Monetary Normalizations and Consumer Credit: Evidence from Fed Liftoff and Online Lending*

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On December 16, 2015, the Federal Reserve initiated "liftoff," a critical step in the monetary normalization process. We use a unique panel data set of 640,000 loan-hour observations to measure the cross-sectional impact of liftoff on interest rates, demand, and supply in the peer-to-peer market for uncollateralized consumer credit. We find that the spread decreased by 17 percent, driven by an increase in supply. Our results are consistent with an investor-perceived reduction in default probabilities and suggest that liftoff provided a strong, positive signal about the future solvency of high credit risk borrowers.

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1. Introduction

Between July 2007 and December 2008, the Federal Open Market Committee (FOMC) lowered its target rate from a pre-crisis high of

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5.25 percent to 0 percent. The federal funds rate then remained near 0 percent for seven years until the FOMC announced "liftoff"—a 25 basis points (bps) hike on December 16, 2015 that signaled an end to emergency measures (FOMC 2015a, 2015b). According to the FOMC's "Policy Normalization Principles and Plans" statement, which marked the return to conventional monetary policy, liftoff constituted the first step in a monetary normalization plan that will ultimately include additional rate hikes and balance sheet adjustments (FOMC 2014; Williamson 2015). Since the FOMC explicitly conditioned normalization on the state of the economy (FOMC 2014), this choice also provided a strong, positive signal about the Federal Reserve's (the Fed's) private assessment of the economy.[1]

We use a unique panel data set of 640,000 loan-hour observations to estimate the Fed liftoff's impact on the peer-to-peer (P2P) market for uncollateralized online consumer credit. The online consumer credit market has been growing rapidly and accounted for around one-third of the U.S. market for unsecured personal loans in 2018 (Balyuk and Davydenko 2019). Furthermore, it is at the forefront of the digitalization of credit, which makes it important for understanding how the online consumer credit market will be affected by future monetary policy. Our work complements the existing empirical literature that identifies the effects of monetary policy on credit availability, consumption, bond interest rates, stock prices, and risk premiums;[2] however, we focus exclusively on the first step of the monetary normalization process, use primary market data, and explore cross-sectional implications.

The existing literature finds that monetary contractions tend to decrease loan supply, increase interest rates, and increase spreads.

[1] James Bullard, President of the Federal Reserve Bank of St. Louis, emphasized the signaling channel in a December 7, pre-liftoff interview: "If we do move in December . . . [it] does signal confidence. It does signal that we can move away from emergency measures, finally" (Bullard 2015).

[2] See Bernanke and Blinder (1992), Bernanke and Gertler (1995), Kashyap and Stein (2000), and Jiménez et al. (2012) on credit availability and Di Maggio et al. (2017) on consumption. For the effect of surprise monetary contractions on bond interest rates, see Cook and Hahn (1989), Kuttner (2001), Cochrane and Piazzesi (2002), Wright (2012), and Hanson and Stein (2015). On stock prices, see Rigobon and Sack (2004) and Bernanke and Kuttner (2005). On risk premiums, see Gertler and Karadi (2015), and for the effects of quantitative easing, see Krishnamurthy and Vissing-Jorgensen (2011).

Our findings differ in sign; and our empirical evidence suggests that the contractionary component of liftoff—an interest rate hike that exceeded expectations—was dominated by the positive signal provided by the choice to proceed with normalization. The signaling effect is particularly strong for low-rated borrowers in the P2P market, who often exhibit subprime characteristics,[3] and, thus, may benefit from improvements in the future outlook of the economy—including the labor market—that lower perceived default probabilities. While we concentrate on the P2P market for uncollateralized consumer loans—which provides us with a laboratory to study the heterogeneous effects of monetary policy signaling—our findings are likely to bear relevance for other risky credit market segments that are also strongly influenced by broader economic developments.

The main results consist of estimates for two outcomes: (i) the change in the spread between high and low credit risk borrowers; and (ii) the change in the average interest rate on uncollateralized consumer loans. We find that the spread between high and low credit risk borrowers decreased by 17 percent. The spread reduction was primarily driven by a decrease in rates for the riskiest borrower segments, which experienced the largest increase in supply of funds. Moreover, we show that the average interest rate on loans in our data set fell by 16.9–22.9 bps. The decrease in the average interest rate is economically significant, and the magnitude of the observed 166 bps reduction in the spread between high and low credit risk borrowers after liftoff is equivalent to approximately one-third of the effect of moving up from Prosper rating category D to C or an improvement in the FICO score from 679 to 690.

These results are robust to the inclusion of all observable loan and borrower characteristics, as well as intraday fixed effects and intraweek fixed effects. We also show that our results are not driven by a change in borrower composition, a collapse in demand, a shift in investor risk appetite, a seasonal adjustment, or Fed undershooting;[4] and are robust to the choice of time window. Both narrow and

[3]Borrowers in the P2P market are typically above the subprime FICO cutoff; however, many exhibit other characteristics associated with subprime borrowing (e.g., missing documentation).

[4]We show that it is unlikely that the Fed undershot with respect to either the federal funds rate adjustment or the announced forward-guidance plan; however,

wide windows (including 3-day, 7-day, and 14-day windows around liftoff) yield statistically significant results. Visual inspection and placebo tests suggest that the change happened precisely at liftoff.

Additional evidence using separate hourly measures for demand and supply allows us to discriminate between different candidate explanations for our main results, and points clearly to a supply-side explanation. We show that demand does not decline after liftoff, which rules out most plausible alternative stories that rely on a demand decrease. To the contrary, investors' propensity to supply funds increases sharply—especially for the riskiest borrower groups. The probability of individual loans getting funded also increases. In sum, we can rule out explanations that are driven by the demand side, including those that rely on borrower composition shifts.

The primary data set we use was scraped at an hourly frequency from Prosper.com, the oldest and second-largest U.S.-based P2P lender. One distinctive feature of this panel data set is that it contains separate measures of demand and supply, unlike time-series market data or bank-based loan origination data. It also contains rejected loans, unlike most bank-based loan data sets. Moreover, it is uncommon that borrowers are discouraged from applying for loans in this platform, since the application cost is low. Demand is constructed by aggregating the amount requested on all loans posted on Prosper at a point in time. Supply measures are constructed using three different definitions: (i) the aggregate amount that has been funded across all loans at a point in time; (ii) the aggregate change in funding over a given time interval; and (iii) the realized probability that a loan will be funded. Exploiting this unique feature of our data set, we show that all measures of supply increased after liftoff, with the largest increase accruing to the high credit risk borrower segment. Demand also increased, but only slightly. Additionally, we also show that the funding gap—the aggregate amount that has been demanded, but not yet supplied—decreased after liftoff, suggesting that the increase in supply was larger than the increase in demand. Overall, these results point to a supply-side explanation for the reduction in the spread and in interest rates.

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our results do not depend on this assumption and would hold if the opposite were true.

We also collected a secondary data set from LendingClub.com by compiling Securities and Exchange Commission (SEC) records. This data set contains a higher number of individual loans but is available only at a daily frequency, since we were unable to track LendingClub originations in real time. This means that we cannot repeat the supply, demand, and funding gap exercises for this data, and we cannot observe interest rates at an intraday frequency. We can, however, replicate the average interest rate and spread results: both decline in the LendingClub data, and the magnitudes of the declines are nearly identical to our original findings. Taken together, both data sets cover more than 70 percent of the U.S. P2P market.

To further establish robustness, we demonstrate that the direction and magnitude of the liftoff results are not common to FOMC decisions by performing the same analysis on the January 27, 2016 decision not to raise rates. In contrast to liftoff, we find that this decision had no statistically significant impact on interest rates. This holds for both wide and narrow time windows, suggesting that there is no common announcement effect. We also perform a sequence of rolling regressions of the interest rate on loan-borrower characteristic controls using a narrow time window. We show that the results are only significant when liftoff is selected as the center of the window.[5] Additionally, the available data allow us to study the subsequent rate hikes on December 14, 2016 and March 15, 2017. We find no significant effect on the average P2P interest rates associated with these policy rate announcements, which confirms the unique role of liftoff in sending a strong positive signal.

The rest of the article proceeds as follows. Section 2 provides an overview of Fed liftoff and the P2P lending market, as well as the expected effects. Section 3 describes the data and how it was collected. Section 4 presents our findings. We discuss the related literature in section 5 and conclude in section 6.

2. Market Setting and Theoretical Framework

We proceed by describing Fed liftoff and market expectations in section 2.1. Thereafter, we describe the P2P lending market in the

[5] In addition to performing robustness tests, we have also discussed the paper with practitioners in the P2P market to ensure that the findings and proposed mechanism are credible.

United States and the Prosper P2P lending platform in section 2.2. Finally, we discuss the theoretical framework that guides our empirical investigation and the expected effects of liftoff in section 2.3.

2.1 Fed Liftoff

During the second half of 2015, the prospect of Fed liftoff was considered by many to be an important event with historic connotations. It marked the end of an unprecedented era of monetary easing and was regarded as an important step towards monetary normalization. On the day prior to liftoff, market participants largely anticipated that the FOMC would vote to raise rates. This is perhaps best reflected in futures contracts, which implied a .84 probability of the federal funds rate range increasing from 0–25 bps to 25–50 bps and a near-zero probability for a rate hike above the 25–50 bps range.[6] This suggests that the FOMC's rate decision overshot, rather than undershot, market expectations. Furthermore, yields on three- to five-year maturity corporate bonds also increased by 17 bps, suggesting that the announced path of forward guidance may have also overshot, pulling up longer term rates after liftoff.

