Can Technology Undermine Macroprudential Regulation?

Evidence from Online Marketplace Credit in China

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Abstract

We study the relationship between FinTech and credit regulation. We exploit the tightening of mortgage LTV caps in various Chinese cities in 2013, and a novel database covering all transactions at RenrenDai, a leading marketplace credit platform. Marketplace loans increase in the affected cities, consistent with borrowers tapping online marketplace credit to circumvent the regulation. Lenders do not adjust their pricing to the influx of new borrowers, although their loans exhibit worse ex-post performance. Symmetric effects are associated with a loosening of LTV caps in 2015 and 2016. Our test provides evidence on the capacity of marketplace credit to undermine credit regulation.

JEL codes: G23; G01; G28.

Keywords: peer-to-peer credit; household leverage; macroprudential regulation; loan-to-value caps; Chinese financial system.

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The years following the financial crisis of 2007-08 have witnessed the development and promotion of macroprudential rules aimed at containing household leverage.[1] Much emphasis has been placed on loan-to-value (LTV) caps, which prevent borrowing beyond a certain fraction of the value of the assets to be purchased with the loan.[2] LTV caps typically restrict household borrowing from traditional financial intermediaries, such as banks. That, however, might be too narrow if households have access to alternative, lightly regulated credit channels that allow them to circumvent limits on borrowing from regulated lenders. In this paper, we study one emerging – and so far neglected – such channel: online marketplace credit.

Using a novel dataset covering the universe of loans at a leading Chinese marketplace lending platform, and exploiting regulatory changes in the Chinese real estate market as shocks to credit demand, we provide comparatively clean evidence of marketplace lending allowing borrowers to bypass LTV caps. Our results point to the more general conclusion that regulatory arbitrage channels that used to be negligible may no longer be so, due to the development of new online financial intermediaries – such as marketplace credit.

By now rivaling traditional consumer loans in size and reach (Morse (2015)), marketplace credit has experienced double-digit growth in developed economies such as the U.S., where lending volumes

[1] There is a vast theoretical and empirical literature on the impact of (household) leverage on consumption, employment and output. Part of the theory contributions stress the role of collateralized lending, with arguments based on Stein (1995) and Kiyotaki and Moore's (1997) works, see e.g. Geanakoplos (2010), Gorton and Ordoñez (2014). Hall (2011) and Guerrieri and Lorenzoni (2017) show that high debt levels can exacerbate downturns even in the absence of collateralized lending. The empirical analyses of Lamont and Stein (1999), Almeida, Campello, Liu (2006), and Baker (2018) show that with high levels of household leverage, income shocks have a strong positive impact on house prices growth. In several studies Adelino, Schoar, and Severino (2012, 2016), Mian and Sufi (2009, 2011), Mian, Sufi, and Trebbi (2015) document the relationship between U.S. household leverage and the severity of the 2007-2009 crisis. Bordo (2008), Claessens, Kose, and Terrones (2012), and Schularick and Taylor (2012) find that crises are typically preceded by periods of rapid credit growth. Mian and Sufi (2010) and Di Maggio and Kermani (2017) document the real economy disruptions associated with high household leverage.

[2] See for instance Allen and Carletti (2011), Crowe, Dell'Arriccia, Igan, and Rabanal (2011), Claessens, Gosh, and Mihet (2013), Jácome and Mitra (2015). Claessens, Gosh, and Mihet (2013) document that LTV-based policies are the most widespread macroprudential tool, both in developed and emerging economies. Out of the 48 countries surveyed in their study, 44% imposed an LTV cap at least once between 2000 and 2010. In contrast, debt-to-income (DTI) caps, the other main macroprudential tool intended to limit household leverage were applied in only 9% of the countries in their sample.

amounted to \$77bn in 2015.[3] The fastest-growing marketplace credit market, however, is China, which is also estimated to be the largest in the world (Deer, Mi, and Yuxin (2015)), with volumes totaling over \$116bn (RMB 789bn) as of December 2018, and corresponding to about 20% of consumption loans to households provided by traditional banks.[4]

A marketplace credit company provides prospective borrowers and lenders with an online platform where they can trade. To see how marketplace credit can act as a channel to elude LTV caps, suppose a borrower intends to take out a mortgage with a bank to purchase a house worth \$100. Suppose further that there is a 50% LTV cap on the mortgage, and that the borrower can only commit \$40 as a down-payment. Other things equal, the LTV cap rules out the mortgage. If, however, the borrower can obtain an additional credit of \$10 on the platform (or if marketplace credit helps her free up funds for an amount of \$10, which can then be added to her down-payment), the bank can issue the mortgage and the LTV cap is circumvented.

Three features of marketplace credit make it especially conducive to this sort of regulatory arbitrage. First, it provides borrowers with a degree of anonymity, since online platforms typically impose much more modest disclosure requirements, and receive much less regulatory scrutiny, in comparison to banks and conventional financial intermediaries. In that respect, marketplace credit is similar to traditional non-bank sources of credit such as family and friends, payday lenders, etc. Second, and unlike those traditional sources, marketplace credit provides access to an unprecedentedly large potential funding pool – in principle, any lender on the platform. Third, marketplace credit companies have a lean structure and conduct most of their business online, without physical branches or loan officers. That reduces their costs, and can help them channel, for a given level of borrower risk, cheaper credit. Thus, borrowers can potentially find on a marketplace credit platform anonymous, abundant, and

[3] "As money pours into peer-to-peer lending, some see bubble brewing", *Bloomberg*, May 15, 2015.
[4] "Chinese P2P lending regulations target hucksters and risk-takers", *Financial Times*, August 24, 2016, and WDJZ (https://www.wdjz.com).

cheap credit that can fund the regulatory arbitrage scheme. Building on these ideas, we assess to what extent marketplace credit poses a vulnerability to LTV-based policies and contributes to fueling household debt creation.

Taking this question to the data confronts us with two empirical challenges. First, we are interested in gauging the capacity of marketplace credit *supply* to undermine LTV caps. But the equilibrium in the market for loans also depends on credit *demand*; and separating demand and supply is difficult, because the econometrician only observes ex post lending outcomes. Thus, an increase in marketplace loans could be due to inefficient lending induced by excess credit supply, but just as well to improved economic prospects raising credit demand. To separate the effects of marketplace credit demand and supply, we need a shock to the demand for marketplace credit which does not separately affect its supply.

Second, in order to trace out credit supply with demand shocks, we must control for potential supply-side drivers, mainly in the form of unobserved heterogeneity among marketplace lenders. For instance, lenders may differ in terms of their proximate knowledge, due to their expertise (Morse (2015)) or their ability to harness soft information for screening and monitoring (Freedman and Jin (2014), Lin, Prabhala, and Viswanathan (2013), Iyer et al. (2016)), or they may be subject to heterogeneous shocks affecting their credit supply. To the extent that lenders' characteristics such as these can vary with the exposure of their borrowers to a demand shock, the resulting simultaneous changes in credit demand and supply can confound the interpretation of any test. Thus, while we study the effects of a change in marketplace credit demand, we want to be able to hold the marketplace lending supply curve fixed.

We address these challenges exploiting the unique features of the setting of our test and the structure of our data. We study marketplace credit around a regulatory change in the Chinese real estate market, which took place in November 2013. The local governments of a number of cities imposed a 16.7% increase in the minimum down-payment required to obtain a mortgage for the purchase of a second home, raising it from 60% to 70% of the property's value. The objective was to slow down the growth in real estate prices, following a policy impulse in this direction by the Chinese central government (we discuss the background to the regulatory change in detail in Section II.A). Anecdotal evidence, however, suggests that real estate investors circumvented the new requirements, borrowing via online marketplace credit platforms to meet the increased down-payment.[5] Importantly, the regulatory change created a positive shock to marketplace credit demand, thus addressing our first empirical challenge.

We exploit this policy intervention in a difference-in-differences setting, studying changes in marketplace credit around this episode, for affected and un-affected cities. We assemble a novel, hand-collected database containing all loan applications and credit outcomes for a leading Chinese marketplace credit platform, RenrenDai (人人贷). Our database contains all the transactions executed within the platform, and it matches each borrower with her lenders.

Our results are consistent with marketplace lending providing an unregulated source of credit with the potential to undermine LTV caps. In the analysis, we are very careful about identification and what we can and cannot conclude; our baseline effects, however, are already visible in Figure 1, which plots loan application volumes at RenrenDai, for "treated" and "control" cities, around the last quarter of 2013. The lines corresponding to treated and control cities closely overlap over the period preceding the regulatory change. Following the last quarter of 2013, however, loan applications in the treated cities increase sharply relative to the control cities, consistent with an influx of applications to help meet the higher down-payments. While RenrenDai loan applications grow in both groups, due to the development of marketplace credit in China during our sample period, in the first six months of 2014 applications in

[5] "China to Crack Down on P2P Lenders," *Financial Times*, March 14, 2016.

the treated cities grow by 50%, as opposed to only 16% in the control cities, consistent with marketplace credit being instrumental to circumventing the regulatory LTV cap.

Our formal tests validate this visual check, and strengthen the case for a causal interpretation. City- and borrower-lender level regressions confirm the evidence from Figure 1. In particular, we leverage the depth of our data with the borrower-lender level regressions, which allow us to trace the impact of the marketplace lending demand shock controlling for lender × date fixed effects. These estimates compare the marketplace credit received by different borrowers from *the same lender at the same point in time*, thus holding credit supply capacity fixed and addressing our second empirical challenge.

Our estimates imply that the increase in marketplace loans we observe accounts for about 35% of the increase in down-payment requirements in the larger cities like Beijing and Shanghai, and between 70% and 100% in smaller cities like Nanchang, Nanjing, and Wuhan. Given that RenrenDai, though an important market player, is but one of a large number of marketplace platforms active in China, and that borrowers may be able to obtain credit on multiple platforms at the same time (Aggarwal and Stein (2016)), these estimates provide a lower bound on the importance of marketplace lending as a channel to circumvent regulatory LTV caps. Consistent with this view, to the extent that the goal of the regulator was to reduce house-price levels or at least slow them down, the regulatory intervention itself appears ineffective: house price growth at the treated cities does not slow down relative to the control cities after November 2013. In contrast, we find that a previous tightening of LTV caps in 2005, before marketplace lending was widely accessible, did slow down house price growth. Corroborating the notion that marketplace lending has neutered the effectiveness of such interventions, we also find that a loosening of LTV caps in 2016 was not associated with faster house prices growth.

The lender × date fixed effects strategy is based on the assumption that any change in credit supply affects proportionally every borrower. If lenders increase more than proportionally the funding of loans in treated cities, our estimates may not be able to fully identify changes in marketplace credit demand. In this situation, we would expect lenders to fund treated cities loans swiftly as they prioritize them over loans originated somewhere else. We find, however, no evidence that loans originated in treated cities are funded more rapidly than those originated in control cities, thus corroborating the validity of our identification strategy.

Our results also suggest that marketplace lenders fail to adjust their screening and loan pricing decisions in the face of the influx of borrowers seeking to circumvent down-payment requirements. We find little evidence of changes in the credit scores and rates of on-site verification for borrowers who obtain a loan after the 2013 episode. In contrast, tighter screening on part of the lenders would predict ex post higher credit scores and higher rates of on-site verification.[6] In addition, we do not detect any significant changes in loan yields or maturities. That is in spite of the fact that default rates increase, by about 45% relative to their pre-2013 levels. These results suggest that lenders on the RenrenDai platform have an “inflexible” lending technology, which does not allow them to adjust their lending decisions, even though they are making loans that turn out to be riskier.

We validate this analysis studying two subsequent changes in LTV caps, which took place in March 2015 and February 2016. In March 2015, every city government in China imposed a reduction in minimum down-payment requirements, down to 40% on second home purchases. In February 2016, all city governments, with the exceptions of Beijing, Guangzhou, Sanya, Shanghai, and Shenzhen, imposed a further reduction in minimum down-payment requirements, this time for first (from 30% to 25% of the property value) as well as second home purchases (from 40% to 30%). In these cases, the demand for

[6] We treat both the credit scoring system and the on-site verification as screening devices available to lenders. In principle, on-site verification is a *signaling* tool available to the *borrowers*, whereby more creditworthy borrowers may choose to have their information directly verified by the platform. But if lenders tighten their screening, they are less likely to make loans to borrowers who are not on-site verified; in that sense the ex post rates of on-site verification reflect screening.

marketplace lending at the treated cities should decrease relative to the controls, reversing the effects observed around the 2013 episode. We find evidence in line with that prediction.

Our findings make three main contributions. First, we contribute to the growing literature that studies the relationship between new financial technologies and credit regulation (e.g. Buchak et al. (2018), de Roure, Pelizzon, and Thakor (2018)). One could view FinTech as a source of shadow banking, with the potential to pose risks to financial stability (Tarullo (2019)). Our evidence indicates that marketplace credit can be a vehicle for regulatory arbitrage; and regulatory arbitrage has long been considered one of the main drivers of the growth of shadow banking. Interestingly, however, a large part of the literature has focused on the elusion of regulatory constraints by financial intermediaries such as banks or other lenders, i.e. on the side of credit *supply* (Adrian and Ashcraft (2012), Buchak et al. (2018), Plantin (2014)). Our results point to the fact that regulatory arbitrage on the side of credit *demand* (enabled or facilitated by the presence of the more lightly regulated marketplace channel) can also be economically very relevant.

