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10 December 2019

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December 2019

(Preliminary and Incomplete)

Abstract

This paper investigates whether borrowers' home values affect their *unsecured* loans' borrowing costs using data from a leading online peer-to-peer (P2P) platform in China. Taking China's 2016 home-purchase restrictions (HPR) policy as an exogenous shock, we employ a difference-in-differences identification strategy to compare the before and after changes between homeowner borrowers from cities with and without the HPR policy. It is found that houseowners' equilibrium interest rates decreased in those restricted cities, but No significant effect observed for borrowers without houses. Two economic channels are assessed, and we find the *perceived collateral* effect but no supporting evidence for the traditional *pure wealth* effect. Our results are robust to a series of alternative estimations. Furthermore, we provide evidence that reductions in borrowing costs are driven by lenders rather than the platform, as homeowner borrowers have faster speed of crowdfunding, more lenders per loan and a higher loan success rate. Homeowner borrowers are also found to default less ex post. Overall, our results contribute to the literature that government housing policy could still matter in an unsecured loan.

Keywords: Peer-to-Peer Lending, Home-purchase Restrictions, Collateral Effects, Pure Wealth Effect.

JEL Classification Codes: D14, G21, G28, R28

[1] We are grateful to Raghavendra Rau, Xuan Tian and seminar participants at Beijing Normal University-Hong Kong Baptist University United International College, Sun Yat-sen University and Zhejiang University for their insight views and comments. All remaining errors are ours.

I. Introduction

It is intriguing whether a borrower's home value determines his/her online peer to peer (P2P) loan outcomes. On one hand, lenders shall inevitably lose their investments when borrowers default, as seizing borrowers' properties is impossible in such *unsecured* loans. Thus, homeowners should not be favored in an unsecured loan market and house values should not affect and are irrelevant to their borrowing costs. On the other hand, soft information such as appearance is found to be crucial in determining the equilibrium loan results due to trust and other psychological reasons (Duarte, et.al, 2012). In addition, homeownerships could also be perceived as an indicator of a more financially trustworthy borrower, and homeowners might use houses as collaterals to seek for alternative funding opportunities (Michels, 2012). As a result, homeowner borrowers shall be impressed and favored by lenders, and an unexpected change in housing values would affect equilibrium outcomes even in an unsecured online P2P lending market.

House prices, which influences a homeowner's balance sheet (Banks & Tanner, 2002; Campbell & Cocco, 2007), are found to have considerable amplification effects in real business cycles via the collateral channel in many calibrated DSGE models (Iacoviello, 2005; Iacoviello & Neri, 2010; Liu, Wang & Zha 2013; Guerrieri & Iacoviello, 2017). At micro-level, housing price fluctuations also cause great wealth variations on household balance sheets (Disney & Gathergood, 2018). Tang (2006) and Carroll, Otsuka, and Slacalek (2011) confirm that the wealth effect of housing is larger than other financial wealth effects when estimating the marginal propensity to consume (MPC), which could strongly impact households' consumptions. Mian, Rao, and Sufi (2013) and Mian et al. (2017) shows that the significant and unequal consumption decrease in US from 2006 to 2009 could largely be attributed to mortgages and house price shocks. Besides, many other household activities will also be influenced by the swing of housing prices, including investment decisions (Schmalz, Sraer, and Thesmar, 2013), labor decisions, educational selections (Lovenheim 2011; Lovenheim & ReyNolds 2013), divorce rates (Farnham et al. 2011), childbirth rates (Lovenheim & Mumford 2013; Dettling & Kearney 2014), and long-term care insurance (Davidoff, 2010).

Yet, literature has mixed explanations over such impacts. On one hand, a larger housing value would, however, convert to a higher living expense, which discourages household financing and expenditure (Buiter, 2008). On the other hand, increase in housing value could translate to a relaxation of households' borrowing constraints, which promote their consumptions (Campbell and Cocco, 2007).

Despite the competing forces, early studies focus on secured loans, where houses are used as collaterals. It is still unclear whether pervious findings could be extended to unsecured loans, where houses are not pledged. Existing empirical findings mainly target on unsecured loans from corporate level. Stulz and Johnson (1985) finds that firms tend to use secured debt to finance profitable projects instead of unsecured ones. Booth and Booth (2006) identifies that firms borrowing on a secured basis from banks have less possible costs than an unsecured basis. Linn and Stock (2005) estimates the variations in senior unsecured debt risk premiums that along with new junior debt issuance. There is few research in the area of the household level, except for Mild et al. (2015) who estimates the risk of default for unsecured loan, Kim (2015) who finds that negative house price shock will lead households to borrow large amounts of unsecured loan to avoid risks, and Bazley (2018) who demonstrates that a rise in house prices is found to have adverse effect for homeowners. In this paper, we attempt to fill this gap and provide suggestive evidence on how households' borrowing costs were affected by sudden change in their housing values. We treat China's 2016 home-purchase restriction policies as an exogenous shock and use a novel transaction level data from a leading online P2P lending platform.

China has witnessed an unprecedented economic growth from past few decades and become the second largest economy worldwide. Real estate, as a pillar industry in China, contributes tremendously on the rapid growth of Chinese economy and household consumptions. However, along with the development of real estate sector, a soaring housing prices has also drawn wide and serious concerns, which leads to a series of home-purchase restriction policies adopted by the central and local governments. Home-purchase restrictions, first launched in 2010 and then in 2016, were implemented in different prefecture cities. The claimed rationales behind the two policy waves were different. Specifically, the 2010's policy was under the context of the recovery of global financial crisis and a general boost in housing prices so that the government decided to apply a nearly nationwide housing purchase restrictions (Du & Zhang, 2015). In contrast, the government merely put the restrictions on the first- and second-tier cities in 2016.

It is widely believed that housing purchase restrictions policies sent a strong signal about housing markets' future prosperities in those cities. The cities with home-purchase restrictions after 2010 still experienced roaring housing prices as shown in Figure 1. Figure 1 also shows that, the growth rate and housing price index of those cities increased even shaper compared with the cities not implementing home-purchase restriction policies in the long run. It is shown that the home purchase restriction policy only has long term effects on suppressing housing prices if their monthly growth rate does not exceed 5% (Li, Cheng, and Cheong, 2017), while most cities in our sample with home-purchase restriction policies had over 5% growth rate on a month-to-month basis from 2010 to 2016. Therefore, due to the learning effect of the previous policy practice, Chinese citizens would possibly to hold a strong expectation of thriving future housing prices in the cities with restriction policy in 2016.

An ideal laboratory to study households' borrowing costs is China's online Peer-to-Peer (P2P) markets, since such data is much more frequent and abundant than that in typical household surveys. This fosters us to investigate how interest rates in online P2P markets were affected by the policy. It is worthy Noted that equilibrium interest rates' changes reflect the changes in how lenders access borrowers' background information after the policy announcement. In is well known that China's P2P crowdfunding activities turn out to be an important channel for household micro financial activities and constitutes a vital aspect of China's FinTech development. By December 2018, 6,618 online P2P platforms have been actively involved in business, with a value of RMB1,794.8 billion transactions in total, according to WDZJ, an authoritative online P2P industry portal in China.

Taken the 2016's home purchase restrictions as an exogenous shock, we employ a difference-in-differences (DD) identification strategy to empirically disentangle the policy effects on households' equilibrium interest rate in their Fintech borrowing. As the timing of city-wide policy announcements was different, it indeed facilities our DD identification. In addition, we also control for city-level time invariant confounding factors and nationwide macroeconomics trends, as they might be correlated with loan level outcomes. As our transaction-level data structure is repeated cross-sectional in nature, we further alleviate the endogeneity concern by incorporating a rich set of borrowers' characteristics, as homeownership status and credit consequences are likely to be correlated with those individual level features (Ramcharan & Crowe, 2013).

In our baseline estimation, we use a subsample of households who are houseowners and find that the policy significantly reduced the borrowing costs for those who lived in the cities with the restrictions comparing to the households who were Not policy constrained. Such effects do Not exist if we switch our sample to the group of borrowers who do Not possess homeownerships. To alleviate the concern that the observed reduction of borrowing costs is purely driven by the unobserved city time-varying factors, we use Non-homeowner borrowers in the same city as another level of the control group and estimate a triple difference-in-differences (DDD) estimations. The above conclusion still holds. The dynamic effect show that such effects is persistent over our sample period.

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In our baseline estimation, we use a subsample of households who are houseowners and find that the policy significantly reduced the borrowing costs by 1.3% for those who lived in the cities with the restrictions comparing to the households who were not policy constrained. Such effects do not exist if we switch our sample to the group of borrowers who do not possess homeownerships. To alleviate the concern that the observed reduction of borrowing costs is purely driven by the unobserved city time-varying factors, we use non-homeowner borrowers in the same city as another level of the control group and estimate a triple difference-in-differences (DDD) estimations. The above conclusion still holds. The dynamic effect show that such effects is persistent over our sample period.

We then turn to the economic mechanisms that shape the results. The first channel is the collateral channel. The policy should relax the borrowing constraint of the borrowers who are houseowners and they are expected to have more favorable loan interest rates since the policy presents as a positive news for the P2P lenders. It is hypothesized that the home purchase restriction policy could be a positive signal sent by local governments, since the 2010's restriction policy was targeted but failed to suppress high housing prices. We construct an indicator for either mortgage or car loan and further differentiates homeowner households into the constrained and unconstrained ones. It is shown that constrained homeowner borrowers did experience a more salient reduction in their borrowing costs for various econometric specifications, based on lenders' impression of the policy shock. We also test the city variations, as the negative effects of P2P borrowing costs could be more pronounced in cities with good macroeconomic condition since these cities' house value might have a stronger appreciation potential. We proxy various local housing appreciation potential using variables such as local GDP, population and real estate investment growth rate to test the validity of the perceived effect hypothesized above. Our results suggest that houseowners that from cities with better past economic performances enjoyed a more reduction in their equilibrium interest rates.

The second possible channel could be the pure wealth effect channel argued by the literature. Households may borrow more and consume more as they feel richer and willing to bear a higher borrowing cost (Campbell & Cocco 2007; Case, Quigley, & Shiller 2013). To empirically test channel, we use borrowers' age as a proxy variable and the DDD results does not support this effect.

