Quantity Restrictions on Investment in Residential Real Estate: A Within-Market Analysis

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Abstract

As part of their efforts to cool the housing market, in late 2010 and early 2011 the Chinese government introduced a series of restrictions on housing demand and the supply of credit. In addition to standard financial sector macro-prudential policies targeting mortgage credit, the government also introduced restrictions on the number of housing units households could buy as investments. This paper examines the effectiveness of these latter policies by exploiting variation in the implementation within cities. We take advantage of the differences in the cross-sectional and temporal introduction of the restrictive policies to test for their effectiveness in both slowing house prices and reducing market activity, the two stated goals of the Chinese government. In doing so, though we also compare these with city differences with the broader policy effects in city level panel tests based on variation in the implementation dates of restrictive policies. Consistently we find the same results: little effects on prices or the rate of price appreciation, but significant declines in transactions. Strong restrictions on financing and in particular on purchases can be effective in near-term slowing of housing market activity, but even over two years do not seem to have a meaningful effect on house price levels.

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1 Introduction

In the wake of the financial crisis regulators have turned increased focus to macro-prudential policies to regulate national housing markets. There is a considerable cross-country literature on the effectiveness of these policies. Here we attempt to address a particular set of policies that limit investment in real estate through quantity controls. The contribution lies in exploiting with-in city variation in the implementation of the policies, which avoids many of the types of problems with cross country analyses in left out variable bias and the non-randomness of policy choice.

The Chinese government introduced house finance and purchase restrictions between April 2010 and mid-2011 to combat a period of intense housing market activity, a boom in new construction, and rapid house price increases that grew out of expansionary policies meant to offset the effects of the Great Financial Crisis. The measures introduced by the Chinese government included limits on the number of properties a household could purchase, increases in the costs of funds, and restrictions on access to credit. We take advantage of the variation in implementation of these policies both across and within cities to test their effectiveness. Of the package of policies introduced by the central government in China, our primary focus is the effectiveness of the policy that prevented buyers from purchasing either a second or third property, depending on their residency status. We take advantage of the two types of variation in the implementation of restrictive policies to identify their effects. Like existing research, we exploit cross-city variation in the timing of the introduction of both financing and purchase restrictions, but because we have project level data, we are able to study the effects of within city variation in the implementation of purchase restrictions., In a small group of cities, the municipal governments chose to exempt certain districts from the purchase restrictions.

We find that the restrictions do not generate economically significant differences in house price changes across areas of cities with restrictions and those without (less than 5 percent), but city districts with purchase restrictions have over 20 percent

lower transaction volume relative to pre-restriction volume compared to the difference for unrestricted districts over the same period.

The remainder of the paper is structured as follows. We begin in Section 2 with an abbreviated discussion of some of the literature globally and in China on macroprudential regulation. We follow this with a brief introduction to Chinese housing policies and the macro-prudential cooling measures in Section 3 Chinese government, a discussion of our identification strategy in Section 4, and a description of the data in Section. Finally, we present the results for each of the test groups, the panel of cities, the within city variation across districts, and then the individual transaction data in Section 6.

2 Literature Review

There has been considerable work on macroprudential policies as a result of the interested in them as a toolbox following the Great Financial Crisis. Both Hanson, Kashyap and Stein (2011) and Galati and Moessner (2013) provide overviews and introductions to the different elements of macroprudential regulatory tools. Central to the definition of this type of policies is that in contrast to the traditional "micro-prudential" policies, which target individual financial institutions as they experience financial stress, macroprudential policies combats general equilibrium problems of overall financial system health through policies that affect lending policies, irrespective of the health of individual lender.

The greater part of research on the effectiveness of these policies are various cross-country studies. Cerutti, Claessens, and Laevenc (2017) assemble a database of over 100 countries and conduct panel analyses of the effectiveness of macroprudential policies. They find that these policies reduce real credit growth, and are more effective for emerging and closed economies where the authors speculate there are fewer mechanisms for policy avoidance. They do not find an effect on house price growth. Focusing on a more limited set of countries and motivated by large and volatile capital flows to Asia, Zhang and Zoli (2016) study the relationship of these policies to the control of housing market booms. Using indices to capture the extent of the package of macroprudential tools and policies for 46 economies, 13 from Asia,

they find that these policies have reduced house price appreciation and reduced lender exposure and strengthened balance sheets, especially in Asia.

There are many fewer papers that use more detailed within country data. Igan and Kang (2011) look at differences across Korea in the intensity of macroprudential policy implementation in the use of loan to value (LTV) and debt to income (DTI) constraints for mortgage lending. limits in different regions of Korea. They find strong short run effects of tightening on transaction volumes, but that prices are slower to respond: volume drops are 16 to 21 percent vs. price appreciation declines of approximately 0.5 percent on a monthly basis. In the second stage of their analysis, they match the more aggregate regional data with a survey of borrower mortgage choices. They observe a mechanism that operates primarily through investors rather than first time buyers. Our approach differs in that we use a difference-in-differences approach using areas with restrictions and tose without. In contrast, their identification comes from in the intensity of tightening and loosening relative to an average effect.

