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How do housing purchase limits affect firm default risks in Mainland China?

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Abstract

The rapid rise in the price of housing in Mainland China in the past decade raised concerns over the potential risks to the economy, leading to the implementation of a policy that placed a limit on housing purchases in many cities in 2010 and 2011. This paper, by using the difference-in-difference method, investigates the effect of the policy on firm default risks. It shows that the impact of housing purchase limits is not homogeneous across cities. While the policy has significantly lowered firm default risks in big cities (especially those cities caught in the first two rounds of policy implementation), it is ineffective in relatively small cities and, in some cases, even caused firm default risks to rise. Furthermore, the effectiveness of the purchase limit on firm default risks becomes weaker when sectors those firms belong to have limited links to the real estate sector.

While the purchase limit focuses on default risks arising from the demand side, the risk arising from the housing supply side remains under the current fiscal system and land sales mechanism. Therefore, the housing purchasing limit should be supplemented by fiscal reforms that could mitigate land price increase and hence lower default risks in the long run.

Keywords: Purchase Limit; Real Estate; Default Risk; Difference in Difference
JEL: C23, G32, R38

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1. Introduction

Since the early 2000s, the real estate sector has become the mainstream of the Mainland economy (Ren et al., 2014). The sector's fast development has not only pushed up property prices, but also induced investment boom in closely linked sectors. According to the National Bureau of Statistics, the housing price nationwide tripled during 2000-2013. In some cities, housing prices were way beyond household affordability. Meanwhile, the annual real estate investment and the fixed asset investment both grew by more than 20% on average. The ensuing concerns on high property prices and over investment prompted the government to take action in order to pull the economy back to its normal course. One of the measures the government took was to introduce housing purchase limits, specifying the condition under which households can buy houses and how many they can buy. On April 17, 2010, the State Council issued a notice, requiring that the municipal government in any city where the housing price was rising too fast should limit the number of units a household can purchase. Following the notice, the Beijing Municipal Government on April 30 announced that each household, no matter local or non-local, could buy one unit at most after the announcement day. This was the first round of policy implementation and Beijing is the sole city to take action.

On September 30, 2010, the Ministry of Housing and Urban-Rural Development (MHURD), the Ministry of Land and Resources (MLR) and the Ministry of Supervision issued a joint notice requiring that in cities where housing prices had increased too fast and were too high while housing supply was tight, should explicitly set the purchase limit. Twelve more cities, including Shanghai, Guangzhou, Tianjin, Nanjing and Hangzhou, decided to set the purchase limit based on this notice (Table 1). This was the second round of policy implementation. As in the first round, a household could buy a housing unit no matter how many units it already had and whether it was local or non-local.[1] The purchase limit therefore in the first two rounds was relatively loose.

On January 26, 2011, the State Council reinforced its policy guidelines ruling that a local household was eligible to buy one (more) housing unit only if the household already owned no more than one; A non-local household was eligible to buy one housing unit if at least one of household members had been working locally for more than one year and possessed no local property for the time being. Thirty more cities, mostly the provincial capitals plus several mid-sized cities along the east coast, announced to follow this new rule (Table 1). This was the third round of policy implementation.

On July 12, 2011, the State Council further stated that it was necessary for Tier-2 and Tier-3 cities to set the purchase limit because housing prices were rising too fast. Three more cities echoed this statement, which was the last group of cities to implement such policies (Table 1).

From beginning to end, 46 cities put the purchase limit in force with the Central Government playing a

[1] Due to China's Hukou system, local and non-local households are treated differently in a city in terms of social welfare and benefits. Urban social security scheme and public non-university education facilities are basically applied to local households.

crucial role in setting the policy theme. Even so, the degree of policy tightness varied across cities. Some cities only targeted local residents, while others targeted local and non-local residents. Some cities tightened the policy once for all, while others tightened several times (Table 2).[2]

While the purchase limit may cool the property market, it could increase the risk of economic slowdown. Checking the risk changes associated with the purchase limit would therefore provide useful information on policy effectiveness, helping future policy making.

In this paper, we investigate the impact of housing purchase limits on default risks. We use firm default likelihood as the risk measure and apply the difference-in-difference (DID) method to carry out the analysis. Specifically, we estimate the default likelihood based on the Black-Scholes-Merton model, and evaluate the policy effects not only for different city groups, but also based on the risk transmission channels. We find that the purchase limit could effectively lower default risks in the Tier-1 and Tier-2 cities, but not in smaller cities. We also find that the effect of the purchase limit on default risks diminished when the associated sectors were less linked to the real estate sector. Despite the effectiveness of the purchase limit on curbing default risks in relatively large cities, default risks arising from supply side (especially land supply) remained as the purchase limit took effect mainly on the demand side. Fiscal reforms therefore should also be taken into consideration to mitigate such risks.

[2] Around 20 cities, including Hangzhou，Shijiazhuang and Xining imposed purchase limits in urban areas, which are not listed in Table 2.

Our study is a natural extension of previous studies on housing purchase measures. We extend the model by Wang and Huang (2013) to accommodate the imperfect competition market, showing how the purchase limit affects the property price and investment, and how the uncertainty in policy duration affects its effectiveness. We complement the literature by explicitly connecting housing purchase policy to default risks in terms of city groups and risk transmission channels. Meanwhile, the effect of land supply on default risks is also examined.

The paper is organised as follows: Section 2 briefs the related literature. Section 3 describes the principle of purchase limits and the empirical model and the data used to quantify the relationship between the purchase limit and firm default risks. Section 4 presents the empirical finding on the effect of purchase limits on default risks across cities. Section 5 investigates the effectiveness of purchase limits along risk transmission channels. Section 6 discusses the default risk from the supply side and its implications on housing policy. Section 7 concludes.

2. Related literature

As the study is about credit risks associated with housing purchase limits, we first briefly review the literature on housing price limit, then on credit risks.

2.1. Literature on real estate market and purchase limits

There is a large volume of research literature on real estate markets, of which one stream focuses on the relationship between real estate market development and economic or industrial activities (see, for example, Leung et al. (2011); Peng et al. (2008); Jin et al. (2012); Green (1997); Coulson and Kim (2000); Liu et al. (2002), Liu and Zhang (2006); Liang et al. (2006); Li (2002); Cao (2003); Liu and Wang (2004); and Liang et al. (2006)). Another stream of literature explores the effect of housing market development on factor markets and the efficacy of resource allocation (see for example, Chang et al. (2010); Fernandez (2007); Deng and Zhang (2011); Yan and Zhu (2013); Wu (2014); Wang and Huang (2013)). A third stream tests housing price bubbles (see for example, Deng at al. (2015); Ahuja et al. (2010); Kuang (2008); and lv (2010)), or identifies the determinates of the property price (see for example, Leung et al. (2011); Peng et al. (2008); Huang et al. (2015); Ahuja et al. (2010); Brian & Gete (2014); Glindro et al. (2011); and Wu et al. (2012)). A fourth stream discusses real estate policies, such as property taxes, down-payment ratio, housing price limit and purchase limit. Studies on housing purchase limits are naturally centred on the Chinese housing market, as purchase limits are rare across the world.[3]

Theoretically, the effectiveness of housing purchase limits depends on housing market conditions. For example, Hu and Sun (2011), treating households who are affected by housing purchase limits as a sub-sample drawn from the whole demand population, prove that the smaller the difference in

[3] Studies on purchase limits often tackle other housing policies as well.

household willingness to purchase properties, the more welfare loss would be. Liu (2013), in a general equilibrium model with properties being assumed to be the substitute of consumption goods, demonstrates that the scale of economy in the real estate sector and other sectors (together with down payment ratio) determines whether the purchase limit can lower or increase housing prices. Wang and Huang (2013) construct a long-term dynamic model where the policy could cause the demand curve to shift parallelly. It shows that the more restrictive the policy, or the longer the policy lasts, the more effective it is to lower the return on real estate investment, and hence the property price, even though property developers may reduce housing supply, causing an upward shift of the supply curve.