Overall, we interpret the interest rate adjustment and forward-guidance path announcement as contractionary relative to expectations; however, our main results do not depend on this assumption. Even if the decisions were expansionary, the interpretation of all results in the paper would remain unchanged.[7]

Finally, while Fed liftoff was widely expected, there was uncertainty about the timing of the move, which drew substantial

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[6] Source: The probability of a federal funds rate increase is based on futures, computed by Bloomberg one day prior to liftoff. The underlying contracts are written for the effective federal funds rate, rather than the Fed's target rate range, which means that the range probabilities are not assumption free. Importantly, however, Bloomberg's calculations were not anomalous and aligned closely with other estimates, including those produced by the Chicago Mercantile Exchange. Interest rates on short maturity debt, such as commercial paper, also increased after liftoff, which reinforces the claim that the Fed did not undershoot relative to expectations.

[7] If the FOMC statement undershot the expected forward-guidance path, this would be captured entirely by changes in rates for near-prime borrowers in our sample. In fact, we find that the reduction in rates is substantially larger for the riskiest borrowers.

attention in discussions among P2P market practitioners. Our identifying assumption is that Fed liftoff was the key event within the narrowest window around liftoff we use (±3 days). Furthermore, we argue that there were no other relevant events that could credibly explain the shift in the P2P lending market, such as substantial and unexpected news from economic data releases, and section 4.1 offers a robustness test.

2.2 The Prosper P2P Lending Platform

The P2P lending market is growing rapidly. In 2018 it reached around one-third of the U.S. market for unsecured personal loans (Balyuk and Davydenko 2019). Our primary data set comprises a panel of loan-hour observations from the P2P lending platform Prosper.com, which operates the oldest and second-largest lending-based crowdfunding platform for uncollateralized consumer credit in the United States, and has been operating since February 2006. As of January 2016, Prosper has more than 2 million members (investors and borrowers) and has originated loans in excess of $6 billion. Borrowers ask for personal uncollateralized loans ranging from $2,000 to $35,000 with a maturity of three or five years. The highest-rated borrowers may have access to traditional sources of credit from banks and credit cards, but the lowest-rated borrowers are unlikely to have such outside options.

After the loan application is submitted, the platform collects self-reported and publicly available information, including the borrower's credit history. Prosper uses a credit model to decide on the borrower's qualification for the loan, to assign a credit score, and to set a fixed interest rate and repayment schedule. The process is fast, and qualified borrowers can expect to receive an offer within 24 hours. The funding phase takes place during a 14-day listing period. Investors review loan listings that meet their criteria and invest (e.g., in $25 increments). A loan can be originated as soon as 100 percent of the funding goal is reached or if a minimum of 70 percent is reached by the end of the listing period. Provided borrowers accept the loan, the total funding volume (net of an origination fee) is disbursed. Prosper services the loan throughout the duration and transfers the borrower's monthly installments to lenders.

According to its website, Prosper assigns rates to loans based on a proprietary measure of expected loss (Prosper rating), the loan term, the economic environment, and the competitive environment. Similarly, LendingClub's website explains that rates are adjusted in response to "macroeconomic conditions, supply and demand on the LendingClub platform, and evolving default and chargeoff rates." Prosper and LendingClub provide lists of average rates and rate ranges associated with their respective proprietary rating groups. For the sample period we study, the minimum value of the best-rated group, the base rate, is lower than 5 percent on both platforms. The maximum value in the worst-rated group is 30.25 percent. Importantly, shifts in these averages and ranges reflect all of the aforementioned pricing factors, as well as changes in how individuals are assigned to different rating groups. For this reason, interest rate change announcements cannot be meaningfully interpreted without first controlling for loan and borrower characteristics.[8]

P2P lending platforms generate fee income that relates to the transaction volume. Specifically, Prosper's fee structure consists of (i) an origination fee of 0.5–5 percent paid by borrowers at loan disbursement; (ii) an annual loan servicing fee of 1 percent paid by lenders; (iii) a failed-payment fee of $15; (iv) a late-payment fee of 5 percent of the unpaid installment or a minimum of $15; and (v) a collection agency recovery fee in the case of a defaulting borrower. The first three fees generate income for Prosper, while the late-payment fee and the collection agency recovery fee are passed on to the lenders. The net profit from late-payment fees is likely to be negligible after accounting for administrative costs. Hence, origination and servicing fees are the key contributors to platform profits.

Given the fee structure, we argue that maximizing of the origination volume is a close approximation to Prosper's interest rate setting problem, conditional on Prosper maintaining appropriate underwriting standards that shield it from potential reputational losses.

[8]Prosper is privately owned and is not obligated to announce rate changes. LendingClub announced an interest rate shift after Fed liftoff. After controlling for loan and borrower characteristics, this shift had a negative impact on the average rate of a constant-quality borrower.

2.3 Expected Effects

The interest rate set for individual Prosper loans can be understood as a function of the risk-free reference rate, economic risk premiums, and market conditions. The risk-free reference rate is influenced by monetary policy. The Federal Reserve targets the overnight federal funds rate and, thereby, affects the nominal risk-free reference rate. Moreover, monetary policy also influences the term structure via expectations of future federal funds rates. The risk premium on Prosper P2P loans comprises credit risk and term risk.[9] Given the uncollateralized nature of the P2P consumer credit segment, the credit risk of individual borrowers is arguably the dominant determinant of the risk premium and of key interest in our study. Moreover, our evidence from section 2.1 suggests that term risk does not appear to be a substantial driver.[10] The dominant role of credit risk also resonates with our cross-sectional analysis. Important factors of influence are unemployment risk, health risk, divorce, or expenditure needs.

When setting the interest rates on individual loans, the Prosper P2P lending platform faces changing market conditions in the form of stochastic supply and demand. One way to understand the interest rate setting problem is to compare it to a joint pricing and inventory control problem with perishable inventory. Such problems have been discussed in the operations research literature.[11] In the context of the P2P lending platform, the inventory corresponds to the funding gap, which is the difference between the cumulative inflows of funds and the target for the outstanding total loan amount for all listings at a given point in time. It is in the interest of the lending platform to safeguard against a scenario where the supply of funds cannot be met by means of an inventory of unfunded loans at a given point in time. The inventory, however, is perishable, since loans are not originated and are permanently delisted if not funded by at least

[9] Recall that the interest rate on Prosper loans is fixed at origination and the average maturity is between three and five years. As a result, investors are exposed to term risk since the short-term risk-free reference rate may not evolve as expected.

[10] This also excludes forward-guidance channels (e.g., Del Negro, Giannoni, and Patterson 2012).

[11] See, e.g., McGill and van Ryzin (1999); Petruzzi and Dada (1999); Elmaghraby and Keskinocak (2003).

70 percent within a 14-day period. Hence, it is undesirable to maintain a large funding gap. At the same time, a positive funding gap or "excess demand" serves an important purpose, as it allows investors to have access to a sufficiently deep pool of loan listings at a given point in time.

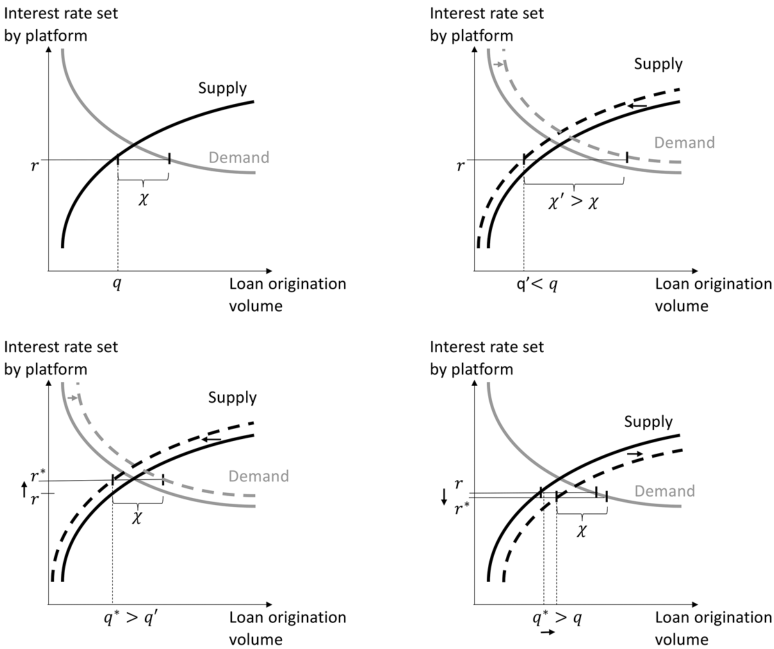
In contrast to other markets, the inventory is not produced, but the interest rate set by the lending platform affects both supply and demand. Moreover, the interest rate is set before an individual loan is listed on the platform and cannot subsequently be adjusted. This differs, for instance, from the case of event admission tickets, which can be discounted when demand is revealed to be weak.[12] In addition, Prosper's interest rate setting is complicated by the fact that newly listed loans compete with previously listed loans, resulting in potential crowding-out effects when rates differ. This latter feature is likely to prevent Prosper from significantly changing the pricing as long as it does not face lasting changes in market conditions. We continue discussing a decomposition of expected effects of such changes in market conditions based on a stylized description of online lending market segment specific supply and demand.

Risk-Free Reference Rate Channel. Based on the existing literature on event studies, which identifies the effect of monetary policy on bond prices, we expect to observe at least partial interest pass-through (e.g., Cook and Hahn 1989 or Kuttner 2001). Namely, an unexpected increase in the reference rate is, in isolation, associated with an increase in the funding costs of P2P borrowers. More specifically, we would expect the propensity of investors to supply funds to decrease for all market segments, because investors earn a lower premium over the risk-free rate. We use graphs to offer a stylized illustration.

The upper left panel of figure 1 depicts market clearing in a given segment of the online lending market, assuming that the platform targets an inventory of > 0 to give investors a sufficiently deep pool of potential investments and to allow for diversification across loans. We depict the inventory, χ, as excess demand and recall that in our sample 25.3 percent of loans are identified as unfunded after the 14-day period. The upper right panel of figure 1 shows the inward

[12] See Sweeting (2012).