Two papers use individual level microdata to study the effects of macroprudential policies. Allen, et al (2016) use loan level data in Canada over a period of changing macroprudential policies on high LTV mortgages, with loosening and then tightening of maximum allowed amortization period and LTV levels. They find households are bound more by the downpayment constraint than the payment constraint in their borrowing in that the bunching at the minimum allowed downpayment does not change when compared with payment amounts. This holds both during loosening and tightening policies. Han, et al (2017) also use Canadian data, studying the imposition of a cap of $1M on properties that can be purchased with high LTV.[1] While the aggregate effects were very low, this did cause a decline in the number of properties listed at just above $1M and an increase in those listed below $1M, but no effect on aggregate volume.

The purchase restrictions introduced in Chinese cities in 2011 have been the subject of a number of papers. Du and Zhang (2015) recognize that identifying the effects of the purchase restrictions at the city level is challenging, especially when other policies

[1] For properties over $1M, regulated lenders required a downpayment of 20%, while for those under $1M the downpayment could be a slow as 5%.

are being implemented. To address this identification challenge they construct a replica of Beijing based on smaller lower status cities that did not adopt purchase restrictions between May 2010 and Nov. 2011. Post-purchase restrictions the difference in price appreciation between Beijing and the replica city is 7.5%, which they identify as the effect of purchase restrictions.[2]

Cao, Huang, and Lai (2015) apply a difference in differences methodology to a panel of 70 cities for which they have National Bureau of Statistics data, of which 39 imposed. To address the problem of city differences they include pre-trend variables and follow two stage approach of Donald and Lang (2007) to difference in differences estimation. They find purchase restrictions associated with a 2.2 – 18% decline in prices relative to the mean depending on their price measure and an over 40 percent decline in sales volume.

To address the problem of unobserved differences across cities, Yan and Ouyang (2017) use propensity score matching to define a more limited, but better matching of treatment and control groups. They end up with four restriction cities that are matched to four control cities based on per capita GDP and population. Their regressions have limited explanatory power. But the difference in mean differences in appears to be a 20% relative decline in the restricted cities.

Unlike the inter-city analysis in other papers, Sun, et al. (2017), estimate the effects of purchase restrictions in a single city, Beijing. Beijing has a large number of households in the housing market that lack official residency status, so the strict restrictions on non-residents might be expected to have an acute effect. They use a regression discontinuity design to identify the existence of a structural break associated with the introduction of purchase restrictions on a variety of real estate market variables. They are not able to isolate the effect of purchase restrictions from other policies introduced at the same time, but they find a combined effect of a 23% decline in house prices post policy constraints. They too find larger effects on volume: a 51-77% reduction.

[2] Du and Zhang (2015) use the same methodology to assess the property tax experiments in Shanghai and Chongqing over this period.

Li, Cheng, and Cheong (2017) use a non-parametric approach that is data driven around the pattern of monthly growth rates in prices. They essentially test for the extent to which price movements immediately following the introduction of purchase restrictions deviates from the overall pattern of price movements. They break their analysis into groups by unit size. While they find some evidence that the restrictions slow house price appreciation, this is less effective for larger units and for situations where rates of house price appreciation are particularly high.

There is also a literature in China that studies the policies that we test. These studies of the effects of the purchase restriction policy on housing prices have been published in Chinese academic journals. Liu (2013) and Wang and Huang (2013) establish different equilibrium models to gauge the effect of the purchase restriction policy. Liu (2013) states that the direction of the housing price movement is unclear given the different conditions. Wang and Huang (2013) suggest that the purchase quota policy may reduce housing prices, but at an insignificant magnitude. Zheng et al. (2016) find that the purchase restriction policy reduces the default risk of listed firms by 25.5%.

All of these studies perform empirical analyses based on intercity comparisons. However, purchase restrictions were not uniformly imposed within cities, and more specifically, purchase restrictive policies only apply to selected administrative districts in many cities. Moreover, the restricted cities differ from the unrestricted cities in terms of economic development, housing market condition, and physical location. The papers cited above rely on a variety of specification techniques to overcome this problem. The contribution of this paper is to use more detailed data that provide housing market measures at the project level. This allows us to take advantage of the differences in the imposition of purchase restrictions within cities. We end up with estimation where the treatment and control groups (different districts within a single city) are much more similar than other papers which look at differences across cities. Rather than imposing econometric structure to address concerns over the unobserved cross-sectional and inter-temporal differences between the control and treatment groups, we use more precise data to allow for a cleaner and more precise difference in differences test.