We carry a series of test to show the robustness of our results. We first adopt an instrumental variable approach, as the choices of restricted cities were Not random. We instruct cities' land supply and run the difference-in-differences instrumental variable (DDIV) estimation. The previous results that people from restricted cities enjoy a reduction in interest rates still hold.

Furthermore, we utilize the relief of home purchase restriction policy that announced and implemented in 2014 to substantiate our arguments. Both our previous DD and DDD results are reversed, which suggests that our baseline conclusions should be generalizable. Though a bunch of personal characteristics have been controlled, there could still be further endogeneity concerns, as unobserved personal characteristics might be omitted and bias our previous estimates. To tackle this valid challenge, we explicitly add individual fixed effects, that is, we focus on borrowers that showed in our transaction records before and after the policy announcement. As a result, individual time-invariant characteristics are controlled. The conclusions still hold for both the 2016 policy restriction and 2014 policy relief. In addition, we winsorize over borrowing interest rates and loan amount for extreme observations and assign weights for the number of observations in different cities for concerns that the numbers of observations in different cities is unbalanced. Both results are robust.

Admittedly, our results are all equilibrium loan interest rates that formed by both borrowers and lenders in the P2P market, and we hypothesize such reduction in interest rate is mainly driven by the lenders rather than the internal algorithm of the P2P platform. To answer this question, we further isolate the lenders' reactions. In particular, we test how the speed of crowdfunding, number of investors per loan and the probability of a successful funding were affected after the restriction policy. All results still indicate that houseowner borrowers residing in cities that implemented the restrictions were more welcome by lenders. Finally, we isolate the borrowers' behaviors and test borrowers' actual post performance after the shock and find that P2P borrowers have 0.1% less defaults after the 2016 home-purchase release policy, which confirms the advantages that treated houseowners might achieve through the perceived house value variation.

Our study contributes to two strands of current literatures. Firstly, it explores the housing wealth effect from a new perspective, which demonstrates the household credit access to unsecured small loans. Existing studies have examined the impact of the asset value on household consumption, investment, and borrowing activities by taking the secured loans into their consideration (Mian, Rao, & Sufi, 2013; Schmalz, Sraer, & Thesmar, 2013; Mian & Sufi, 2018). Along with the development of the micro-loan market in China, an increasing number of households choose to borrow from the P2P market and their activities gradually attract more attention from researchers (Lin and Viswanathan, 2015; Li, Liu & Tian, 2018). This study complements the effect of housing value fluctuation literatures over the government home-purchase restriction policy shock and try to build the causal relationship between the housing price variations and borrowing activities in the P2P market.

Secondly, our study replenishes P2P crowdfunding literatures by standing on the supplying side of the market through the P2P market mechanism in China. Recent empirical literature already touched the determinants of P2P borrowing costs and the impact of economic shocks on P2P activities. Age, income, positive financial prospects and housing tenure are found to be significant in determining online borrowing costs in Del and Young (2006)'s research. Michels (2012) finds that volunteer and unverifiable disclosures significantly lessen borrowing costs in p2p lending platform. Braggion, Manconi, and Zhu (2018) recognizes the relationship between tightening loan-to-value caps from banks and increasing borrowing loan amount from P2P market. Li, Liu, and Tian (2018)'s study identify that policy uncertainty negatively affects households' access to small loans in P2P lending platform. Ramcharan and Crowe (2013) further considered housing fluctuations could have a significant influence on P2P lending credit availability. Based on the background of 2008 financial crisis, they found that the declining housing price will make houseowners face lower funding success rates, higher interest rates, greater credit rationing, and quicker loan delinquency.

We emphasize on how P2P borrowers and investors respond to the new information reflected in the housing policy shock. Our research echoes the Ramcharan and Crowe (2013)'s study and further confirms the relationship between the house price fluctuation and P2P credit availability, but our identification is different. This study targets on the home-purchase restriction polices from different cities, which is positively related to the housing price and could be considered as external shock because of the information asymmetry between the government and the housing market. Moreover, the market mechanism of the US P2P lending is different from the one in China. Renrendai P2P lending in China employ the posted-price mechanism instead of auction model used by Prosper.com before 2010. Under posted-price mechanism, the borrowing interest rate and amount are set by borrowers, while investors only voice their opinions by specifying the investment amount and duration. Our research is different from Braggion, Manconi, and Zhu (2018) that their study mainly focused on the causal relationship between increasing banking mortgages down payments and rising borrowing loan amount from P2P market. While our study targets on how the house purchase policy affect houseowners' P2P borrowing costs. In addition, their identification is from the demand sides that the tightening mortgage down-payment requirement leads P2P borrowers to increase higher borrowing amount. Our study targets on equilibrium interest rates and estimate the effect from lender's perceptive. We also support the house wealth effect through the borrowing collateral channel in P2P platform and exploit diverse house wealth effects of heterogeneity in household-level and city-level.

The remaining paper is organized as follows. Section II presents the theoretical foundation of the study. Section III describes the data and summary statistics. Section IV explains the estimation model and results, Section V concludes.

II. Theoretical Foundation

Previous studies generally identify the housing wealth shock by measuring how those shocks affect housing return. For example, Glewwe and Jacoby (2004) believes that local Economic growth is an important wealth effect that stimulates residential household level activities. While, other researchers track the effects of government policies on households' behavior, like the important roles that monetary policy play in propagating the shock transmission through the credit channel (Bernanke & Gertler, 1995; Benmelech & Bergman, 2012; Iacoviello, 2005; Kaplan et al., 2018). Tax policy and credit supply expansion policy embrace the similar effects (Sommer & Sullivan, 2018). Empirical evidences are found that households' debt and consumption responded actively to U.S tax policy (Souleles, 1999), and the effects for debt were particularly strong for those who were liquidity unconstrained (Agarwal et al., 2007). Di Maggio & Kermani (2017) studies the heterogeneous impact of banking deregulations on different states in the U.S. This exogenous variation of credit supply due to anti-predatory lending contributed to the local house prices and employment rate. Cai (2016) also indicates that the implications of agriculture insurance provision raises household borrowing size while decreases loan interest rates for Chinese rural households

The mechanism that drives the bond between the swings of the house value and household consumptions and decisions is widely explained through two major assumptions: pure wealth effect and borrowing collateral effect (Sinai & Souleles, 2005; Cooper, 2013; Berger, 2015, Cloyne et al, 2019). The pure wealth effect considers that the house is one type of financial asset, the rising house prices increase households' Nominal housing wealth, households may borrow more and consume more as they feel richer (Campbell & Cocco 2007; Case, Quigley, & Shiller 2013). However, Nominal wealth is Not real wealth (Sinai & Souleles 2005; Buiter 2010). Because the increase in the housing value may offset by the increase in the future rental cost, the pure wealth effect should be obvious for the one with short horizon. Many studies examine the pure wealth effect through age profile. Old homeowners are regarded as the cohort with short horizon, they may expect to sell the house and exit the housing market in the near future and convert the Nominal wealth to real wealth. In contrast, young home owners have the long horizon, the Nominal wealth would be offset by future rental cost. As a result, the pure wealth effect can be tested by looking at the heterogeneous effect with respect to age (Campbell & Cocco 2007, Attanasio et al., 2009, Mian & Sufi, 2011). Gan (2010) studies the relationship between house value and credit card spending in Hong Kong, and find that pure wealth effect, which is identified by looking at how the consumption respond to the house value shock across households with different number of houses, can partly explain the relationship. Households without borrowing constraints could benefit from the pure wealth effects captured by their lifetime budget constraints. The canonical certainty-equivalent life-cycle model, on the contrary, suggests an invariant household behavior after predictable future income fluctuations (Modigliani & Brumberg, 1954; Carroll, 2001: Friedman, 2018).

However, Flavin and Nakagawa (2008) and Buiter (2010) questioned about the pure wealth effect and stated that the effect of housing appreciation or depreciation on the net wealth of the house owners is ambiguous since housing wealth should be treated as both an asset and an expenditure good. A complex mechanism is included behind the housing wealth changes for house owners and Non-house owners. For instance, Cho (2011) shows that housing price fluctuation would work in opposite directions for household with house and without house.

Therefore, many research studies the relationship between house prices and Economic activities concentrating on the borrowing collateral channel (Guerrieri & Iacoviello, 2017). Housing value makes up the greatest part of the household's portfolio and could be the largest type of collateral. As the relax to credit market would increase the borrow demand (DeFusco 2017), the collateral effect implies that the value of collateral would increase along with the housing price appreciation, so that the increased collateral value would decrease the borrowing cost, especially for households who are experiencing a borrowing constraint (Campbell & Cocco 2007). Aoki et al. (2004) explain the effect of housing value on household consumption via credit market by considering credit frictions in their general equilibrium model. The collateral channel works by amplifying and propagating monetary policy shocks on housing demand and consumption. Iacoviello (2005) distinguish the effect of different type of shocks and theoretically prove that positive demand shock improves the household or firm's debt capacity and increase the consumption and investment. A number of empirical evidences show that the housing wealth impact the borrowing consumption via collateral channel. Cooper (2013) finds the collateral channel, instead of wealth effect channel, could explain the relationship between the Non-housing consumption and housing value by looking at the heterogeNous effect across groups of households with different level of borrowing constraints. Cloyne et al (2019) uses a rich dataset to verify the collateral channel by examining the heterogeneous effect of LTV, age, income and income growth on the elasticity of borrowing to housing price. They find the elasticity is strongly respond to high LTV ratio, even controlling for the other 3 factors, suggesting collateral channel can be used to explain their findings of positive relationship between housing value and loan amount. A rise in housing values translates to an increase in collateral values and thus makes households' borrowing constraints Non-binding. This encourages leverage and consumption through the classic consumption Euler equation. Later studies confirm that housing wealth helps to alleviate credit constraints for household and even of their potential investments (Schmalz, Sraer, & Thesmar, 2013; Corradin & Popov, 2015). Corradin and Popov (2015) echoes the previous research that housing wealth is able to lessen credit constraints for potential entrepreneurs based on the collateral channel.