Figure 1. P2P Consumer Credit Market Supply and Demand



shift in supply associated with an unexpected increase in the reference rate. Arguably, unsophisticated loan applicants are likely to be less responsive to interest rate changes. Nevertheless, the unexpected increase in the reference rate may increase their costs for alternative funding. Consequently, we may expect to see an increase borrowers' propensity to list a loan on the platform, which corresponds to an outward shift in demand as depicted in the lower left panel of figure 1. In case the interest rate, r, set by the platform is unchanged, the excess demand will be higher, $\chi' > \chi$, and the loan origination volume lower, $q' < q$. A platform expecting the change in market conditions to persist will increase the rate to $r > r$ to balance the market at its excess demand target level, as depicted in the lower left panel. This also increases the loan origination volume to $q^* > q'$.

Credit Risk Channel. In isolation, a reduction in perceived credit risk increases the attractiveness of the online lending market for investors. Consequently, we would expect an increase in the propensity of investors to supply funds to online lending. This is depicted as an outward shift in supply in the lower right panel of figure 1. Everything else equal, the excess demand is reduced below its target. Arguably, this is even more so in a case in which the reduction in perceived credit risk improves outside options of loan applicants, causing an inward shift in the demand schedule. A platform expecting the change in market conditions to persist will decrease the interest rate to $r^* < r$ in order to balance the market at its excess demand target level, thereby increasing the origination volume to $q^* > q$.

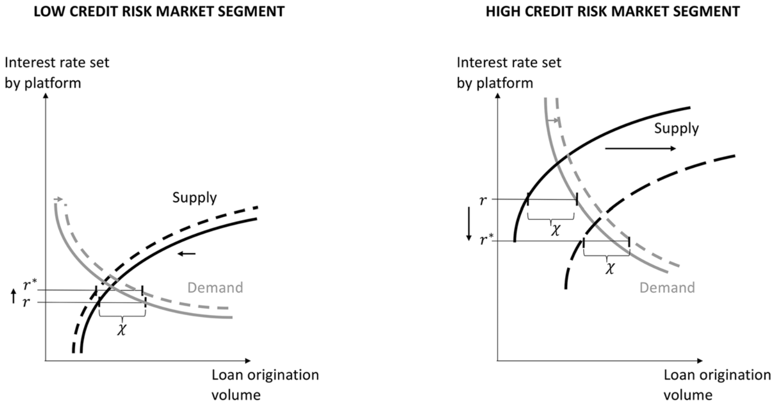
Liftoff Signaling Channel. We next discuss the combined effect of the risk-free rate channel and the credit risk channel. This is because monetary contractions can also affect credit risk, the key determinant of the risk premium in the P2P segment for consumer credit. Regarding the credit risk channel, there can be two opposing effects. First, the empirical literature finds that surprise monetary contractions are associated with an increase in credit spreads (e.g., Gertler and Karadi 2015). Second, credit spreads are known to be countercyclical and are regarded as a leading indicator for economic activity (e.g., Gilchrist and Zakrajsek 2012).[13] As a result, a monetary contraction that ushers in monetary normalization may be associated with a reduction in credit spreads if the decision sends a strong positive signal about the state of the economy. This is true even more so if the normalization is conditioned on an improvement in the economic outlook.

More specifically, taking a significant step towards monetary normalization, such as the Fed liftoff decision to move away from near-zero rates, constitutes a strong positive signal about the Fed's private assessment of future employment and growth prospects.[14] This interpretation is supported by empirical studies that demonstrate the Fed's good nowcasting performance (Faust and Wright 2009) and suggest that the disclosure of information by central banks plays an important role in coordinating market expectations and

[13]This countercyclical nature of credit spreads has been rationalized most prominently in the financial accelerator proposed by Bernanke and Gertler (1989).

[14]Following the end of quantitative easing in October 2014, liftoff can be regarded as the first step towards monetary normalization, with the reduction of the Fed's balance sheet being the second step (FOMC 2014).

Figure 2. P2P Consumer Credit Market Specific Supply and Demand



Note: Market specific supply and demand for the low credit risk segment (left panel) and for the high credit risk segment (right panel).

provides relevant macroeconomic information to markets (Swanson 2006; Ehrmann and Fratzscher 2007; Campbell et al. 2012; Boyarchenko, Haddad, and Plosser 2016; Ehrmann, Eijffinger, and Fratzscher 2016).

For uncollateralized consumer credit, the assessment of future employment prospects is an important determinant of perceived credit risk. Moreover, the default risk of high credit risk borrowers is arguably most sensitive to changes in the economic outlook. Hence, we would expect a strong credit risk channel associated with the positive signal of a monetary normalization, which outweighs the risk-free rate channel, to crystallize in a reduction of the spread between high and low credit risk borrowers. We provide a formalization in online appendix B and the outcome is illustrated in figure 2 (see http://www.ijcb.org for online appendix).

The left panel of figure 2 shows in a stylized way the low credit risk market segment where the credit risk channel is weak. Due to the small increase of the risk-free reference rate during liftoff, we expect a rather small inward shift in supply. At the same time, there may be a small outward shift in demand due to the deteriorating outside options of loan applicants. Taken together, the two effects both tend to increase the excess demand and, thereby, warrant a small interest rate increase by the platform for the lowest credit risk segment that appears to be approximately the same size as the risk-free rate increase. Conversely, the credit risk channel is considerably stronger in the high credit risk segment. Here, the supply shift outward is much larger, as depicted in the right panel of figure 2. This warrants a substantial interest rate reduction for the borrowers with the low credit ratings to balance the market and achieve the platform's objective. In sum, we expect a strong credit risk channel associated with the positive signal of a monetary normalization to show as a reduction of the spread between high and low credit risk borrowers. Prediction 1 summarizes the liftoff channel, which is consistent with our empirical work.[15]

PREDICTION 1. *If we observe that liftoff is associated with a reduction in the average funding costs of P2P borrowers, then the spread between high and low credit risk borrowers should decline.*

Given the importance of investor propensity to supply of funds, the availability of high-frequency flow-of-funds information allows us to further discriminate between supply and demand effects. An observed reduction in interest rates on Prosper loans may be driven by supply or demand factors. First, we would expect a reduction in perceived default probabilities on P2P loans to be associated with higher loan attractiveness, leading to an increase in the supply of funds, as measured by a decrease in the funding gap (the aggregate amount that has been demanded, but not yet supplied), and an increase in the funding speed and the funding success. As Prosper learns about such a lasting change in market conditions, it reduces the interest rates on individual loans to attract more borrowers and, therefore, match the supply increase. Second, an observed reduction in interest rates on Prosper loans is also consistent with a lasting reduction in demand, where Prosper responds to a demand reduction

[15]The conditional statement in prediction 1 describes a necessary and sufficient condition under the plausible assumption that the risk-free rate channel dominates the credit risk channel for borrowers in the lowest credit risk categories. To see this, recall that a perceived reduction of credit risk has a stronger effect for the high credit risk market segment (recall figure 2 and online appendix B). As a result, the average funding cost of P2P borrowers can only decline if there is a sufficiently high reduction of credit risk for high credit risk borrowers that outweighs the risk-free rate channel, which crystallizes in the reduction of the spread.

by reducing rates. Prediction 2 follows and our empirical analysis validates the liftoff signaling channel described previously.

PREDICTION 2. *(a) If we observe that liftoff is associated with a reduction in the funding costs of P2P borrowers, but not with a reduction in demand, then we should see a decrease in the funding gap, and an increase of the funding speed and success probability. (b) If we see a reduction in the spread between high and low credit risk borrowers, then the change in supply should be largest for high credit risk borrowers.*

3. Data and Descriptive Statistics

Our primary data set comprises loan-hour observations from the Prosper P2P lending platform.[16] We collected hourly observations of loan funding progress and loan-borrower characteristics from Prosper's website between November 20, 2015 and January 20, 2016 using web scraping.[17] In total, our sample covers 326,044 loan-hour observations.[18] Among the 4,257 loan listings in the data set, 3,015 loans can be identified as having successfully originated using the 70 percent funding rule.[19] Loan listings occur continuously around

[16]To provide external validity, we use data from LendingClub.com, another P2P lending platform. This secondary data set comprises loan-level origination data from the U.S. P2P lending platform LendingClub.com starting from December 2014, which we obtained from the public SEC records. The LendingClub.com and Prosper.com platforms both specialize in uncollateralized consumer credit and target a very similar slice of the market. As a result, the descriptive statistics for our secondary data set are similar, with an average loan size of $15,775.86, an average interest rate of 12.92 percent, and an average debt-to-income (DTI) ratio of 19.85 percent.

[17]We use scraping to obtain hourly microdata about loans posted on Prosper.com. Specifically, we collected all information posted publicly about Prosper loans—including their funding and verification statuses—using custom Bash and Python scripts.

[18]Our sample starts from November 20, 2015 and is updated hourly until the current date. Initially, we used a sample of 640,000 loan-hour observations, which overlaps with two FOMC meetings: December 15–16, 2015 and January 27–28, 2016. We decided to drop the data after January 20, 2016—about one week before the January meeting—to avoid picking up interest rate changes related to the January FOMC meeting. The complete sample of 640,000 loan-hour observations is, however, used for a placebo test.

[19]Recall that, according to the Prosper documentation, a loan is originated when reaching a funding status of at least 70 percent. However, the funding

the clock. The loan terms are fixed by Prosper and posted online once the funding phase starts. The verification status of a loan does occasionally improve as more documents are verified by Prosper.