3 Chinese Macroprudential Intervention in the Housing Market.

The movement to a private housing market in China began with Deng Xiaoping's 1980 statement and subsequent State Council support that urban residents should be able to build or purchase their own units. This support was translated into more concrete action with the 1988 Constitutional changes to allow the transfer of land use rights and the 1991 recognition of property rights in the private market, the latter were not formalized in the PRC State Constitution until 2004. The final major transformative step was the ending of the housing welfare system in 1998 with the announcement of the 23rd Decree[3] by the State Council in 1998. The transition has been dramatic: from 1.45b m^2 of floor space real estate completed in 1995 to 3.5b m^2 in 2015 (China Statistical Yearbook 2015). Along with this increase in investment has come a stunning increase in prices: Wu, Deng, and Liu (2014) estimate hedonic prices rose by over 140% between 2004 and 2009, Fang, Gu, Xiong, and Zhou (2015) report annual real house price growth of slightly over 13 percent in major Chinese cities, and Wu, Deng and Gyourko (2015) estimate real land prices in key Chinese cities rose by a factor of five between 2004 and 2015.

The period studied in this paper follows several years of intense housing market activity. In the wake of the world financial crisis, China pursued a program of stimulus led by an almost $US 600b investment program announced in Nov 2008.[4] Some pointed to this stimulus and the associated increase in liquidity as driving a real estate boom, characterized by rapid price increases and investors owning apartments as pure stores of wealth: some estimated up to 30 percent of new apartments being purchased and left vacant.[5] The overheating of the housing market in prices, investment volume, and new construction following the post crisis stimulus eventually triggered a set of policies by the Chinese central government intended to rein in the market. These actions involved central directives, but the implementation occurs locally. The provincial government forwards the messages from the central government to the municipal and lower level governments. It is then up to a local

[3] The 23rd Decree states that work units are no longer allowed to develop new housing units for their employees.

[4] Reported in the *New York Times*, Nov 9, 2008 "China Unveils Sweeping Plan for Economy", http://www.nytimes.com/2008/11/10/world/asia/10china.html

[5] "China's Looming Real-Estate Bubble; A massive Keynesian spending program has misallocated capital and set the stage for a crisis." *Wall Street Journal* (on-line), Aug. 20, 2010.

government's discretion to customize these policies and determine the timeline based on local economic conditions.

This paper uses the two sets of macroprudential policies introduced by the State Council to study the effects of these policies on housing market activity. These directives, the "Ten National Rules" (effective April 17, 2010) and the "Eight National Rules" (effective January 26, 2011, raised minimum loan to value ratios and interest rates, placed restrictions on certain types of borrowing, and limited the number of residential properties a household could purchase, all in an effort to curb the soaring housing prices by suppressing demand. The Housing Provident Fund underwriting was changed, raising the minimum LTV on the first unit purchased, if it has a floor area greater than 90 m^2, from 20 to 30%, and the minimum LTV for a second unit purchased from 40 to 50%.[6] Lending by commercial banks was also tightened, with minimum LTVs increased by 10 to 20 percentage points, with higher LTVs for second homes, and in some cases no financing for a third home. Interest rates were raised, with a minimum allowed rate of 1.1 times the "benchmark rate." Under the purchase restriction policies, residents, those who have official city residency status or *hukou*, were allowed to purchase a second or third home, and non-residents were not. In both cases quantity was constrained.

4 **Identification**

As we note above, the contribution of our paper includes the analysis of the same question using data from a single source over multiple levels of geographic aggregation through different tests to develop a more robust and comprehensive assessment of the effects of purchase restrictions on housing market conditions. This informs our identification strategy as our methodology to test the effects of purchase restrictions exploits the latitude given to local governments to implement the policy directives from the central government in the housing market. Across cities, stricter underwriting policies on residential lending, both for Housing Provident Fund and

[6] The Housing Provident Fund (HPF) is a mandatory savings plan for government, state owned enterprise, and some private business employees. Individual contributions are matched by employers with withdrawals limited to purchase owner-occupied real estate. This is a buyer's lowest cost financing, but the amounts are limited and typically need to be supplemented with bank financing. Studies and summaries of the HPF include Tang and Coulson (2017), Xu (2016), Yang and Chen (2014), and Yeung and Howes (2006).

commercial banking loans and purchase restrictions were introduced at different times, providing variation that we exploit in panel regressions of cities over time. Most of our analysis focuses on the imposition of quantity restrictions on apartment purchases. The variation in policy implementation for the financing restrictions is by the month they were introduced, which is a narrow form of variation as only 12 months elapsed from the first policies in Beijing (May 2010) and the last in Xuzhou (May 2011), with most between October 2010 and February 2011. In contrast, some local governments did not restrict purchases at all. And among some cities that did restrict purchases, the local government imposed the restrictions on some districts and exempted others. We exploit all three forms of variation in a staged estimation strategy that applies different tests depending on the degree of aggregation of the data and the appropriateness of test assumptions.

The first set of tests use city level aggregation and utilizes a panel structure for the data. We estimate the response of city level monthly mean transaction price, appreciation in the mean from the previous year, and total units sold to whether lending and purchase restrictions are in force in the city. The pre-treatment and post-treatment periods for the treatment cities are sorted by the policy implementation date in the respective cities, with a simple dummy to indicate when the set of restrictions are in place in the city along with continuous macroeconomic measures and city and time fixed effects. The dates vary from May 2010 to May 2011, with most cities bunched between Jan and May 2011. This estimation using city aggregate values and a binary variable for the presence of restrictive policies is similar to that in Li, Cheng, and Cheong (2017). We also include a dummy for those cities that imposed purchase restrictions for the period they are in place. This allow us to differentiate between the tighter underwriting for mortgage financing and the purchase restrictions, where the former was introduced in all cities, and the latter not so.