These theories allow us to explore the house wealth effect and the heterogeneity of the effect by taking advantages of the quasi-natural experiment of the home-purchase restriction based on the sample of Chinese P2P platform. The effect of the home-purchase restriction on Chinese P2P borrowers' activities could be explored by the identification of the channel of the house wealth effect, including the pure wealth effect and the borrowing collateral effect. The test of the heterogeneity of the effect could further exploit the mechanism that how house-level and city-level characteristics influence the house wealth effect.

III. Institutional Background

House-purchase restriction (HPR) policy has long been acknowledged for its short-term rather than long run effectiveness in controlling housing prices (Yang, 2018). The policy could be especially pertinent to China, as real estate valuation accounts for more than two thirds of households' wealth. A healthy housing market, one of the pivotal growth drivers of China's economy, has far-reaching implications for not only individuals but also the national economy. HPR policy normally stipulates the maximum number of houses a person could purchase; or it could require a minimum down payment ratio, mortgage interest rate, and person's residency status (namely Hukou in Chinese) in certain areas[2]. In addition, local governments sometimes attach certain minimum periods an owner must hold years before resale, which disincentivizes speculative purchase (Wang 2017, Deng and Zheng 2018). In this section, we discuss some institutional background regarding the 2010 HPR policy, the 2014 HPR relief policy and the 2016 HPR tightening policy respectively.

The 2010 HPR Policy

In response to 2008's global financial crisis, the Chinese government implemented a bunch of expansionary policies, and loosening mortgage loans turned to be a nature consequence of banks' excessive funding, and China's housing market became thriving (Zhang, 2009). To stem the soaring housing prices, both the central and local governments rolled out a series of restriction policies. On April 17, 2010, the central government announced "Notice of the State Council on Resolutely Curbing the Soaring of Housing Prices in Some Cities" (*国务院关于坚决遏制部分城市房价过快上涨的通知*). Soon after, Beijing municipal government issued a detailed HPR policy in response to State Council on April 30, where each family could only purchase one new house.[3] Consequently, a total of 40 major cities including 16

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[2] Authority could require the minimum years that a homebuyer must work in the restricted city. Proofs such as social security or income tax payment must be shown when purchasing the house in cities implementing the HPR policy.

[3] Sun and etc. (2013) discussed the details of Beijing's HPR policy that each family with Beijing Hukou can own a maximum of two homes while families without local Hukou are not allowed to buy any more only if they provide documents to prove their income tax payment and social security contributions for straight five consecutive years.

first-tier and second-tier cities (Hangzhou, Guangzhou, Nanjing, Shanghai, Tianjin etc.) initiated HPR policies. It was considered as the most strict and wide-range housing restriction policy in China (Du and Zhang, 2015). As a result, speculation activities on housing market were suppressed but the HPR was not successful in cooling down people's expectation about the bright future of the housing market. Many newly sold apartments remain in high vacancy, showing that buyers had high expectation on future capital gains. IMF's global financial stability report also forecasted an even greater run-up in housing prices in some areas, which could cause financial instability (IMF, 2011). Thus, the authorities' MPR policy was only effective in short run, but could induce a sharper increase in expected valuation of houses in the long term. Panel B in Figure 1 shows the house prices rebounded in 2012 and increased to a historical level in 2014.

The 2014 HPR Relief Policy

Although the implementation of 2010 restriction policies curb the overheated housing prices within a short time, the strong expectation of investors on the housing market prompted the property market to rebound gradually in 2012 and remained steady upsurge in 2013. Yet in 2014, high house inventories raised extra attention and an alarming Economic slowdown emerged thereby residential property market receding (Cao et al., 2015). Housing prices started to decline in an increasing number of cities while the residential property inventories have increased sharply.[4] China's National Bureau of Statistics showed that prices for newly built homes fell broadly in July 2014 and the weakness spread to more cities, suggesting a downward trend of the property market (New York Times, 2014).[5] Since June 26, 2014, Hohhot stepped first to loosen the local property market restrictions. Hangzhou, Jinan, Nanning and more than 30 cities quickly followed up. As of August 16, only 5 of the 46 cities in China, namely Beijing, Guangzhou, Shanghai, Shenzhen and Sanya, did not relieved the HPR policy.

[4] See from http://www.gov.cn/xinwen/2014-10/31/content_2773645.htm for details

[5] See from https://cn.nytimes.com/business/20140818/cc18home/zh-hant/ for details

The 2016 HPR Tightening Policy

Policies released by the central government in the early 2016, according to the Guardian, plainly sent a positive signal to the market as it set destocking houses as the main task.[6] However, this led to an overwhelmingly optimistic expectation from investors and speculators to enlarge spending on houses. The "China housing bubble" came back to a headline-making theme in 2016, thereby local governments further tightening restriction policies, particularly towards 'speculative' housing purchases, to moderate property price inflation. Different from the 2010 HPR policy, the central government delegated the power of policymaking on house restrictions to local governments rather issuing any official support documents in advance.[7] It was considered as a complete shock to the market. On May 27, 2016, Shanghai surprisingly announced the implementation of restriction policy by increasing the number of work years from two to five for non-local residents. Following by Hangzhou and Beijing, a total of 21 cities intensively proclaimed to tighten or add new locations or districts into the restrictive areas during the National Holiday.[8] In Hangzhou, before the HPR policy announcement at 17:00 on the 18th September, the housing sale record displayed no more than 700 deals. Yet until 00:00 in that evening, this figure had risen to 3089, among which 2859 deals were located in the restrictive areas.[9] The restriction policy shock was entirely out of people's expectation, which validates our shock-based analysis in section V.

IV. Data and Summary Statistics

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[6] See from http://www.guandian.cn/blogComment/20160111/169710.html

[7] See from https://www.reuters.com/article/idCNL4S1E71D5 for the detailed policy list with regard to the housing market in 2016.

[8] The 21cities include Beijing, Chengdu, Dongguan, Foshan, Fuzhou, Guangzhou, Hangzhou, Hefei, Jinan, Langfang, Nanchang, Nanjing, Shanghai, Shenzhen, Suzhou, Tianjing, Wuhan, Wuxi, Xiamen, Zhengzhou, Zhuhai.

[9] The normal business hour of the property transaction center was extended until 0:00 on that day to facilitate last minute trading. See from http://finance.sina.com.cn/china/dfjj/2016-09-19/doc-ifxvyqvy6737641.shtml for details

The empirical analysis is based on the household-level data collected from Renrendai, one of the largest P2P online lending platforms in China. Our sample consists of all P2P funded loans from January 2016 to July 2017, 9 months before and after October 2016, the month with a bunch of announcements of the home-purchase restriction policies. The household residing in the 21 cities (Table 1) which adopted home-purchase restriction are attributed in the treatment group while others are in the control one. The P2P sample includes funded borrowers' borrowing interests, amount, number of lenders, duration of the funding process, as well as their individual characteristics. To summarize, our sample includes 249,309 households, of which 107,699 household has house and 141,612 has No any house properties. For houseowners, 32,702 of them are in the treatment group where home-purchase restriction policies are adopted while 74,997 of them are in control regions.

[Insert Table 1 Here]

Table 3 provides the summary statistics of the key variables for the period January 2016 to September 2016 before the announcement of home-purchase restriction policies. Gender is a dummy variable with one is female and zero is male. Marriage is a dummy variable with one is married and zero otherwise. Age indicates the age of the borrower. Salary is a variable indicating a borrower's monthly income level, where n=0 represents whose wage is No more than 1000 RMB, n=1 means monthly income is between 1000-2000 RMB, n=2 means monthly income is between 2000-5000 RMB); n=3 means monthly income is between 5000-10000RMB; n=4 means monthly income is between 10000-20000RMB; n=5 means monthly income is between 20000-50000 RMB; n=6 means monthly income is above 50000 RMB. Education is a variable indicating the education level of borrowers, where n=0 (if the borrower is high school certificate and below), n=1 (if the borrower is college-degree holder), n=2 (if the borrower is university- degree holder), n=3(if the borrower is with postgraduate degree and above). Work years is a variable showing the working experience of borrowers, where n=0 (if the working experience is No more than 1 year), n=1(if a borrower has 1-3 years' working experience), n=2 (if a borrower has 3-5 years' working experience), n=3 (if a borrower has more than 5 years' working experience). Car is a dummy variable with one has car and zero otherwise. Job position is a dummy variable with one working for salariats and zero otherwise. SOE is a dummy variable with one working for state-own companies and zero otherwise. Loan is a dummy variable with one having house loan or car loan and zero otherwise.

Based on the characteristics of the P2P borrowers from the sample, the treated houseowners' average borrowing interests (10.26%) are significantly lower than that of the controlled houseowners (10.29%). Meanwhile, the average loan size (98000 RMB), borrowing duration (11.21 hours), and number of lenders (130.1) for the treated borrowers are significantly higher than those of controlled houseowners, which are 87,000 RMB, 9.818 hours, and 113.5, respectively. Also, the treated region borrowers exhibit the differences with more females (0.299 versus 0.311), less marriage (0,756 versus 0.763), younger age (37.51 versus 38.36), and higher education level (1.270 versus 1.392), shorter working experience (2.044 versus 2.161 years), loan percentages (62.4% versus 53.9%), stable salary payment percentages (0.176 versus 0.133), and the average amount of salary (3.941 versus 3.289) compared to the borrowers from the control region. The treated regions have significantly higher GDP growth rate (9.1% versus 8.5%), population growth rate (0.6% versus 0.3%), real estate investment growth rate (12.9% versus 1.7%), and residential investment growth rate (14.5% versus 9.20%), as well as lower wage growth rate (9.4% versus 10.5%) than the control regions.

[Insert Table 3 Here]

V. Estimation Strategies and Results

The progressively implementation of the home-purchase restriction policies introduced variations in house wealth across time periods, cities, and household borrowing activities. According to these variations, we use both DD and DDD estimations in the empirical analysis.