The data set contains loan information, such as size, purpose, interest rate, maturity, and monthly payment; and borrower information, including employment status, income bracket, debt-to-income ratio, and a credit score issued by Prosper. Panel A of table 1 gives summary statistics for the full sample of borrowers with loans posted. The loan size varies from \$2,000 to \$35,000, but has an (unweighted) sample average of \$13,100. The majority of loans have a three-year maturity. Loan purpose categories include business, consumption (e.g., auto, boat, vacation, etc.), debt consolidation, special loans (e.g., baby and adoption, medical, etc.), and others. More than 75 percent of the listings are in the debt consolidation category. The average interest rate, without taking into account the loan-borrower characteristics, is 14.22 percent. Figure 3 shows two histogram plots of the interest rates, divided into pre- and post-liftoff subsamples. After liftoff, the interest rate distribution appears more skewed to the left. This is consistent with the direct observation from descriptive statistics that the average interest rate drops from 14.29 percent to 14.15 percent after liftoff.

Prosper provides rich information about borrowers on its website, including a credit rating that is mostly based on the borrower's Fair Isaac Corporation (FICO) score and credit history. Prosper assigns one of seven credit ratings to each borrower: AA, A, B, C, D, E, and HR, which are monotonically increasing in the perceived credit risk.[20] For our analysis, we later group credit ratings into three bins: high ratings (AA and A), middle ratings (B and C), and low ratings (lower than C). This classification helps us to divide the borrowers into three groups of similar sizes. The employment status is another important variable in assessing the borrower's default

phase continues if the funding status reaches the 70 percent level before the end of the listing period.

[20]While it was possible to translate Prosper's credit ratings from the FICO scores (Butler, Cornaggia, and Gurun 2017), we expect that Prosper now uses additional information to assign credit ratings, such as behavioral user data, the user's history on the platform, and social media data.

Table 1. Descriptive Statistics

Panel A: Full Sample

	Mean	SD	Min.	Max.	Obs.		Obs.	Pct.		Obs.	Pct.
Size	13.10	7.13	2.00	35.00	4,257	Business	93	2.18	\$1–24,999	175	4.11
Int. Rate	14.22	6.46	4.32	30.25	4,257	Cons.	415	9.75	\$25,000–49,999	1,682	39.51
DTI	27.32	12.33	1	68	4,257	Debt	3,222	75.69	\$50,000–74,999	1,213	28.49
Maturity	3.77	0.97	3	5	4,257	Other	344	8.08	\$75,000–99,999	601	14.12
Verif.	2.30	0.76	1	3	4,257	Special	183	4.30	\$100,000+	586	13.77
Δfunding	0.95	3.91	0	99	322,600	Total	4,257	100	Total	4,257	100

Panel B1: Sample before the Liftoff

	Mean	SD	Min.	Max.	Obs.
Size	13.05	7.25	2.00	35.00	2,029
Int. Rate	14.29	6.46	4.32	30.25	2,029
DTI	27.10	12.24	1	63	2,029
Maturity	3.85	0.99	3	5	2,029
Verif.	2.30	0.76	1	3	2,029

Panel B2: Sample after the Liftoff

	Mean	SD	Min.	Max.	Obs.
Size	13.14	7.01	2.00	35.00	2,228
Int. Rate	14.15	6.46	4.32	30.25	2,228
DTI	27.52	12.41	1	68	2,228
Maturity	3.69	0.95	3	5	2,228
Verif.	2.30	0.76	1	3	2,228

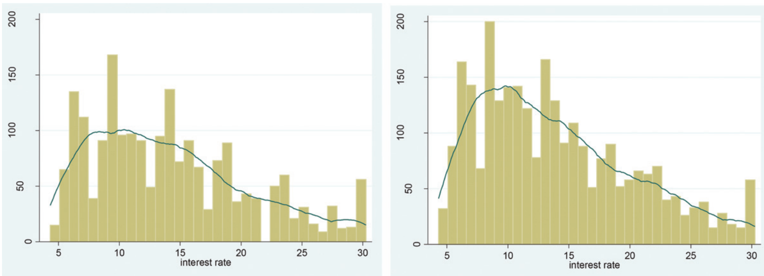
(*continued*)

Table 1. (Continued)

Panel C1: ES = Employed						Panel D1: CR = High					
	Mean	**SD**	**Min.**	**Max.**	**Obs.**		**Mean**	**SD**	**Min.**	**Max.**	**Obs.**
Size	13.80	7.43	2.00	35.00	3,166	Size	13.28	6.44	2.00	35.00	1,198
Int. Rate	13.66	6.35	4.32	30.25	3,166	Int. Rate	7.28	1.37	4.32	9.43	1,198
DTI	27.35	12.05	1	68	3,166	DTI	24.84	10.21	1	62	1,198
Maturity	3.77	0.97	3	5	3,166	Maturity	3.80	0.98	3	5	1,198
CreditBin	0.95	0.76	0	2	3,166						
Panel C2: ES = Self-Employed						**Panel D2: CR = Middle**					
	Mean	**SD**	**Min.**	**Max.**	**Obs.**		**Mean**	**SD**	**Min.**	**Max.**	**Obs.**
Size	10.59	3.66	2.00	15.00	520	Size	14.38	7.84	2.00	35.00	1,825
Int. Rate	17.42	6.40	5.76	30.25	520	Int. Rate	13.06	2.21	9.49	16.97	1,825
DTI	23.60	12.12	1	63	520	DTI	27.87	12.52	1	66	1,825
Maturity	3.74	0.97	3	5	520	Maturity	3.79	0.98	3	5	1,825
CreditBin	1.34	0.66	0	2	520						
Panel C3: ES = Unemployed						**Panel D3: CR = Low**					
	Mean	**SD**	**Min.**	**Max.**	**Obs.**		**Mean**	**SD**	**Min.**	**Max.**	**Obs.**
Size	11.49	7.07	2.00	35.00	571	Size	11.02	6.11	2.00	30.00	1,234
Int. Rate	14.37	6.27	4.32	30.25	571	Int. Rate	22.65	3.90	17.61	30.25	1,234
DTI	30.54	13.12	1	63	571	DTI	28.90	13.53	2	68	1,234
Maturity	3.75	0.97	3	5	571	Maturity	3.69	0.95	3	5	1,234
CreditBin	1.04	0.73	0	2	571						

Notes: The sample includes all loan listings on Prosper.com over the period between November 20, 2015 and January 20, 2016. The loan size is measured in thousands of dollars. The interest rates are quoted in percentage points. DTI is the monthly debt-service-to-income cost. ES is the employment status. CR is short for the borrower credit rating. CreditBin takes on the value 0 if CR = Low, 1 if CR = Middle, and 2 if CR = High. Verif. denotes the verification stage. It takes on a discrete value from 1 to 3, where 3 indicates that most of the documents have been verified by Prosper. Δfunding is the hourly percentage change in the funding status. Cons. Denotes the purpose consumption.

Figure 3. Histogram of Loan Interest Rates



Note: Histogram of interest rates for loans in our observed period, before (left panel) and after (right panel) Fed liftoff on December 16, 2015.

risk, which contains three categories: employed, self-employed, and unemployed.[21]

We track all observed loans with an hourly frequency by scraping Prosper's website to update the sample. The major advantage of an hourly data set is that we see funding status changes over time. This provides an up-to-date snapshot of the P2P lending market, which is potentially reacting to the monetary policy announcement. Furthermore, this data set enables us to construct an hourly measure of fund inflows to different loans and determine the size of aggregate demand at any hour in our sample. The loan-hour observations are used to calculate the funding gap, defined as the gap between cumulative inflow of funds and the loan amount target, for each listing, borrower group, and the whole market. The funding gap is an essential variable for understanding Prosper's interest rate setting problem and interest rate dynamics as discussed in section 2.3.

4. Results

Section 4.1 presents our main findings on interest rates and spreads for the P2P lending market after Fed liftoff. These results speak to prediction 1. Section 4.2 suggests a mechanism for the interest rate

[21] A few employed borrowers indicate their employment status as "full-time." The last category is reported as "other" in Prosper, but we interpret it as unemployed.

and spread results by exploring measures of supply, demand, and the funding gap in the P2P market. The analysis of supply and demand speaks to prediction 2. Finally, section 4.3 provides external validity and corroborates the employment outlook as a channel driving the investor-perceived reduction in default probabilities after liftoff.

4.1 Interest Rates and the Credit Spread

We analyze interest rates of loans listed within ±3-day, ±7-day, and ±14-day windows around December 16, 2015, the date of Fed liftoff. Our longest window—hereafter, "LONG" —spans the entirety of our main sample for Prosper, which runs from November 20, 2015 to January 20, 2016. Note that this window starts with the first day of data collection and ends one week prior to the first 2016 FOMC meeting.

The baseline model regresses the interest rate of loans posted around the Fed's liftoff decision and a large number of observed loan-borrower characteristics. Table 2 summarizes the results for our sample with various window sizes. We use the following specification:

$$\begin{aligned}\text{InterestRate}_{i,t} = \alpha + \alpha_h + \alpha_d + \beta_1 \text{Liftoff}_t + \gamma_1 \text{LoanCharacteristics}_i \\ + \gamma_2 \text{BorrowerCharacteristics}_i + \epsilon_{i,t},\end{aligned} \tag{1}$$

where α captures the constant term, while α_h and α_d control for hour-of-day and day-of week effects, respectively.[22] The inclusion of loan-borrower controls and fixed effects ensures we compare interest rates of loans with similar characteristics before and after liftoff. Liftoff_t is an indicator that takes on a value of 1 if the loan i is posted at a time t, which is after the Fed liftoff announcement. The estimated value of β_1 is between -0.169 and -0.229 and is highly significant at multiple time windows. Hence, the average interest rate for loans drops by 16.9–22.9 bps post-liftoff, after controlling for all loan and borrower characteristics. When narrowing the event

[22]Platforms tend to post loans in groups throughout the day. Additionally, investor visits to the platforms are likely to be clustered around certain hours of the day and certain days of the week. Controlling for hour-of-day and day-of-week effects captures recurring variation in borrower and lender density on the platform. Since such changes are predictable, it is possible that the platforms could adjust interest rates accordingly. We do not, however, find large effects from the inclusion of such fixed effects.