The problem with using the city panel is that policy choice and city type are highly correlated. Purchase restrictions were imposed in all Tier1, in all but one Tier 2 cities (see the appendix for a list of cities), but in only 3 of 17 Tier 3 cities.[7] While authors such as Cao, Huang, and Lai (2015) and Yan and Ouyang (2017) have used a

[7] The one exception is Chongqing, which is listed as a Tier 2 city, despite being a "direct-controlled municipality (*zhixiashi)* and thus administratively equivalent to a province, along with Beijing, Shanghai, and Tianjin. With a population of 33m it is orders of magnitude larger than other Tier 2 cities.

difference in differences methodology with a Chinese city panel to test the effects of purchase restrictions, the assumptions of randomness in treatment and no time varying excluded differences seems to be a problematic when comparing across cities of significantly different levels of economic and political importance. The test could easily be viewed as a test of Tier 3 city housing market outcomes relative to Tier 1 and Tier 2 rather than a test of specific policies. We treat the broad cross-city panel as a general descriptive exercise. For purposes of comparison with our second tests we do apply a difference in differences test to these data, but only among Tier 3 cities. This way the treatment and non-treatment cities are of similar type. In the appendix we show the results of logit tests on Tier 3 cities for the probability of having a treatment (imposing restrictions), which suggest that restrictions and housing market conditions may be somewhat endogenous.[8]

Our second test takes advantage of the variation in treatment, the imposition of purchase restrictions, within in a city. In a small set of cities, local governments imposed purchase restrictions in some districts, but not in others: typically, restrictions in the central districts and not for units in projects in suburban districts. We apply the standard difference in differences methodology for purchase restrictions, looking at change in housing market outcomes for development projects in districts with purchase restrictions compared to those in districts without over the period before and after the introduction of limits on the number of units a household could purchase. We do this at both the city level, comparing between districts, and also with border regressions, limiting the analysis to projects within 3 km of the border between restrictive and non-restrictive districts.

5 Data

The data used in the analysis are from the Chinese Real Estate Index System (CREIS). CREIS records housing transaction data in China from information published by the central, provincial and local governments on a weekly or monthly basis. Transactions data are reported at the city, project, and deeds levels, where the

[8] No coefficients are statistically different from zero. However, this is likely a result of the small sample size with a non-linear specification. The estimated coefficient son both price level and city size are close to being statistically different from zero, so it is not unreasonable to suggest that authorities in higher house price cities were more likely to impose purchase restrictions.

first two are aggregate data and the last individual transactions.

The aggregate data includes the average unit price (Chinese Yuan per square meter), the total value of sales (Chinese Yuan), and total area (m^2) transacted. Aggregate city data are available for 105 cities, but while reporting started in 2005, the data are not uniformly available as some cities only began reporting statistics in 2010 or later. Only using cities with aggregate data prior to 2010 leaves with a panel of 46 cities. For city level regressions, we also include per capita GDP collected directly from the National and Municipal Bureaus of Statistics in China, but this like other possible macro-economic measures is an annual value for each city.

The project level data are also monthly data. Variables include transaction information the average unit price (Chinese Yuan per square meter), total units sold, total transacted area (m^2), total monetary value of sales (Chinese Yuan) along with project location. The data cover 49,525 projects in 126 cities from as early as 2005. However, pre-2010 the coverage is very sparse in most cities. While nine cities had variation in purchase restrictions within the city by district, not all of these cities had sufficient reported project level data in the period prior to 2011.[9] We keep 2,014 projects in four cities (Hefei, Guangzhou, Chengdu, and Qingdao) that qualify for our first DID analysis based on having sufficient observations over multiple months in the period prior to the implementation of the policy restrictions in each city.

We provide summary statistics for these data at both the city and project aggreates in Table 1. Panel A shows city level aggregate data and Panel B corresponds to the project level statistics. In both panels we break the data down between purchase restricted and unrestricted cities (districts) and for periods before and after the imposition of the financing and purchase restrictions.

At the city level there are clear differences between the groups. In Panel A, the cities without purchase restrictions (all Tier 1, all but one Tier 2, and three Tier 3 cities) have significantly higher GDP per capita, average sales price, and transaction volume than do the unrestricted cities (Chongqing and most of the Tier 3 cities). After the cooling policies, average prices continue to rise for both groups, around 30% in the

[9]Changsha, Chengdu, Guangzhou, Hefei, Nanchang, Nanjing, Qingdao, Wuxi, and Xi'an are restricted by districts.

unrestricted cities compared with a 24% increase in the restricted cities, but there are clear differences in transaction volumes. The fall in the restricted cities but remain unchanged in the unrestricted cities. . This unconditional result indicates that the purchase restriction policy may be effective in cooling the housing market to some extent.