Empirical studies reveal that the purchase limit is effective in lowering housing prices or transaction volumes in the short run. For instance, Du and Zhang (2015), by choosing 21 cities that didn’t launch housing purchase limits as a control group to conduct a counter-factual analysis, estimate that the purchase limit lowered the price index for newly constructed properties in Beijing by 7.7%. On the other hand, Sun et al. (2014), by applying the regression discontinuity design (RDD) technique to a large housing transaction and lease dataset, detect that the policy caused the secondary properties in Beijing to fall by 17-32%, while rents were largely unaffected. Their findings are consistent with Wang and Huang (2013), who apply the fixed-effect model and the correlated random trend model to 70 city price indices. They confirm that the policy is more significant in curbing the price for secondary properties than for newly constructed ones. Guo et al. (2012) apply the TEI@I method (i.e., integration of text mining, econometrics and intelligence) to investigate the policy impact on housing transaction volume. They discover that the policy had larger effect in Beijing than in Shanghai and Shenzhen, but its effectiveness faded as time passed. Qiao (2012), by applying the DID method to 70-city panel data, estimates that the policy on average had lowered the housing price by 2.5% while raising transaction volume by 0.15%. Zhang and Zheng (2013), also applying the DID method to 70-city panel data, differentiate the tightness of the policy in terms of population origins and property locations, concluding that the purchase limit was more effective to curb housing prices when it targeted both non-local and local residents and suburb areas of a city, as housing speculation was citywide and appeared to be the most powerful driving force behind the rising prices and booming housing investment.

2.2. Literature on credit risks

The reduced form approach and the structural approach are two ways to deal with credit risks. The reduced form approach includes discriminant analysis, the binary response model, and the duration model. Discriminant analysis is pioneered by Beaver and Altman (1968), which generates credit scores (such the Z-score) from financial statements. The binary response model employs Logit or Probit regressions to explain the firm default rate with fundamental variables, where the firm default rate is constructed based on historical firm failure. The duration analysis, on the other hand, uses discrete-time or Cox proportional hazard function to link default probability or default intensity to explanatory variables (See Ohlson (1980); Zmijewski (1984); Shumway (2001); Hillegeist at al. (2004);

Campbell at al. (2008); Altman at al. (2011); and Duan at al. (2012)). Shumway (2001) proves the equivalence between the discrete-time hazard model and the multi-period Logit model when hazard function is the cumulative density function of firm failure in the Logit model.

Structural approach on default risks is pioneered by Black and Scholes (1973) and Merton (1974), where zero-coupon debt is considered in modelling. Since then, further developments have been done by researchers to relax assumptions on default boundary, the asset movement process, interest rates, dividends, taxes, bankruptcy costs and risk appetite (Sundaresan (2013)). In general, the structural models can be classified into endogenous- and exogenous-default groups. The endogenous-default group is explored, for example, by Leland and Toft (1996), Anderson and Sundaresan (1996), and Anderson et al. (1996) and the exogenous-default group is explored, for example, by Longstaff and Schwartz (1995), Collin-Dufresne and Goldstein (2001), and Huang and Huang (2003). The two groups differ in the way the default threshold is treated. In the endogenous-default models, the timing of default is optimally chosen by borrowers, while in the exogenous-default models, the default point is predetermined or imposed in terms of simple rules. Leland (2004) compares the predictive power between endogenous and exogenous models for default likelihood of debts with different grades. He finds that, the exogenous models, with an appropriately chosen boundary, could always replicate the default predictions by the endogenous models, but not vice versa. Both groups of models predict the shape and level of default likelihood better in long horizon than in short horizon. Tarashev (2008) revisits these models, finding that in general these models deliver unbiased short and long term estimates. In addition, he points out that the difference in predictive power for different debt maturities documented by Leland (2004) lies in the nonlinear relationship between model inputs and the default likelihood and can be remedied by taking into consideration the Jensen inequality effect. Afik et al. (2016) assess the one-equation and two-equation applications of the Merton model, such as the volatility restriction method (Jones et al. (1984); Ronn and Verma (1986); Hillegeist et al. (2004); Eom at al. (2004); and Campbell et al. (2008)), the KMV method, the market value proxy method and its variants (Charitou et al. (2013); Bharath and Shumway (2008); and Afik et al. (2016)), as well as the barrier option method (Dionne and Laajimi (2012)), in default predictions. They find that the simplified applications perform better than its more complex and computational intensive variants, and the default predictions are more sensitive to asset return and volatility than to the default boundary. Duan and Wang (2012) contrast the single-equation and two-equation methods discussed in Afik et al. (2016) with the transformed-data MLE method initially introduced by Duan (1994), arguing that the MLE method is superior to other methods, especially when dealing with high-leverage features of financial firms. The MLE method is also able to provide statistical properties of the estimates.

In this paper we adopt the MLE method to estimate the default risk, which is then put in the DID regression functions as the dependent variable to gauge the impact of housing purchase limits across city groups and along the risk transmission channels.

3. Research framework and data

We first demonstrate how the purchase limit could affect property prices and investment, and further the default risk. Then we specify the difference-in-difference method, followed by data description.

3.1. Principle of the purchase limits

The effect of the purchase limit on housing price and investment can be demonstrated in a housing demand-supply diagram for a monopolistic competition market. In such a market the property developers enjoy a certain degree of pricing power in local areas while still facing competition from peers. As shown in Figure 1, the supply curve is simply the marginal cost curve *MC* and the demand curve is l_1 with the marginal revenue curve MR_1. Initially the equilibrium is reached at point A with price p^* and housing unit Q^* , where the condition that marginal revenue is equal to marginal cost is satisfied. The implementation of the purchase limit suppresses housing demand, causing the demand curve to shift down to l_2 with the new marginal revenue curve MR_2. The new equilibrium is located at point B with price P_1 and housing unit Q_1, where the marginal revenue is equal to the marginal cost.

It should be mentioned that the property developers might reduce (or delay) housing investment and supply so that the *MC* curve moves inward to *MC'*. In this case the new equilibrium would be likely located at point B' with price P_1' and housing units Q_1', so that $P_1<P_1'<P^*$, and $Q_1'<Q_1<Q^*$. No matter how the *MC* curve behaves, both the price and the units built in general tend to drop under the

purchase limit.[4] It can be proved that, the stricter the policy or the longer the policy duration is, the more the price will decline (see the appendix for the proof).[5]

While Figure 1 only shows how the purchase limit affects property price and property development, its impact on default risks lies in the linkage between the real estate sector and other sectors. There are several channels through which the real estate sector is linked to other sectors and hence the default risk is propagated (Chan et. al., 2015): (1) The purchase limit affects the business activities of firms that are vertically integrated with the real estate sector; (2) Other sectors use their property as collateral to obtain bank loans. The purchase limit affects the value of collateral and hence firm financing behaviour;[6] (3) The purchase limit affects local government revenue from land sales, which in turn affects government expenditure and hence firm business activities. A change in firm business activities will affect firm value, which is reflected not only in firm balance sheets, cash flow statements and income statements, but also in equity prices, thus providing necessary information for default risk estimation. Following the Black-Scholes-Merton framework, we define firm default likelihood as a normal distribution function *N(.)*. Specifically, the 1-year default likelihood *DL* of a firm is given by

$$DL = N(-(V/D+(\mu-\delta^2/2))/\delta) \qquad (1)$$

[4] This result implies that the demand curve is parallelly shifting down. If the demand curve shifts downward just slightly and the remaining buyers are much more insensitive to the price change (i.e., the demand curve becomes steeper), then the new equilibrium price might change little. That would happen if the effect of substitution between big units and small units as well as investment motivation was strong.