Table 2. Baseline Regressions

	Dependent Variable: Interest Rate			
	(1)	**(2)**	**(3)**	**(4)**
Explanatory Variables				
Liftoff	-0.195^{*}	-0.229^{***}	-0.173^{***}	-0.169^{***}
	(-1.74)	(-3.10)	(-3.17)	(-4.36)
Additional Controls	√	√	√	√
Controls				
Loan Characteristics	√	√	√	√
Borrower Characteristics	√	√	√	√
Main Effects				
Weekday FE		√	√	√
Hour FE	√	√	√	√
Window Size	±3d	±7d	±14d	LONG
Adj. R^2	0.971	0.972	0.972	0.970
Observations	445	987	1,818	4,257

Notes: The dependent variable is the interest rate, in percentage points, posted on Prosper. The variable Lifoff_t is a dummy that equals 1 after the liftoff announcement on December 16, 2015. The borrower characteristics controls include debt-to-income ratio, income group, prosper credit rating, and employment status. The loan characteristics include the loan size, maturity, purpose, and verification stage. We also include weekday fixed effects, hour-of-the-day fixed effects, and additional covariates, such as cross-products of loan-borrower characteristics and the liftoff dummy, to validate the robustness of our findings. We run the regression for different window sizes (±3-day, ±7-day, ±14-day, LONG), including in the main sample over the period November 20, 2015 to January 20, 2016. We drop the weekday dummies in the ±3-day regression because of multicollinearity. t statistics are shown in parentheses. The results are robust to standard error clustering at time or borrower location. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

window to ±3 days around liftoff, we still observe a drop in average interest rates of a similar magnitude, as shown in column 1.[23]

Selection effects are an important concern. Unlike Jiménez et al. (2012), we cannot use lender-borrower fixed effects, since we cannot observe and track the identity of individual investors on the platform. For the same reason, we cannot employ time-lender fixed effects. Moreover, we are also unable to employ time-borrower fixed effects, since individual borrowers are not applying for multiple

[23]We have to drop weekday fixed effects in the ±3 days regression, due to the multicollinearity between the weekday dummies and the liftoff variable.

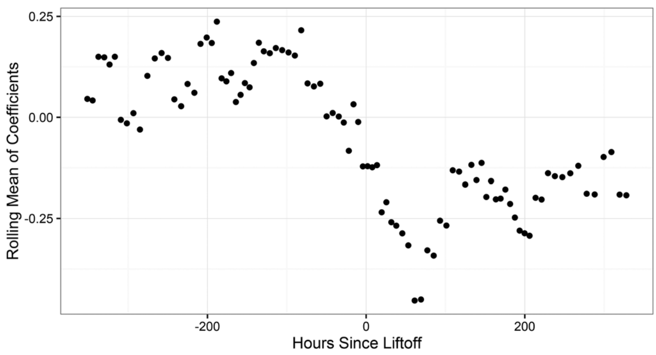
loans. To rule out the possibility that the regression results are mainly driven by the econometric model's (mis-)specification, we run two additional estimations to check the validity of the interest rate reduction result. The first robustness check expands the baseline regression by including the cross products of various loan-borrower characteristics (DTI, maturity, verification, etc.) and the liftoff dummy as regressors. The interest rate reduction survives this test, as documented in table A.11 of the online appendix. In the second robustness check, we regress the interest rate on all combinations of loan-borrower characteristics and the liftoff dummy. After obtaining the coefficients on liftoff, we run a sample mean test of the coefficient differences for the groups sharing similar loan-borrower characteristics before and after liftoff. The t-statistics suggest that the interest rate is lower after liftoff and the difference is significantly negative. The estimation results are available in table A.3 of the online appendix. We conclude that changes in borrower composition or substitution into shorter maturity loans are not driving our main results.

Both visual inspection and placebo tests suggest that the change in P2P lending rates happened precisely at liftoff.[24] In figure 4, we first recover the residuals from a regression of the interest rate on all loan-borrower information. We then compute the mean of the residuals for all loans posted in the same hour and plot the three-cohort rolling mean over time. We observe a clear drop in the average level of interest rates after the liftoff, controlling for all observable loan-borrower characteristics.[25]

[24]Notably, there is both a small increase in the rate prior to liftoff and a small decrease after that, but prior to liftoff. It is possible that these small movements could have been generated by other announcements or movements in the expected probability of liftoff, which we address in a simulation exercise.

[25]While Prosper and LendingClub occasionally announce rate changes, this communication is primarily directed at investors and is voluntary for Prosper. Additionally, these announcements may be accompanied by reallocations of borrowers across internal credit rating bins. For this reason, the meaning of interest rate change announcements is unclear. LendingClub, for instance, announced a rate increase in late December, while Prosper made no such announcement. In the data, however, the net effect of all changes appears to be a decline in average rates and spreads for borrowers with similar characteristics on both platforms. We also observe unannounced shifts in rates associated with credit bins in the data, which reinforces this point.

Figure 4. Time Trend in the Interest Rate After Controlling for Loan and Borrower Composition

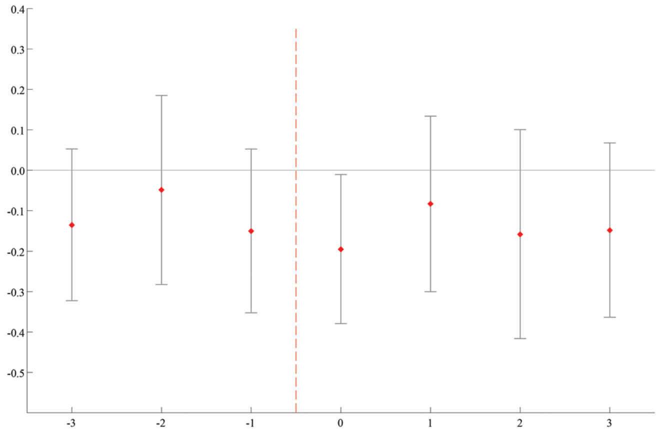


Notes: We recover the trend by performing a regression of the interest rate on all loan-borrower controls and computing the means of the residuals for all loans posted in the same hour. Finally, we plot the three-cohort rolling mean of the cohort-specific means over a ± 14-day window around liftoff.

In a separate exercise, we run a placebo test that conducts a rolling regression of the interest rate with loan-borrower characteristic controls and the narrowest window of ± 3 days. Within the window, we define a pseudo-liftoff variable $D(\tau)_t$ to replace Liftoff_t from equation (1). The variable $D(\tau)_t$ is a dummy whenever t is in the second half of the time window, where $\tau = -3, \cdots, 3$ refers to the number of days since the liftoff date. Figure 5 illustrates that only the time dummy coinciding with the liftoff dummy is significantly different from zero. This suggests that our results are unlikely to be driven by pre-existing trends or other news events unrelated to liftoff.

The estimated coefficients in regression (1) also confirm the presence of the usual channels for default risk in Prosper data. The coefficients on credit risk and unemployment, reflected in Prosper credit scores, are positive, indicating that the interest rate is higher for borrowers with higher perceived credit risk. Detailed estimation results are provided in table A.4 of the online appendix. Since our panel data contain loan listings with various characteristics, we estimate the model on data in different categories that are defined using the borrower's employment status and credit score. The equation we estimate is still the baseline regression, but we divide the data into

Figure 5. Pseudo-Liftoff Rolling Regression



Note: Point estimates and 90 percent confidence interval of the pseudo-liftoff coefficient estimates from a rolling regression of the interest rate with loan characteristics controls over a ±3-day window.

subsample categories. We find a statistically significant interest rate reduction of approximately 40 bps for borrowers with lower Prosper credit ratings (lower than A). The interest rate reduction is significant for both employed and unemployed borrowers, but the drop is 6 bps larger for unemployed borrowers.

To further establish robustness, we also expand the sample to include observations until February 26, 2016, a few days before the March FOMC meeting. We run a regression to measure the impact of the January 27, 2016 FOMC decision to keep the federal funds rate range at 0–25 bps on Prosper loan interest rates. The results are reported in table A.5 of the online appendix. We find that the January announcement did not have a statistically significant impact on the P2P lending rate. This suggests that the reduction in interest rates at liftoff cannot plausibly be attributed to a placebo effect, since no such effect is present at the January 27 meeting, where there was neither strong Fed signaling nor an unexpected adjustment in interest rates. In a further expansion of the sample to the end of March 2017, we extend the baseline interest rate regression to include two more FOMC decisions to increase the policy rate.[26] After

[26]These decisions are announced on December 14, 2016 and March 15, 2017.

identifying the press conference time in the scraped data, we reestimate the regression to evaluate the average interest rate changes in the platform around these rate hikes. Table A.6 of the online appendix shows that these two policy rate hikes did not lead to significant interest rate changes on the Prosper platform in short time windows. In the longest time window we consider, the later policy rate increase event generates a rate increase on the Prosper platform. This confirms that the strong reduction in perceived credit risk in the uncollateralized consumer credit market was unique to liftoff, which supports the important role played by the signaling channel at liftoff.