Panel B of Table 1 reports the summary statistics at the project level. The observations in the data are project-specific monthly aggregate values, where we distinguish between projects in districts with purchase-restrictions and those without in four cities (Hefei, Guangzhou, Chengdu, and Qingdao). As in Panel A we compare values before and after the implementation of the purchase restriction policy. Sales volumes are lower in the core-area restricted districts than in the unrestricted districts, and as one might expect for more central areas, prices are higher and average unit sizes lower. Unit prices rise in both types of districts after the imposition of purchase restrictions, all districts are subject to the financing restrictions, but volumes, both units sold and total area sold, fall in both district types.

Table 1. Summary Statistics

Panel A. City Level Statistics for 46 Cities

Purchase-Restricted Cities (31 cities)

	Full Sample (2009 to 2013)			Before Restriction			After Restriction		
VARIABLES	Mean	Median	S.D	Mean	Median	S.D	Mean	Median	S.D
Sales Volume	5,361.61	4,124.50	4,396.22	6,214.10	4,951.50	4,933.86	5,600.43	4,654.00	4,014.09
Transacted Areas (Thou.)	557.77	427.90	448.95	648.30	536.55	504.27	577.55	488.60	402.16
Unit Price	9,581.01	8,262.00	4,495.74	8,758.79	7,689.00	4,164.71	10,880.33	9,271.50	4,941.62
Total Value (Bill.)	4.98	3.63	5.13	5.39	4.06	5.61	5.94	4.43	5.50
Per GDP	82,460.10	60,840.20	64,939.31	74,705.51	54,943.35	61,483.13	105,263.25	81,843.92	78,745.27

Unrestricted Cities (15 cities)

	Full Sample (2009 to 2013)			Before No. 1 [2011]			After No. 1 [2011]		
VARIABLES	Mean	Median	S.D	Mean	Median	S.D	Mean	Median	S.D
Sales Volume	3,302.44	1,984.50	4,104.32	3,233.55	1,807.00	4,572.13	3,351.69	2,112.00	3,738.72
Transacted Areas (Thou.)	328.05	206.30	381.98	307.95	170.70	407.43	342.93	228.65	361.71
Unit Price	5,520.42	5,254.00	1,931.30	4,702.36	4,405.00	1,732.24	6,120.46	5,789.00	1,849.28
Total Value (Bill.)	1.90	1.06	2.37	1.50	0.80	2.01	2.19	1.33	2.56
Per GDP	66,091.65	39,683.93	66,479.05	56,404.29	37,668.16	59,040.19	74,259.43	48,864.98	71,221.31

Panel B. Project Level Statistics for 4 Cities

Purchase-Restricted Districts (1747 projects in 4 cities)

	Full Sample			Before Restriction			After Restriction		
VARIABLES	Mean	Median	S.D	Mean	Median	S.D	Mean	Median	S.D
Sales Volume	27.80	9.00	51.69	33.65	11.00	58.68	26.77	9.00	50.29
Transacted Areas	2641.79	971.50	4685.91	3215.61	1105.00	5492.35	2541.20	951.00	4522.47
Total Value (Mill.)	27.70	9.94	53.65	29.73	9.09	59.95	27.33	10.07	52.41
Unit Price	11813.84	8972.00	8786.67	9129.39	7167.00	5989.63	12304.68	9300.00	9121.76
Average Transacted Areas	112.00	95.00	106.10	113.10	97.19	67.79	111.81	94.66	111.47

Unrestricted Districts (1172 projects in 4 cities)

	Full Sample			Before Restriction			After Restriction		
VARIABLES	Mean	Median	S.D	Mean	Median	S.D	Mean	Median	S.D
Sales Volume	35.58	14.00	70.79	40.44	17.00	67.41	34.96	14.00	71.18
Transacted Areas	3490.47	1594.50	6770.54	4244.89	2053.00	6847.36	3395.61	1550.50	6755.17
Total Value (Mill.)	21.77	9.95	41.79	25.13	11.38	47.40	21.33	9.78	40.97
Unit Price	6558.96	5743.00	3400.97	5719.89	5122.50	2597.24	6669.66	5836.00	3478.24
Average Transacted Areas	122.44	97.67	98.92	132.37	102.39	96.84	121.19	97.05	99.12

6. Results

Our results are structured to provide increasing level of geographic specificity to the same question regarding the effect of difference in difference type tests for each level of geographic assessment. The particular tests differ by geography, but all look for changes in prices and sales volumes around a policy implementation date between areas that impose the policies and those that do not.

The first two tables of regression results, Tables 2 and 3, use the city panel dataset to test the effects of both conventional macro-prudential financing restrictions, higher downpayment requirements and higher lending rates, and the separate purchase restrictions that limit the number of units a household can purchase in a particular city. The former was applied in all cities in the panel, but at different dates over a year, the latter to 15 of the 46 cities, and if applied, then at the same time as the financing restrictions were introduced. Both tables use observations from the city-specific 12 months prior to the implementation of the policies and then 12 months after. The introduction month is excluded. All regressions include city and month fixed effects, where the latter in particular addresses the strong seasonality effect of the Spring Festival, and year effects for shared national temporal effects.