Second, our paper contributes to the literature on the drivers of household leverage. Financial (il)literacy (Lusardi and Tufano (2009)), real estate prices (Mian and Sufi (2011), Crowe and Ramcharan (2013)), and import competition (Barrot et al. (2017)) have been found to be important factors behind household debt. Our findings suggest a new, and so far neglected factor: The development of financial technology and the disintermediation of financial services.

Third, our test speaks to the ongoing debate on the systemic impact of household leverage, and on the design of policies to contain it. Much of the literature has focused on U.S. data, and two views prevail. One view focuses on credit supply, and blames financial innovation and incentives in the financial sector for the buildup of mortgage debt leading to the 2007-2009 crisis (Mian and Sufi (2009), Claessens et al. (2010)). A second view focuses on credit demand, on the grounds that household leverage growth encompassed not only lower-income borrowers, but also the middle-class ((Adelino, Schoar, and

Severino (2012, 2016), Foote, Gerardi, and Willen (2012), Foote, Loewenstein, and Willen (2016), Albanesi, De Giorgi, and Nosal (2017)). Our findings present fresh evidence from a different context – China – and time period – 2010-2017 – and highlight the role of both credit demand (to meet the down-payment requirements) and credit supply (from marketplace lending). They also point to a vulnerability of LTV caps, a central instrument in the macroprudential toolkit (Allen and Carletti (2011), Crowe et al. (2011), Claessens, Gosh, and Mihet (2013), Jácome and Mitra (2015)). A potential solution would be to monitor other indicators than LTV (for example, debt-to-income ratios), as well as the borrowers' overall indebtedness. The risk, however, is to throw out the baby with the bathwater, losing the flexibility that makes marketplace credit a viable business in the first place.

The remainder of the paper is organized as follows. Section II provides the institutional background and lays out our empirical predictions with the aid of a simple model. Section III presents our data and identification strategy. Section IV reports our baseline findings on changes in marketplace lending volumes around the 2013 tightening of LTV caps, and Section V on subsequent changes in loan screening, pricing, and performance. Section VI presents similar tests around the 2015 LTV cap relaxation. Section VII discusses the policy implications of our findings. Section VIII concludes.

II. Background: Regulatory changes in the real estate market in 2013-2016

A. 2013 change in minimum down-payment requirements

Most of the analysis in our paper revolves around a regulatory change in the Chinese real estate market, which took place in November 2013. Since about 2011, China had experienced strong growth in real estate prices. Over the period 2012Q4-2013Q4, the *100-City Price Index*, a broad index of house prices published by the China Index Academy (中国指数研究院), rose by 14%; and among the cities with over 5 million inhabitants comprised in our sample, the mean increase in house prices was 11% and the largest increase over 78%. In comparison, the inflation rate over that period was about a mere 2.6%.

In response to the perceived overheating of the real estate market, the General Office of the State Council issued on 26 March 2013 a “Notice on Further Improving Regulation of the Real Estate Market” (国务院办公厅关于继续做好房地产市场调控工作的通知) instructing local regulators to contain house prices.[7] Historically, Chinese authorities had pursued the objective of house prices moderation with two main levers: taxation on house sales and limits to house purchases.[8] The notice called for a stricter enforcement of those policy tools, which were in fact already in place in a number of cities. In addition, for the first time in an official regulatory communication since 2011, it raised the possibility of an increase in minimum down-payment requirements on home mortgages.[9] The increase was not mandated, but rather the notice left it up to provincial and city governments whether to undertake it or not (we return to this point in Section VII). Conditional on a decision to tighten down-payment requirements, the implementation and enforcement of the regulation was delegated to local branches of the People’s Bank of China.[10]

Following the publication of the notice, regulators in Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Ningbo, Shanghai, Shenzhen, Suzhou, and Wuhan imposed a 16.7% increase on the minimum down-payment required to obtain a mortgage for the purchase of a second home, raising it from 60% to 70% of the property’s value. The stated objective of the intervention was to slow down the growth in real estate prices.[11] As we mentioned, anecdotal evidence suggests that real estate investors

[7] The notice was addressed to provincial governments, ministries and commissions of the State Council, and other directly affiliated institutions, as well as city governments, ministries, commissions, and affiliated institutions at the local level. Also see “Shanghai Raises Home Down-Payment Requirement as Prices Jump”, *Bloomberg*, November 8, 2013, and “China’s Nanjing, Hangzhou Raise 2nd Home Down Payments”, *Bloomberg*, November 27, 2013.

[8] As of 2013, 32 major cities in China with population over 5 million imposed a limit on the number of homes that a given household or individual could own. In addition, throughout the country a 20% sale tax is levied on house purchases. Neither the home purchase limits nor the sale tax changed during the period we study.

[9] Before the policy change of November 2013, only in March 2005 there was a change in LTV ratios that involved a precise group of Chinese cities, and can be treated as a quasi-natural experiment. The other changes were China wide and they constitute movements in LTV caps only in the time series.

[10] Unlike the central banks of other major economies, the People’s Bank of China is actually a department of the State Council. It (or its branches) may thus implement policies determined by the administrative authorities, such as the provincial and city governments in this case.

[11] “Shanghai Raises Home Down-Payment Requirement as Prices Jump”, *Bloomberg*, November 8, 2013, and “China’s Nanjing, Hangzhou Raise 2nd Home Down Payments”, *Bloomberg*, November 27, 2013. Most cities increased mortgage

circumvented the new requirements, borrowing via marketplace credit platforms to meet the increased down-payment.[12] Our main tests take this notion to the data, and examine its implications for the relationship between marketplace credit and macroprudential regulation.

B. March 2015 and February 2016 changes in minimum down-payment requirements

In a further set of tests, we also look at a symmetric change in regulation, which took place in March 2015 and February 2016. In 2014 the real estate market in China cooled off. The prices of the property market in all major and medium-sized cities across the country gradually declined, the volume of transactions shrank and the stock of unsold real estate increased significantly (Qin and Mo (2015)). Following these developments, in March 2015, the People's Bank of China, the Ministry of Housing and Urban-Rural Development, and the China Banking Regulatory Commission jointly issued the Notice on Issues Related to Personal Housing Loan Policies (关于个人住房贷款政策有关问题的通知) and reduced the minimum down payment ratio for the second homes in every Chinese city to 40%.

In 2015, the Chinese economy experienced a significant slowdown. In March, the government cut its GDP growth target to 7% – down from an earlier target of 8%, as well as from growth rates averaging over 10% in the previous 5 years. To cushion the effects of slower growth, a broad stimulus package was launched, which included measures to support the housing market. In February 2016, the General Office of the State Council released a “Notice of the People’s Bank of China and the China Banking Regulatory Commission on Issues concerning Further Improving the Differential Housing Credit Policies” (中国人民银行中国银行业监督管理委员会关于进一步完善差别化住房信贷政策有关问题的通知), instructing all Chinese cities with the exceptions of Beijing, Guangzhou, Sanya,

down-payment requirements in November, with the only exception of Beijing, which increased them in May. For that reason, the visual analysis of Figure 1 is focused on the last quarter of 2013. In the tests discussed below, however, Beijing is considered a treated city starting in the second quarter of 2013.

[12] “China to Crack Down on P2P Lenders,” *Financial Times*, March 14, 2016.

Shanghai, and Shenzhen to relax minimum down-payment requirements on the purchase of both first and second homes, from 25% to 20% for first houses and from 40% to 30% for second homes.

II. Data and identification

A. Data

We base our analysis on a large, loan- and loan application-level database from a leading Chinese online marketplace credit platform, RenrenDai (人人贷). RenrenDai was launched in 2010, and quickly developed into one of the main players in the Chinese marketplace credit sector, with cumulative turnover of RMB 25bn ($3.7bn, as of February 2017) and close to 4 million registered accounts (2017 1st quarter). Among the over 2,000 Chinese marketplace credit platforms active as of December 2016, RenrenDai ranks, by turnover, in the top 1%. Our database spans the period from October 2010, when RenrenDai first opened to the public, until February 2017. In total, the data contain 955,174 loan applications, made by 746,647 individual borrowers, and involving 351,333 lenders.

Table 1 reports summary statistics for our data, over a window from May 2012 until December 2014.[13] The average loan has a size of RMB 57,991 ($8,700), with an annualized interest rate of 12.75% and duration 27 months. The average RenrenDai borrower has a pre-tax monthly income of RMB 11,787 ($1,768), or about RMB 141,000 ($21,216) yearly. Based on data from the China Household Finance Survey, the mean after-tax yearly income for Chinese individuals with outstanding debt, living in non-rural areas in the provinces where RenrenDai is active, is RMB 74,000 ($10,963).[14] With an average income tax rate of about 40%,[15] therefore, RenrenDai borrowers appear in line with the population

[13] The window ends with the last quarter of 2014, as in March 2015 down-payment requirements on second home purchases are reduced in every Chinese city.
[14] The China Household Finance Survey is administered by the Southwestern University of Finance and Economics. The data are based on the 2011 wave of the survey (the only one available at the time of writing).
[15] Income taxes are progressive in China (cf. e.g. https://www.ecovis.com/focus-china/individual-income-tax-iit-china-ground-rules/). The 40% average tax rate is based on a back-of-the-envelope calculation for an individual with a pre-tax income of RMB 130,000 as in our data.

average. The loan face value is typically about 40% of the borrower's annual income. In comparison, Morse (2015) reports average interest rates of about 14%, loan duration of 41 months, and loan face value of 20.5% of the borrower's annual income. We thus observe higher loan-to-income ratios and shorter durations, but similar interest rates as in the U.S. There is also sparse information on the purpose of the loans; the most common purposes are "Short Term Liquidity Needs" (49%), "Consumption/General" (37%), and "Entrepreneurship" (6.8%). The data also report each borrower's credit score, based on RenrenDai's internal scoring system. There appears to be relatively little variation in credit scores: the average score is 173 (with standard deviation about 28), the median is 180, and the maximum is 181.

For each borrower in our data, in addition to her income level we are able to observe a number of characteristics, including demographics such as gender, age, city of residence, etc. Additional data items are disclosed by the borrowers on a voluntary basis, such as education, home ownership, and whether or not they have a mortgage. Average borrower age is about 38 years; around 50% of borrowers have a college degree, and 65% are male. As in the U.S. (Balyuk (2017)), the median RenrenDai borrower is not a home owner.[16] Disclosing more information allows the borrower to obtain a higher credit score on RenrenDai's internal rating system, so that borrowers have an incentive to greater disclosure. In our data, 99.86% of all successful loan applications are associated with borrowers who disclose at least some of these non-mandatory items. The median borrower in our data only obtains one loan; there are, however, repeat borrowers, with up to 148 loans in their history on RenrenDai.

Similar to studies based on U.S. marketplace credit data (e.g. Balyuk (2017), Morse (2015)), we are not able to directly observe lender characteristics, but we can characterize them by looking at the features of the lenders' loan portfolios. In Table 1.C, we report the characteristics of the average lender

[16] This value refers to the entire May 2012-May 2015 period. Table 2, where the data are restricted to the period prior to November 2013, reports a lower value. That is consistent with an influx of borrowers who are home owners in the period subsequent to November 2013, as we discuss below.

on a given loan (the mean number of lenders per loan is 42; median: 27). On average, lenders hold a portfolio of 240 loans, with a total face value of RMB 429,940 ($64,491). Income per capita in China in 2013 was $7.077; in Beijing and Shanghai, the two largest cities in the country, it was $15,143 and $14,560 respectively.[17] Thus, the average lender's investment is between 4 and 9 times per capita income; that is consistent with anecdotal and survey evidence indicating that investors on RenrenDai are households belonging to the emerging Chinese middle class (Deer, Mi, and Yuxin (2015)). The average lender has an experience, computed as the number of loans she funded since registration on RenrenDai, of about 5,000 loans. Finally, lenders can choose to make their loans directly to borrowers, or delegate the allocation of their funds across different loans to automatic investment plans, based on a "robo-advisor" technology that matches lenders to borrowers mostly based on returns and maturity preference parameters set by the lender. The main automatic investment plan on RenrenDai is called Uplan (U 计划), and around 66% of all loans are made through it.