[5] This also means that, if economic agents anticipate the policy is short-lived, then it will become less effective.

[6] This kind of acceleration principle is also applicable to the real estate sector itself (Case et al, 2000).

where V is the firm's implied asset value with its volatility δ and trend growth rate μ, and D is the corporate debt.

In principle, if the purchase limit curbs rapid housing price inflation and exuberant housing investment, cooling the economy through the real estate sector's direct and indirect linkages with other sectors, then the value and equity price of the firms in the related sectors would adjust accordingly, leading to a change in the distance to default and hence their default likelihood in the first place. In general the distance to default tends to rise and firm default likelihood to decline under purchase limits. Nevertheless, there is a possibility that the purchase limit depresses the economy too much so as to increase firm default likelihood. We find both scenarios are likely to take place.

3.2. Empirical method

Default risks vary across firms and across time, on which the impact of the purchase limit can be identified after controlling for fixed effects for firm characteristics over time. We apply the DID method to carry out the task. Specifically the regression reads

$$Y_{irt} = \beta_0 + \beta_1 Q_{rt} + \gamma X + \delta_{ir} + \theta_t + \varepsilon_{irt} \quad (2)$$

where i denotes firms, r cities in which firm i is located, and t the period. The default risk variable, $Y_{irt} = ln((1-DL_{irt})/DL_{irt})$, is a monotonically decreasing function of default likelihood DL for firm i in city r at period *t*. Q_{rt} is a dummy variable for purchase policy: $Q_{rt}=1$ means that the purchase limit is in effect in city *r* at period *t* and $Q_{rt}=0$ otherwise. X is a vector of control variables including firm performance *(Perform*), liquidity (*Liquid)* and *leverage (Lever)*. As in typical DID models, δ_{ir} controls for fixed effect in firm and city level, while θ_t controls factor effects on default risks over time, with ε_{irt} the error term.[7] β_0, β_1 and γ are coefficients, or vector of coefficients, among which β_1 is our focus.

Model (2) assumes that the impact of the purchase limit on default risks is homogenous across firms. To investigate the effect of the purchase limit each round on default risks, the following model is applied:

$$Y_{irt} = \beta_0 + \beta_1 Q_{rt}^1 + \beta_2 Q_{rt}^2 + \beta_3 Q_{rt}^3 + \beta_4 Q_{rt}^4 + \gamma X + \delta_{ir} + \theta_t + \varepsilon_{irt} \qquad (3)$$

where Q_{rt}^j (*j=1,2,..,4*) is the dummy variable for purchase policy: $Q_{\mathrm{rt}}^j=1$ means that the j^{th} round purchase limit is in effect in city *r* at period *t* and $Q_{\mathrm{rt}}^j=0$ otherwise.

A framework similar to Model (5) is applied to investigate the transmission channels through which the purchase limit affects default risks. In addition, it is extended to discuss the impact of supply side factors on default risks, where the land price is added as an additional explanatory variable.

[7] *Perform*, *liquid*, *lever* are all in one lag to address endogeneity issues. The dummy variable Q_{rt} might also be endogenous, as the first two rounds of purchase limits were implemented in large cities where the rising property price was easy to spot, on which we make a cross-check later. Nevertheless, fixed-effect dummy and control variables could to certain extent alleviate this problem.

3.3. Data

Data for 2484 listed firms, including stock prices, firm liabilities and equity, and firm revenues, are from Wind, ranging from 2003Q1-2013Q2. Except for stock prices that are on daily basis, firm-level data are on a quarterly basis abstracted from firm financial reports. The city-level data, such as land prices and real estate investment, are from CEIC. The default likelihood *DL* for each firm in each quarter is estimated by using Equation (1), following the estimation for the values of *V*, *δ* and *μ* with the maximum likelihood function proposed by Duan (1994):

$$L(\mu,\delta) = -[(n-1)/2]\ln(2\pi)-[(n-1)/2]\ln(\delta^2)$$

$$-\sum_{t=2}^{n} \ln V_t(\delta) - \sum_{t=2}^{n} \ln N(d_t) - \sum_{t=2}^{n} (\ln V_t(\delta) - \ln V_{t-1}(\delta) - u)^2 \tag{4}$$

conditional on

$$E = VN(d)-DN(d - \delta) \tag{5}$$

$$d = [\ln(V/D)+(\delta^2/2)]/\delta \tag{6}$$

Here corporate equity *E* is defined as the share of an owner's equity times the stock price, and corporate debt *D* as the short-term liabilities plus a half of long-term liabilities. Logically both *E* and *D* are affected directly or indirectly by business activities associated with the purchase limit. The same estimation methodology and the resultant default likelihood are applied in Chan et al. (2015).[8]

The estimated default risks are shown in Figure 2, where the cities are grouped into two sets, with one

[8] As daily data are used to estimate *DL* in a one-year rolling window, *E* and *D* taken or calculated from last quarter's financial reports are simply treated as daily value in the current quarter.

set of cities putting the purchase limit in force and the other not. The city-level default likelihood for each group is the mean of the default likelihood for the corresponding firms. Default risks appeared to be low before 2007. They climbed up in 2007-2008, peaked in 2008Q2 on the eve of the global financial crisis and then declined following government stimulus measures.[9] It appears that the cities which had implemented purchase limits had, in general, lower default risks than others after the policy took effect. We discuss it further in the following section.

We construct some control variables with firm characteristics based on the original firm-level data:[10]

(1) Perform. It is an indicator for firm profitability, i.e.,

$$Perform = \ln(1-RE/\hat{V})$$

where *RE* is retained earnings of each firm and $\hat{V}$ the estimated firm asset value. By definition, *Perform* is a decreasing function of retained earnings.

(2) Liquid. It is a measure of firm liquidity, i.e.,

$$Liquid = \ln(CA/\hat{V})$$

where *CA* is the current assets of each firm.

(3) Lever. It is a measure of firm leverage, i.e.,

$$Lever = \ln(\hat{V}/TL)$$

where *TL* is the total liabilities of each firm.

[9] According to our estimation, the rise in default likelihood was two months ahead of the financial crisis. Therefore it is a leading indicator for economic activities.

[10] Altman et al. (2011) uses firm characteristics to explain the default risk variable. Chan at al. (2015) follow the same method to check how important the firm-specific variables are in explaining the volatility of default risks.

(4) *Outpop*. It is the share of non-operating revenue in total revenue.

To investigate the transmission channels through which the purchase limit affects default risks as well as supply side factors associated with default risks, we collect or calculate the share of firm, non-core operating revenue in its total revenue (*outpop*), the growth rate of the urban real estate price (*Hprg*), the urban real estate investment (*Rinv*, in 100 million yuan), and the land price (*Lprice*, in thousand yuan) originated from CEIC. The statistics of the main variables are presented in Table 3.