The effects of the policies are broadly similar. No effect on the level of prices, a slightly above one percentage point decline in the rate of price appreciation, and large (greater than 35 percent) decrease in sales volumes. Where they differ is in the effects on transaction volume of the finance restrictions in the Tier 3 cities. The finance restrictions have no effect on volume for these cities as the interaction effect is effectively as large as the implementation effect of the actual policy. In contrast, the purchase restrictions do effect volumes in the Tier 3 cities. In Table 3 the policy Tier 3 interaction is essentially zero, so that unlike with the general financial restrictions, purchase restrictions affect transactions for all city types.

Table 2. Effect of Financing Restrictions
City Panel – One Year Prior/Post Restrictions

	Ln(Price)		Y on Y Price Growth		Ln(Total unit sales)	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Finance Policy	-0.010	-0.004	-0.008**	-0.013**	-0.332***	-0.381**
	(0.022)	(0.047)	(0.004)	(0.006)	(0.096)	(0.155)
Policy*Tier2		0.018		0.003		-0.051
		(0.049)		(0.008)		(0.163)
Policy*Tier3		-0.072		0.010		0.342*
		(0.054)		(0.007)		(0.192)
ln(Per GDP)	0.135	0.076	-0.013	-0.009	0.628	0.925*
	(0.139)	(0.146)	(0.019)	(0.018)	(0.598)	(0.527)
Time Trend	0.007***	0.007***	-0.000	-0.000	0.005	0.001
	(0.002)	(0.002)	(0.000)	(0.000)	(0.006)	(0.005)
Constant	3.260***	3.395***	0.171	0.165	-2.340	-3.113
	(0.808)	(0.840)	(0.107)	(0.108)	(4.420)	(4.472)
Observations	1,473	1,473	1,452	1,452	1,511	1,511
R-squared	0.944	0.945	0.026	0.026	0.803	0.808
City FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES

Notes: *Policy* dummy =1 if tighter lending standards are in place in city. Heteroscedasticity-consistent standard errors clustered at the city level are shown in parentheses under the coefficients estimated. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

Table 3. Effect of Purchase Restrictions
City Panel – One Year Prior/Post Restrictions

	Ln(Price)		Y on Y Price Growth		Ln(Total unit sales)	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Purchase Policy	0.011	0.002	-0.012***	-0.013**	-0.385***	-0.371**
	(0.019)	(0.047)	(0.004)	(0.005)	(0.077)	(0.151)
Policy*Tier2		0.016		0.000		-0.013
		(0.049)		(0.008)		(0.159)
Policy*Tier3		-0.025		0.001		-0.034
		(0.060)		(0.009)		(0.191)
ln(Per GDP)	0.127	0.120	-0.009	-0.009	0.741	0.741
	(0.145)	(0.147)	(0.016)	(0.017)	(0.564)	(0.575)
Time Trend	0.007***	0.007***	-0.000	-0.000	0.002	0.002
	(0.002)	(0.002)	(0.000)	(0.000)	(0.005)	(0.005)
Constant	3.432***	3.460***	0.172*	0.173*	-1.647	-1.645
	(0.791)	(0.800)	(0.092)	(0.093)	(4.347)	(4.388)
Observations	1,473	1,473	1,452	1,452	1,511	1,511
R-squared	0.944	0.944	0.027	0.027	0.806	0.806
City FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES

Notes: *Policy* dummy =1 if tighter purchase restrictions are in place in city. Heteroscedasticity-consistent standard errors clustered at the city level are shown in parentheses under the coefficients estimated. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

The stated objective of the restrictive policies was to tame high and accelerating house prices. However, the primary effect seems to be limited to decreases in market activity. Since the restrictions include limits on access to capital for the purchase of investment properties, the second or third property purchased, we would expect to see policies affect the volume of sales, rather than prices alone. This is also consistent with the contention that housing markets clear in volume and prices, not just prices (Stein, 1995; Clayton, Miller, & Peng, 2010).

For the second group of tests we examine difference within cities, between districts that had purchase constraints, and those that didn't. We cannot separately test for the financing restrictions because these did have within city variation in their implementation. In all cases regressions include project and month fixed effects. Here we use the difference in differences methodology, where restrictions are the treatment. As we note above, districts are not random and the decision to exclude district from purchase restrictions cannot be considered random. Figure 1 makes it clear that the differentiation is between the urban core and the periphery. We only do the analysis for Tier 3 cities as this is the group with actual within group variation in policy implementation.[10] We preform the DiD regressions over varying window sizes and present the results in separate tables for city aggregates for price level (Table 4) and transaction volume (Table 5).[11] We only show the interaction between the treatment group (*treat*), the cities that impose purchase restrictions, and the period during which the effects are in place (*policy*) because we have city, month, and year fixed effects that subsume the individual effects of these variables. The results here are similar to those in the larger city panel: purchase restrictions have no effect on prices (Tables 4), but significant effects (greater than 30 percent decline in Table 5) on transaction volume. The effects peak at a 45 percent decline in the first nine months after the introduction of restrictions, but decline from there so that by 18 months the effect is not statistically different from zero between the two groups of cities.