5.1 The Impact of the 2016 Home-purchase Restriction on Houseowners

Common Trend Analysis

To analyze the how home-purchase restriction affect P2P borrowing costs, we estimate the common trend assumptions for the pre-policy trend, which is defined as four quarters before

September 26, 2016, in the following regression to examine whether DD is an applicable approach for our study:

$$Interest_{irt} = \beta_0 + \beta_1 Timetrend_t + \beta_2\ Treat_{ir} + \beta_3 Timetrend_t * Treat_{ir} + \beta_4\ Control_{ir} + \alpha_{irt} \quad (1)$$

In the regressions, i, r, t are borrower, city, and month indices, correspondingly. $Interest_{irt}$ is P2P funded interest of the borrowing activities. $Timetrend_t$ indicates quarterly trends before September 26, 2016. $Treat_{ir}$ is a dummy variable with one for the cities implementing 2016 home-purchase restriction while zero otherwise. This test tracks for four quarters, three quarters, two quarters, and one quarter before the announcement date of home-purchase restriction policy in each city, respectively. City, month*year, job characteristics (office_type, salary, office_size) fixed effects are controlled in all estimations. Standard errors are clustered at city level. Individual characteristics (age, gender, work_year, married, graduation), loan amount, and city trend as selected as control variables. As presented in Table 4, houseowners were in a similar trend for borrowing interests before the announcement of home-purchase restriction in 2016, we could conclude that the common trend assumption is valid since all the interaction terms, β_3, are insignificant, which demonstrates that there is a parallel trend between the city with and without implementing 2016 home-purchase restriction before this policy shock.

[Insert Table 4 Here]

5.2 DD and DDD Analysis for houseowners

Based on the common trend assumption, we could further test the effect of home-purchase restriction on the houseowners through the following DD and DDD model:

$$Interest_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i + \beta_2\ Control_{ir} + \alpha_{irt} \quad (2)$$

$$Interest_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i * has_{house} + \beta_2 Treatpost_{2016_i} + \beta_3 has\_house + \beta_4 Control_{ir} + \alpha_{irt} \quad (3)$$

where $Treatpost\_2016_i$ is an indicator equal to one for treatment regions after the 2016 policy shock and zero otherwise. $Interest_{irt}$ is P2P funded interest of the borrowing activities. $Control_{irt}$ indicates various control variables including borrowing amount and individual characteristics, including gender, married, age, work years, and education. The dependent variable is borrowing costs. Treatpost_2016 is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. α_{irt} represents a full set of city, month, year, and job characteristics fixed effects, with standard errors clustered at the city and month*year level to account for any correlations of the error terms within each firm.

From DD results shown in Table 5, we could further identify that the treated houseowners' P2P borrowing interest rates drop more significantly rather than those of control regions after the announcement of home-purchase restriction in their cities. This outcome means that treated houseowners could tend to achieve 1.3% borrowing cost reduction in the P2P platform since lenders perceive that the announcement of home-purchase restriction indicates a positive signal for treated houseowners. We could also find that the P2P borrowing interest rates for treated households without house has no significant differences compared to those of control regions after the announcement of home-purchase restriction in their cities. As a result, unlike houseowners, lenders could not perceive such an effect on Non-houseowners since their financial situation will not be affected by the announcement of the policy or the effect is ambiguous. This estimation result also supports that the lower borrowing cost for treated houseowners is merely due to the house value variation instead of city trend variation.

Following the spirit of Cai (2016), we further implement the following DDD model to confirm the results using the whole sample (houseowners and Non-house owners). This DDD framework aims to control for potential city-specific effects in our study (Cai, 2016), since some cities might have some unique potential trends during the sample period and factors other than the policy shock could affect the result. Based on this estimation, we target on the differences between houseowners in the treated regions and the control regions by considering the differences in Non-houseowners before and after the shock. From Table 5, we confirm the previous research results that the DDD interaction term for interest of the borrowing is significantly negative. This outcome shows that treated houseowners could experience 2.6%

decrease of interest rates after the shock compared with all the other households since their house value variation could be perceived by the P2P lenders.

[Insert Table 5 Here]

5.3 Channels of the House Wealth Effect on Borrowing Outcomes

Since the home-purchase restriction will lead to a rising housing price in those treatment regions, which represents a higher house wealth for the borrowers with houses. To identify the channel of house wealth effect in our study, we conducted the following DD and DDD model to test the impact of the home-purchase restriction on Non-houseowners. According to the theories discussed in Section II, the channel of the effect of house value swings on household consumptions and decisions are generally explained through pure wealth effect or borrowing collateral effect (Sinai & Souleles, 2005; Cooper, 2013; Berger, 2017; Cloyne et al, 2019). The pure wealth effect means that the rising house prices increase real housing wealth for households, which encourages households borrow more and consume more as they feel richer, especially for elder people (Campbell & Cocco 2007; Case, Quigley, & Shiller 2013). The collateral effect concentrates on lenders' perceptions and indicates that the value of house collateral would increase along with the housing price appreciation. The rising collateral value would be considered as a positive signal by lenders and would further decrease the borrowing cost, especially for households who are facing borrowing or collateral constraints (Campbell & Cocco 2007).

In order to examine the channel of house wealth effect in our estimation, we consider the financial constraints using the following a DDD model:

$$Interest_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i + \beta_2\ Control_{ir} + \beta_3 Treatpost\_2016_i * Loan_{ir} + \beta_4 Loan_{ir} + \alpha_{irt} \quad (5)$$

$$Interest_{irt}\ =\ \beta_0 + \beta_1 Treatpost\_2016_i * has_{house} + \beta_2\ Control_{ir} + \beta_3 Treatpost_{2016_i} * has\_house * Loan_{ir} + \beta_4 Treatpost\_2016_i * Loan_{ir} + \beta_4 has\_house * Loan_{ir} + \alpha_{irt} \quad (6)$$

where $Loan_{ir}$ is a dummy variable indicates that whether household has an existing loan (car or house loan). One is with loan and zero represents without loan. The definitions of other variables are the same as equation (2).

The results from Table 6 demonstrate that houseowners with financial constraints experience a stronger impact from the home-purchase restrictions, with 6.5% reduction of the borrowing costs. This result matches the existing study in the collateral channel that houseowners with financial constraints involve in a more obvious health wealth effect (Cooper, 2013; Corradin & Popov, 2015; Cloyne et al., 2019).

Most existing literature studies the pure wealth effect through age profile (Campbell & Cocco 2007; Attanasio et al. 2009; Mian & Sufi 2011). They find that the pure wealth effect can be tested by looking at the heterogeneous effect with respect to age. As a result, our study further use age as an interaction term in the DDD model to test the pure wealth effect as follows:

$$Interest_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i + \beta_2\ Control_{ir} + \beta_3 Treatpost\_2016_i * age_{ir} + \alpha_{irt} \quad (7)$$

$$Interest_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i * has\_house + \beta_2\ Control_{ir} + \beta_3 Treatpost_{2016_i} * has\_house * age_{ir} + \beta_4 Treatpost\_2016_i * age_{ir} + \beta_4 has\_house * age_{ir} + \alpha_{irt} \quad (8)$$

The definitions of the variables are the same as equation (2). The insignificant research result from Table 6 shows that there is no strong correlation between age and the effect of policy shock. Therefore, the pure wealth effect is not appropriate in explaining the house wealth effect of the study.

[Insert Table 6 Here]

We further explore the housing value collateral effect through city variations in housing prices. Since the cities with good macroeconomic condition tends to have a stronger appreciation potential in house market. We proxy various local housing price appreciation potential using variables such as GDP, population, and real estate investment growth rate to test how houseowners from cities with heterogenous economic performances being perceived in P2P market after the shock. The following DDD model was applied:

$$Interest_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i + \beta_2\ Control_{ir} + \beta_3 city\_characteritics_i + \beta_4 Treatpost\_2016_i * city\_characteritics_i + \alpha_{irt} \quad (4)$$

Where city characteristics indicates city macro-economic data, including GDP_growth_i, $Population\_growth_i$, and $Investment\_growth_i$ represent three- city GDP growth rate, city population growth rate, and city residential investment growth rate, respectively. The definitions of other variables are the same as equation (2). Results from Table 6 demonstrate that treated houseowners' P2P interest rate has a significantly negative relationship with their residential city's GDP growth rate, population growth rate, and residential investment growth rate. This result could be summarized that the better economic situation of the city, the greater housing collateral value could be perceived by lenders and finally lower borrowing costs that treated households could enjoy in their P2P borrowing activities. The result matches the study from Glewwe and Jacoby (2004) that local economic growth is a significant wealth effect that stimulates residential household activities.

[Insert Table 7 Here]

In sum, based on the signs of home-purchase restriction, the increasing housing value perceived by lenders as more collateral advantages, help treated P2P borrowers, especially the ones with financial constraints, reduce the borrowing costs by 6.5% through the borrowing collateral effect.

Dynamic Effects

The release of the 2016 house-purchase restriction policy may impact household borrowing activities for a certain period. Furthermore, the magnitude and the significance of the effect could vary over time. Through the following regression, our study estimates the dynamic effect of the 2016 house-purchase restriction policy on household P2P borrowing activities to check those dynamic possibilities.

$$Interest_{irt} = \beta_0 + \beta_1 Treatpost_i + \beta_2 Dynamic_{Quarter_t} + \beta_3 Treatpost_i * Dynamic_{Quarter_t} + \alpha_{irt} \quad (9)$$

where $Dynamic\_Quarter_t$ contains a set of quarter dummies before and after the 2016 house-purchase restriction policy. α_{irt} represents a full set of city, month* year, and job characteristics fixed effects, with standard errors clustered at the city level to account for any correlations of the error terms within each firm. The definitions of other variables are the same as equation (2).

The results in Figure 2 indicate that the effect of house-purchase restriction policy on houseowners' P2P borrowing interest rates becomes significant after the shock. The impact of the shock persists for the rest of four quarters of the post period, which shows that lenders' perception on this policy shock lasts for at least four quarters after the shock.

[Insert Figure 2 Here]

VI. **Robustness Check**

6.1 The Instrument Variable Approach

Following the spirit of Saiz (2010) and Graham (2018) that variation in house prices as predicted by the local housing supply, we use city land supply as the instrumental variable for the housing price variations. The city land supply directly affected housing price and then the implementation of the house purchase restriction policies. It is, however, unclear that how city land supply affects P2P borrowing/lending behaviors because this instrument captures only the land supply which is based on the geographic situation of the city. Thus, the land supply our instrument should reasonably satisfy the exclusion restriction. Table 8 reports the two-stage regression results. Column (1) reports the second-stage regression results on P2P borrowing costs. The marginal effect (coefficient estimates) on the instrumented treatpost_2016 is negative and significant, suggesting that 2016 home purchase restriction policy leads to a lower borrowing costs for the treated groups.