4. Basic results

We first estimate Models (2)-(3) by pooling all the cities in the sample together followed by placebo test and cross-check by RDD method. Alternatively, we conduct regressions based on the tightness of the purchase limit and on the different tier of the cities.

4.1. Are the impacts of purchase limits on default risks homogeneous?

The estimates by DID method are reported in Table 4, where Regression (1)-(3) are based on Equation (2) and Regression (4) is based on Equation (3). Regressions (1)-(2) refer to the classical DID regression without additional control variables. Since the dependent variable *Y* is a monotonically decreasing function on the default likelihood, it shows that the purchase limit lowers the credit risk significantly. Given that the sample mean of the dependent variable is 4.89, which is equivalent to a default likelihood of 7.5%, an increment of 0.493 in Regression (2) means the credit risk is reduced by 27 basis points, or 35.9%, due to the purchase limit. In Regression (3), lagged variables describing firm characteristics as well as Y_{-1} are added to the model. It shows that the dependent variable increases by 0.296, equivalent to a reduction in the credit risks by 19 basis points, or 25.5%, due to the purchase limit.

In Regression (4) we remove the restrictive assumption of homogeneous effects of the purchase limit. Clearly, each round of purchase limits has a distinctive effect on credit risks. The first and second rounds of purchase limit significantly reduce the default risk, however, with decreasing effects. The impact of the purchase limits becomes weaker and insignificant in the third round. More interestingly, the coefficient to the fourth round of the purchase limit turns out to be negative and significant, meaning the purchase limit increases firm default risks. This is because the cities in the first two rounds of policy implementation are relatively large, where housing demand is strong and urban industries are more diversified. Thus the purchase limit lowers default risks without destroying the local economy. In small cities where housing demand is relatively weak, or where the real estate sector is a sole dominating sector, the purchase limit is either non-binding, or would bring the local economy to a standstill, leading to an increase in default risks. In short, purchase limits in general are effective in reducing default risks, but this is not true for every city. The policy, indeed, should be differentiated across cities and time horizon.

The control variables also merit noting. One of the control variables is the lagged dependent variable Y_{-1}, to which the coefficient is significant, meaning that the default risk could stay for certain periods, other things being equal. Other control variables include firm profitability (*Perform*), firm liquidity (*Liquid*), and firm leverage (*Lever*), which are also in lagged form. While the sign of the coefficient to *Lever* seems puzzling, the coefficients to *Perform* and *Liquid* coincide with common sense that the good firm performance and high liquidity tend to reduce default risks.

4.2. Placebo test

The key assumption of the DID method is that the test group (i.e., cities that imposed purchase limits) and the control group (i.e., cities that didn't impose purchase limits) have no distinct growth path in credit risks before the policy implementation. Otherwise, the estimates would be biased. Furthermore, if the two groups have no significant difference in their growth path in credit risks before the policy implementation, then the causal effect of default risks on policy implementation (i.e., the endogeneity issue) should be limited. We therefore conduct pPlacebo test in order to verify this assumption for the sample period of 2003Q1-2010Q1. The result is presented as Test (1) in the second column of Table 5, where the dummy $Qr=1$ if City r is a city that imposed the purchase limit, and $Qr=0$ otherwise. The coefficient to Qr is insignificant, suggesting that the parallel growth path in credit risks of the two groups cannot be rejected before the purchase limit was imposed.[11]

Another question is whether agents had anticipated the purchase policy before it was actually imposed. If so, the policy effect would have been observed before they were imposed. To test it, we visualise that the purchase limit was imposed in 2009Q1, 2009Q1 and 2008Q4 respectively, the time when the stimulus package was launched. Test (2), Test (3) and Test (4) list the test results. The coefficient to Q_{rt} is insignificant in all three tests, indicating that it is the policy itself, rather than anticipation of policy implementation, which affects the default likelihood. We also visualise the policy was imposed in 2008Q1 when the global financial crisis erupted. As shown in Test (5), the coefficient is still insignificant, supporting the view that the decline in default likelihood is caused by the purchase policy itself.

4.3. RDD method: a cross check

As we mentioned earlier, Sun et al. (2014) applied the RDD method to detect the impact of purchase policy on Beijing property prices. The RDD method is applicable to a sample when there is a break in the observations at a threshold. The RDD method assumes that the test group and the control group around the threshold would have had a similar behaviour in the absence of the policy, and hence can be mimicked by a flexible function of a continuous forcing variable (in addition to some covariates). Any difference in the outcome observed below and above the threshold is viewed as the policy effect. In our case, the threshold is the time when a purchase limit is imposed. Figure 3 shows a scatter of the

[11] We also test each city group classified by different rounds of policy implementation versus the control group, and confirm that each test group and the control group have no distinct growth path in the outcome before purchase limits were imposed.

default likelihood, where the horizontal axis (*dt*) represents the number of quarters below (with negative value) or above (with positive value) the threshold. Indeed, there is a visible break in the default likelihood at the threshold. The average default likelihood was trending up below the threshold while trending down above the threshold (Figure 3).

One advantage of the RDD method is its robustness to the endogeneity problem of the policy variable incurred by missing variables. In addition, the control group and the test group are perfectly matched. As a cross check, we estimate the policy effect by using the RDD method. The nonparametric RDD regression reads

$$y_{irt} = \beta_0 + \beta_1 Q_{rt} + \beta_2 dt_r + \beta_3 Q_{rt} dt_r + \gamma X_{irt} + \delta_{ir} + \theta_t + \varepsilon_{irt} \qquad (7)$$

where dt_r denotes an observation's distance to the threshold in city *r*. while the slope of the left fit and the right fit in Figure 3 is captured by β_2 and (β_2+β_3) respectively, the policy effect is captured by β_1. The sample only includes the cities that imposed purchase limits. The results are reported in Table 6. Regressions RDD(1), RDD(2), and RDD(3), with whole sample, reveal that the purchase limit Q_{rt} has significant effects on default likelihood. As shown in regressions RDD(4)-RDD(6), when the bandwidth becomes smaller, the coefficient to Q_{rt} is still significant.

We also run parametric RDD regressions by replacing the first order term of dt_r and $Q_{rt}dt_r$ with their 2nd-order polynomials in the regression function. Though the results are not reported here, the coefficient to Q_{rt} is significant in all regressions except the one with the narrowest bandwidth (i.e., 2008Q2-2013Q2). Even so, the sign of the coefficient in that regression is still correct. This cross check suggests the results from the DID method are pretty robust.

4.4. Housing purchase limits under alternative classification of city groups

Here we further investigate the heterogeneous effects of the purchase limit on default risks. As shown in Table 2, the tightness of the purchase limit is different across groups of cities. We divide the cities that had imposed purchase limits into four groups. While the purchase limit in Group 1 and Group 2 targeted only the local and non-local residents respectively, Group 3 contains cities where the purchase limit was set for the local and non-local residents. Group 4 contains cities where the purchase limit not only targeted the local and non-local residents, but was also upgraded at least once. We run a DID regression for each group against the cities that did not impose the purchase limits. It shows the purchase limit targeting only local residents has no significant effect on default risks, while those targeting only non-local residents, or local and non-local residents, have a significant effect on default risks. The most restrictive purchase limit in Group 4 comes out with the most significant and the largest effect on credit risks (Table 7). The results suggest that capital and population inflows are critical for the development of local housing markets. The key to the success of the purchase limit is restricting non-local residents to purchase local houses regardless of fairness.