[10] Tier 3 are also not provincial capitals like Tier 2 cities or the richer more internationally integrated migrant destination cities like the four Tier 1 cities.

[11] We do not present the results of tests for differential changes in the rate of price appreciation because these tests do not add anything beyond the results for changes in price levels. All price appreciation difference in differences test yield not statistically different from zero results and all have low explanatory power, with R^2 values below 0.10.

Table 4 - City Panel DiD with Tier 3 (17) Cities
Dependent Variable: ln(Price)

VARIABLES	(1) -6m to 6m	(2) -12m to 12m	(3) -18m to 18m	(4) -24m to 24m
Treat*After	-0.039	-0.040	-0.063	-0.035
	(0.039)	(0.028)	(0.042)	(0.023)
After	0.065	0.106***	0.029	0.036*
	(0.101)	(0.036)	(0.023)	(0.019)
Time Trend	0.008	0.001	0.008***	0.007***
	(0.009)	(0.003)	(0.001)	(0.001)
Constant	3.672	8.061***	3.797***	4.522***
	(5.406)	(1.746)	(0.850)	(0.452)
Observations	219	411	574	712
R-squared	0.943	0.930	0.919	0.912
City FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES

Notes: *Treat* is a dummy taking a value 1 if the city is subject to a purchase restriction during full sample period and 0 otherwise. *After* is a dummy taking a value 1 after the policy of purchase restriction is enacted, and 0 otherwise. Heteroscedasticity-consistent standard errors clustered at the city level are shown in parentheses under the coefficients estimated. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

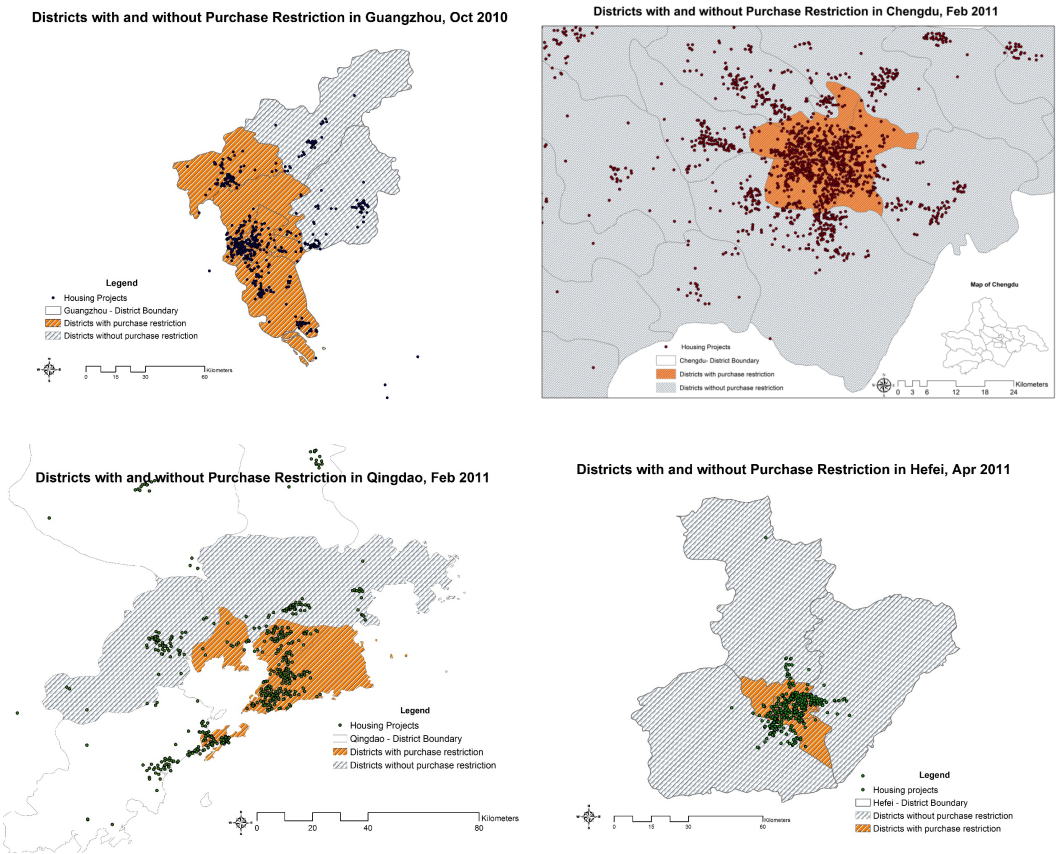
Table 5 - City Panel DiD with Tier 3 (17) Cities
Dependent Variable: Transaction Volume

