26)
Revolver limits$_{t-1}$	<0.001	0.23**	0.23**	0.23**	0.23**				
	(0.07)	(2.47)	(2.48)	(2.48)	(2.47)				
Revolving accounts$_{t-1}$						0.92***	0.92***	0.92***	0.92***
						(57.07)	(56.99)	(57.03)	(57.06)
RDD model	Quadratic + splines	Linear	Quadratic	Linear + splines	Quadratic + splines	Linear	Quadratic	Linear + splines	Quadratic + splines
State & year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Robust SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	810	810	810	810	809	823	823	823	823
Adj. R^2	0.22	0.22	0.22	0.22	0.19	0.91	0.91	0.91	0.91
R^2	0.27	0.27	0.27	0.27	0.24	0.92	0.92	0.92	0.92

Table VI
P2P Lending and Access to Bank Credit: "Donut" RDD

This table reports the results of the "donut" local fuzzy RDD regression of variables related to access to bank credit on receiving a P2P loan, in which observations with funding rates between 70% and 70.1% are omitted. The indicator for crossing the funding threshold of 70% is the (excluded) instrument for the lagged P2P loan origination. The baseline bandwidth is 10%. Column (1) reports the first-stage estimates and Columns (2)–(9) report the second-stage estimates. The basic regression specification is the same as in Equations (2a) and (2b), respectively. All variables are defined in Tables I, II, and IV. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. The estimates of the intercept and fixed effects are omitted for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent variable:	P2P loan_{t-1}	Revolver limits ($'000)				Revolving accounts (#)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\widehat{\text{P2P loan}}_{t-1}$		41.0***	38.6***	39.1***	25.3*	-0.0016	0.034	0.037	0.30
		(3.03)	(2.92)	(2.92)	(1.73)	(-0.00)	(0.09)	(0.09)	(0.73)
Above_{t-1}	0.56***								
	(8.81)								
Distance	-0.47	-129***	-124***	-74.0	-103	1.66	1.58	0.48	-3.61
	(-0.44)	(-2.95)	(-2.89)	(-1.08)	(-0.36)	(1.09)	(1.08)	(0.33)	(-0.53)
Distance × Above_{t-1}	-9.45***			-101	322			2.19	3.41
	(-2.97)			(-1.02)	(0.74)			(0.71)	(0.31)
Distance^2	-5.08		-691		-299		11.2		-40.9
	(-0.49)		(-1.47)		(-0.11)		(0.71)		(-0.62)
Distance^2 × Above_{t-1}	92.1***				-3,988				74.3
	(2.96)				(-1.61)				(0.82)
Revolver limits_{t-1}	<0.001	0.22**	0.22**	0.22**	0.22**				
	(0.04)	(2.41)	(2.43)	(2.42)	(2.41)				
Revolving accounts_{t-1}						0.92***	0.92***	0.92***	0.92***
						(54.18)	(54.16)	(54.19)	(53.84)
RDD model	Quadratic + splines	Linear	Quadratic	Linear + splines	Quadratic + splines	Linear	Quadratic	Linear + splines	Quadratic + splines
State & year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Robust SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	750	750	750	750	748	763	763	763	763
Adj. R^2	0.25	0.24	0.25	0.25	0.30	0.91	0.91	0.91	0.91
R^2	0.30	0.29	0.30	0.30	0.34	0.92	0.92	0.92	0.92

Table VII
P2P Lending, Credit Demand, and Creditworthiness

This table reports the results of the OLS regressions with the total dollar amount of revolving balances (Columns (1)–(2)), revolver utilization ratio (Columns (3)–(4)), and credit scores (Columns (5)–(6)) as the dependent variables, and the P2P loan indicator as the regressor of interest. The analysis is restricted to outcomes after the first application. The regression specification is the same as in Equation (1). All variables are defined in Tables I and II. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. The estimates of the intercept and fixed effects are omitted for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent variable:	Revolving balance ($'000)		Revolver utilization (%)		FICO score	ScoreX score
	(1)	(2)	(3)	(4)	(5)	(6)
P2P loan$_{t-1}$	-1.48***	-2.26***	-5.12***	-7.45***	8.78***	-3.65***
	(-10.77)	(-19.87)	(-22.21)	(-40.45)	(24.59)	(-8.36)
Log (income)	3.91***	2.81***	4.96***	2.05***	-2.88***	-10.7***
	(13.70)	(17.96)	(12.41)	(8.44)	(-7.41)	(-19.19)
Log (employment)	0.11	0.15*	0.091	-0.070	0.44*	-0.039
	(1.33)	(1.70)	(0.65)	(-0.55)	(1.87)	(-0.13)
Homeowner	2.47***	4.33***	3.51***	9.33***	1.87**	21.7***
	(6.64)	(12.62)	(7.35)	(20.35)	(2.48)	(20.79)
Debt-to-income	22.2***	12.4***	27.1***	8.59***	-10.6***	-43.8***
	(11.10)	(13.34)	(10.17)	(7.68)	(-5.08)	(-13.52)
FICO bins	Yes	No	Yes	No	No	No
ScoreX bins	No	Yes	No	Yes	No	No
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	Borrower	Borrower	Borrower	Borrower	Borrower	Borrower
Observations	127,452	152,156	127,452	152,156	127,452	152,156
Adj. within R^2	0.04	0.04	0.06	0.02	0.02	0.04
Adj. R^2	0.97	0.96	0.89	0.87	0.87	0.85