[Insert Table 8 Here]

6.2 The Impact of the 2014 Home-purchase Release on Houseowners

Also, we use the relief of home purchase restriction policy that announced and implemented in 2014 to substantiate our arguments. Before the analysis of the 2014 home-purchase relieve and its impact on P2P borrowing costs, we estimate the evolution of houseowners in the treatment and control groups before the policy shock. Based on the same methodology as the 2016 shock, we could conclude from Table 9 that the common trend assumption is valid since all the interaction terms, β_3, are insignificant.

We further test the effect of home-purchase release on the houseowners through the following DD and DDD model:

$$Interest_{irt} = \beta_0 + \beta_1 Treatpost\_2014_i + \beta_2\ Control_{ir} + \alpha_{irt} \quad (10)$$

$$Interest_{irt} = \beta_0 + \beta_1 Treatpost\_2014_i * has\_house + \beta_2 Treatpost_{2014_i} + \beta_3 has\_house + Control_{ir} + \alpha_{irt} \quad (11)$$

where $Treatpost\_2014_i$ is an indicator equal to one for treatment regions after the 2014 policy shock and zero otherwise. The definitions of other variables are the same as equation (2). α_{irt} represents a full set of city, month, year, and job characteristics fixed effects, with standard errors clustered at the city and month*year level to account for any correlations of the error terms within each firm.

[Insert Table 9 Here]

From DD and DDD results shown in Table 10, we could identify that the treated houseowners' P2P borrowing interest rates increase significantly rather than those of control regions after the announcement of home-release restriction in their cities. This outcome means that treated houseowners could tend to achieve a higher borrowing cost to crowdfund in the P2P platform since lenders perceive that the announcement of home-purchase release indicates a negative signal for treated houseowners. We could also find that the P2P borrowing interest rates for treated households without house has No significant differences compared to those of control regions. As a result, the 2014 house purchase release policy has a counter effect on P2P borrowing costs compared to the 2016 house purchase restriction policy. This result supports our main findings and mechanism while further alleviating potential time trends of the city effects on the changes of the P2P borrowing costs in 2016 shock.

[Insert Table 10 Here]

6.3 The Impact of the 2016 Home-purchase Release on Houseowners based on Borrower Fixed Effects

Although we apply DID estimation based on three exogenous shocks to account for the potential endogeneity, a concern related to the repeated cross-sectional nature of tour P2P lending data setting is still valid, since we still suffer from the omitted variable bias from various possible unobserved individual characteristics. Therefore, we carry a serious of robustness checking estimations by adding borrower's fixed effect to alleviate the endogeneity consideration. The equation and the definitions of variables are the same as equation (2). However, we only control for loan amount and include city and borrower fixed effects, with standard errors clustered at the city and borrower level to account for any correlations of the error terms within each firm. The borrower's fixed effect targets on the same person who participated in the P2P lending market both before and after the shocks. The robustness test result from Table 11 confirms that our main conclusions remain unchanged, the borrowing costs drops 5.2% after the 2016 house purchase restriction policy compared to this house owner's borrowing costs before the 2016 policy, while the borrowing costs increase 46.8% after the 2014 house purchase release policy compared to this houseowner's borrowing costs before the 2014 policy. This result suggests that our previous results are Not driven by the unobserved time-invariant personal characteristics.

[Insert Table 11 Here]

6.4 Winsorized Interest Rate and Loan Amount

To account for the consideration that the extreme observations in borrowing interest rates and loan amount might gave an impact on the main research results the 95% and 99% winsorization over borrowing interest rates and loan amount was applied into the previous DiD settings in cities with and without implementing 2016 house purchase restriction policies. The result is robust with the previous finding as shown in Table 12.

[Insert Table 12 Here]

6.5 Weighted Regression by Cities' Transactions

To account for the concerns that the numbers of observations in different cities is unbalanced, we conduct robustness test by giving weights for the number of observations in different cities. As shown in Table 13, based on the weighted numbers of observation in different cities, the research results are robust with the previous findings.

[Insert Table 13 Here]

6.6 The Speed of Crowdfunding, Number of Investors per loan and the Probability of a Successful Loan

Admittedly, our results are all equilibrium loan interest rates that formed by both borrowers and lenders in the P2P market, and we hypothesize such reduction in interest rate is mainly driven by the borrowers rather than the internal algorithm of the P2P platform. To answer this question, we attempt to isolate the lenders' reactions. In particular, we test how the speed of crowdfunding, number of investors per loan and the probability of a successful funding were affected after the restriction policy. We still apply the previous DD settings for these three tests.

$$dur\_min_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i * has\_house + \beta_2 Treatpost_{2016_i} + \beta_3 has\_house + \beta_4\ Control_{ir} + \alpha_{irt} \quad (12)$$

$$number\_lenders_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i * has\_house + \beta_2 Treatpost\_2016_i + \beta_3 has\_house + \beta_4\ Control_{ir} + \alpha_{irt} \quad (13)$$

$$success_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i * has\_house + \beta_2 Treatpost_{2016_i} + \beta_3 has\_house + \beta_4 Control_{ir} + \alpha_{irt} \quad (14)$$

Where $number\_lenders_{irt}$, dur_Min_{irt} are the numbers of lenders who invest their funds to achieve borrowers' funding goal and duration of the P2P borrowing activities. And *success* is a dummy variable with one as household successfully funded from P2P platform while zero fails. For equation (12) (13), the definitions of variables are the same as equation (2). α_{irt} represents a full set of city, month, year, and job characteristics fixed effects, with standard errors clustered at the city and month*year level to account for any correlations of the error terms within each firm. The estimation result from Table 15 shows that the speed of crowdfunding is 3.556 minutes less for treated houseowners compared with controlled ones after the shock, which demonstrates that treated houseowners could borrow significantly faster in the P2P platform compared with their counterparts due to the home-purchase restriction. In addition, the estimation result from Table 15 shows that the number of lenders per loan increase by 9.3 for the treated houseowners, which shows that more lenders are inclined to invest their money to the treated borrowers after the shock. This result echoes to our main finding regarding the advantages that treated houseowners could achieve after the shock and responds to the previous theories of house borrowing collateral effect since housing value has also been seen as an explicit asset valued by banks or other financial intermediations.

[Insert Table 14 Here]

[Insert Table 15 Here]

For equation (14), *Control$_i$* is the control variable that includes interest rate or borrowing amount. α_{irt} represents city, year, month and borrower fixed effects, with two-way standard error clustered at both borrower and city level. The definitions of the other variables are the same as equation (2). We only use borrower fixed effect since we only eye on the borrowers who borrow before and after the policy shock. This could avoid the inconsistent borrowing behaviors before and after the shock. Other variables and sets follow those stated before. Since the data of success rate could only be access before 2016, we only target on the 2014 home-purchase release policy. The estimation result from Table 16 shows that success rate for treated P2P household borrowing activities drop 2.2% compared to the control group. This result echoes to our main finding regarding the lenders perceives as a negative effect for treated houseowners' house value after the shock and thus lenders are less inclined to respond to in the treated borrowers' borrowing requests.

[Insert Table 16 Here]

VII. Discussion

the Effect of the 2016 House-purchase Restriction on P2P household default rates

Our previous study only evaluates the advantages that the affected household might be perceived by the lenders after the policy shock. We also like to see the borrowers' actual post performance after the shock. Therefore, we target on P2P household default rates conditioning on the success of a deal. We still apply the previous DD settings for these three tests.

$$default_{irt} = \beta_0 + \beta_1 Treatpost\_2016_i * has\_house + \beta_2 Treatpost_{2016_i} + \beta_3 has\_house + Control_{ir} + \alpha_{irt} \quad (15)$$

Where $default$ is 1 represents P2P borrowers with P2P defaults and 0 otherwise, the definitions of variables are the same as equation (2). α_{irt} represents a full set of city, month, year, and job characteristics fixed effects, with standard errors clustered at the city and month*year level to account for any correlations of the error terms within each firm. The estimation result from Table 17 shows that P2P borrowers have 0.1% less defaults after the 2016 home-purchase release policy. This result echoes to our main finding regarding the advantages that treated houseowners could achieve after the shock, which makes them less likely to default.

[Insert Table 17 Here]

VIII. Conclusion

Household borrowing costs and related activities in P2P lending platform are subject to great variations for diverse reasons. Government policies are often the causes to create such distortions. Based on a quasi-natural experiment in the announcement of home-purchase restriction policies in multiple of cities in China in late September and early October, our study uses a series of DD and DDD models to estimate the impact house-purchase restriction policy on houseowners' P2P borrowing interest rates. We identify that home-purchase restriction is perceived by the lenders as a positive signal for borrowers increasing house value, which is led to decrease treated houseowners' P2P borrowing interest rates and slowing down the speed of the P2P crowdfunding, while increasing the number of lenders per loan who fund this borrowing requests through the channel of borrowing collateral effect. Since the home-purchase restriction policy implies the increase of the house wealth for the houseowners, lends would consider that houseowners tend to have a higher borrowing collateral value that decreases their default risks and strengthen their financial position in the financial market. We further proxy the heterogenous effect of city macroeconomic conditions as housing price appreciation potential on the policy shock. The results show that P2P interest rates decrease sharper for treated houseowners whose cities have a higher GDP growth rate, population growth rate, and residential investment growth rate. Therefore, economic growth of the city is able to increase the house value, which offers household a stronger financial position and more advantages in the P2P borrowing activities. This estimation further comfirms the collateral effect.

The further estimation considering household loans and age confirms the borrowing collateral effect that houseowners with borrowing constraints benefit more from the policy effect while age profile does Not make significant impacts on this relationship. In addition, we use the 2014 home-purchase release policy as a counter shock of the 2016 home-purchase release policy. The results reverse the findings from 2016 shock and confirms the borrowing collateral effect of the variation of the borrowing costs. The robustness check results using borrower fixed effects and land supply as instrumental variable also support our findings.