B. Identification approach

The structure of our data helps us address the identification challenges discussed in the introduction. In particular, to each lender on the RenrenDai platform is associated a unique ID code, and the typical lender invests in multiple loans at the same time. This allows us to control for unobserved lender heterogeneity and hold credit supply fixed with a fixed effects strategy. Intuitively, our test compares two loans, made by the *same lender*, *at the same point in time*, to two different borrowers, Fang and Wei. Fang is exposed to the increase in down-payment requirements; Wei is not. Because the marketplace lender is the same on both loans, any factor affecting the *supply* of credit from the lender, related e.g. to her lending capacity, market strategy, technology etc. can thus be ruled out, allowing us to focus on the difference in credit

[17] Source: World Bank National Accounts Data.

demand between borrowers Fang and Wei. Operationally, we exploit the wealth of information at our disposal by running our tests on loan-lender level data, with lender × date fixed effects.[18]

We analyze changes in marketplace loans, comparing affected and un-affected real estate markets around the 2013 and 2015 changes in minimum mortgage down-payment requirements described above. The baseline test takes the form of a difference-in-differences regression:

$$L_{blt} = \alpha + \beta Treated_{bt} + \gamma Post_t + \delta(Treated_{bt} \times Post_t) + \mu' x_{blt} + \varepsilon_{blt} \quad (7)$$

where L_{blt} denotes a loan associated with borrower b and lender l at time t. $Treated$ is an indicator variable equal to 1 if the borrower is located in one of the cities affected by the change in minimum down-payment requirements. $Post$ is an indicator variable equal to 1 in the period subsequent to the change in down-payment requirements. To be immune to the Bertrand, Duflo, and Mullainathan (2004) critique of standard errors in difference-in-differences tests, we collapse the data and take averages over two periods, before and after the change in down-payment requirements, and then take first differences, estimating:

$$\Delta L_{bl} = \alpha + \delta Treated_{bt} + \mu' \Delta x_{bl} + \eta_{bl} \quad (7')$$

where ΔL denotes the change in loan applications around the regulation change, associated with borrower b and lender l.

Given the features of the data at our disposal, we can estimate model (7)-(7’) on different levels of granularity, allowing to control for alternative potential confounding factors. In the simplest specification, we estimate equation (7’) on the city-date level data, i.e. studying the behavior of all loans (applications) in a given city at a given point in time around each change in down-payment requirements.

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[18] This approach is close in spirit to the fixed effects strategies adopted in the literature on bank liquidity shocks (e.g. Khwaja and Mian (2008); Schnabl (2012); Chodorow-Reich (2014)). Note, however, that studies in that literature typically control for *borrower* fixed effects, as their objective is to hold credit demand constant, to examine the effects of credit supply shocks. In our case, we want to hold credit supply constant, and thus control for *lender* fixed effects.

In a second specification, we estimate model (7) on the individual loan-lender level, i.e. where each observation corresponds to a borrower-lender pair, that is a given loan, associated with a given lender and borrower. This specification allows us to exploit the full depth of our data, and hold the credit supply curve fixed, saturating the model with lender × date fixed effects as discussed (this is equivalent to including lender fixed effects in equation (7')).

The nature of the experiment also helps us with identification. As the 2013 changes of LTV caps pertained only the purchase of second homes, we should find that our results are driven by marketplace borrowers that are homeowners, a conjecture that we verify in our tests.

C. Comparison of treatment and control groups prior to November 2013

Our main tests are focused on the 2013 increase in down-payment requirements. The cities that experience it include four of the ten largest cities in China (Beijing, Guangzhou, Shanghai, and Shenzhen), and overall make up about 22% of the population of urban China.[19] In addition, the treatment affects both "Tier 1" (Beijing, Guangzhou, Shanghai, and Shenzhen) and "Tier 2" (Changsha, Hangzhou, Nanjing, Ningbo, Suzhou, and Wuhan) cities. We take all other Chinese cities with active borrowers on RenrenDai and population over 5 million as our control group; the overall sample comprises 52 cities, with an aggregate population of 463 million; in those cities are located 41,174 RenrenDai borrowers as of November 2013, corresponding to 84% of the platform's active borrowers.

In Table 2, we compare the loans associated with the treatment and control cities along observable dimensions, prior to November 2013. Panel A focuses on borrowers. Borrowers from treated and control cities do not exhibit significant differences in terms of monthly income (RMB 11,413 and 11,634 on average), age (about 39 for both groups), gender (58% males for both groups), or the number of loan

[19] Communiqué of the National Bureau of Statistics of the People's Republic of China on the Major Figures of the 2010 Population Census. We restrict the sample to cities with an average population of at least 5 million during our sample period (all the results are robust to including smaller cities).

applications since registering on RenrenDai (2.36 and 1.56). Treated borrowers are modestly more likely to have a college degree (49% have one, compared to 46% for the control group), and less likely to be home owners (18%, compared to 28% for the control group; t-stat: -2.22).[20] Panel B compares lenders across the two groups. In terms of portfolio size, concentration, experience, and participation to Uplan, there are no significant differences between the treated and control groups, in statistical as well as economic terms. Finally, in Panel C the treated and control cities are compared in terms of macroeconomic variables. We detect no significant differences along the dimensions of per capita GDP (level and growth), population growth, household net debt to income, real wages (growth), house price index, unemployment rate in the city, and RenrenDai penetration rates.

In sum, we do not observe large differences along observable dimensions between the treatment and control groups prior to the increase in down-payment requirements of November 2013. That confirms the intuition from Figure 1, which shows parallel trends in marketplace lending in the two groups in the pre-down-payment increase period, and validates the difference-in-differences setting for our test.

IV. Baseline tests

A. City-level estimates

We run a first set of regressions on city-level data. We estimate model (7') by time-averaging, collapsing the data, and taking first differences, as described above, to control for serial correlation in the standard errors (Bertrand, Duflo, and Mullainathan (2004)). The results are reported in Table 3. The estimates in Table 3 support the evidence from Figure 1, as well as the arguments illustrated in Section II. They imply that, over the period following the 2013 rise in down-payment requirements, loan applications in the

[20] These values are based on observations prior to November 2013, explaining the difference from the average home ownership rates in Table 1, which are based on the entire sample.

treated cities increase by about 53%, and the RMB volume of actual marketplace loans by 29%, both of which appear economically substantial.[21]

Separate tests in Table 4 also show that house price growth does not slow down in the treated cities – despite the fact that that was precisely the aim of the regulatory intervention. The estimates, reported in columns (1) and (2), have as dependent variable the monthly change in house prices in a given city around the 2013 policy change. The implied effects are near zero and the coefficients, if any, are positive, implying an increase in house price growth in the treated cities relative to the control cities. In sum, it appears that the rise in down-payment requirements was largely ineffective in slowing down house price growth at the treated cities.

We contrast this result to an analogous regulatory intervention from March 2005, when the People's Bank of China mandated a tightening of LTV caps in cities that displayed "fast growing" house prices. Following the announcement, Hangzhou, Ningbo, and Shanghai raised the down-payment requirements from 20% to 30% on both first and second home purchases. Marketplace lending only became widespread in China in the 2010s, and was non-existent as of 2005.[22] To the extent that our test captures an effect associated with the availability of non-bank lending channels in general, the 2005 regulatory intervention should be similarly ineffective; if instead marketplace lending affects the effectiveness of LTV regulation, we should observe significant differences between house price growth changes in 2005 and 2013. The results, reported in Table 4, support the latter hypothesis: We find that house price growth rates drop by 4-7%, consistent with this kind of regulatory intervention being effective in 2005, but no longer so in 2013.[23] Similarly, we find that house price growth does not

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[21] These effects are calculated as follows. The coefficient on $Treated$ measures the increase in borrowing for the average loan made at the treated cities relative to the control cities, on a *monthly* basis over the period from November 2013 until December 2014. The monthly basis implies that the increase in the average loan size is divided over the 13-month period after the policy change; thus the increase in size for the average loan is equal to the coefficient estimate times 13 months. In specification (1), that is $0.061 \times 13 = 53\%$; and likewise in specification (4) it is $0.022 \times 13 = 29\%$.
[22] The first marketplace lender in China was PaipaiDai established in 2007.
[23] House prices data for the 2005 experiment are available only at yearly frequency. As a result, we use a longer event window of 7 years between 2002 and 2008. A shorter event window would not change the results.

accelerate after February 2016, when down-payment requirements are loosened throughout China (columns (5)-(6)) both on first and second home purchases.

As a final check, we visually inspect the time series of traditional bank lending at the treated and control cities in Figure 2.[24] Over the period 2009-2015, the volumes of credit at the two groups of cities move in parallel; in particular, we do not detect any deviation from the pre-2013 trend at the treated cities, relative to the control cities, around 2013. Moreover, Figure 2 does not indicate a generalized growth of credit at the treated cities over and above the control cities; that suggests that the effect that we capture is specific to marketplace credit, and does not manifest itself in other sources of credit.

B. Loan-level estimates

The city-level evidence is consistent with the notion that borrowers use marketplace lending to circumvent the increase in down-payment requirements. A rise in marketplace loans, however, can be in general the result of a combination of shifts of the credit demand and credit supply curves. For instance, a faster development of marketplace lending, or a greater popularity of marketplace lending as a form of investment at the treated cities, might generate similar effects as the ones we observe in Table 3. To control for credit supply side effects, we estimate model (1) on data matching individual lenders and borrowers, controlling for lender × date fixed effects. As discussed above, this allows us to hold credit supply fixed, and isolate the effect of a shock to credit demand.

The estimates are reported in Table 5.A. Specifications (2)-(4) include lender × date fixed effects; as a benchmark, specification (1) reports the corresponding estimates without them. Overall, the estimates are in line with those of Table 3, and consistent with an increase in marketplace lending demand to circumvent the down-payment requirement increase. Economically, the effects are also meaningful.

[24] Traditional bank lending equals the total amount of credit extended by Chinese banks over our sample period. Detailed data on the separate components of bank credit (e.g. mortgages, consumer credit, credit to businesses) on a province or city level are, to the best of our knowledge, not available.

The estimation window employed in the tests of Table 5.A covers a 28-month period around the 2013 increase in down-payment requirements, from May 2012 until December 2014. The estimates are based on monthly data, and they imply a 6.4-9.3% monthly increase in marketplace borrow at the treated cities, relative to the control cities. Taking the 7.9% midpoint of that range, over the 13-month period following November 2013 that implies approximately a 102% increase in marketplace loans, or an increased borrowing close to the average loan size of about RMB 58,000 (about $8,700).

The value of a medium-size apartment (70 sq. meters) in 2013 in Nanjing (the median among our treatment group cities in terms of house prices) is RMB 875,000 (about $129,630), so that the increase we document accounts for 66% (= RMB 58,000 / RMB 87,500) of the 10-percentage point increase in down-payment requirements. Across the set of treated cities, the effects implied by the estimates of Table 5.A account for between 31% (relative to Beijing house prices) and about 100% (Wuhan house prices) of the increase in down-payment requirements.[25] Given that RenrenDai, though an important market player, is but one of a large number of online marketplace lending platforms active in China, and that borrowers may be able to obtain credit on multiple platforms at the same time (Aggarwal and Stein (2016)), these figures likely provide a lower bound on the importance of marketplace lending as a channel to circumvent the new requirement.

We also separately analyze the intensive margin (whether repeat borrowers increase their borrowing on RenrenDai after November 2013) and the extensive margin (whether one-time borrowers are more likely to turn to RenrenDai, or borrow larger amounts, once down-payment requirements increase). To do so, we estimate two additional regressions, in columns (5) and (6). In column (5) (intensive margin), the sample is restricted to borrowers who are active on RenrenDai (have at least one loan) both before and after November 2013. In column (6) (extensive margin), the sample is restricted

[25] We obtain city-level data on house prices per square meter from the databank of China Index Academy, a leading real estate research organization in China.

to borrowers who are active (have at least one loan) only before or only after 2013. The coefficient estimate on $Treated$ in the intensive margin regression is 0.014, statistically indistinguishable from zero; the corresponding estimate in the extensive margin regression is 0.067 (t-stat: 2.48). The difference between the two coefficients is close to the estimated coefficient on $Treated$ in specifications (1)-(4), suggesting that the effect is driven by the *extensive* margin: in other words, an influx of one-time borrowers after the 2013 increase in down-payment requirements explains our baseline effect.

These results are robust to a number of checks, summarized in Table 5.B. First, we restrict the set of treated cities to Tier 2 cities (i.e. we exclude Beijing, Guangzhou, Shanghai, and Shenzhen). The distinction between Tier 1 and Tier 2 cities is based on an informal hierarchy popularized in the media; Tier 1 cities are larger, richer, and typically more expensive than the average control city. When we estimate a modified version of equation (7)-(7') on the sample of Tier 2 cities, we find a coefficient of 0.049, somewhat smaller than the baseline. This estimate implies an economic effect accounting for about 44% of the implied increase in down-payment requirements in Nanjing. Second, we also find similar effects, economically stronger than the baseline of Table 5.A, if we restrict the sample to loans made by lenders who were active on the RenrenDai platform prior to November 2013, either as registered users (specification (2)) or by having made a loan (specification (3)). This restriction ensures a homogeneous set of lenders before and after November 2013. Our regressions already control for lender × date fixed effects, and the restriction further attenuates potential concerns about a correlation between changes in the composition of credit supply and changes in credit demand around the regulatory intervention. Table 5.B also documents that the increase in marketplace borrowing at the treated cities is driven by loans to home owners (specifications (4)-(5)). This is consistent with the notion that the LTV cap tightening only affects second-home mortgages. Importantly, separating home owners from non-home owners allows us to include city fixed effects in these regressions, which absorb the impact of any unobserved factors related to the local economy that change around November 2013.