Alternatively, we divide the cities that had imposed the purchase limit into different tiers according to their population and output. Here Tier-1 cities include Beijing, Shanghai, Guangzhou, Shenzhen and Tianjin. Tier-2 cities are the 36 biggest cities, excluding Tier-1 cities, and Tier-3 cities are the biggest 70 cities excluding Tier-1 and Tier-2 cities. Tier-4 cities are the rest of the cities in the sample. Again we run a DID regression for each group against the cities that did not impose the purchase limit. As expected, the purchase limit has a significant impact on default risks in Tier-1 cities, followed by Tier-2 cities (Table 8). However, the coefficient to Q_{rt} becomes negative and insignificant for Tier-3 and Tier-4 cities, echoing the previous finding that it is not necessary to impose housing purchase limits in smaller cities, as they are either ineffective or could even cause higher default risks.[12]

5. Purchase limits and the risk transmission channels

We have argued earlier that the purchase limit could in general lower the property price, leading to a fall in investment, which tends to lower default risk in the real estate sector. Such an effect would spread through sectoral network or production chains. In this section we examine how the purchase limit works through risk transmission channels.

5.1. Real estate price, real estate investment and purchase limits

[12] Rather than grouping samples by cities, we also group firms in terms of their ownership (i.e., State-owned enterprises vs. Private firms) and geographic locations (i.e., East, Central, and West). We find the purchase limits are effective in reducing credit risks both for private and SOE firms, and the difference in the degree of effectiveness is not large. The purchase limits are also effective in reducing default risks for firms in different locations. The degree of effectiveness is more significant in East than in other regions. We do not report the detailed results here.

We first check the impact of the price limit on urban housing price growth (*Hprg*) and property investment (*Rinv* in logs), then the impact of property investment on default risks. The regression results are presented in Table 9. As expected, the coefficients to Q_{rt} in Columns 2-3 are significantly negative, meaning the purchase limit tends to lower both the property price and property investment. Specifically, the purchase limit lowers the growth of property prices by 0.36 percentage points and property investment by 11.3%. The coefficient to property investment in Column 4 is also significantly negative, meaning the higher real estate investment tends to increase default risks. Combining these results suggests that the purchase limit in general lowers default risks through housing price and investment adjustment.

5.2. Production chains and the impact of purchase limits

The real estate sector links to other sectors through sectoral network and production chains, and default risks from the real estate sector can spill over to other sectors through such linkages. The impact of the purchase limit on default risks tends to diminish along production chains as risk spill-over weakens with the distance of other sectors to the real estate sector. In this sense, the impact of the purchase limit on default risks in the real estate sector and its closely linked sectors should be larger than that in its loosely linked sectors. To verify this, we divide all the sectors in the sample into three groups. The first group is the real estate sector itself, the second group consists of the sectors closely linked to the real estate sector, and the third group is the rest of the sectors.[13] Table 10 reports the regression results for all the three groups of sectors. While the purchase limit has a significant effect in reducing the default risk in real estate sector and its closely linked sectors, it has no significant effect on default risks in loosely linked sectors.

6. Supply side effect, purchase limits and default risks

The purchase limit aims to lower default risks arising from the demand side. However, the default risk could also emerge from the supply side, which is likely due to the current land supply system. After the fiscal reforms in 1994, the local governments only contain a small share of the fiscal revenue while the big chunk has been taken away by the central government. As such, the local governments have to rely on land revenue to finance their expenditure. Since 2003, the land transfer agreement has been gradually replaced by land auction, which has greatly increased the land price.[14] During 2003-2012 the land price by auction for residential properties on average increased by 20% annually (Figure 4). We verify that the fast rising land price could lead to a higher property price and heavy investment, thus increasing default risks. The corresponding regression results are reported in Table 11.[15] In the first regression (Column 2), the land price (in logs) is the only explanatory variable while in the second regression (Column 3) both the land price and the purchase limit are the explanatory variables. In both

[13] According to default risk correlation, sectors that are closely linked to the real estate sector include Construction, Glass, Metal, Chemical, Plastic, Cement, Coal, Ship, IT, Electric, General and special equipment, Whole sale and retails, Hotel and restaurant, Heating-Gas-Electricity, Transportation and storage, Timber and Furniture, and Instruments.

[14] The full name for this kind of land sales is “Bid invitation, auction, and listing”.

[15] The land auction price is annually, so we assume that quarterly land price is the same as the corresponding annual land price for our regression purpose.

cases, the land price significantly increases default risks. It appears that, the effect of the purchase limit on default risks could well be offset by the effect of rising land prices, suggesting the fiscal reform to stabilise the land price should be considered as a long-term method to mitigate default risks associated with the rising housing prices.

7. Concluding remarks

China introduced housing purchase limits to curb fast rising property prices from 2010. This paper applies firm-level and city-level data to investigate the impact of this policy on credit risks. The study finds that in general, purchase limits significantly reduce credit risks and were more effective in the first two rounds of policy implementation, as the cities imposing such a policy were relatively large and had significant capital and population inflows. However, the purchase limit has a negligible effect on credit risks and can even cause credit risks to rise in smaller cities, as housing demand there is already weak or their industries are concentrated in the real estate or closely linked businesses.

The study finds that the scheme of land sales is one important factor causing credit risks to rise. Local governments prefer the land auction scheme in order to obtain maximum extra-budget fiscal income to support their expenditure, which has inevitably pushed up the housing price and hence the firm credit risk. As the distortion resulting from land sales of the local governments cannot be removed all at once, the purchase limit serves to mitigate the credit risks from the demand side.

However, the policy is by no means a long-term solution for the real estate market, in that the social welfare loss incurred could be large. When the aggregate demand is weakening, the purchase policy obviously becomes ineffective. As such, most cities (except several Tier-1 cities) have suspended the purchase limit since October 2013. Nevertheless, the purchase limit in Tier-1 cities could survive longer, witnessed by housing price surge in Shenzhen and Shanghai in 2015-2016 and the following policy tightening.[16]

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[16] In August 2015, the MHURD, PBOC, SAFE, Ministry of Commerce, National Development and Reform Commission and the State Administration for Industry and Commerce jointly announced lowering of the capital requirement for foreign real estate investment companies, and allowing foreign nationals working or studying in China and overseas companies' Chinese units to buy properties. However, housing purchasing policies in the three Tier-1 cities have been tightened further. In March 2016, the Shanghai Municipal Government protracted non-locals' tax payments or contributions to social security scheme from three consecutive years to five consecutive years at least before they are eligible to buy properties in the city. In addition, the minimum down payment for the second property was raised from 40% to 50% for ordinary properties and to 70% for high-end properties. Meanwhile, the Shenzhen municipal government protracted non-locals' tax payments or contributions to social security scheme from one year to three consecutive years at least before their housing purchase. In addition, the minimum down payment for the second property was raised from 30% to 40%.