Our research contributes to the literature by exploring the effect of government housing policies on households and investors at the Chinese online micro-financing market. We examine the collateral channel of the house wealth effect on household borrowing activities based on housing price swings. We also shed new light on the individual and city factors influencing P2P borrowing activities by connecting macro-economic shocks with micro-financing variations.

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Table 1 The Specific Date of Restrictive House-purchase Policy for 21 cities in 2016

City Name	Restrictions Date
Beijing	30-09-2016
Tianjing	01-10-2016
Langfang	01-04-2016
Shanghai	27-05-2016
Nanjing	26-09-2016
Wuxi	02-10-2016
Suzhou	04-10-2016
Hangzhou	28-09-2016
Hefei	02-10-2016
Fuzhou	07-10-2016
Xiamen	06-10-2016
Nanchang	08-10-2016
Jinan	03-10-2016
Zhengzhou	01-10-2016
Wuhan	03-10-2016
Guangzhou	04-10-2016
Shenzhen	04-10-2016
Zhuhai	04-10-2016
Foshan	07-10-2016
Dongguan	07-10-2016
Chengdu	01-10-2016

Source: Cao et.al, (2015)

Table 2 The Specific Date of Restrictive House-purchase Policy Relief for 35 cities in 2014

City Name	Restrictions Relief Date
Nanjing	21-09-2014
Hangzhou	29-08-2014
Tianjin	01-08-2014
Zhengzhou	09-08-2014
Chengdu	22-07-2014
Wuxi	30-08-2014
Hefei	02-08-2014
Jinan	10-07-2014
Wuhan	24-09-2014
Xiamen	01-07-2014
Fuzhou	01-08-2014
Nanchang	12-08-2014
Shijiazhuang	26-09-2014
Taiyuan	04-08-2014
Hohhot	24-06-2014
Shenyang	10-06-2014
Dalian	03-09-2014
Changchun	19-07-2014
Harbin	16-08-2014
Xuzhou	01-08-2014
Ningbo	30-07-2014
Wenzhou	30-07-2014
Jinhua	01-08-2014
Qingdao	01-08-2014
Changsha	06-08-2014
Nanning	01-10-2014
Haikou	22-07-2014
Sanya	07-10-2014
Guiyang	01-09-2014
Kunming	11-08-2014
Xian	01-09-2014
Lanzhou	03-09-2014
Xining	10-09-2014
Yinchuan	22-08-2014
Urumqi	01-08-2014

Source: Cao et.al, (2015)

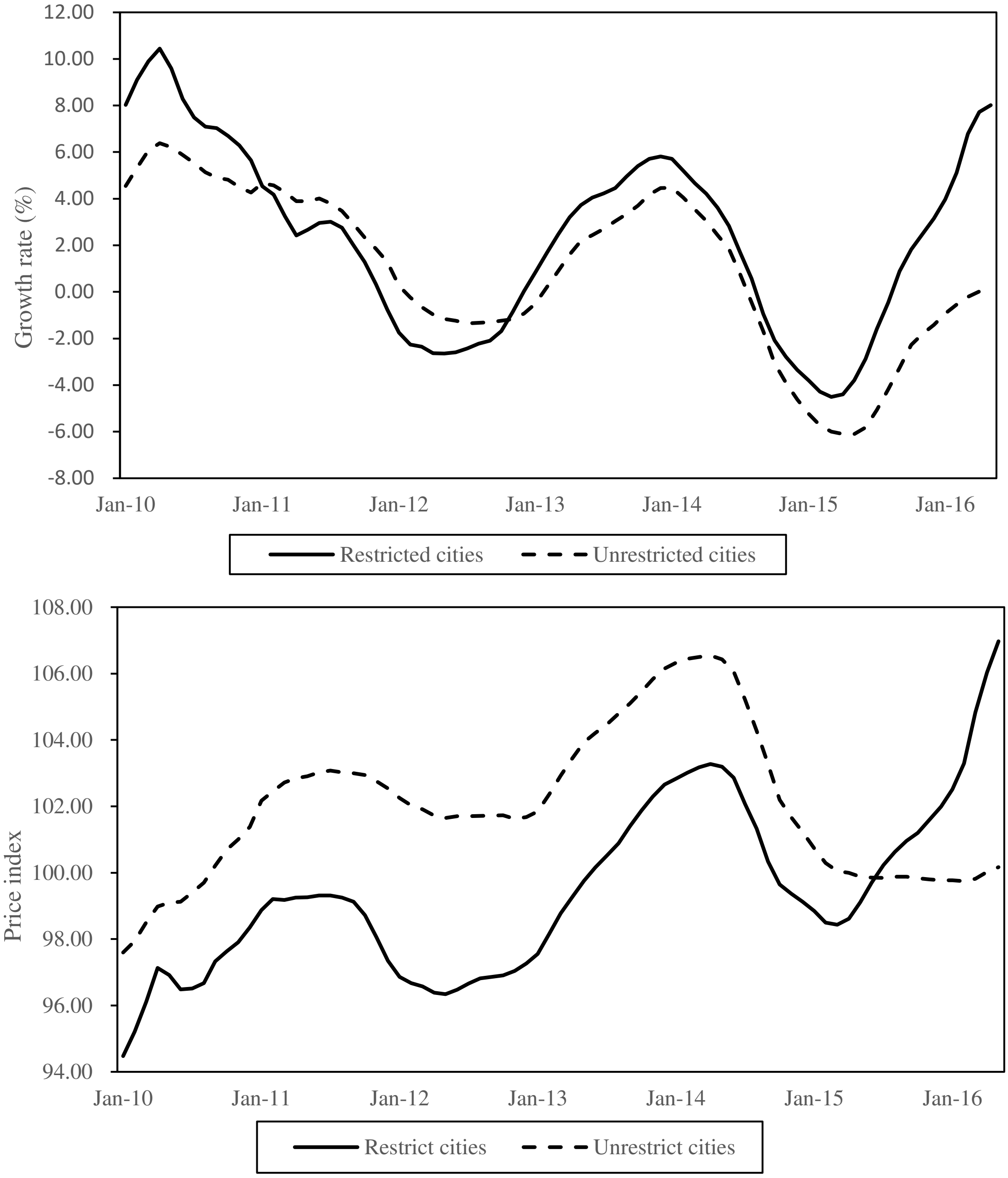

Figure 1 Housing price index fluctuation over 2010-2016

The figure plots the housing price index and month-on-month growth rate over 2010-2016 for 70 cities. Restricted cities are the cities implementing the housing purchase restriction policy between September of 2010 and March of 2011. Data is from WIND database.

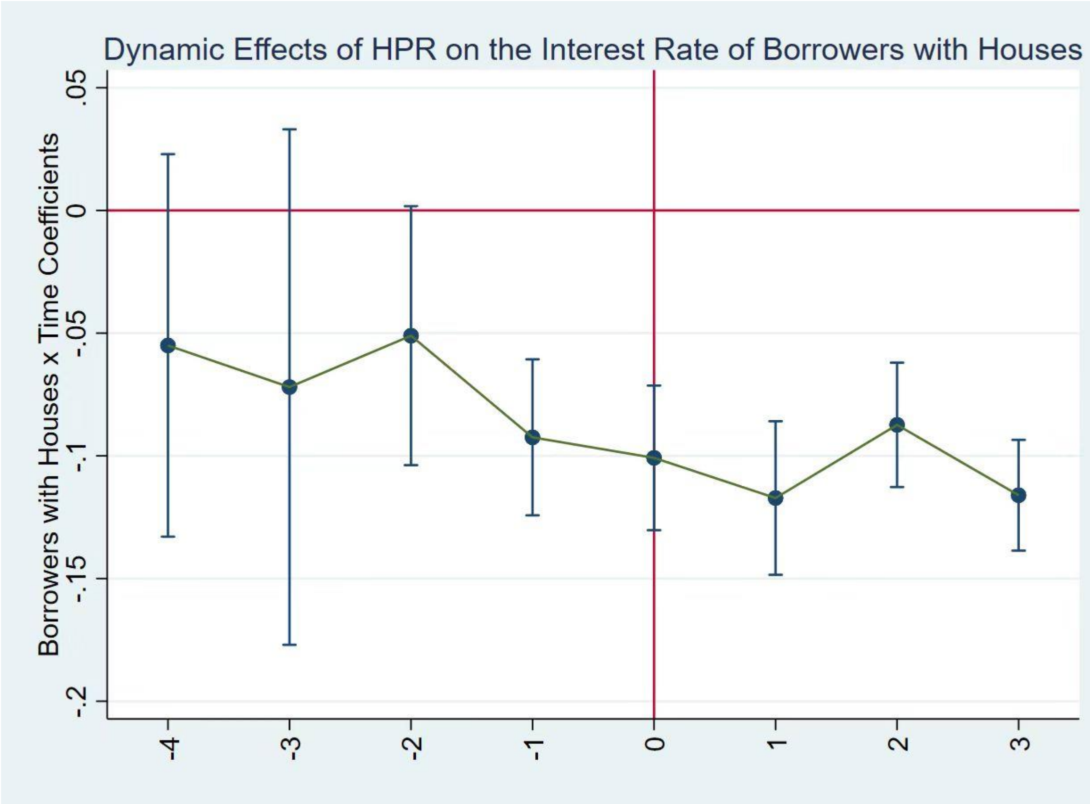

Figure 2 Dynamic effects of HRP on the Interest Rate of Borrowers with Houses

Table 3 Summary statistics

This table presents summary the mean for characteristics of funded loans, borrowers and cities in the pretreatment periods (1 Jan 2016 and 26 Sep 2016). Data on loans, borrowers is from Renrendai P2P platform. City characteristic data is from CSMAR. Standard deviations are in brackets. For The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