C. Further tests: Borrower and lender characteristics

Further analysis provides a richer characterization of these findings. In Table 6 we partition the sample based on lender characteristics: lending via Uplan or direct lending, experience, and portfolio size. Specifications (1) and (2) shows that our effect is mainly associated with lenders who make loans as part of Uplan, which account for the majority of loans in our data (specifications (1)-(2)).[26] Moreover, borrowers in the treated cities receive financing from lenders regardless of the level experience: in both specifications (3) and (4), the coefficient on $Treated$ is positive and statistically significant. The estimated effect is, in fact, larger in magnitude for lenders with above-median experience (0.074 as opposed to 0.030; the difference is statistically significant, with an F-statistic of 7.29). Having a longer experience on RenrenDai as a lender, apparently, is no obstacle to increasing lending at the treated cities. Finally, specifications (5)-(6) distinguish lenders based on their portfolio size. The coefficients on $Treated$ is positive and significantly different from zero both for lenders with below and above median portfolio size. The coefficient in the subsample of lenders with large portfolios is larger (0.083 as opposed to 0.044; the difference is statistically significant, with an F-statistic of 6.76). That suggests that the increase in marketplace lending at the treated cities after November 2013 is driven primarily by larger lenders. To the extent that lenders with larger loan portfolios are likely financially more sophisticated, it appears that such sophistication does not prevent them from increasing lending at the treated cities.[27]

[26] There does not appear, however, to be any form of specialization of Uplan loans towards the treated cities. In fact, the proportion of loans to borrowers located in the treated cities made via Uplan or direct lending is nearly identical: 31% (Uplan) and 32% (direct lending), in terms of the number of loans, and 32% via both channels in RMB terms. The effect is driven by loans made via Uplan simply because Uplan accounts for a large component of the loans made on the platform (around 70%).

[27] At the same time, even lenders in the largest portfolio size quartile do not appear to have especially large amounts invested via RenrenDai. The average portfolio size in that quartile is about RMB 530,000 ($78,518), and the largest portfolio in our sample has size about RMB 4,200,000 ($630,000), consistent with the view that we are looking at individual lenders (rather than e.g. institutions), as we remarked above. Moreover, and similarly to the remarks we made about loans made via Uplan as opposed to direct lending, there does not seem to be a specialization of lenders with large portfolios towards the treated cities: 32% of their loans go to borrowers located in the treated cities, and a nearly identical fraction for the lenders with smaller portfolios.

Taken together, these findings suggest that marketplace lending supply responds to the credit demand generated by the 2013 increase in down-payment requirements as predicted by our discussion of Section II. Marketplace lenders are able to supply an economically substantial amount of credit, based on the effects discussed above. The expansion of marketplace credit is driven by a broad range of lenders. In particular, delegating portfolio choice to the Uplan "robo-advisor", longer experience, or greater sophistication do not appear to make lenders less likely to fuel the increased marketplace lending.[28]

D. Further tests: Lending Speed

The lender × date fixed effects identification strategy is based on the assumption that any change in credit supply affects proportionally every borrower. If lenders increase more than proportionally the funding of borrowers located in treated cities, we may not be able to disentangle credit demand from credit supply, and our regressions would yield biased estimates.

To address this possibility, we look at how quickly loans are funded. Funding speed can be interpreted as a measure of willingness to supply credit once demand has been set by the borrowers. On RenrenDai (as in other marketplace lending platforms), borrowers first post the amount they wish to borrow and the conditions attached to the loan; and only subsequently lenders observe these variables and decide whether to bid or not on a given loan. If lenders are eager to finance loans in treated cities, they will prioritize them over others, and fund them faster in respect to those originated in control cities.

In Table 7, we relate lending speed to the LTV policy change. Columns (1)-(2) examine how long it takes for a loan to be fully funded. Columns (3)-(4) check how long it takes for a loan to receive the

[28] Throughout our analysis we implicitly assume that borrowers use marketplace funds to purchase a home in the city where they live. A possible concern is that borrowers in control cities borrow funds on RenrenDai to buy a house in a treated city. In principle, this possibility would make our control and treatment groups more alike, working against our test and suggesting that our estimates represent a lower bound of the effects of interest. In addition, every city in our treated group has home purchase restrictions in place that actually prevent residents from other cities to purchase a second home in the areas under their jurisdiction. For instance, only a registered resident in Shenzen is allowed to buy a second home in Shenzen, ruling out the possibility that a marketplace borrower in, say, Chengdu (a city in our control group) may borrow on the platform to fulfil the down payment requirement set by another city.

first bid. Columns (1) and (3) control for borrower as well as city macroeconomic characteristics; columns (2) and (4) also control for loan characteristics such as the amount requested by the borrower, loan spread and maturity, borrower credit score, and whether the borrower was onsite verified. This allows us to compare similar loans originated either in treated or in control cities. In every specification, we observe no statistically significant difference in funding speed between treated and control cities. This result is consistent with the notion that lenders do not increase more than proportionally credit supply to borrowers located in treated cities; overall, it validates our lender × date fixed effects strategy.

V. Other loan features; loan performance

The influx of marketplace borrowers at the treated cities, and the fact that even experienced and sophisticated investors appear prone to fueling it, raise the possibility of an increased risk exposure for marketplace lenders. We now study if the lenders protect themselves against that risk by adjusting loan contract terms, as well as the ex-post performance of their loans.

A. Screening, pricing, and duration of loans

At the same time as they fund marketplace loans that, as we documented, are consistent with regulatory arbitrage, RenrenDai lenders can in principle limit their risk exposure by adjusting the features of the loans in which they invest. We consider three central loan contract features: the degree of screening to which the borrower is subject, pricing, and duration.

Our first measure of screening is on-site verification. Borrowers on RenrenDai self-declare their characteristics such as income, age, etc. In addition, they may also submit to on-site verification, where an officer from You Zhong Xin Ye Financial Information Services Ltd. (友众信业金融信息服务(上海)有限公司), a sister company of RenrenDai, verifies that the information they provided is true by visiting them at their stated address. If lenders respond to the influx of new borrowers by stepping up screening and tightening their lending standards, they may be willing to invest in a given loan only if the borrower has been on-site verified. We should therefore expect higher rates of on-site verification among the loans made after the last quarter of 2013. In Table 8 (specification (1)) we find, instead, the opposite: After November 2013, borrowers in the treated cities are 7.5 percentage points less likely to be onsite verified (about 10% relative to the unconditional average).

By a similar logic, tighter screening predicts ex post higher borrower credit scores on the loans. There is no centralized credit bureau in China, and no consumer credit score equivalent to and/or with similar broad coverage as a FICO score. For that reason, a number of Chinese marketplace platforms, including RenrenDai, have developed their own credit scoring systems, which are visible to the lenders, as an aid in their portfolio allocation decisions. Our estimates of specification (2) of Table 8 provide little evidence of an increased borrower credit score. That has two possible interpretations: (a) Lenders do not align their investments more closely with credit scores after November 2013, or (b) RenrenDai's credit score is uninformative. Neither interpretation suggests tighter screening. Taken together, these results indicate that the lenders simply do not become more discriminating after November 2013.

In line with these findings, the pricing and duration of loan contracts issued after 2013 also do not change appreciably. We find no significant changes in yield spreads (specification (3)), nor in duration (specifications (4)), after 2013. In sum, marketplace lenders treat the influx of borrowers from the treated cities just like their old borrowers, and lend to them at conditions that are no different. This suggest that lenders make no adjustments to their lending terms following 2013. The interesting question is, of course, whether this can be rationalized ex post, for instance because the "new" loans perform similarly to the "old" ones.

B. Loan performance

We test for this possibility by looking at four measures of loan performance: delinquencies (the proportion of months during which the borrower is delinquent over the loan's life), loan default (an indicator equal to 1 if a given loan experiences a default), the log-RMB amount of a defaulted loan, and the log-RMB outstanding amount of the loan at the time of default. When looking at delinquencies and default indicators, the sample size shrinks because of a truncation problem: for some ongoing loans, default may simply not have been declared yet. When looking at the log-RMB measures of on the size of the default, the sample shrinks further, as it is restricted to defaulted loans.[29]

The evidence, reported in Table 9, indicates a deteriorating loan performance at the treated cities following 2013. Delinquencies increase by 1 percentage point (specification (1)), and defaults by 0.6 percentage points (specification (2)), relative to the control cities. Lower than the estimates reported by Morse (2015) for the U.S., default rates are on average 1.3% among RenrenDai loans (see Table 1).[30] Our estimates imply, therefore, that default frequency increases by 46% in relative terms, which appears economically substantial. The losses imposed on lenders also appear to concentrate on the largest loans: the estimates of specification (3) indicate that the defaulted loans are 2.6 times larger at the treated cities than at the control cities. Finally, specification (4) indicates that the RMB size of the default (the part of the loan that does not get repaid) is 31% larger.

Taken together, the evidence presented in this section and the previous one indicates that: (i) Following the 2013 tightening of LTV caps, marketplace borrowing rises abnormally at the treated cities; (ii) marketplace lenders do not respond by adjusting their screening procedures, nor do they alter the

[29] One caveat is that, to the best of our knowledge, no information on the recovery rates for defaulted loans on RenrenDai is publicly available. Because we cannot account for recovery, therefore, our estimates can be interpreted as an upper bound on the economic loss associated with the defaults.

[30] In the second half of 2015, there was a wave of defaults on marketplace loans across mainland China, with much higher default rates than the 2% average associated with the entire sample ("China's Unregulated P2P Lending Sites are Still Spreading Financial Instability", *China Economic Review* July 28, 2015; "China Imposes Caps on P2P Loans to Curb Shadow-Banking Risks", *Bloomberg News*, August 24, 2016). We are able to observe the increase in defaults in our data; however, given its timing, it has a minimal impact on our estimates around the 2013 increase in minimum down-payment requirements. Below we discuss an additional test, centered around a similar LTV cap change in September 2015; we discuss the possible impact of the 2015 default wave on that test in the next Section.

pricing and duration of their loans in response; and (iii) Default rates among “new” post-2013 borrowers are systematically higher. This suggests that the “lending technology” of the RenrenDai lenders is not flexible enough to induce them to tighten their lending standards in response to the influx of borrowers in the treated cities after November 2013, even though the loans they make turn out to be riskier.

VI. Evidence on the 2015 and 2016 regulatory changes

As explained, in March 2015 a reverse policy intervention was implemented across the country. As part of a broader stimulus package, minimum down-payment requirements on second homes were lowered to 40% of the asset’s purchase value, this time in the entire country. Based on the same argument behind our test around November 2013, this should curb the demand for marketplace lending for home owners. In February 2016, the minimum down-payment requirement was reduced on both first and second home purchases, by 5 percentage points (from 25% to 20%) and 10 percentage points (from 40% to 30%) respectively.

One caveat to correctly interpret the tests we are about to present is that the demand for marketplace lending is expected to decrease, under the assumption that the existing overall credit demand as of March 2015 incorporates a component of borrowers who resort to marketplace credit to meet existing down-payment requirements. That appears plausible, based on our findings on the effects of the 2013 policy intervention discussed in the preceding sections.

With that caveat in mind, we run tests similar to the ones presented in Section IV. First, we examine changes in lending volumes following March 2015. Because the change in regulation affected the entire country, we compare marketplace lending to home owners and non-home owners. The results are reported in Table 10.A. In line with our predictions, we find a reduction in lending volumes to home owners related to non-home owners, suggesting that home owners reduced their reliance on marketplace credit.[31]

Second, we look at lending around the February 2016 loosening of down payment requirements in every Chinese city with the exceptions of Beijing, Guangzhou, Sanya, Shanghai, and Shenzhen. The results are illustrated in Table 10.B. Consistent with our expectations, we find that marketplace lending drops at the treated cities, both driven by the extensive margin and intensive margin. In other words, existing borrowers reduced their reliance on marketplace credit, and one-time borrowers obtain smaller loans at the treated cities. In economic terms, the effects are slightly smaller to the ones presented in Section IV, but with the reverse sign.

VII. Discussion and policy implications

In light of the evidence reported in the previous sections, we now discuss (1) the design of our test, and (2) the policy implications of our results. Regarding the design of our test, one potential question is why in November 2013 the local governments of the treated cities, but not those of the control cities, decided to tighten mortgage down-payment requirements. As we mentioned, the regulatory change was a response to a rise in real estate prices. But house prices were rising across the entire China, not just the treated cities: in the 6-month period leading to November 2013, house prices relative to per capita incomes rose on average by 17.67% in the treated cities, and by 17.62% in the control cities (all non-treated Chinese cities with over 5 million inhabitants in 2013, Table 2).

[31] The estimates are somewhat smaller compared to those of 2013. The decline in down-payment requirements on second home mortgages may have induced first-time home buyers to expect a similar policy on first home mortgages, leading them to delay the house purchase and to rely less on marketplace lending. As the behavior of first-time buyers becomes similar to the behavior of homeowners, the economic effects we find are also attenuated. Notice that the effects of changes in expectations ought not to be symmetric when there is a tightening of down-payment requirements, as in our 2013 experiment. In that case, expecting tighter LTV caps, first-home buyers may anticipate the purchase of a house using their own savings so to avoid both the payment of a higher down-payment and the use of market place credit. This would keep the behavior of first home buyers distinct from the behavior of home-owners.