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Table 1: Four rounds of the purchase limits

Rounds of policy	Policy start period	Implementing cities
Round 1	2010Q1	Beijing
Round 2	2010Q4	Shanghai, Tianjin, Nanjing, Dalian, Hangzhou, Ningbo, Fuzhou, Xiamen, Guangzhou, Shenzhen, Haikou, Sanya
Round 3	2011Q1	Harbin, Changchun, Shenyang, Hohhot, Urumqi, Yinchuan, Taiyuan, Shijiazhuang, Xining, Lanzhou, Xian, Jinan, Qingdao, Zhengzhou, Xuzhou, Wuxi, Hefei, Suzhou, Chengdu, Wuhan, Zhoushan, Shaoxing, Jinhua, Wenzhou, Nanchang, Changsha, Guiyang, Kunming, Nanning, Foshan
Round 4	2011Q3	Taizhou, Quzhou, Zhuhai

Table 2: Tightness of the purchase limits across cities

No restriction to non-locals	Restrictive to non-locals	Restrictive to non-locals, tightened more than once
Hohhot, Urumqi Yinchuan, Taiyuan Zhoushan, Changsha	Beijing, Shanghai, Tianjin, Nanjing, Dalian, Ningbo, Xiamen, Lanzhou, Xian, Shenzhen, Haikou, Sanya, Harbin, Changchun, Taizhou, Zhengzhou, Hefei, Wuhan, Jinhua, Wenzhou, Foshan	Beijing, Shanghai, Lanzhou, Tianjin, Shenzhen, Haikou, Zhengzhou, Hefei, Wuhan, Wenzhou, Kunming

Table 3: Statistics of main variables

Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
Y	54904	4.6	6.1	-16.64	87.8
Quota	64753	0.24	0.43	0	1
Perform	54682	-0.18	0.32	-6.69	14.91
Liquid	54174	0.72	8.96	0	1385.27
Lever	55055	1.75	0.69	-5.91	11.38
Outpop	37757	0.04	1.62	-0.01	263.78
Rinv	2517	4.58	5.66	0	28.58
Hprg	2517	4.94	5.26	-17	64.2
Lprice	2127	0.613	0.464	0.001	4.176

Table 4: Basic regression results

	Regression (1)	Regression (2)	Regression (3)	Regression (4)
	Y	*Y*	*Y*	*Y*
Q_{rt}	0.545***	0.448**	0.296**	
	(0.209)	(0.199-)	(0.119)	
Q_{rt}^1				0.591***
				(0.114)
Q_{rt}^2				0.310*
				(0.167)
Q_{rt}^3				0.219
				(0.169)
Q_{rt}^4				-0.575***
				(-0.159)
Y_{-1}			0.431***	0.431***
			(0.015)	(0.015)
$Perform_{-1}$			-0.658***	-0.659***
			(0.146)	(0.146)
$Liquid_{-1}$			0.297**	0.299**
			(0.125)	(0.125)
$Lever_{-1}$			-0.315*	-0.313*
			(0.164)	(0.164)
Company FE	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes
City dummy*time trend	Yes	Yes	Yes	Yes
Sector dummy* time trend		Yes	Yes	Yes
Ownership*time trend		Yes	Yes	Yes
Observations	53,700	48,805	45,505	50,332
R-square	0.192	0.216	0.349	0.331

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Placebo test

	Test (1)	Test (2)	Test (3)	Test (4)	Test (5)
		2009Q2	2009Q1	2008Q4	2008Q1
Q_r	-0.063				
	(0.061)				
Q_{rt}		0.071	0.059	-0.053	-0.155
		(0.168)	(0.156)	(0.146)	(0.134)
Company FE	Yes	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes	Yes
City dummy*time trend		Yes	Yes	Yes	Yes
Sector dummy* time trend	Yes	Yes	Yes	Yes	Yes
Ownership*time trend	Yes	Yes	Yes	Yes	Yes
Observations	27,214	45,505	45,505	45,505	45,505
R-square	0.284	0.349	0.349	0.349	0.349

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Other control variables are the same as in the last column of Table 4 and not reported here.

Table 6: Nonparametric RDD regressions

	RDD(1)	RDD(2)	RDD(3)	RDD(4)	RDD(5)	RDD(6)
	2003Q1 -2013Q2	2003Q1 -2013Q2	2003Q1 -2013Q2	2005.Q2 -2013.Q2	2006.Q4 -2013.Q2	2008.Q2 -2013.Q2
Q_{rt}	0.447***	0.356***	0.286**	0.282**	0.273**	0.232*
	(0.140)	(0.121)	(0.130)	(0.126)	(0.129)	(0.133)
dt_r	-0.197***	-0.112***	-0.057	-0.090	-0.154***	0.080
	(0.027)	(0.019)	(0.040)	(0.071)	(0.029)	(0.072)
$Q_{rt}dt_r$	0.438***	0.255***	0.173***	0.165*	0.153*	0.065
	(0.126)	(0.085)	(0.062)	(0.085)	(0.083)	(0.077)
Control Variables		Yes	Yes	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes	Yes	Yes
City dummy*time trend			Yes	Yes	Yes	Yes
Sector dummy* time trend			Yes	Yes	Yes	Yes
Ownership*time trend			Yes	Yes	Yes	Yes
Observation	33,493	31,265	31,265	28,798	25,162	20,815
R-sq	0.186	0.336	0.343	0.324	0.331	0.326

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Other control variables are the same as in the last column of Table 4 and not reported here.

Table 7: Effect of purchase limits on default risks with different policy tightness

	Group 1	Group 2	Group 3	Group 4
	Y	*Y*	*Y*	*Y*
Q_{rt}	0.175	0.314***	0.338**	0.440-***
	(0.381)	(0.121)	(0.133)	(0.149)
Company FE	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes
City dummy*time trend	Yes	Yes	Yes	Yes
Sector dummy* time trend	Yes	Yes	Yes	Yes
Ownership*time trend	Yes	Yes	Yes	Yes
Observations	16,353	43,392	35,043	33,065
R-square	0.368	0.351	0.348	0.346

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Other control variables are the same as in the last column of Table 4 and not reported here.

Table 8: Effect of purchase limits on default risks with different city scale

	Tier-1	Tier-2	Tier-3	Tier-4
	Y	*Y*	*Y*	*Y*
Q_{rt}	0.372**	0.373**	-0.390	-0.205
	(0.181)	(0.175)	(0.562)	(0.200)
Company FE	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes
City effects*Time trend	Yes	Yes	Yes	Yes
Sector dummy* time trend	Yes	Yes	Yes	Yes
Ownership*time trend	Yes	Yes	Yes	Yes
Observations	27,897	29,734	13,782	12,323
R-square	0.346	0.368	0.375	0.384

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Other control variables are the same as in the last column of Table 4 and not reported here.

Table 9: Impact of purchase limits through property price and investment changes

	Hprg	*Rinv*	*Y*
Q_{rt}	-0.362***	-0.113***	
	(0.07)	(0.008)	
Rinv			-0.153*
			(0.086)
Company FE	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes
City effects*Time trend	Yes	Yes	Yes
Sector dummy* time trend	Yes	Yes	Yes
Ownership*time trend	Yes	Yes	Yes
Observations	2,133	32,538	26,635
R-square	0.429	0.874	0.201

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Other control variables are the same as in the last column of Table 4 and not reported here.

Table 10: Production chain and the purchase limits

	Real estate sector	Closely linked sectors	loosely linked sectors
	Y	*Y*	*Y*
Q_{rt}	0.509**	0.469**	0.188
	(0.221)	(0.196)	(0.335)
Company FE	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes
City effects*Time trend	Yes	Yes	Yes
Sector dummy* time trend	Yes	Yes	Yes
Ownership*time trend	Yes	Yes	Yes
Observations	14,246	15,221	11,008
R-square	0.397	0.356	0.353

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Other control variables are the same as in the last column of Table 4 and not reported here.