	Has house			Has No house	All sample
	Control	Treated	Difference		
VARIABLES	(1)	(2)	(3)	(4)	(5)
amount	87332.88	97766.27	-10433.4***	74255.82	82832.55
	(46853.46)	(51866.5)		(46240.19)	(48134.89)
interest	10.287	10.263	0.030***	10.161	10.225
	(0.479)	(0.488)		(0.51)	(0.499)
Duration(hour)	9.818	11.205	-1.392***	11.519	10.81
	(16.418)	(18.795)		(19.097)	(18.071)
No. investor	113.468	130.114	-16.600***	100.215	109.806
	(130.61)	(147.412)		(124.655)	(130.981)
gender	0.311	0.299	0.012***	0.34	0.323
	(0.463)	(0.458)		(0.474)	(0.468)
age	38.362	37.514	0.850***	34.531	36.465
	(8.547)	(8.258)		(8.196)	(8.539)
graduation	1.27	1.392	-0.122***	1.277	1.291
	(0.717)	(0.682)		(0.681)	(0.697)
married	0.763	0.756	0.007	0.608	0.69
	(0.426)	(0.43)		(0.488)	(0.462)
houseLoan	0.539	0.624	-0.085***	0	0.302
	(0.498)	(0.484)		(0)	(0.459)
workYears	2.161	2.044	0.117***	1.837	1.994
	(1.022)	(1.045)		(1.072)	(1.06)
salary	3.289	3.941	-0.652***	3.223	3.355
	(1.206)	(1.247)		(1.147)	(1.211)
GDP growth rate	0.085	0.091	-0.006***	0.09	0.088
	0.045	0.018		(0.029)	(0.035)
population growth rate	0.003	0.006	-0.002***	0.007	0.006
	(0.012)	(0.024)		(0.021)	(0.018)
residential investment growth rate	0.092	0.145	-0.053***	0.128	0.116
	(0.145)	(0.051)		(0.104)	(0.119)
Observations	34654	13162		41101	88917

Table 4 Test common trend prior to policy intervention

The table reports common trend before the home-purchase restriction policy shock, which shows the common trend assumption of DD estimation for all of the households. The dependent variable is borrowing cost. treated is a dummy variable with one for the cities implementing home-purchase restriction while zero otherwise. Lag_4_Quarters, Lag_3_Quarters, Lag_2_Quarters, Lag_1_Quarters is 4 quarters, 3 quarters, 2 quarters, and 1 quarter before the home-purchase restriction policy shock in each city, respectively. City, month*year, job characteristics (office_type, salary, office_size) fixed effects are controlled in all estimations. Standard errors are clustered at city level. Column 1 adds loan amount as control variable. Column 2 adds individual characteristics (age, gender, work_year, married, graduation) and loan amount as control variables. Column 3 adds individual characteristics and city_trend as control variables. Column 4 adds individual characteristics (age, gender, has_house, work_year, married graduation), loan amount, and city_trend as control variables. The definition of variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Interest Rate	(1)	(2)	(3)	(4)
Lag_4_Quarters*treated	0.020	0.020	0.025	0.026
	(0.62)	(0.62)	(0.78)	(0.81)
Lag_3_Quarters*treated	-0.002	-0.002	0.006	0.002
	(-0.05)	(-0.05)	(0.19)	(0.06)
Lag_2_Quarters*treated	-0.031	-0.031	-0.030	-0.028
	(-1.52)	(-1.52)	(-1.48)	(-1.43)
Lag_1_Quarter*treated	-0.007	-0.007	-0.004	-0.004
	(-0.58)	(-0.60)	(-0.38)	(-0.34)
Controls (Loan Amount)	Yes	Yes	No	Yes
Controls (Individual Characteristics)	No	Yes	Yes	Yes
Controls (City Trend)	No	No	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Job Characteristics FE	Yes	Yes	Yes	Yes
Observations	454,489	454,489	454,489	454,489
R-squared	0.788	0.788	0.787	0.791

Table 5 The effect of the 2016 housing value fluctuation on borrowing costs

The table reports the DD and DDD estimation results of the effect of the 2016 house wealth fluctuation on the borrowing cost. The results are for the subsample with households having house, the subsample with households without house, and the full sample with households with or without house. The dependent variable is borrowing costs. Treatpost_2016 is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Column (1), (3), (5) are basic regression without any control variables. Column (2), (4), (6) add individual characteristics (age, gender, work_year, married, graduation) and loan amount as control variables. The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable=Interest Rate	(1)	(2)	(3)	(4)	(5)	(6)
Treatpost_2016	-0.013***	-0.013***	0.012	0.009		
	(0.001)	(0.002)	(0.011)	(0.011)		
Treatpost_2016 × Has house					-0.026***	-0.028***
					(0.009)	(0.009)
Controls (Loan Amount)	No	Yes	No	Yes	No	Yes
Controls (Individual Characteristics)	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Job Characteristics FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	100,546	100,546	130,313	130,313	215,606	215,606
R-squared	0.440	0.449	0.392	0.398	0.511	0.519

Table 6 Mechanism tests of the effect of housing value change on borrowing costs

The table shows the results of the mechanism tests of the effect of housing value change on borrowing outcomes. The dependent variable is borrowing cost. Column (1) and (3) report DD estimation results for subsample with households having house. Column (2) and (4) report DDD estimation results for full sample with households with or without house. Treatpost_2016 is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. Age is borrowers' age. Loan is a dummy variable equals to 1 if the household has house loan or car loan and 0 otherwise. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Individual characteristics (age, gender, work_year, married, graduation) and loan amount are added as control variables. The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Interest Rate	(1)	(2)	(3)	(4)
Treatpost_2016 × Loan	-0.042***			
	(0.011)			
Treatpost_2016 × Has house × Loan		-0.065***		
		(0.019)		
Treatpost_2016 × Age			0.001	
			(0.000)	
Treatpost_2016 × Has house × Age				0.001
				(0.000)
Controls (Loan Amount)	Yes	Yes	Yes	Yes
Controls (Individual Characteristics)	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Job Characteristics FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	96,062	215,606	96,062	215,606
R-squared	0.547	0.519	0.546	0.519

Table 7 Heterogeneity of the 2016 house value effect on borrowing costs: city characteristics

The table presents heterogeneous effect of the house value change from city characteristics. The dependent variable is borrowing cost. The results are for sub-sample with households having house(s). Treatpost_2016 is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. GDP Growth, Population Growth Rate, and investment Growth Rate is the GDP, population, and fixed assets investment growth rate at each city, respectively. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Column (1), (3), (5) are basic regression without any control variables. Column (2), (4), (6) add individual characteristics (age, gender work_year, married, graduation) and loan amount as control variables. The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Interest Rate	(1)	(2)	(3)	(4)	(5)	(6)
Treatpost_2016 × GDP Growth Rate	-0.170***	-0.264***				
	(0.037)	(0.087)				
Treatpost_2016 × Population Growth Rate			-0.062***	-0.001		
			(0.016)	(0.041)		
Treatpost_2016 × Investment Growth Rate					-0.069***	-0.071***
					(0.002)	(0.008)
Controls (Loan Amount)	No	Yes	No	Yes	No	Yes
Controls (Individual Characteristics)	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Job Characteristics FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	98,394	98,394	98,394	98,394	98,394	98,394
R-squared	0.437	0.446	0.437	0.446	0.437	0.446

Table 8 Instrument Variable test on Borrowing Cost

This Table shows the IV test of House Restriction Policy on online borrowing cost. Construction Land Supply and Planned Construction Land Supply are employed as Instrumental variables for Treatpost_2016, which is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Individual characteristics (age, gender, work_year, married, graduation) and loan amount are control variables. The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)
Dependent variable =Interest Rate	IV=Construction Land Supply	IV=Planned Construction Land Supply	IV=Planned Construction Land Supply & Construction Land Supply
Treatpost_2016	-0.781***	-0.781***	-0.781***
	(0.150)	(0.150)	(0.150)
Controls (Loan Amount)	Yes	Yes	Yes
Controls (Individual Characteristics)	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Job Characteristics FE	Yes	Yes	Yes
Observations	136,357	136,357	136,357
R-squared	-0.282	-0.282	-0.282

Table 9: 2014 Relief Event (Test common trend)

The table reports common trend before the 2014 home-purchase release policy shock, which shows the common trend assumption of DD estimation for all of the households. The dependent variable is borrowing cost. treated is a dummy variable with one for the cities implementing home-purchase restriction while zero otherwise. Lag_4_Quarters, Lag_3_Quarters, Lag_2_Quarters, Lag_1_Quarters is 4 quarters, 3 quarters, 2 quarters, and 1 quarter before the 2014 home-purchase release policy shock in each city, respectively. City, month*year, job characteristics (office_type, salary, office_size) fixed effects are controlled in all estimations. Standard errors are clustered at city level. Column 1 adds loan amount as control variable. Column 2 adds individual characteristics (age, gender, work_year, married, graduation) and loan amount as control variables. Column 3 adds individual characteristics and city_trend as control variables. Column 4 adds individual characteristics (age gender has_house work_year married graduation), loan amount, and city_trend as control variables. The definition of variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Interest Rate	(1)	(2)	(3)	(4)
Lag 4 Quarters	-0.076	-0.080	-0.075	-0.056
	(0.060)	(0.062)	(0.066)	(0.065)
Lag 3 Quarters	-0.070	-0.072	-0.071	-0.075
	(0.081)	(0.080)	(0.076)	(0.079)
Lag 2 Quarters	-0.066	-0.079	-0.076	-0.092**
	(0.051)	(0.048)	(0.047)	(0.048)
Lag 1 Quarter	0.000	-0.015	-0.010	-0.053
	(0.039)	(0.038)	(0.038)	(0.037)
Controls (Loan	Yes	Yes	No	Yes
Controls (Individual	No	Yes	Yes	Yes
Controls (City Trend)	NO	No	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Job Characteristics FE	Yes	Yes	Yes	Yes
Observations	551,177	551,162	551,162	201,835
R-squared	0.812	0.815	0.813	0.520