The combined evidence of Figures 1 and 2 and Table 2 indicates that, prior to November 2013, the treated and control cities were on parallel trends in terms of traditional and marketplace credit, and that they did not exhibit significant differences in terms of an extensive list of observable economically relevant variables. That, combined with the fine mesh of fixed effects employed in our tests, considerably raises the bar for any alternative interpretation of our results based on some omitted factor driving systematic differences between treatment and control cities. Given similar ex ante conditions and a similar recent rise in real estate prices, then, why were the policy choices different in those two groups?

We do not pretend to have an exhaustive explanation of the policy choices of local governments in the period surrounding November 2013. Having said that, anecdotal evidence provides some indication as to the factors influencing regulators. Although the General Office of the State Council notice of March 2013 did indicate to the local authorities that they should slow down the growth in real estate prices, as we discussed there was no official mandate to prefer targeting LTV caps over the existing levers of sales taxes and regulatory limits to house purchases. That left local authorities some leeway in defining whether and how to intervene. Anecdotal evidence, as well as a growing empirical literature, suggests that macroprudential policies can be unpopular with the general public, particularly if they have the result of curbing credit (Horvath and Wagner (2016), Haldane (2017), and Müller (2018)). It is therefore likely that a relatively "strong" regulatory authority at the province level was in a better position to increase down-payment requirements. Consistent with this view, we find that treated province governors and party chairmen are more senior: on average the governors are 2.8 years closer to retirement in comparison to the control cities (56% relative to the 5-year duration of a governor's mandate and about 100% of the average governor tenure we observe in the data), suggesting that they may have greater authority, or that they may be less concerned about potential negative career consequences of their policy.

A second question is what policy implications we can draw from our findings. Since the financial crisis of 2007-2009, evidence has accumulated documenting the potential negative effects of household leverage, and how high levels of debt exacerbate the business cycle (Lamont and Stein (1999), Almeida, Campello, and Liu (2006), Mian, Rao, and Sufi (2013), Mian, Sufi, and Verner (2017), Baker (2018)). Macroprudential tools, including LTV caps, have been the focus of much of the debate on how to design policies to contain household leverage (Allen and Carletti (2011), Aikman et al. (2019)), and there is evidence showing that they can be effective (Almeida, Campello, and Liu (2006), Igan and Kang (2011), Claessens, Gosh, and Mihet (2013)). Our results, however, point to a vulnerability of LTV caps to regulatory arbitrage, fueled by lightly regulated credit channels such as marketplace platforms. Understanding the policy implications of this finding revolves around three further questions.

First, is the type of regulatory arbitrage we document confined to marketplace credit, or could it be run with alternative credit channels? A salient channel could be credit card debt, which has similar interest rates as marketplace credit.[32] Institutional features of the Chinese financial system suggest that is unlikely: Credit card penetration rates in China, as well as the volume of credit card debt, are still relatively low (consistent with per capita GDP being low in comparison to more mature economies).[33] Abstracting from the Chinese context, in the U.S. credit card debt has a worse impact on the borrower's FICO score than online marketplace loans, and also in the rest of the world it is generally more transparent to banks and other mortgage lenders.[34] In sum, it takes marketplace credit, or something with similar contractual and regulatory characteristics as marketplace credit, to run the kind of regulatory arbitrage we analyze.

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[32] Credit card interest rates are on average about 20%; for online marketplace credit in the U.S., Balyuk (2017) reports interest rates of about 18% and Morse (2015) a range of 13-19%. Our own data indicate interest rates at the lower end of that range (Table 1).

[33] As of 2013, the average Chinese had 0.29 cards, in contrast to the average American who had over 3. Moreover, outstanding credit card debt is small (2% of GDP in 2013, versus 11% in the U.S.), and credit card debt must typically be repaid on a much shorter horizon than marketplace credit (1 month versus an average loan duration of 2-3 years in our data). These features suggest that credit cards are unlikely to play as a relevant a role as marketplace credit in the Chinese context.

[34] Raising the limit on one's credit card triggers a "hard" inquiry, which remains on the borrower's credit report for two years (affecting her FICO score), regardless of whether or not the limit increase is granted; in contrast, applying for an online marketplace loan only results in a "soft" inquiry, which is not reported on the credit report and does not affect the FICO score. In addition, credit card debt is classified as "revolving", whereas marketplace loans are classified as "instalment" debt; revolving debt has a worse impact on the FICO score.

Second, given our evidence, should we consider curbing marketplace credit via regulation, and if so, to what extent? There are theoretical and empirical arguments suggesting that marketplace credit in its current form can be welfare-increasing. One is that precisely its light regulation allows it to serve a pool of potentially profitable borrowers who would otherwise remain unbanked. Another reason is that the entry of the marketplace lending technology, and its lower loan origination costs, creates competition for bank lenders (Buchak et al. (2018)), Morse (2015), de Roure, Pellizzon, and Thakor (2018)). Moreover, marketplace credit is disintermediated and lenders can directly match their investment horizon to the loan maturities available on the platform. This allows the lenders to extend loans without the rollover risk associated with maturity transformation. These benefits must be weighed against the possibility of abuse and regulatory arbitrage of the sort we document, and assessing their relative importance seems far from trivial.

Third, assuming that we would like to limit marketplace credit, how does one determine and enforce that limit? An ongoing discussion among Chinese regulators has focused on the introduction of caps to the amount of lending and borrowing that a given individual can do on marketplace platforms. The proposed caps, however, would typically not be binding against the regulatory arbitrage that we analyze: a proposal from August 2016 indicated a target cap of RMB 200,000 for household borrowing on a given platform, i.e. over five times larger than the about RMB 36,000 increase in marketplace borrowing at the treated cities following November 2013 (Huang (2018)).

An alternative approach is to broaden the scope of mortgage credit regulation to ratios other than LTV, such as debt-to-income (DTI), which take into account the entire debt position of the prospective borrower; and indeed the literature on macroprudential regulation discusses DTI as a relevant additional tool (Crowe et al. (2012)). That, however, requires setting up a credit registry; and monitoring marketplace loans implies collecting information to a level of detail which, to the best of our knowledge, is unprecedented in most developed economies. This problem is coupled with two additional issues. First, DTI and LTV caps serve different policy goals and they are complementary rather than alternative policy instruments.[35] As a result, tighter DTI caps cannot fully compensate for the circumvention of LTV caps via marketplace credit. Second, tighter DTI caps can be procyclical, in that they can prevent (efficient) borrowing to smooth consumption over the business cycle. That seems in contrast with the aims of macroprudential regulation, which involve precisely preventing the financial system from amplifying business cycle fluctuations.

VIII. Conclusion

We investigate the capacity of marketplace credit to undermine loan-to-value (LTV) caps in mortgage markets. We rely on a novel, hand-collected database containing all lending transactions at RenrenDai, a leading Chinese marketplace platform, and focus on the increase in 2013 of minimum down-payment requirements on second-home mortgages at several major Chinese cities. This tightening of LTV caps should raise the demand for marketplace credit by borrowers, who try to circumvent the new down-payment requirement. Consistent with this argument, marketplace loans increase at the treated cities relative to the control cities following the new LTV cap. Importantly, the structure of our data allows us to separate credit demand and supply effects, using a lender × date fixed effects strategy – we are thus able to isolate the capacity of the marketplace credit channel to fuel household debt. We validate this analysis with evidence from a reverse change in LTV caps in 2015 and 2016, when city governments lowered minimum down-payment requirements, resulting in a drop in marketplace credit demand. We find little evidence that marketplace lenders adjust their policies in response to the expected characteristics of their borrowers, suggesting that the information benefits of marketplace credit that have been observed by part of the literature may be limited. Our results indicate that marketplace credit can act as a channel to circumvent LTV caps affecting loans made by traditional credit providers. The rapid

[35] DTI caps assure that borrowers take on affordable mortgages, whereas LTV caps prevent speculation in the housing market by forcing the borrower to have “skin in the game”.

growth of online marketplace credit in recent years and its largely unregulated and informal nature suggest that a policy solution may not be trivial. More broadly, our findings add to the growing body of results suggesting that, at least in part, FinTech development is driven by regulatory arbitrage; complementing those earlier results, they suggest that regulatory arbitrage on the side of credit demand, not only supply, may also be economically important.

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Table 1 Summary statistics

The table reports summary statistics. Panel A describes loan characteristics, panel B borrower characteristics, and panel C lender characteristics. All variables are defined in detail in Appendix A. The sample consists of all loans on the RenrenDai platform, over the period May 2012-Dec 2014 around the November 2013 tightening of mortgage down-payment requirement) for borrowers located in metropolitan areas in mainland China with population above 5 million.

	Mean	St. dev.	Min	Median	Max	N
A. Loan characteristics						
Loan amount (RMB)	57,991	62,864	3,000	49,100	3,000,000	68,477
Interest rate (%)	12.75	0.99	8.00	13.20	24.40	68,477
Interest rate spread (%)	8.02	1.10	3.00	8.20	19.60	68,477
Duration (months)	26.62	9.83	1.00	24.00	36.00	68,477
On-site verification (0/1)	0.77	0.42	0	1	1	68,438
Borrower credit score	172.61	28.33	0.00	180.00	182.00	68,314
Proportion of months delinquent (%)	1.76	10.75	0.00	0.00	100.00	68,477
Default (0/1)	0.013	0.11	0	0	1	68,468
Time to first bid (seconds)	33,971	141,281	0	623	2,771,210	68,413
Time to fully fund a loan (seconds)	35,459	141,620	0	1,193	2,771,332	68,413
B. Borrower characteristics						
Income (monthly RMB)	11,787	13,745	0	5,000	50,000	68,477
Age	38.23	8.47	23	37	57	68,477
College degree (0/1)	0.50	0.50	0	1	1	68,474
Male (0/1)	0.65	0.48	0	1	1	68,477
Home owner (0/1)	0.45	0.50	0	0	1	68,477
Number of applications since registration	1.47	4.59	1	1	148	68,477
Total amount borrowed since registration (RMB)	66,446	120,993	3,000	52,900	9,000,000	68,477
Number of lenders per loan	41.84	57.38	1	27	1,841	68,438
C. Lender characteristics						
Portfolio size (RMB)	429,940	548,805	4,712	313,384	4,214,200	68,332
Portfolio size (nr. loans)	240.24	154.34	4.00	204.53	1,925.00	68,332
Uplan lending (% of RMB)	65.75	31.22	0.00	84.85	100.00	68,332
Uplan lending (% of loans made)	70.44	30.55	0.00	90.19	100.00	68,332
Experience (number of loans since registration)	8,822	64,270	1.00	1,891	5,413,695	68,332

Table 2 Comparison of treatment and control groups pre-November 2013

The table compares the characteristics of borrowers and lenders on loans associated with cities in the treatment (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Ningbo, Shanghai, Shenzhen, Suzhou, and Wuhan) and control groups (all other Chinese cities with over 5 million inhabitants) 18 months prior to the November 2013 increase in minimum mortgage down-payment requirements. All variables are defined in detail in the appendix. The column labeled "Treated" reports the average of each characteristic for the treatment group, the column "Control" for the control group, the column "Difference" their difference, and the column "t-statistic" the t-test statistic for the difference, based on standard errors clustered around cities.