Although Fed liftoff was partially anticipated by the market (see section 2.1), the difference in the pre-announcement trend for different segments of the P2P lending market was negligible, especially close to the FOMC's policy meeting. We next narrow in on a window of ± 7 days around the announcement date to pin down the effect on the credit spread between less risky and risky borrowers. We divide the loan listing observations into three groups: employed borrowers with high credit ratings (AA and A), unemployed borrowers with middle or low credit ratings (not AA or A), and others. We focus on the first two groups in the regression, using the unemployed and lower credit rating borrower groups as the benchmark to control for any shared trend before the liftoff decision. The sample size is reduced to 355 loan listings, of which one-third are from unemployed borrowers with a low credit rating.

$$\begin{aligned} \text{InterestRate}_{i,t} = {} & \alpha + \alpha_h + \alpha_d + \beta_0 1\{EMP, High\}_i \\ & + \beta_1 \text{Liftoff}_t + \beta_2 1\{EMP, High\}_i \times \text{Liftoff}_t \\ & + \gamma_1 \text{LoanCharacteristics}_i \\ & + \gamma_2 \text{BorrowerCharacteristics}_i + \epsilon_{i,t}. \end{aligned} \tag{2}$$

Table 3 reports the estimation results with different controls. Columns 1–4 show results with all possible controls at the loan level, three dummies corresponding to before-after group differences, and the cross-product of group and liftoff time periods. It appears that the interest rate spread before liftoff between the two borrower groups is around 960 bps, and the gap is reduced by 166 bps after liftoff. This indicates that the spread between the high credit risk

Table 3. Before/After Regressions on the Interest Rates for Different Groups

	Dependent Variable: Interest Rate			
	(1)	**(2)**	**(3)**	**(4)**
Explanatory Variables				
Liftoff	−1.810***	−1.884***	−1.891***	−1.934***
	(−2.81)	(−2.92)	(−2.87)	(−2.94)
$1\{EMP,High\}$	−10.360***	−10.376***	−9.605***	−9.629***
	(−21.52)	(−21.37)	(−17.61)	(−17.55)
$1\{EMP,High\} \times$ Liftoff	1.536**	1.654**	1.601**	1.658**
	(2.01)	(2.16)	(2.08)	(2.15)
Controls				
Loan Characteristics			√	√
Borrower Characteristics			√	√
Main Effects				
Weekday FE		√		√
Hour FE		√		√
Window Size	±7d	±7d	±7d	±7d
Pre-liftoff, Int. Rate Mean $1\{EMP,High\} = 0$	17.805	16.085	19.974	19.315
F-test (Liftoff, $1\{EMP,High\} \times$ Liftoff)	4.165	4.402	4.312	4.484
Adj. R^2	0.663	0.668	0.671	0.675
Observations	355	355	355	355

Notes: We focus on ±7-day windows centered around the liftoff date. The interest rate is regressed on the liftoff dummy, borrower riskiness (Employment and Credit Rating), and their interaction terms. Additional controls include loan characteristics, borrower characteristics, and time dummies. The empirical specification treats the borrower with high credit ratings and employment as the focus, and benchmarks their interest rate variation with unemployed borrowers who receive a low credit rating from Prosper. t statistics are shown in parentheses. We report the F-test statistics for the joint significance of "Liftoff" and "$1\{EMP,High\} \times$ Liftoff." Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

borrowers with the lower credit rating and the good borrowers drops by around 17 percent on average, after controlling for all observable loan-borrower characteristics and possible time trends. Our findings on the spread are also robust to the window size, but have lower significance when a window shorter than ± 7 days is used. Our findings are also robust to the choice of econometric specification and standard error clustering. Moreover, as we demonstrate in table A.7 of the online appendix, they also survive the inclusion of the variance risk premium (Bollerslev, Tauchen, and Zhou 2009) as a control for shifts in risk appetite over time.[27]

In a final robustness exercise, we perform a simulation to determine whether other macro news events surrounding liftoff could have plausibly explained the reduction in rates at liftoff. Since the inclusion of time dummies does not allow us to control for macroeconomic and financial events in the window around liftoff, we construct a non-overlapping sample that spans the period between January 2016 and December 2017. We use the loans in this sample to compute the average daily interest rate and then take the first difference. We then regress the first difference in the average rate on the forecast errors for all of the indicators that (i) had announcements in the liftoff window; and (ii) had a sufficient number of observations in the extended sample. This includes the surprise series for jobless claims, retail sales, core inflation, housing starts, the Federal Reserve Bank of Chicago national activity index, personal income, and the Federal Reserve Bank of Philadelphia manufacturing index. Cumulating the surprises over a seven-day window around liftoff, we find a change of –0.9 bps, which is considerably smaller in magnitude than the –22.9 bps we measure at liftoff. We conclude from this that it is unlikely that news announcements surrounding liftoff could credibly explain the observed shift in online lending rates.

To conclude, we find robust evidence that the Fed liftoff announcement was associated with a sharp drop in the average interest rate of around 16.9–22.9 bps. Moreover, the spread between high and low credit risk groups experienced a relatively large drop of around 17 percent after liftoff. The decrease in the average interest rate is economically significant, and the magnitude of the observed

[27]See the online appendix for more details about the variance risk premium's construction.

166 bps reduction in the spread between high and low credit risk borrows after liftoff compares to approximately one-third of the effect of moving up from Prosper rating category D to C or an improvement in FICO score from 679 to 690. Our empirical findings confirm prediction 1, which suggests that the spread between high- and low-risk borrowers should decrease if the risk-free rate channel is outweighed by the credit risk channel, as suggested by the reduction in P2P lending rates after liftoff. While it is perhaps counterintuitive at first glance that the increase of the risk-free reference rate is associated with a reduction in interest rates, especially for borrowers with low credit ratings and no stable labor income, we will argue in the remainder of the paper that a reduction in perceived default probabilities, induced by positive Fed signaling, is the most plausible explanation for these findings. That is, the positive liftoff signaling dominates the credit risk channel, especially for riskier market segments.

We proceed by linking our main results to supply-side factors in section 4.2. Thereafter, section 4.3 provides evidence for external validity and discusses the employment outlook as a key driver of perceived default risk.

4.2 Supply and Demand Analysis

In addition to our main data set, we also obtained hourly updates of loan funding progress for each listing. The granular data allows us to construct measures of supply that can be used to gain a better understanding of the channels described in section 2.3 by testing predictions 2a and 2b. The loan funding progress is of key interest in this section and we use a loan-level indicator variable for loans being funded. Moreover, the additional measures of funding increase and funding speed are at the funding increment level, which is even more granular. To isolate the liftoff channel, we examine how liftoff affects the funding gap and find that it drops significantly. We also show that the funding gap reduction appears to be driven by an increase in supply, rather than a demand reduction. Our supply measures—funding speed and funding success—both increase, especially for high credit risk borrowers, validating predictions 2a and 2b. Taken together, the results support the mechanism for the post-liftoff reduction in average interest rates, discussed in section 4.1.

The funding gap, defined as the size of the unfunded portion of the loan at each time t for loan listing i, provides a natural metric for the P2P platform when choosing individual interest rates to maximize the origination volume. We can aggregate the funding gap for the whole sample and also for different categories (e.g., according to credit ratings and/or employment status). This allows us to distinguish between different market segments.

Demand and supply in the lending market are endogenous to the interest rate decision in equilibrium, making it difficult to identify the driving forces behind observed interest rate changes after liftoff. However, the funding gap, which is defined as

$$\begin{aligned} \text{FundingGap} = \ & \text{RequestedLoanAmount} \\ & - \text{FundedLoanAmount}, \end{aligned} \tag{3}$$

is a key variable in the P2P platform's profit maximization problem. Specifically, the platform maximizes the origination volume by assuring that the funding gap remains narrow, especially after lasting changes in supply and demand conditions.

The first two columns in table 4 show the corresponding regressions for the effect of liftoff on the funding gap measure. We first study the impact of liftoff on the aggregate funding gap over time with the following regression:

$$\begin{aligned} \text{FundingGap}_t = \ & \alpha + \alpha_h + \alpha_d + \beta_1 \text{Liftoff}_t \\ & + \gamma \text{LoanBorrowerCharacteristics}_t + \epsilon_t. \end{aligned} \tag{4}$$

Columns 1 and 2 in table 4 present results for the aggregate funding gap over time. Consistent with prediction 2a, we find that it is reduced after liftoff, dropping significantly by around \$400,000. This result is robust to inclusion of intraday and intraweek fixed effects, as well as average loan and borrower characteristics, including the size of the loan itself. Speaking to prediction 2b, we explore the funding gap in different market segments classified by credit riskiness, we run the regression of the funding gap in market segment j:

$$\begin{aligned} \text{FundingGap}_{j,t} = \ & \alpha + \alpha_h + \alpha_d + \beta_0 1\{EMP, High\}_j + \beta_1 \text{Liftoff}_t \\ & + \beta_2 1\{EMP, High\}_j \times \text{Liftoff}_t + \epsilon_{j,t}. \end{aligned} \tag{5}$$

Table 4. Before/After Regressions for the Aggregate Funding Gaps and Demand

Dependent Variable	(1) Funding Gap	(2) Funding Gap	(3) Demand	(4) Demand
Explanatory Variables				
Liftoff	−0.474***	−0.383***	0.031***	0.017**
	(−23.12)	(−10.84)	(5.81)	(2.23)
Controls				
Loan Characteristics		√		√
Borrower Characteristics		√		√
Main Effects				
Weekday FE		√		√
Hour FE		√		√
Window Size	LONG	LONG	LONG	LONG
Adj. R^2	0.113	0.555	0.023	0.397
Observations	1,403	1,403	1,403	1,403

Notes: We focus on the LONG window size, using the main sample over the period November 20, 2015 to January 20, 2016. We regress funding gaps and demand (in millions of USD) on liftoff, and intraday and intraweek dummies. We include all borrower types in the aggregation. Additional controls include sample average loan characteristics and average borrower characteristics. t statistics are shown in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 shows the results. In columns 1 and 2 we use a ±7-day window, centered around the liftoff announcement, to study the dynamics of the funding gap in two distinct groups: employed borrowers with high credit ratings and unemployed borrowers with low credit ratings. We find that the funding gap is higher for employed borrowers with high credit ratings. Furthermore, it increases after the liftoff decision by $57,000 (summing up β_1 and β_2 in column 2). Taken together, this differential impact of the liftoff on the funding gap for different borrower groups also reinforces our second main finding in section 4.1 on the spread reduction between low and high credit rating borrowers. This is because a lasting reduction in the funding gap for low credit rating borrowers is associated with downward pressure on the interest rates of these borrowers.