Table 10 The effect of the 2014 housing value fluctuation on borrowing cost

The table reports the DD and DDD estimation results of the effect of the 2014 house wealth fluctuation on the borrowing cost. The results are for subsample with households having house, subsample with households without house, and the full sample with households with or without house. The dependent variable is borrowing cost. Treatpost_2014 is defined as 1 for cities after implementing 2014 home-purchase release policy and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Column (1), (3), (5) are basic regression without any control variables. Column (2), (4), (6) add individual characteristics (age, gender, work_year, married, graduation) and loan amount as control variables. The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Interest Rate	(1)	(2)	(3)	(4)	(5)	(6)
Treatpost_2014	(0.042)	(0.036)	(0.092)	(0.087)		
	(2.28)	(2.76)	(1.23)	(1.20)		
Treatpost_2014 × Has_house					0.077**	0.075**
					(0.034)	(0.035)
Controls (Loan	NO	Yes	NO	Yes	NO	Yes
Controls (Individual Characteristics)	NO	Yes	NO	Yes	NO	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Job Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	113,130	113,126	95,616	95,611	193,163	193,154
R-squared	0.505	0.521	0.520	0.529	0.584	0.593

Table 11: The effect of the housing value fluctuation on borrowing costs based on borrower fixed effects

The table reports the DD and DDD estimation results of the effect of the house wealth fluctuation on borrowing cost. The results are for subsample with households having house, subsample with households without house, and the full sample with households with or without house. The dependent variable is borrowing costs. Treatpost_2016 is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. Treatpost_2014 is defined as 1 for cities after implementing 2014 home-purchase release policy and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. City and borrower fixed effects are controlled in all estimations. Standard errors are clustered at city and borrower level. loan amount is the control variable. The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Interest Rate	(1)	(2)
Treatpost_2016 × Has house	-0.052***	
	(0.018)	
Treatpost_2014 × Has house		0.468***
		(0.139)
Controls (Loan Amount)	Yes	Yes
City FE	Yes	Yes
Borrower FE	Yes	Yes
Observations	23,871	11,236
R-squared	0.720	0.654

Table 12: Winsorized interest rate and loan amount

The table reports the robustness check results of the effect of the 2016 house-purchase restriction on P2P household borrowing costs based on winsorized interest rate and loan amount. The results are for the subsample with households having house and the full sample with households with or without house. Treatpost_2016 is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Column (1), (3), (5) (7) are basic regression without any control variables. Column (2), (4), (6), (8) add individual characteristics (age, gender, work_year, married, graduation) and loan amount as control variables. Column (1) (2) (5) (6) winsorize interest rates and loan amount at 95%. Column (3) (4) (7) (8) winsorize interest rates and loan amount at 99%. The definition of variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Interest Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatpost_2016	-0.015***	-0.014***	-0.013***	-0.013***				
	(0.000)	(0.002)	(0.001)	(0.002)				
Treatpost_2016 × Has house					-0.023***	-0.024***	-0.026***	-0.027***
					(0.008)	(0.007)	(0.009)	(0.008)
Controls (Loan Amount)	No	Yes	No	Yes	No	Yes	No	Yes
Controls (Individual Characteristics)	No	Yes	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Job Characteristics FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	100,546	100,546	100,546	100,546	215,606	215,606	215,606	215,606
R-squared	0.460	0.467	0.440	0.450	0.481	0.489	0.513	0.522

Table 13: Weighted numbers of observation in different cities

The table reports the robustness check results of the effect of the 2016 house-purchase restriction on P2P household borrowing costs based on the weighted numbers of observation in different cities. The results are for the subsample with households having house and No house, and the full sample with households with or without house. Treatpost_2016 is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Column (1), (3), (5) (7) are basic regression without any control variables. Column (2), (4), (6), (8) add individual characteristics (age, gender, work_year, married, graduation) and loan amount as control variables. The definition of variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Interest Rate	(1)	(2)	(3)	(4)	(5)	(6)
Treatpost	-0.010***	-0.009***	0.019*	0.015		
	(0.001)	(0.002)	(0.010)	(0.010)		
Treatpost × Has house					-0.031***	-0.031***
					(0.009)	(0.009)
Controls (Loan Amount)	No	Yes	No	Yes	No	Yes
Controls	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Job	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.452	0.461	0.389	0.395	0.544	0.551

Table 14 The effect of the housing value fluctuation on duration of the P2P crowdfunding procedures.

The table reports the DDD estimation results of the effect of the house wealth fluctuation on the duration of the P2P crowdfunding procedures. The dependent variable is duration of the P2P crowdfunding procedures in minutes. The results are for full sample with households with or without house. Treatpost_2016 is defined as 1 for cities after implementing the 2016 home-purchase restriction and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Individual characteristics (age, gender, work_year, married, graduation) and loan amount are added as control variables. The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Duration_minutes	(1)	(2)
Treatpost_2016 × Has house	-2.796*	-3.556**
	(1.592)	(1.653)
Controls (Loan Amount)	No	Yes
Controls (Individual Characteristics)	No	Yes
City FE	Yes	Yes
Job Characteristics FE	Yes	Yes
Year FE	Yes	Yes
Month FE	Yes	Yes
Observations	50,781	50,781
R-squared	0.012	0.015

Table 15 The effect of the housing value fluctuation on the number of investors who invest in the P2P crowdfunding procedures

The table reports the DD and DDD estimation results of the effect of the 2016 house wealth fluctuation on the number of investors who invest in the P2P crowdfunding procedures. The results are for subsample with households having house, subsample with households without house, and the full sample with households with or without house. The dependent variable is the number of investors who invest in the P2P crowdfunding procedures. Treatpost_2016 is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Column (1), (3), (5) are basic regression without any control variables. Column (2), (4), (6) add individual characteristics (age, gender, work_year, married, graduation) and loan amount as control variables. The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =No. of Investors	(1)	(2)	(3)	(4)	(5)	(6)
Treatpost_2016	5.493	5.689*	10.291***	3.526		
	(3.994)	(2.874)	(3.376)	(3.028)		
Treatpost_2016 × Has house					12.883***	9.300***
					(3.861)	(3.013)
Controls (Loan Amount)	No	Yes	No	Yes	No	Yes
Controls	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Job	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	100,546	100,546	130,313	130,313	215,606	215,606
R-squared	0.079	0.343	0.080	0.310	0.112	0.359

Table 16 The effect of the housing value fluctuation on borrowing success rates

The table reports the DDD estimation results of the effect of the house wealth fluctuation on borrowing success rates. The results are for full sample with households with or without house. The dependent variable is borrowing success rates. Treatpost_2014 is defined as 1 for cities after implementing 2014 home purchase release policy and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. City and borrower fixed effects are controlled in all estimations. Standard errors are clustered at city and borrower level. loan amount and interest rate are the control variables. The definition of the variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Success Dummy	(1)	(2)	(3)	(4)
Treatpost 2014 × Has house	0.023***	0.026***	0.017**	0.021***
	(0.008)	(0.009)	(0.008)	(0.008)
Controls (Loan Amount)	No	Yes	No	Yes
Controls (Interest)	No	No	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	227,025	227,025	227,025	227,025
R-squared	0.630	0.648	0.634	0.651

Table 17: Ex-post Default (Late/No Payment) the effect of the 2016 house-purchase restriction on P2P household default rates

The table reports the robustness check results of the effect of the 2016 house-purchase restriction on P2P household default rates. The results are for the subsample with households having house, the subsample with households having No house, and the full sample with households with or without house. The dependent variable is default rate, which is 1 with P2P defaults and 0 otherwise. Treatpost_2016 is defined as 1 for cities after implementing 2016 home-purchase restriction and 0 otherwise. Has_house is defined as 1 for P2P household with house and 0 otherwise. City, year, month, and job characteristics fixed effects are controlled in all estimations. Standard errors are clustered at city and month*year level. Column (1), (3), (5) (7) are basic regression without any control variables. Column (2), (4), (6), (8) add individual characteristics (age, gender, work_year, married, graduation) and loan amount as control variables. The definition of variables refers to Appendix table. ***, ** and * stand for significant at the 1%, 5% and 10% levels, respectively.

Dependent variable =Default Dummy	(1)	(2)	(3)	(4)	(5)	(6)
Treatpost_2016	-0.002*	-0.002*	0.000	0.000		
	(0.001)	(0.001)	(0.001)	(0.001)		
Treatpost_2016 × Has house					-0.001**	-0.001**
					(0.000)	(0.000)
Controls (Loan Amount)	No	Yes	No	Yes	No	Yes
Controls	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Job Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	99,947	99,947	129,802	129,802	214,747	214,747
R-squared	0.282	0.284	0.063	0.064	0.388	0.388

Appendix Table A1: Variable Definitions

VARIABLE	DESCRIPTION
Dependent Variables	
Funding Success	A dummy variable with one if the listing posted by a borrower is successfully funded; zero otherwise
Interest Rate	The percentage of principle charged by loan platform to a borrower
Duration of processing	Logarithm of minutes took by the borrower of the funding process
No. of lenders	The total number of lenders who are willing to invest on the loan
Loan amount	Logarithm of loan amount received by the borrower.
Gender	A dummy variable with one if the borrower is a female; zero otherwise
Marital Status	A dummy variable with one is married and zero otherwise
Age	The age of the borrower.
Salary	A variable indicating a borrower's monthly income level, where n=0 represents whose wage is No more than 1000 RMB, n=1 means monthly income is between 1000-2000 RMB, n=2 means monthly income is between 2000-5000 RMB; n=3 means monthly income is between 5000-10000RMB; n=4 means monthly income is between 10000-20000RMB; n=5 means monthly income is between 20000-50000 RMB; n=6 means monthly income is above 50000 RMB.
Graduation	A variable indicating the education level of borrowers, where n=0 (if the borrower is high school certificate and below), n=1 (if the borrower is college-degree holder), n=2 (if the borrower is university- degree holder), n=3(if the borrower is with postgraduate degree and above)
Has house	A dummy variable with one if the borrower owns a house; zero otherwise
House/Car Loan	A dummy variable with one if the borrower has either a house loan or a car loan; zero otherwise
Work Years	A variable showing the working experience of borrowers, where n=0 (if the working experience is No more than 1 year), n=1(if a borrower has 1-3 years' working experience), n=2 (if a borrower has 3-5 years' working experience), n=3 (if a borrower has more than 5 years' working experience)
GDP Three-Years' Average Growth Rate	Three-year （2013-2015）average city GDP growth rate
Population Two-Years' Average Growth Rate	Three-year (2013-2015）average city population growth rate
Residential Investment Three-Years' Average Growth Rate	Two-year (2014-2015) city residential investment growth rate