	Treated	**Control**	**Difference**	**t-statistic**
A. Borrower characteristics				
Income (RMB)	11,413	11,634	-221	-0.243
Age	39.08	38.83	0.25	0.626
College degree (0/1)	0.49	0.46	0.03	1.035
Male (0/1)	0.58	0.58	0	-0.010
Home owner (0/1)	0.18	0.28	-0.10	-2.217**
Number of applications since registration	2.36	1.56	0.81	0.851
Total amount borrowed since registration (RMB)	73,173	61,802	11,371	1.333
Number of lenders per loan	33.05	34.25	-1.20	-0.724
Number of loans per capita growth (%)	75.16	88.37	-13.21	-1.403
B. Lender characteristics				
Portfolio size (RMB)	655,649	638,804	16,845	0.309
Portfolio size (nr. loans)	271.40	263.20	8.20	0.658
Uplan lending (% of RMB)	71.30	69.70	1.60	0.385
Uplan lending (% of loans made)	75.30	73.50	1.80	0.416
Experience (nr. loans made by lender since registration)	5,100	5,841	-741	-1.387
C. Macroeconomic characteristics				
Province GDP per capita (RMB)	60,805	45,831	14,974	1.272
Province annual GDP per capita growth (%)	8.35	11.26	-2.91	-1.363
Province annual population growth (%)	0.01	0.01	0.00	0.565
House price index	0.20	0.15	0.05	0.792
% change in house prices (past 6 months)	17.69	17.59	0.10	0.233
Household net debt-to-income	-0.72	-0.42	-0.30	-1.260
Annual real wage growth (%)	0.41	0.70	-0.29	-0.920
Unemployment rate (%)	13.70	14.10	0.40	-0.190
RenrenDai penetration (applications per 10,000 inhabitants)	2.31	1.93	0.38	0.900
Growth of RenrenDai penetration (Jan 2012-Oct 2013) (%)	75.2	88.3	-13.1	1.403

Table 3 Marketplace lending around the 2013 increase in mortgage down-payment requirements: City level
In columns (1)-(4), the table reports the estimates of:

$$L_{ct} = \alpha + \beta Treated_c + \gamma Post_t + \delta(Treated_c \times Post_t) + \mu' x_{ct} + \varepsilon_{ct}$$

Each observation corresponds to a given city c on a given calendar month t. The dependent variable is the log-loan amount associated with the aggregate loan applications or RMB loan volume in the city. $Treated$ is an indicator variable equal to 1 if the city belongs to the treatment group (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Ningbo, Shanghai, Shenzhen, Suzhou, and Wuhan). $Post$ is an indicator variable equal to 1 over the period following the change in mortgage down-payment requirements; in all specification the sample period covers May 2012-Dec 2014 around the down-payment requirement increase. To control for serial correlation in the standard errors, we time-average and collapse the data (Bertrand, Duflo, and Mullainathan (2004)), and estimate:

$$\Delta L_c = \alpha + \delta Treated_c + \mu' \Delta x_c + \eta_c$$

where ΔL_c denotes the changes in loan applications in columns (1)-(2) and log-loan amount that are actually granted in columns (3) and (4) from before to after the change in down-payment requirements. In all specifications, the vector of control variables x includes province GDP per capita level and growth rates, province population growth rates, city GDP per capita growth rates, and yearly real wage growth, city unemployment rate, and city net household debt over income. All specifications also include city house price % growth over the past 6 months and growth of the RenrenDai platform between Jan 2012 and Nov 2013. The standard errors (reported in parentheses) are clustered at the province level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	Credit volumes			
	Applications		**Loans**	
	(1)	(2)	(3)	(4)
$Treated$	0.061***	0.041***	0.017*	0.022***
	(0.023)	(0.016)	(0.009)	(0.010)
Controls	N	Y	N	Y
R^2	0.29	0.61	0.18	0.59
N	52	52	52	52

Table 4 House prices around changes in mortgage down-payment requirements: City level
In columns (1)-(6), the table reports the estimates of:

$$House\ prices\ growth_{ct} = \alpha + \beta Treated_c + \gamma Post_t + \delta(Treated_c \times Post_t) + \mu' x_{ct} + \varepsilon_{ct}$$

Each observation corresponds to a given city c on a given calendar month t. The dependent variable is the house prices growth rates in the city. $Treated$ is an indicator variable equal to 1 if the city belongs to the treatment group (Hangzhou, Ningbo, and Shanghai in March 2005; Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Ningbo, Shanghai, Shenzhen, Suzhou, and Wuhan in November 2013; and all Chinese cities with at least 5 million inhabitants except Beijing, Guangzhou, Sanya, Shanghai, and Shenzhen in February 2016). $Post$ is an indicator variable equal to 1 over the period following the change in mortgage down-payment requirements. The regressions in columns (1) and (2) are computed over the window 2002 and 2008, at an annual frequency; regressions in columns (3) and (4) are computed over the window May 2012 - December 2014 at a monthly frequency; regressions in columns (5) and (6) are computed over the window October 2015 - Feb 2017 at a monthly frequency. To control for serial correlation in the standard errors, we time-average and collapse the data (Bertrand, Duflo, and Mullainathan (2004)), and estimate:

$$\Delta House\ prices\ growth_c = \alpha + \delta Treated_c + \mu' \Delta x_c + \eta_c$$

where $\Delta House\ prices\ growth_c$ denotes the change in houses prices growth rates from before to after the change in down-payment requirements. In all specifications, the vector of control variables x includes province GDP per capita level and growth rates, province population growth rates, city GDP per capita growth rates, and yearly real wage growth, city unemployment rate, and city net household debt over income. All specifications also include city house price % growth over the past 6 months and growth of the RenrenDai platform between Jan 2012 and Nov 2013. The standard errors (reported in parentheses) are clustered at the province level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	2005		**2013**		**2016**	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treated$	-0.040**	-0.073***	0.003	0.000	0.005	0.007
	(0.018)	(0.026)	(0.003)	(0.003)	(0.004)	(0.005)
Controls	N	Y	N	Y	N	Y
Region FE	Y	Y	Y	Y	Y	Y
R^2	0.24	0.53	0.11	0.74	0.08	0.32
N	34	34	51	51	48	48

Table 5 Marketplace lending around the 2013 increase in mortgage down-payment requirements: Lender-borrower level

The table reports the estimates of:

$$\Delta L_{lb} = \alpha + \delta Treated_{bc} + \mu' \Delta x_{bc} + \varepsilon_{lb}$$

Each observation corresponds to a given pair borrower b-lender l. The dependent variable is the change in the natural logarithm of loans made by lender l to borrower b (average after the 2013 increase in down-payment requirements minus average before that). $Treated$ is an indicator variable equal to 1 if borrower b is located in a city c that belongs to the treatment group (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Ningbo, Shanghai, Shenzhen, Suzhou, and Wuhan). Following Bertrand, Duflo, and Mullainathan (2004), the equation is estimated on changes around the down-payment requirement increase, after collapsing and time-averaging the data around the policy intervention. All specifications except (1) include lender fixed effects, corresponding to controlling for a lender-specific intercept before and after the 2013 increase in down-payment requirements. In Panel A, specifications (1)-(4) focus on loan volumes in the full sample, specification (5) on the sub-sample of borrowers who borrow on RenrenDai both before and after the down-payment increase, and specification (6) on the subset of borrowers who borrow on RenrenDai only after. Panel B reports a number of additional checks: Specifications (1) only include the treated cities belonging to "Tier 2" cities (all treated cities other than Beijing, Guangzhou, Shanghai, and Shenzhen). In specifications (2) and (3), the sample is restricted to include only lenders who have been active on RenrenDai prior to November 2013, either because they registered (specification (2)), or because they actually made a loan (specification (3)). Specifications (4) and (5) report the estimate of:

$$\Delta L_{lb} = \alpha + \delta Treated_{bc} \times House\ owner_b + \beta_1 Treated_{bc} + \beta_2 House\ owner_b + \mu' \Delta x_{bc} + \varepsilon_{lb}$$

where $Treated$ is additionally interacted with an indicator variable $House\ owner$ equal to 1 if borrower b owns a house. In both panels and all specifications, price and province controls include the % change in the house price index in the previous 6 months, province GDP per capita, and province GDP per capita growth rate. City controls include city GDP per capita growth rate, city unemployment rate, and real wage growth. Household finance controls include city household net debt over income. Platform penetration controls include province population growth rate, and growth of the RenrenDai platform between Jan 2012 and Nov 2013. The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

A. Baseline

	Full Sample				**Intensive margin**	**Extensive margin**
	(1)	(2)	(3)	(4)	(5)	(6)
$Treated$	0.137**	0.070*	0.093***	0.064**	0.014	0.067**
	(0.053)	(0.035)	(0.031)	(0.026)	(0.015)	(0.027)
Controls:						
Price and Province controls	Y	Y	Y	Y	Y	Y
City controls	N	N	Y	Y	Y	Y
Household finance controls	N	N	Y	Y	Y	Y
Platform penetration controls	N	N	N	Y		
Region FE	Y	Y	Y	Y	Y	Y
Lender FE	N	Y	Y	Y	Y	Y
R^2	0.07	0.40	0.41	0.41	0.58	0.40
N	2,811,813	2,802,047	2,802,047	2,802,022	66,377	2,724,416

B. Additional Results

	Treated city: Tier 2	Pre-2013 active lenders: Registered	Lent	Borrower is a homeowner:	
	(1)	(2)	(3)	(4)	(5)
$Treated$	0.049*	0.081**	0.086**	0.047	-
	(0.028)	(0.032)	(0.034)	(0.034)	-
$Treated \times Home\ owner$				0.064**	0.052*
				(0.031)	(0.028)
Controls:					
Price and Province controls	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Household finance controls	Y	Y	Y	Y	Y
Platform penetration controls	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	Y
Lender FE	Y	Y	Y	Y	Y
City FE	N	N	N	N	Y
R^2	0.38	0.28	0.24	0.48	0.49
N	2,387,330	2,054,695	1,933,372	2,792,273	2,792,273

Table 6 Credit volumes and lender characteristics

The table reports the estimates of regressions with identical specification as in Table 5, estimated over alternative sub-samples defined by lenders' characteristics. Specifications (1)-(2) focus on whether the lender makes a loan via Uplan or direct peer-to-peer, specifications (3)-(4) on whether the lender's experience is low or high (below/above the median), and specifications (5)-(6) focus on whether the lender's portfolio size is small or big (below/above the median). The row labeled F test (p-value) below columns (1)-(2), (3)-(4), and (5)-(6) reports the F test statistic for the difference between the estimates of the coefficients on the $Treated$ indicator in the corresponding columns. The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	Lending channel		**Experience**		**Portfolio Size**	
	Direct	**Uplan**	**Low**	**High**	**Small**	**Big**
	(1)	(2)	(3)	(4)	(5)	(6)
$Treated$	0.019	0.110***	0.030***	0.074***	0.044**	0.083***
	(0.020)	(0.028)	(0.011)	(0.026)	(0.017)	(0.027)
Controls:						
Price and Province controls	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y
Household finance controls	Y	Y	Y	Y	Y	Y
Platform penetration controls	Y	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	Y	Y
Lender FE	Y	Y	Y	Y	Y	Y
R^2	0.62	0.37	0.70	0.37	0.60	0.37
N	699,455	2,098,721	1,403,582	1,396,908	1,386,137	1,413,897
F test	6.45		7.29		6.76	

Table 7 Marketplace loan funding speed around the 2013 increase in down-payment requirements
The table reports the estimates of:

$$y_{bt} = \alpha + \beta Treated_b + \gamma Post_t + \delta(Treated_b \times Post_t) + \mu' x_{bt} + \varepsilon_{bt}$$

Each observation corresponds to a given loan, made to a given borrower b on a given calendar date t. The dependent variable y_{bt} is the natural logarithm of the number of seconds between the time of posting of a loan offer on RenrenDai and the time of the last bid (columns (1)-(2)); and the natural logarithm of the number of seconds between the time of posting of a loan offer on RenrenDai and the time of the first bid (columns (3)-(4)). $Treated$ is an indicator variable equal to 1 if the city belongs to the treatment group (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Ningbo, Shanghai, Shenzhen, Suzhou, and Wuhan). $Post$ is an indicator variable equal to 1 over the period following a change in mortgage down-payment requirements. In all specifications, the vector of control variables x includes city fixed effects, calendar month fixed effects, administrative region × calendar month fixed effects, city house price % growth over the past 6 months, borrower age, income, number of applications the borrower, house owner, purpose of loan, size of borrowers' employers, growth of the RenrenDai platform between Jan 2012 and Nov 2013, and yearly macroeconomic controls province GDP level and past growth rates, city GDP per capita growth, province population growth, city unemployment rate, real wage growth, and city household net debt over income. Specifications (2) and (4) further include control variables on loan conditions: interest rate spread, loan duration, onsite verification (Y/N), borrower credit score, and the log-amount of loan size. The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	Time to fund a loan		**Time to first bid**	
	(1)	(2)	(3)	(4)
$Treated \times Post$	0.120	-0.062	0.131	-0.048
	(0.167)	(0.129)	(0.179)	(0.156)
Controls	Y	Y	Y	Y
City FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Region × Month FE	Y	Y	Y	Y
Loan conditions	N	Y	N	Y
R^2	0.51	0.58	0.57	0.63
N	64,888	64,725	64,861	64,698

Table 8 Marketplace loan pricing and screening of the borrowers around the 2013 increase in down-payment requirements

The table reports the estimates of:

$$y_{bt} = \alpha + \beta Treated_b + \gamma Post_t + \delta(Treated_b \times Post_t) + \mu' x_{bt} + \varepsilon_{bt}$$

Each observation corresponds to a given loan, made to a given borrower b on a given calendar date t. The dependent variable y_{bt} is the on-site verification indicator (specification (1)), the borrower's credit score ((2)), the interest rate spread associated with the loan ((3)), and the natural logarithm of the loan's time to maturity ((4)). $Treated$ is an indicator variable equal to 1 if the city belongs to the treatment group (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Ningbo, Shanghai, Shenzhen, Suzhou, and Wuhan). $Post$ is an indicator variable equal to 1 over the period following a change in mortgage down-payment requirements. In all specifications, the vector of control variables x includes city fixed effects, calendar month fixed effects, administrative region × calendar month fixed effects, city house price % growth over the past 6 months, borrower age, income, number of applications the borrower, house owner, purpose of loan, size of borrowers' employers, growth of the RenrenDai platform between Jan 2012 and Nov 2013, and yearly macroeconomic controls province GDP level and past growth rates, city GDP per capita growth, province population growth, city unemployment rate, real wage growth, and city household net debt over income. The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	On-site Verification	**Credit Score**	**Spread**	**Duration**
	(1)	(2)	(3)	(4)
$Treated \times Post$	-0.075*	-0.032	0.000	-0.032
	(0.041)	(0.021)	(0.000)	(0.033)
Controls	Y	Y	Y	Y
City FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Region × Month FE	Y	Y	Y	Y
R^2	0.52	0.24	0.50	0.53
N	64,888	64,764	64,927	64,927