Table 11: Land price and credit risks

	Y	*Y*
Lprice	-0.163***	-0.142**
	(0.066)	(0.066)
Q_{rt}		0.352***
		(0.126)
Company FE	Yes	Yes
Seasonal FE	Yes	Yes
City effects*Time trend	Yes	Yes
Sector dummy* time trend	Yes	Yes
Ownership*time trend	Yes	Yes
Observations	28,831	28,831
R-square	0.446	0.446

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Other control variables are the same as in the last column of Table 4 and not reported here.

Figure 1: Purchase limits and real estate price

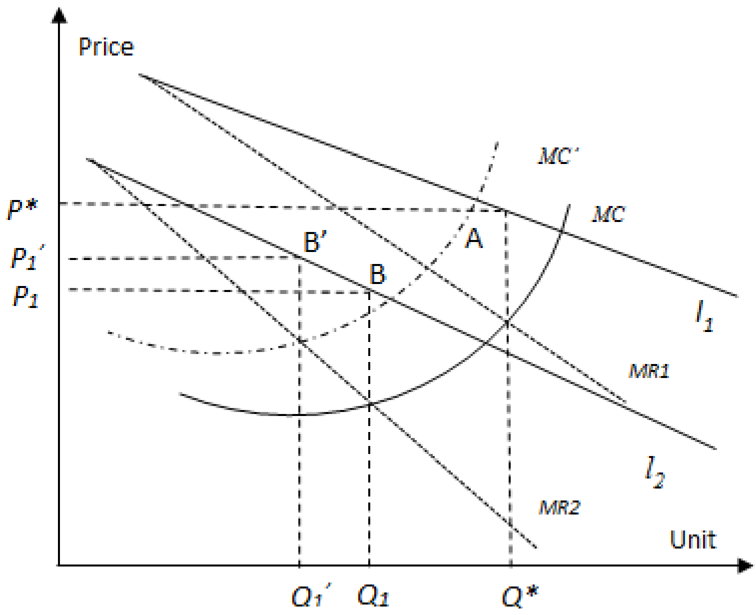


Figure 2: Default risks in cities with or without purchase limits

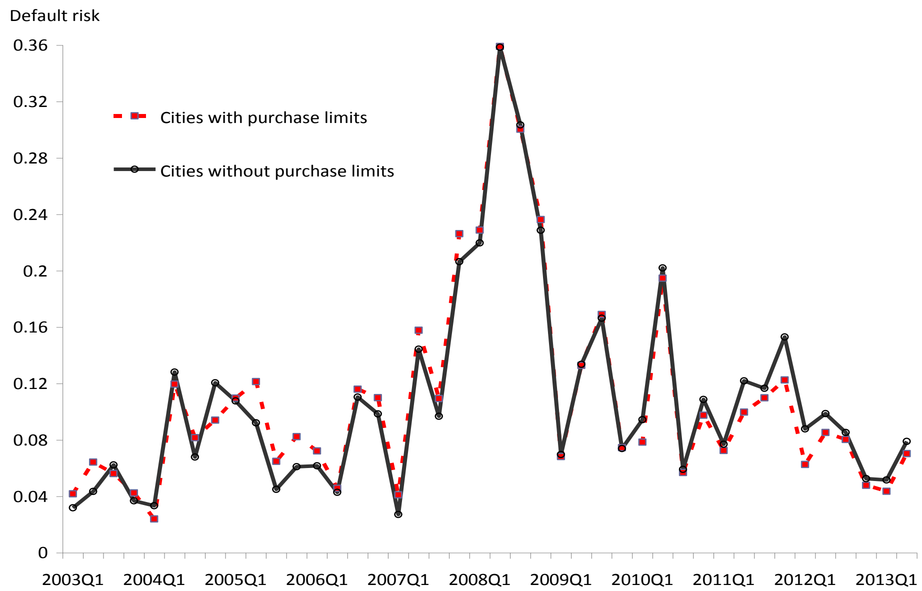


Figure 3: Scatter plot of default likelihood against the distance to the threshold

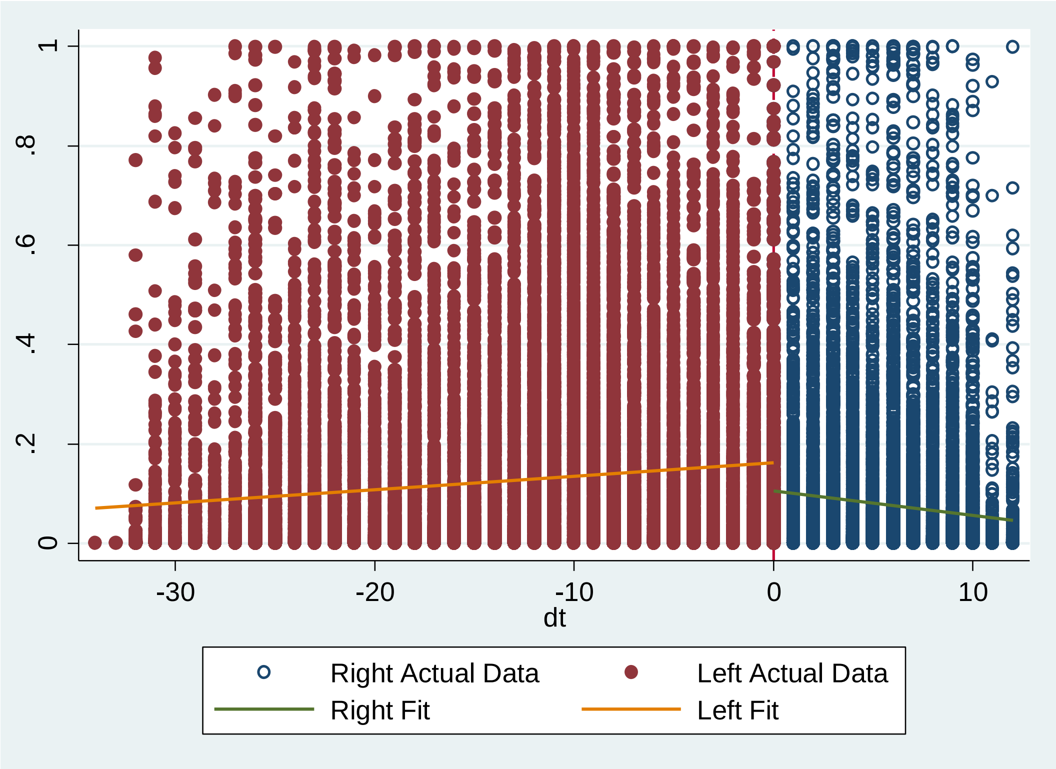
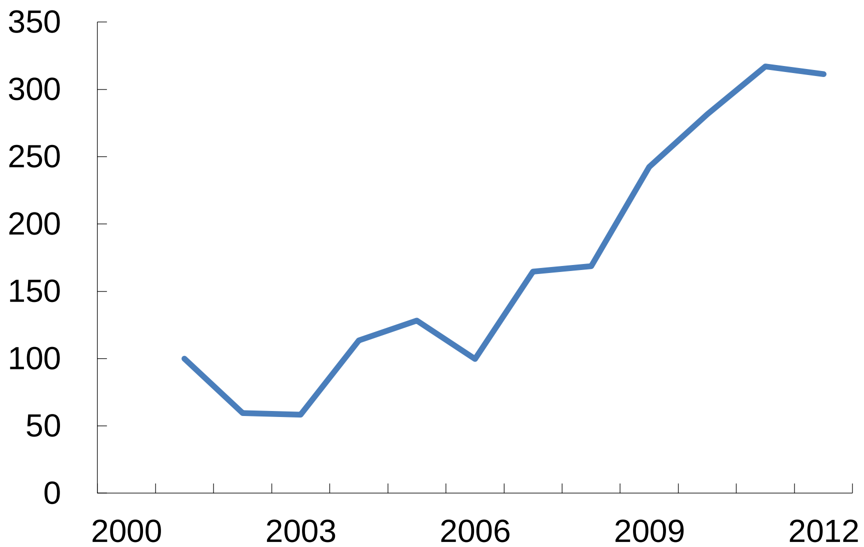