We next test whether the funding gap reduction was driven by an increase in supply or a decrease in demand. We investigate aggregate new demand in different market segments of the P2P lending

Table 5. Before/After Regressions for the Funding Gaps and Demand of Different Groups

Dependent Variable	(1) Funding Gap	(2) Funding Gap	(3) Demand	(4) Demand
Explanatory Variables				
Liftoff	-0.047^{***}	-0.044^{***}	0.005^{*}	0.006^{**}
	(−7.99)	(−9.81)	(1.70)	(2.01)
$1\{EMP, High\}$	0.181^{***}	0.181^{***}	0.031^{***}	0.031^{***}
	(31.09)	(41.40)	(10.36)	(11.77)
$1\{EMP, High\} \times$ Liftoff	0.101^{***}	0.101^{***}	0.030^{***}	0.030^{***}
	(12.03)	(16.03)	(6.87)	(7.77)
Controls				
Main Effects				
Weekday FE		√		√
Hour FE		√		√
Window Size	±7d	±7d	±7d	±7d
Pre-liftoff, $\{UnEMP, Low\}$	0.232	0.184	0.028	0.007
F-test	72.683	130.616	14.312	18.484
Adj. R^2	0.828	0.903	0.463	0.583
Observations	650	650	650	650

Notes: We focus on the ±7-day windows centered around the liftoff date to study the aggregate funding gap and demand in different market segments. This table shows regressions of funding gaps and demand (in millions of USD) on liftoff, borrower-loan characteristics (Employment and Credit Rating), and intraday and intraweek dummies. The two borrower categories are defined as borrowers with high credit ratings and employment, versus unemployed borrowers with low credit ratings from Prosper. We report the F-test statistics for the joint significance of "Liftoff" and "$1\{EMP, High\} \times$ Liftoff." t statistics are shown in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

platform. A decrease in demand would suggest that the mechanism behind the reduction in the funding gap and reduction in interest rates is not identified. To the contrary, we find that demand increases slightly after liftoff, reinforcing our supply-driven hypothesis. The following regression uses aggregate new demand as the dependent variable:

$$\begin{aligned}\text{Demand}_t = \alpha + \alpha_h + \alpha_d + \beta_1 \text{Liftoff}_t \\ + \gamma \text{LoanBorrowerCharacteristics}_t + \epsilon_t. \end{aligned} \quad (6)$$

Columns 3 and 4 in table 4 show that new demand increases after liftoff for all groups by \$17,000. This provides strong evidence that the interest rate reduction results are not driven by a collapse of demand in the market.

To capture the demand shift in market segment j, we also employ the following regression:

$$\begin{aligned}\text{Demand}_{j,t} = \alpha + \alpha_h + \alpha_d + \beta_0 1\{EMP, High\}_j + \beta_1 \text{Liftoff}_t \\ + \beta_2 1\{EMP, High\}_j \times \text{Liftoff}_t + \epsilon_{j,t}.\end{aligned} \tag{7}$$

Hour-of-day and day-of-week fixed effects are included as α_h and α_d. In columns 3 and 4 in table 5, we separate the market into high and low credit risk segments using a ±7-day window around liftoff. We find that the increase is stronger for borrowers with high creditworthiness, which is consistent with the interest rate changes and funding gap dynamics in these segments.

Finally, we construct three separate measures of loan funding supply. A post-liftoff increase in these variables supports the hypothesis that the average interest rate reduction was driven by an increase in supply. Furthermore, taken together with the reduction in the interest rate spread, it also supports the hypothesis that perceived default probabilities fell, leading to a stronger inflow of funds.

We first test the supply increase hypothesis using the realized probability that a loan listing is funded $Pr(1\{LoanFunded\} = 1)$ as a measure of supply. The logit regression for a loan posted at time t is

$$\begin{aligned}1\{LoanFunded\}_i = \alpha + \alpha_h + \alpha_d + \beta_1 \text{Liftoff}_t \\ + \ \gamma_1 \text{LoanCharacteristics}_i \\ + \ \gamma_2 \text{BorrowerCharacteristics}_i + \epsilon_{i,t}.\end{aligned} \tag{8}$$

We also use other measures of supply to study whether the funding gap changed, such as

$$\text{Funding Increase}_{i,t} = \Delta(\text{Funding Percentage})_{i,t} \tag{9}$$

for each loan posting at time t. A loan is more likely to be funded after liftoff (reaching at least 70 percent of the total funding target) if the increase is large. With this approach, we can exploit variation

Table 6. Before/After Regressions for the Funding Success Measures

Dependent Variable	(1) $\mathbf{1}\{LoanFunded\}$	(2) Funding Increase	(3) Funding Speed
Explanatory Variables			
Liftoff	0.238**	0.137***	0.028**
	(2.39)	(11.23)	(1.98)
Controls			
Loan Characteristics	√	√	√
Borrower Characteristics	√	√	√
Main Effects			
Weekday FE	√	√	√
Hour FE	√	√	√
Window Size	LONG	LONG	LONG
R^2	0.094	0.098	0.015
Observations	2,858	237,296	237,296

Notes: We focus on the LONG window size, using the main sample over the period November 20, 2015 to January 20, 2016 and the loan listings where we observe the whole funding process. Funding success is regressed on a liftoff dummy, loan-borrower characteristics (as in previous regressions), and intraday and intraweek dummies. The funding success variable is measured as the probability of getting funded, the funding increase, and the funding speed. t statistics are shown in parentheses. Results are from OLS regressions, except for a logit regression with the funding probability $1\{LoanFunded\}$. The variables Funding Increase and Funding Speed are in percentage (%). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in the loan-time observations. Similarly, we replace the dependent variable in equation (5) with the funding speed increase:

$$\text{Funding Speed}_{i,t} = \Delta(\text{Funding Increase})_{i,t}, \tag{10}$$

to calculate the speed of reaching the funding target. We select loans posted on the Prosper website from November 20, 2015 to January 5, 2016, such that we observe the whole funding process of the loan listings.

The estimation results are reported in table 6. In column 1, the logistic regression for funding probability yields a coefficient estimate of 0.24, which translates into an odds ratio of 1.27 or a 5.37 percent increase in the funding probability after liftoff. Moreover, this result is statistically significant. The second column shows that the funding increase is larger after liftoff by 0.14 percentage point.

The last regression, which uses funding speed as the dependent variable, indicates that liftoff increased the rate of funding progress by 0.03 percentage point over time.

Taken together, the results are in line with predictions 2a and 2b. Moreover, the supply results, coupled with the average interest rate and spread reductions, suggest that liftoff may have been associated with a reduction in the perceived probability of default. Section 4.3 demonstrates this further by showing that improvements in the expected future state of the economy, as measured by changes in the real yield curve, are associated with a reduction in interest rates in the P2P market. Finally, we discuss how unemployment at the state level affects the rates that borrowers receive, even when we control for employment status at the individual level, and link it to the credit risk channel.

4.3 External Validity

This paper emphasizes the role that Fed liftoff played as a strong, positive signal about future macroeconomic conditions. In the P2P segment of the online credit market, it translated into a lower perceived default probability and, thus, a lower interest rate. In this section, we provide evidence for the external validity of these findings over time and across markets. Moreover, we discuss the employment outlook as an explanation for the investor-perceived reduction in default probabilities after the signaling effect of liftoff.

First, we generalize the link between improvements in the expected economic outlook and our key findings on the interest rate and credit spread. If the improvement of future economic conditions affects the P2P lending rate, then changes in the slope of the real yield curve, a proxy for measuring future economic development used in the literature (Harvey 1988, Estrella and Hardouvelis 1991), should induce interest rate adjustments in the market we study. In table A.8 of the online appendix, we regress the interest rates observed in the Prosper market on the slope, defined as the difference between the five-year TIPS yield and the one-month real interest rate.[28] An increase in the real slope is usually associated

[28]The construction of the real interest rate and the data sources are explained in the online appendix.

with an improvement in fundamental economic conditions. We find that interest rates for high credit risk borrowers decrease by 2.03 percent for every percentage-point increase in the real slope variable $\text{Slope}_t^{(5)}$. We also see that the credit spread between borrowers with low credit rating and borrowers with high credit rating is reduced by 21.5 percent for every percentage-point increase in the real slope.

The effect of the real yield-curve slope on P2P lending rates is large and statistically significant. Replacing the 5-year real slope with the 10-year real slope yields does not change the direction and does not substantially change the magnitude. Furthermore, if we include the real slope as an explanatory variable, the impact of liftoff becomes less significant. This suggests that the information revealed by liftoff is similar to the information embodied by real yield-curve slope adjustments, which provides further support for the claim that liftoff was interpreted as a positive signal about future economic conditions.

Second, we validate our key findings by studying LendingClub, another major P2P lending platform in the United States. We obtain daily loan origination reports of LendingClub to the U.S. Securities and Exchange Commission for the same sample period from November 20, 2015 to January 20, 2016. The reports provide interest rates and loan-borrower information variables for all loan postings that have been successfully originated on the LendingClub platform. Unfortunately, the reports do not contain information about loans that have not been funded and cannot be used to construct intraday measures of demand and supply in the market. We explore the interest rate data for originated loans and report the regression results for the liftoff dummy and different interest dynamics for high- versus low-risk borrowers in table A.9 of the online appendix. We find that the average interest rate drops and the credit spread narrows after liftoff. This result confirms our findings from the Prosper data set and suggests that the monetary policy signaling associated with the Fed liftoff decision also affected other lending markets where many borrowers exhibit risky characteristics.