Table 9 Marketplace Loan performance following the 2013 increase in down-payment requirements

The table reports the estimates of regression specifications analogous to Table 8, focused on loan performance. In specification (1), the dependent variable is delinquency, defined as the percentage of months during the borrowing period in which the borrower is delinquent; in specification (2), it is a default indicator. Specifications (3)-(4) focus on the loss conditional on default on a given loan; because these tests are restricted to loans that default, the number of observations is reduced. In specification (3), the dependent variable is the natural logarithm of the RMB loan amount; in specification (4), it is the natural logarithm of the outstanding amount of the loan at the time of default (i.e. the amount of the loan that has not yet been repaid when default occurs). In all specifications, the vector of control variables x is the same as in Table 8 with an additional variable: total number of lenders per loan. The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

			Loss given default	
	Delinquency	**Default**	**Loan size**	**Outstanding loan amount**
	(1)	(2)	(3)	(4)
$Treated \times Post$	0.010***	0.006**	2.559***	0.308***
	(0.003)	(0.003)	(0.610)	(0.087)
Controls	Y	Y	Y	Y
City FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Region × Month FE	Y	Y	Y	Y
R2	0.23	0.13	0.25	0.19
N	64,927	64,918	786	786

Table 10 Marketplace lending around the 2015 and 2016 changes in down-payment requirements

Panel A reports the estimates of regressions analogous to Table 5, estimated around the March 2015 change in down-payment requirements. The estimations are computed over the window October 2014-September 2015. In this case, the $House\ owner$ indicator equals 1 for house owners. Panel B report the estimates of regressions analogous to Table 5, estimated around the February 2016 change in down-payment requirements. The estimations are computed over the window September 2015-February 2017. In this case, the $Treated$ indicator equals 1 for all Chinese cities with at least 5 million inhabitants except Beijing, Guangzhou, Sanya, Shanghai, and Shenzhen. In all panels and specifications, control variables include province GDP per capita, province GDP per capita growth rate, province population growth rate, growth of the RenrenDai platform between Jan 2012 and Dec 2014, and the % change in the house price index in the previous 6 months. In column (3) panel A, additional city fixed effects are included. The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

A. Credit volumes – March 2015

	(1)	(2)	(3)
$Home\ owner$	-0.045***	-0.023***	-0.024***
	(0.007)	(0.003)	(0.004)
Controls	Y	Y	Y
Region FE	Y	Y	Y
Lender FE	N	Y	Y
City FE	N	N	Y
R^2	0.016	0.309	0.315
N	18,420,341	18,393,983	18,393,983

B. Credit volumes – February 2016

	Full Sample		**Intensive margin**	**Extensive margin**
	(1)	(2)	(3)	(4)
$Treated$	-0.083**	-0.052**	-0.044***	-0.051**
	(0.036)	(0.024)	(0.014)	(0.024)
Controls:	Y	Y	Y	Y
Region FE	Y	Y	Y	Y
Lender FE	N	Y	Y	Y
R^2	0.01	0.25	0.42	0.24
N	14,890,092	14,867,378	280,654	14,531,514

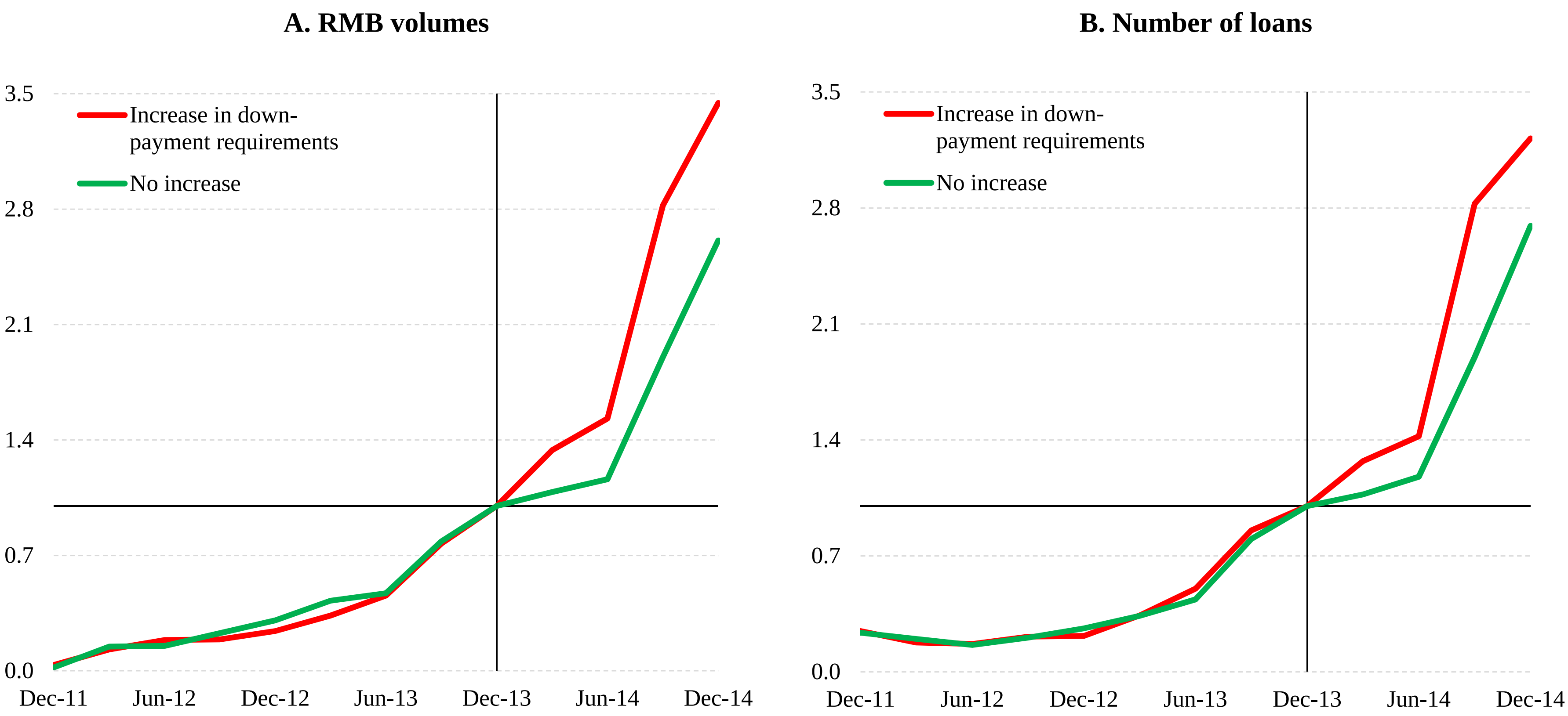

Figure 1 Marketplace loan applications at RenrenDai around the 2013 increase in down-payment requirements
The graphs plot the marketplace loan applications on the RenrenDai platform, for treated and control cities, around the 2013 increase in mortgage down-payment requirements. In panel A, the vertical axis reports the city-level RMB loan applications volume per capita, averaged across all treated cities (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Ningbo, Shanghai, Shenzhen, Suzhou, and Wuhan) and control cities (all other Chinese cities with population above 5 million). In panel B, the vertical axis reports the number of loan applications per capita, averaged across treated and control cities. We normalize each series so as to equal 1 on the date of the change in down-payment requirements (the fourth quarter of 2013), such that the vertical axis represents the relative change in marketplace loan applications compared to that date. The graph shows that, after the down-payment requirements increase, the growth in marketplace loan applications in the treated cities is higher than in the control cities.

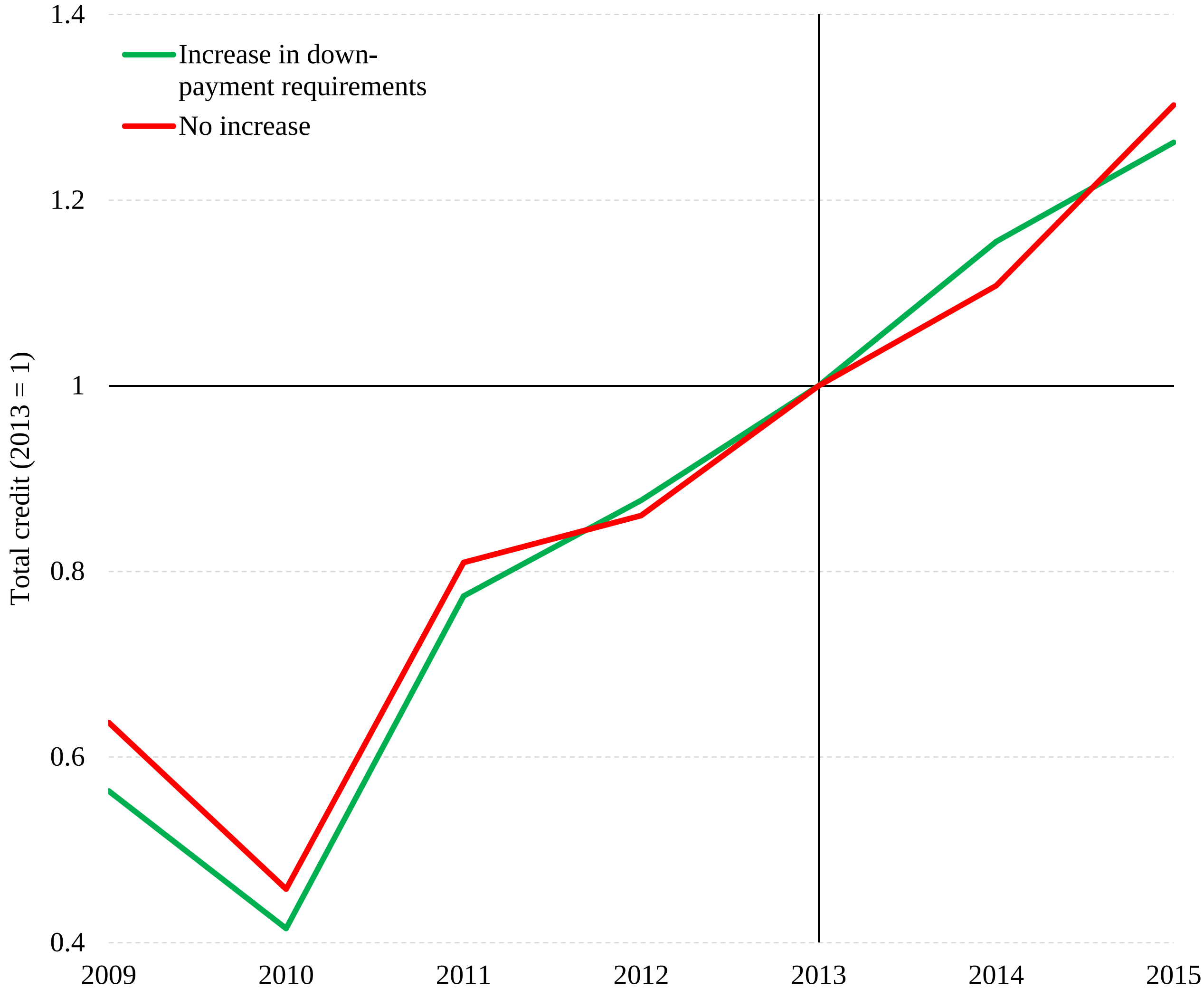

Figure 2 Total credit by Chinese financial institutions around the 2013 increase in down-payment requirements
The graphs plots the total credit extended by Chinese financial institutions, for treated and control cities, around the 2013 increase in mortgage down-payment requirements. The vertical axis reports the city-level RMB total credit, averaged across all treated cities (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Ningbo, Shanghai, Shenzhen, Suzhou, and Wuhan) and control cities (all other Chinese cities with population above 5 million). In all cases, total credit is rescaled so as to equal 1 in 2013. The graph shows no noticeable difference in the growth of total credit between treated and control cities after the increase of down-payment requirements.

Appendix A: Variable definitions

Variable	Definition
A. Loan characteristics	
Loan amount (RMB)	Amount of the loan in RMB.
Interest rate (%)	Annual interest rate applied to the loan.
Interest rate spread (%)	Annual interest rate minus the corresponding one-year Shibor rate.
Duration (months)	Maturity of the loan, expressed in number of months.
On-site verification (Y/N)	Indicator variable that takes the value of 1 if an officer from RenrenDai verified that the information provided by the borrower on the internet platform is true, by visiting the borrower at her stated address.
Credit score	Credit score assigned to the borrower by RenrenDai.
Proportion of Months Delinquent (%)	The proportion of months, over the loan's life, during which the borrower is delinquent. A borrower is delinquent if she misses or delays the monthly payment of the interest and/or the monthly repayment of the principal.
Default (0/1)	Indicator variable that takes the value of 1 if a loan is declared in default and 0 otherwise.
Time to fund a loan	The number of seconds between the time of posting of a loan offer on RenrenDai and the time of the last bid.
Time to first bid	The number of seconds between the time of posting of a loan offer on RenrenDai and the time of the first bid.
B. Borrower characteristics	
Income (RMB)	Borrower's monthly income at the origination of the loan. RenrenDai provides this information in brackets: between 0 and 1,000, between 1,001 and 2,000, between 2,001 and 5,000, between 5,001 and 10,000, between 10,001 and 20,000, between 20,001 and 50,000, and above 50,000 RMB.
Age	Age of the borrower at the origination of the loan.
College degree (0/1)	Indicator variable that takes the value of 1 if the borrower has a college degree or higher education level.
Male (0/1)	Indicator variable that takes the value of 1 if the borrower is a male.
Home Owner (0/1)	Indicator variable that takes the value of 1 if the borrower owns a house and 0 otherwise.
Number of applications since registration	Number of loan applications, at the time of the loan origination, made by the borrower since her registration in RenrenDai.
Total Amount Borrowed since registration	Total RMB borrowed by the borrower on RenrenDai at the time of the loan origination since her registration
C. Lender characteristics	
Portfolio size (RMB)	Size of lenders's portfolio, measured in RMB.