Figure 4: Land auction price for residential properties (Year 2000=100)



Appendix

This analysis is an extension to Wang and Huang (2013) by incorporating monopolistic competition to the model. The purchase limit is assumed to be in effect in the period of [0, *T*]. After time *T*, the purchase limit is simply terminated. A real estate developer has land stock s(0) at the beginning, which is also the maximal housing quantity he could provide all over the time. The developer chooses the housing quantity *q(t)* to develop and sell each period to maximise the present value of profits subject to the housing demand and development costs:

$$max_{q(t)} \int_0^{\infty} e^{-rt} [p(t) - c(t))q(t)]dt \qquad (A.1)$$

$$s.t. \;\; \dot{s(t)} = -q(t);\; s(0) = s_{0;}$$

$$p(t) = h\left(\frac{q(t)}{\alpha}\right), \quad t \in [0, T];$$

$$p(t) = h\big(q(t)\big), \quad t \in (T, \infty)$$

$$c(t) = k\left(\frac{q(t)}{\alpha}\right), \quad t \in [0, T]$$

$$c(t) = k(q(t)), \quad t \in (T, \infty)$$

where *p(t) = h(.)* is the inverse of the demand function, and *c(t)=k(.)* the housing development cost. The parameter α (0<α<1) represents the strictness of the purchase limit. The smaller the α is, the stricter the purchase policy is. The corresponding current value Hamiltonian is

$$H(t) = [p(t) - c(t)]q(t) - \lambda(t)q(t) \qquad (A.2)$$

where λ(t) is the shadow price. The Hamiltonian results in the following optimal solutions

$$[p(t) + p'_q(t)q(t)] = \left[c(t) + c'_q(t)q(t)\right] + \lambda(t) \qquad (A.3)$$

$$\dot{\lambda(t)} = r\lambda(t) \qquad (A.4)$$

where p'_q(t) and c'_q(t) are separately the derivatives of *p(t)* and *c(t)* with respect to *q(t)*. In fact, $[p(t) + p'_q(t)q(t)]$ in (A.3) is the marginal revenue of housing sales that is monotonically decreasing, and $\left[c(t) + c'_q(t)q(t)\right]$ is the marginal costs of housing development that is monotonically increasing. (A.3) shows that the marginal revenue of housing sales equals to the marginal costs of housing development plus the opportunity costs of housing sales. It follows from (A.4) that

$$\lambda(t) = \lambda(0)e^{rt} = \lambda_0 e^{rt} \qquad (A.5)$$

where $\lambda(0) = \lambda_0$ is the initial value of λ(t), and λ(t) satisfies the transversality condition:

$$\lim_{t\to\infty} e^{-rt}\lambda(t)s(t) = 0 \qquad (A.6)$$

Since *p(t)*, $p'_q(t)$, *c(t)*, $c'_q(t)$ are all the function of *q(t)*, let

$$f(q) = \left[p(t) + p'_q(t)q(t)\right] - \left[c(t) + c'_q(t)q(t)\right], \qquad (A.7)$$

then *f(q)* is a monotonic function on *q(t).* (A.7) and (A.3) mean that then *q(t)* can be rewritten as the inverse function of $\lambda(t)$. Taking into consideration the purchase limit condition yields

$$q(t) = \alpha f^{-1}(\lambda_0 e^{rt}), \;\; t \in [0, T] \qquad (A.8)$$

$$q(t) = f^{-1}(\lambda_0 e^{rt}), \;\; t \in (T, \infty) \qquad (A.9)$$

where λ_0 is implied in the following equation

$$\int_0^T \alpha f^{-1}(\lambda_0 e^{rt})dt + \int_T^{\infty} f^{-1}(\lambda_0 e^{rt})dt = s_0 \qquad (A.10)$$

that is obtained from the integral of $s(\dot{t}) = -\mathrm{q(t)}$ and the transversality condition.

In the period of [0,T], the property price can be expressed as

$$p(t) = h(f^{-1}(\lambda(t)) = h\big(f^{-1}(\lambda_0 e^{rt})\big) \quad \text{(A.11)}$$

and its derivative with respect to α is

$$\frac{\partial \mathrm{p(t)}}{\partial \alpha} = \frac{\partial h}{\partial f^{-1}}\frac{\partial f^{-1}}{\partial \lambda(\mathrm{t})}\frac{\partial \lambda_0}{\partial \alpha}e^{rt} \quad \text{(A.12)}$$

Since the slope of the demand curve is negative, $\frac{\partial h}{\partial f^{-1}}$*<0.* Also since the inverse function has the same monotonicity as the function, $\frac{\partial f^{-1}}{\partial \lambda(\mathrm{t})}$< 0. Taking derivatives to the both sides of (A.10) and applying $\frac{\partial f^{-1}}{\partial \lambda(\mathrm{t})} < 0$ yields

$$\frac{\partial \lambda_0}{\partial \alpha} = -\left[\int_0^T f^{-1}(\lambda_0 e^{rt})\mathrm{dt}\right](\int_0^T ae^{rt}\frac{\partial f^{-1}}{\partial \lambda_t}dt + \int_T^\infty e^{rt}\frac{\partial f^{-1}}{\partial \lambda_t}dt)^{-1} > 0 \quad \text{(A.13)}$$

Substituting (A.13) into (A.12) yields

$$\frac{\partial \mathrm{p(t)}}{\partial \alpha} > 0 \quad \text{(A.14)}$$

(A.14) states that, when the purchase limit cause a parallel shift of the demand curve, then more restrictive the policy is, the more the price would fall. Furthermore, the partial derivative of *p(t)* with respect to the policy duration *T* is

$$\frac{\partial \mathrm{p(t)}}{\partial \alpha} = \frac{\partial h}{\partial f^{-1}}\frac{\partial f^{-1}}{\partial \lambda(\mathrm{t})}\frac{\partial \lambda_0}{\partial \mathrm{T}}e^{rt} \quad \text{(A.15)}$$

Again taking derivatives to the both sides of (A.10) and applying $\frac{\partial f^{-1}}{\partial \lambda(\mathrm{t})} < 0$ yields

$$\frac{\partial \lambda_0}{\partial \mathrm{T}} = (1-\alpha)f^{-1}(\lambda_0 e^{rt})(\int_0^T ae^{rt}\frac{\partial f^{-1}}{\partial \lambda_t}dt + \int_T^\infty e^{rt}\frac{\partial f^{-1}}{\partial \lambda_t}dt)^{-1} < 0 \quad \text{(A.16)}$$

Substituting (A.16) into (A.15) yields

$$\frac{\partial \mathrm{p(t)}}{\partial \mathrm{T}} < 0 \quad \text{(A.17)}$$

(A.17) states that, when the longer the purchase limit stands, the more the property price would fall.