Table VIII
P2P Lending, Credit Demand, and Creditworthiness: "Donut" RDD

This table reports the results of the "donut" local fuzzy RDD regression of variables related to demand for bank credit and credit scores on receiving a P2P loan, in which observations with funding rates between 70% and 70.1% are omitted. The indicator for crossing the funding threshold of 70% is the (excluded) instrument for the lagged P2P loan origination. The basic regression specification is the same as in Equations (2a) and (2b), respectively. The baseline bandwidth is 10%. Different columns report different modifications of the basic specifications. All variables are defined in Tables I and II. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. The estimates of the intercept, fixed effects, and controls are omitted for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

RDD model:		Linear	Quadratic	Linear + splines	Quadratic + splines
Dependent variable	$\widehat{\text{P2P loan}}_{t-1}$	(1)	(2)	(3)	(4)
Revolving balance ($'000)	Coefficient	2.27	2.48	2.62	1.82
	t-statistic	(0.73)	(0.80)	(0.84)	(0.49)
Revolver utilization (%)	Coefficient	0.46	1.14	1.27	5.37
	t-statistic	(0.11)	(0.29)	(0.32)	(1.19)
FICO score	Coefficient	4.70	4.76	2.12	1.44
	t-statistic	(0.24)	(0.23)	(0.10)	(0.05)
ScoreX score	Coefficient	-23.3	-25.6	-27.1*	-29.7*
	t-statistic	(-1.51)	(-1.64)	(-1.72)	(-1.73)

Table IX
P2P Lending, Total Debt, and Delinquencies

This table reports the results of the OLS regressions with the total dollar amount of debt (Columns (1)–(2)), debt-to-income ratio (Columns (3)–(4)), and probability of delinquency on any debts (Columns (5)–(6)) as the dependent variables, and the P2P loan indicator as the regressor of interest. The analysis is restricted to outcomes after the first application. The regression specification is the same as in Equation (1). All variables are defined in Tables I and II. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. The estimates of the intercept and fixed effects are omitted for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent variable:	Total debt ($'000)		Debt-to-income		Delinquency (1/0)	
	(1)	(2)	(3)	(4)	(5)	(6)
P2P loan$_{t-1}$	6.93***	5.55***	0.038***	0.042***	-0.0053*	-0.014***
	(11.57)	(8.96)	(44.08)	(42.61)	(-1.72)	(-4.59)
Log (income)	0.99***	1.77***	-0.17***	-0.20***	-0.012***	-0.013***
	(3.20)	(4.63)	(-40.54)	(-30.13)	(-3.41)	(-3.81)
Log (employment)	0.77*	0.70	0.0033***	0.0047***	-0.0016	-0.00083
	(1.89)	(1.55)	(6.08)	(6.24)	(-0.80)	(-0.41)
Homeowner	204***	201***	0.0046**	0.014***	0.022***	0.026***
	(61.56)	(66.26)	(2.27)	(6.64)	(2.78)	(3.46)
Debt-to-income					-0.064***	-0.055***
					(-2.93)	(-3.62)
FICO bins	Yes	No	Yes	No	Yes	No
ScoreX bins	No	Yes	No	Yes	No	Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	Borrower	Borrower	Borrower	Borrower	Borrower	Borrower
Observations	120,386	143,682	127,454	152,158	127,454	152,158
Adj. within R^2	0.51	0.42	0.44	0.38	0.001	0.002
Adj. R^2	0.98	0.97	0.87	0.84	0.90	0.87

Table X
P2P Lending, Total Debt, and Delinquencies: "Donut" RDD

This table reports the results of the "donut" local fuzzy RDD regression of variables related to total debt and delinquencies on receiving a P2P loan, in which observations with funding rates between 70% and 70.1% are omitted. The indicator for crossing the funding threshold of 70% is the (excluded) instrument for the lagged P2P loan origination. The basic regression specification is the same as in Equations (2a) and (2b), respectively. The baseline bandwidth is 10%. Different columns report different modifications of the basic specifications. All variables are defined in Tables I and II. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. The estimates of the intercept, fixed effects, and controls are omitted for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

RDD model:		Linear	Quadratic	Linear + splines	Quadratic + splines
Dependent variable	$\widehat{\text{P2P loan}}_{t-1}$	(1)	(2)	(3)	(4)
Total debt ($'000)	Coefficient	6.24	5.58	5.19	28.4
	t-statistic	(0.28)	(0.27)	(0.25)	(1.10)
Debt-to-income	Coefficient	0.031	0.029	0.028	0.027
	t-statistic	(0.77)	(0.69)	(0.67)	(0.49)
Delinquency (1/0)	Coefficient	-0.019	-0.037	-0.038	0.056
	t-statistic	(-0.16)	(-0.30)	(-0.30)	(0.41)