Finally, an additional result strengthens the hypothesis that liftoff reduced the perceived default probabilities of P2P borrowers. Borrowers in states with higher unemployment rates received higher interest rates, even after controlling for borrower and loan characteristics, including their own employment status. The additional finding, which is reported in online appendix section A.3, suggests that a channel exists in the P2P market for macroeconomic factors to affect perceived default probabilities and, therefore, individual loan interest rates. More specifically, we argue that liftoff cannot be reduced to an increase in the risk-free rate, since it was paired with a signal about the economic outlook, which had implications for perceived default probabilities. This resonates with the view that monetary policy is reacting to changes in macroeconomic conditions (e.g., Rigobon and Sack 2003) and with the extensive literature on the signaling role of central bank communication (e.g., Blinder et al. 2008).

5. Related Literature

Our paper relates to several different strands of literature. First, our work complements the existing empirical literatures on the bank lending channel and on event studies. We use primary market data and attempt to capture the impact of a rare monetary normalization event, which means that we cannot achieve identification using repeated observations of the same event category. In this sense, we are closer methodologically to the literature on the bank lending channel of monetary policy (Kashyap and Stein 2000),[29] but with the advantage that we observe loan outcomes at an hourly frequency instead of a monthly or quarterly frequency.

We employ panel data to study how a monetary normalization affects uncollateralized consumer credit with a focus on the cross-sectional dimension.[30] One way to establish identification, which has been employed in the literature on the bank lending channel, is to use a difference-in-differences (DID) specification (see, e.g., Heider, Saidi, and Schepens 2019). In our setting, we observe an exogenous shock that affects one group more than another, and where one of the main objects of interest is the difference in outcomes across group.

[29]See also Jiménez et al. (2012, 2014) and Di Maggio et al. (2017). For negative rates and unconventional monetary policy, see Heider, Saidi, and Schepens (2019) on bank lending and Mamatzakis and Bermpei (2016) on bank profitability.

[30]There exist only a few works on monetary policy interest rate pass-through to consumer credit. See Ludvigson (1998) for monetary policy transmission and automobile credit and Agarwal et al. (2018) for a recent study on credit cards.

While we use fixed effects to estimate the impact of liftoff on different groups, this can be interpreted as a double difference: one over time and one across groups. Our cross-sectional regressions reveal the different impact of liftoff on borrowers with heterogenous characteristics. Taking differences across borrower groups cancels out the effect of the liftoff event on risk-free rates and term premiums. What remains is the differential effect on perceived default probabilities. Since high-rated borrowers have very low default probabilities, a positive signal about solvency cannot reduce their interest rates substantially. Thus, while our estimate captures the lower bound of the magnitude of the effect, it is likely to be close to the actual treatment effect on the high credit risk segment.

This paper also relates to the extensive literature on monetary policy signaling with an interest in both the disclosure of monetary policy actions and revelation of information about macroeconomic variables (Andersson, Dillén, and Sellin 2006; Blinder et al. 2008). While the desired degree of transparency about the central bank's information on economic fundamentals has been intensely debated,[31] the literature suggests that the central bank information disclosure plays an important role in coordinating market expectations and provides relevant macroeconomic information to market participants (Swanson 2006; Ehrmann and Fratzscher 2007; Campbell et al. 2012; Boyarchenko, Haddad, and Plosser 2016; Ehrmann, Eijffinger, and Fratzscher 2016; Schmitt-Grohé and Uribe 2017).[32] Relatedly, Faust and Wright (2009) document the Fed's good nowcasting performance. Moreover, in line with our findings on the P2P lending market, perceived probabilities of default play an important role (e.g., in the context of bank lending policies (Rodano, Serrano-Velarde, and Tarantino 2018), and employment risk appears to be a key contributing factor (e.g., as a predictor of mortgage defaults (Gerardi et al. 2015).

Our work focuses on the distributional impact of the monetary normalization process within online credit markets. Specifically, we examine heterogeneity in the response to liftoff across

[31] See, e.g., Morris and Shin (2002), Angeletos and Pavan (2004), Hellwig (2005), Svensson (2006), and Cornand and Heinemann (2008).

[32] Furthermore, monetary policy action might also provide a signal about inflationary shocks to unaware market participants (Melosi 2016).

credit risk types. This is closely related to the growing literature on distributional effects of monetary policy.[33] In particular, the effects we measure capture something similar to the interest rate exposure channel described in Auclert (2019), but instead pick up the differential impact of monetary policy signaling, rather than policy rate shocks.

We also contribute to the growing literature on P2P lending and on consumer credit, more broadly.[34] P2P lending targets a slice of the consumer credit market—namely, high-risk and small-sized loans—that is neglected by traditional banks (De Roure, Pelizzon, and Tasca 2016). A number of papers employ the P2P market as a laboratory to study different aspects of lending, such as the role of informational frictions, using U.S. data from Prosper.com[35] and LendingClub.com, as well as from other platforms.[36] To our knowledge, the only other paper prior to ours that has attempted to link online lending markets to macroeconomic developments is Crowe and Ramcharan (2013), which studies the effect of home prices on borrowing conditions. More recent work by Chu and Deng (2019) and Huang, Li, and Wang (2019) find for the United States and for China that more accommodative monetary policy is associated with

[33]See Doepke and Schneider (2006) and Albanesi (2007) for the distributional impact of inflation on wealth, and Erosa and Ventura (2002) for the regressivity of inflation as a consumption tax. Gornemann, Kuester, and Nakajima (2012) evaluate the impact of monetary policy in an environment with heterogeneous agents.

[34]For a recent review of the literature on crowdfunding, see Belleflamme, Omrani, and Peitz (2015).

[35]Papers using Prosper.com data study the role of soft information, such as the appearance of borrowers (Pope and Sydnor 2011; Duarte, Siegel, and Young 2012; Ravina 2012; Gonzales and Loureiro 2014), screening of hard information in lending decisions (Iyer et al. 2015; Hildebrand, Puri, and Rocholl 2016; Faia and Paiella 2017; Balyuk 2018), herding of lenders (Zhang and Liu 2012), geography-based information frictions (Lin and Viswanathan 2016; Senney 2016), the auction pricing mechanism that existed prior to 2011 (Chen, Ghosh, and Lambert 2014; Wei and Lin 2016), and the ability of marginal borrowers to substitute between financing sources (Butler, Cornaggia, and Gurun 2017).

[36]Papers using data from LendingClub.com study adverse selection (Hertzberg, Liberman, and Paravisini 2018), retail investor risk aversion (Paravisini, Rappoport, and Ravina 2016), P2P as a substitute for bank lending (Tang 2019), and bank misconduct (Bertsch et al. 2020). Franks, Serrano-Velarde, and Sussman (2016) use auction data from FundingCircle.com to study information aggregatoin and liquidity.

an expansion of credit especially to riskier borrower segments, which the authors link to the risk-taking channel of monetary policy. Our paper complements this work by highlighting the signaling role in the context of a monetary policy normalization. In line with the key role of employment risk for our mechanism, Lam (2019) highlights the important role played by the loan applicants' employment length for lenders' funding decisions on LendingClub.com.

Finally, there is a large literature on household credit that spans a broad range of topics from mortgage debt to the different types of consumer credit (e.g., Bertola, Disney, and Grant 2006; Agarwal and Ambrose 2007). Nourished by increasing household indebtedness in many advanced economies over the last decade, the field has enjoyed increased attention (Guiso and Sodini 2013). Early papers studying the impact of FinTech on mortgage and consumer credit include Buchak et al. (2018), Fuster et al. (2018), and Berg et al. (2020). We differ from this work in that we study P2P markets; however, there are credit markets that have similar characteristics and are, therefore, closely related. For instance, credit cards are close substitutes for P2P personal loans. We expect access to new alternative sources of finance to be relevant for the spending behavior of consumers.

6. Conclusion

This paper contributes to the emerging literature on monetary normalizations by measuring the effect of Fed liftoff on the P2P segment of the uncollateralized online consumer credit market. We compile a unique panel data set of loan-hour observations from the online primary market for uncollateralized consumer credit. This allows us to monitor the funding process in real time, and to separately measure supply and demand. We find that liftoff reduced the spread between high and low credit risk borrowers by 17 percent and lowered the average interest rate by 16.9–22.9 bps. This change was not caused by Fed undershooting, a reduction in demand, a change in borrower composition, or a shift in risk appetite, but appears to be driven by a drop in investor-perceived default probabilities. We also use a separate data set to demonstrate that this effect generalizes to over 70 percent of the P2P market; and also show that these findings are not common to all FOMC announcements.

In addition to our interest rate results, we exploit a unique feature of our data set to demonstrate that (i) supply increased after liftoff; and (ii) demand did not fall. This is consistent with the narrative that liftoff revealed the Fed's strong, positive assessment of the future state of the economy. Borrowers in the P2P market are particularly sensitive to such assessments, since many of them have risky characteristics, including partial documentation and uncertain unemployment statuses. Indeed, we find that the net effect of the interest rate hike and FOMC signaling (i.e., proceeding with normalization) was small for highly rated borrowers, but was large and negative for borrowers with poor credit histories. This suggests that the effect we identify may be difficult to measure in other markets, such as the market for corporate or government debt, where default probabilities are less sensitive to signaling about future employment probabilities. Our findings are most easily generalizable to the uncollateralized consumer credit market.

Overall, our work complements the empirical event studies literature on monetary contractions, but is closer methodologically to work on the bank lending channel of monetary policy. We contribute to the literature by providing one of the first assessments of a critical stage in the monetary normalization process; and use a unique panel data set that allows us to monitor funding in real time and to disentangle supply and demand. Our results suggest that monetary normalizations may actually decrease interest rates for borrowers with poor credit histories by lowering their perceived default probabilities. This may, of course, depend on the content of the signals a central bank sends about its monetary normalization plan. In this case, the FOMC explicitly announced that liftoff would be contingent on the state of the economy, which framed the event as a positive revelation about the Fed's private assessment.

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