Portfolio size (nr. loans)	Size of lender's portfolio, measured in number of loans.
Uplan lending (% of RMB)	% of the lender's portfolio (measured in RMB) invested via Uplan.
Uplan lending (% of loans made)	% of the lender's portfolio (measured in number of loans) invested via Uplan.
Experience (number of loans since registration)	Experience of the lender, measured as the number of loans funded between the origination of the loan and the registration of the lender on RenrenDai.
Number of Lenders per loan	Number of lenders funding a particular loan issue on RenrenDai

D. Macroeconomic variables

Province GDP per capita	GDP per capita of the province where the borrower's city is located, retrieved from the CSMAR database.
Province annual GDP per capita growth	Annual GDP per capita growth of the province where the borrower's city is located, retrieved from the CSMAR database.
Province annual population growth (%)	Annual population growth of the province where the borrower's city is located, retrieved from the CSMAR database.
Monthly % change in house prices (past 6 months)	Average growth of house prices in the city during the past 6 months, retrieved from the China Index Academy databank.
Household net debt to income	Total city household debt minus total city households bank deposits divided by city GDP, retrieved from the CSMAR database.
Annual Real wage index growth.	Average growth of real wages per worker in the city. Real wages per worker is average nominal wage per worker in the city divided by the city's CPI (base, Shanghai in November 2013), retrieved from the CSMAR database.
Unemployment rate	Number of unemployed individuals in the city divided by the city labor force, retrieved from the CSMAR database.
RenrenDai penetration	Number of loan applications per city in a given year divided by city population (in thousands) in the same year.
Growth of RenrenDai penetration	% Growth of RenrenDai penetration between January 2012 and October 2013.
Region	Indicator variable describes the eight economic zones defined by the Development Research Center of the State Council in 2005. It takes the value of 1 if the provinces are Yunnan, Guizhou, Sichuan, Chongqing, and Guangxi. It takes the value of 2 if the provinces are Hubei, Hunan, Jiangxi, and Anhui. It takes the value of 3 if the provinces are Shanxi, Shaanxi, Henan, and Inner Mongolia. It takes the value of 4 if the provinces are Fujian, Guangdong, and Hainan. It takes the value of 5 if the provinces are Shanghai, Jiangsu, and Zhejiang. It takes the value of 6 if the provinces are Beijing, Tianjin, Hebei, and Shandong. It takes the value of 7 if the provinces are Liaoning, Jilin, and Heilongjiang. The remaining provinces take the value of 8.

APPENDIX B: Predicted impact of the change in LTV caps

To analyze the impact of online marketplace credit on the effects of changes in regulatory LTV caps (mortgage down-payment requirements), we rely on a framework that builds on Holmstrom and Tirole's (1997) workhorse fixed investment model. The rise in down-payment requirements to borrow from traditional lenders is analogous to a "collateral squeeze," which curbs credit in Holmstrom and Tirole's model. We show that the availability of marketplace lending allows borrowers to circumvent the tightened LTV cap, sterilizing its effects such that the levels of new credit are not reduced. These results allow us to formulate the key empirical predictions for our test.

First, we consider an economy populated by households (borrowers) and competitive traditional, regulated lenders ("banks"). At a later stage, we introduce unregulated ("marketplace") lenders. Households seek credit to acquire real estate, and when borrowing from a bank they are subject to an endogenous down-payment requirement $\bar{A}$ (derived below), plus an additional margin δ imposed by the regulator. We model the 2013 tightening of the LTV cap as an increase in δ, and study its effects on the total amount of debt promised interest payments level, and default rates in the economy.

As in Holmstrom and Tirole (1997), borrowers are subject to moral hazard. They are able to generate future cash flows $Y \in \{0, y\}$, which they use to pay back their loans. The probability of positive cash flows $\Pr(Y = y) = p$ takes values in $\{p_L, p_H\}$, with $p_H - p_L = \Delta_p > 0$. A borrower needs to exert "effort" to raise the success probability to p_H, and the borrower's utility from not exerting effort is B.

Each would-be borrower has assets-in-place A, representing to her ability to meet a down-payment requirement, and needs to borrow $I - A$ to make her real estate purchase. If the borrower does not default, she splits her cash flow with the bank such that $y = d_b + d_l$, where d_l is her payment to the bank and d_b is the portion of cash flow she retains. If the borrower defaults, the bank recovers a value

$R < I$ (e.g. as the result of a foreclosure process).[36] R is exogenously given.[37] Intuitively, the bank wants to induce p_H, providing the borrower with an incentive contract.

The participation constraint for the bank is $p_H d_l + (1 - p_H)R \geq I - A$, i.e. the bank must expect a larger payoff if it makes the loan than if it holds on to its cash $I - A$. This implies:

$$d_l \geq \frac{1}{p_H}[I - A - (1 - p_H)R]. \tag{1}$$

Since banks are competitive, (1) holds with equality. The incentive compatibility constraint for the borrower is $p_H d_b \geq p_L d_b + B$, i.e. the borrower must prefer to exert effort, so that:

$$d_b \geq B/\Delta_p. \tag{2}$$

Combining (1) and (2) with the resource constraint $y = d_l + d_b$, as long as $y \geq B/\Delta_p + d_l$ we have the following condition for the bank to make a loan:

$$A \geq \bar{A} = I - p_H \left[y - \frac{B}{\Delta_p} + (1 - p_H)\frac{R}{p_h}\right] \tag{3}$$

Expression (3) implies that only borrowers with sufficiently high assets-in-place (i.e. able to meet the down-payment requirements) obtain credit.

To analyze the equilibrium of the credit market in this setting, suppose that there is a continuum of borrowers indexed by their assets-in-place A, distributed according to a cdf $G(A)$. The total amount of credit in equilibrium is then $I[1 - G(\bar{A})]$. Denoting the bank's required interest rate by i_l, by definition $d_l = (I - \bar{A})(1 + i_l)$, so that from expression (1) we have:

$$i_l = \frac{1}{p_H}\left[1 - \frac{R(1-p_H)}{I-\bar{A}}\right] - 1 \tag{4}$$

and the aggregate interest owed in the economy is $i_l[1 - G(\bar{A})]$.

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[36] This ingredient is not present in Holmstrom and Tirole's (1997) original formulation. We introduce it for two reasons. First, it simplifies the exposition in our setting. Second, it better reflects the reality of real estate mortgages, where the lending bank's recovery can correspond to the value of the property, following the foreclosure.

[37] One could think of an extension of this analysis where the value of R is determined in equilibrium. Household leverage could then affect the value of collateral R and impose fire sale externalities, similar to the arguments of Shleifer and Vishny (1992). Such externalities could provide a rationale for the regulator's intervention (i.e., raising δ with the aim of limiting credit growth).

Consider now the effects of an LTV cap tightening, in which the regulator mandates that borrowers possess an additional $\delta > 0$ over and above $\bar{A}$, in the form of a mandatory minimum down-payment requirement (in the language of Holmstrom and Tirole (1997), this is equivalent to a "collateral squeeze"). The resulting total amount of lending is $I[1 - G(\bar{A} + \delta)]$. As the function $G(\cdot)$ is monotone increasing, this is less than $I[1 - G(\bar{A})]$, i.e. the new LTV cap curbs the level of debt in the economy. Because the bank has a smaller exposure to each borrower, moreover, interest rates decrease via expression (4), and the aggregate interest costs are reduced. Finally, because there are fewer borrowers, there are also fewer aggregate defaults $(1 - p_H)[1 - G(\bar{A} + \delta)]$. In sum: tightening the LTV cap reduces new credit, as well as aggregate interest payments and defaults.

Suppose next that an additional set of lenders is introduced, which we refer to as the marketplace lenders. Unlike the banks, marketplace lenders are not subject to regulation enforcing a minimum down-payment requirement on their loans. Moreover, the marketplace lenders are assumed to be competitive and "small," in the sense that they cannot lend more than δ to any borrower. These assumptions mimic the features of the marketplace lending market in China (Deer, Mi, and Yuxin (2015)), as well as what we find in our data (discussed in the next section). A simple strategy for a borrower who fails to obtain credit from the bank because her assets-in-place are below $\bar{A} + \delta$, then, is to borrow $\bar{A} + \delta - A$ from the marketplace lenders, so as to be able to make the full $\bar{A} + \delta$ down-payment. We study whether this strategy can be sustained in equilibrium, and its implications.

In addition to being unregulated, the marketplace lenders differ from banks in two main respects. First, they are not collateralized, i.e. in the event of default their payoff is equal to 0. Second, they do not condition their lending decisions on the borrower's assets-in-place, but simply take her default risk as given. This implies that the participation constraint for the marketplace lenders is:

$$p_H d_{P2P} \geq \bar{A} + \delta - A, \tag{5}$$

where the marketplace lenders' payoff is scaled by p_H because the borrower also receives credit from the bank, which already provides the incentive to exert effort.[38]

A sufficiently large y ensure that in equilibrium borrowers turn to the bank first, and if they do not have sufficient assets-in-place they also borrow from the marketplace lenders to meet the down-payment requirement. Because the marketplace lenders are competitive, the marketplace participation constraint (5) holds with equality, and $d_{P2P} = (\bar{A} + \delta - A)/p_H$. The participation constraint for the bank (1) is now modified as $d_l = \frac{1}{p_H}[I - (\bar{A} + \delta) - (1 - p_H)R]$, and the incentive constraint for borrowers remains $d_b \geq B/\Delta_p$. The endogenous minimum level of assets-in-place $\bar{A}$ required to obtain credit from the bank is pinned down by the resource constraint $y = d_l + d_{P2P} + d_b$, and because the $\bar{A} + \delta$ terms in d_l and d_{P2P} cancel out, $\bar{A}$ is again given by expression (3). In other words, regardless of the size of the increase in the down-payment requirement δ, an identical mass $1 - G(\bar{A})$ of borrowers obtains credit, and the total level of debt in the economy is unchanged. Also note that for any size of the loan, marketplace credit is more expensive, because the bank is collateralized. The interest rate demanded by the marketplace lenders, implied by (5), is: $i_{P2P} = \frac{1}{p_H} - 1$, which is larger than $i_l = \frac{1}{p_H}\left[1 - \frac{R(1-p_H)}{I-(\bar{A}+\delta)}\right] - 1$, so that indeed a given borrower will only turn to the marketplace lenders if she cannot meet the down-payment requirement. In sum: the objective of the tightened LTV cap is to curb new credit. The availability of marketplace credit, however, sterilizes the cap, leaving new credit unchanged.

This analysis allows us to formulate our key empirical predictions. The 2013 increase in down-payment requirements corresponds to an increase in δ. Changes in δ do not affect the overall level of credit in the economy, but simply shift demand into and out of marketplace lending. Therefore:

[38] As we verify below, in equilibrium borrowers turn to the bank first, and only if they do not have sufficient assets-in-place A they also borrow from the marketplace lenders. This allows the marketplace lenders to "free ride" on the incentives provided by the bank. marketplace credit is still more expensive, because the recovery under default is 0 (while the bank recovers R). Alternatively, one could assume that the probability of default remain "low" (p_L) for marketplace loans, without changing the main conclusions.

Following the 2013 increase in down-payment requirements, we will observe a larger volume of new marketplace loans in cities that raise mortgage down-payment requirements (treatment group) than in other cities (control group). The effect will be the opposite around the 2015 relaxation of down-payment requirements.[39]

[39] This model provides a simple framework to form expectations on the impact of marketplace lending on the effectiveness of the 2013 (and, in a further test described below, 2015) policy intervention in the mortgage markets. A byproduct of its simplicity is that aggregate interest payments remain unchanged after the increase in down-payment requirements, because $\frac{\partial}{\partial\delta}(d_l + d_{P2P}) = 0$. Similarly, aggregate defaults are unaffected by down-payment requirement and remain equal to $(1 - p_H)[1 - G(\bar{A})]$, because individual borrower default risk depends on the realization of future cash flows Y. In particular, in this framework a default on the bank loan also implies a default on the marketplace loan as the borrower's cash flows are 0; but in practice mortgage maturities tend to be considerably longer than the average duration of a marketplace loan (2-3 years in our data, Table 1). A more flexible model might incorporate that feature; it might also generate increasing default rates as borrowers turn to marketplace lending (consistent with the evidence we discuss in section V). We feel that such a model is beyond the scope of our study, as our focus is mainly empirical.