VARIABLES	(1) -6m to 6m	(2) -12m to 12m	(3) -18m to 18m	(4) -24m to 24m
Treat*After	-0.451***	-0.184*	-0.085	-0.151
	(0.110)	(0.104)	(0.088)	(0.181)
After	0.107	-0.280**	-0.343***	-0.295***
	(0.199)	(0.122)	(0.073)	(0.069)
Time Trend	-0.010	0.015*	0.019***	0.016***
	(0.019)	(0.009)	(0.003)	(0.003)
Constant	13.799	-1.811	-3.988*	-2.005
	(11.524)	(5.434)	(2.040)	(1.586)
Observations	203	382	543	686
R-squared	0.865	0.809	0.804	0.793
City FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES

Notes: *Treat* is a dummy taking a value 1 if the city is subject to a purchase restriction during full sample period and 0 otherwise. *After* is a dummy taking a value 1 after the policy of purchase restriction is enacted, and 0 otherwise. Heteroscedasticity-consistent standard errors clustered at the city level are shown in parentheses under the coefficients estimated. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

The challenge with these city level difference in differences tests is the weakness of the assumption that cities randomly choose to impose restrictions and that over time there are no trend effects that differ systematically between cities that imposed purchase restrictions and those that do not. We revisit this test with a smaller group of four cities that imposed restrictions on some districts within the city and not on others. The cities are Chengdu, Guangzhou, Hefei, and Qingdao. Guangzhou is a Tier 1 city, the other three are Tier 2, so they are different city types than the Tier 3 cities analyzed in Tables 4-5 with implied annual price appreciation rates over the window periods of 7-11%. As we note above in the identification section, we are still dealing with non-random treatment because the city governments are deciding in which districts to restrict purchases. Figure 1 shows the distribution of projects across city districts for the four cities.

Figure 1 – Within City Purchase Restrictions



These tests are still subject to the problem with non-randomness in treatment by design because the treatment breaks down by central vs. suburban districts. We expect within city excluded variable issues to be less problematic than it for the between city panels used by Cao, Huang, and Lai (2015) and Yan and Ouyang (2017). Logit regressions project sites in the appendix (Appendix Table A-3) do indicate that projects with higher mean prices located in smaller districts, both of which reflect a location in more in the centre of an urban region. Our contention, is that comparing across districts within a city within should present less of an excluded bias or trend effect that is correlated with the treatment timing than the same tests across cities.

We present our results in Tables 6-7 for price levels, year on year price change, and transaction volumes. The unit of observation is project level aggregate or mean values in a given month for approximately 2,800 projects across the four cities. All regressions are structured where *After* is the dummy variable that designates the time period after policies were put in place and *Treat* is the dummy variable that takes on the value of one for projects in the districts that will have or have purchase restrictions. We do not include *Treat* on its own in the regressions because it is a linear combination of the project level fixed effects that are right hand side control variables, and which encompass city or district level fixed effects. The regressions include month fixed effects for seasonality, a shared time trend, and controls for mean unit size in a district.

The identification that utilizes project level within city variation in the application of purchase restrictions is consistent with the cross-city panel analysis. First, mean prices in projects in districts that had purchase restrictions did not differ statistically before and after the introduction of the policies when compared to those in districts where restrictions were not put in place (Table 6). This is even though prices over this period were rising in both areas. As Table 7 shows, transaction volumes drop off more after the introduction of restrictions in projects in the districts with purchase restrictions than in those without, by 16-22%. This is on top of a shared decline in average project transaction volume of up to 55% for the largest windows comparing two years after. This relative difference is consistent with the national and local lending restrictions having a larger effect on volumes than the purchase restrictions,

though the latter are still important.

Table 6 – Within City District Level DiD – Project Level Data Dependent Variable: ln(Price)

VARIABLES	(1) -6m to 6m	(2) -12m to 12m	(3) -18m to 18m	(4) -24m to 24m
Treat*After	**0.000**	**0.006**	**0.016**	**0.025**
	(0.023)	**(0.024)**	**(0.027)**	**(0.030)**
After	0.029	0.052**	0.088***	0.102***
	(0.021)	(0.024)	(0.026)	(0.028)
ln(size)	0.071	0.090**	0.049	0.068
	(0.050)	(0.039)	(0.036)	(0.041)
Time Trend	0.009***	0.006***	0.002**	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Constant	3.241***	4.803***	7.480***	8.617***
	(0.811)	(0.678)	(0.544)	(0.492)
Observations	8,545	12,514	17,403	22,190
R-squared	0.885	0.849	0.837	0.831
Month FE	YES	YES	YES	YES
Project FE	YES	YES	YES	YES

Notes: *Treat* is a dummy taking a value 1 if a district is subject to a purchase restriction during full sample period and 0 otherwise. *After* is a dummy taking a value 1 after the policy of purchase restriction is enacted, and 0 otherwise. Standard errors are clustered at the project level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

Table 7 – Within City District Level DiD – Project Level Data
Dependent Variable: Transaction Volume

VARIABLES	(1) -6m to 6m	(2) -12m to 12m	(3) -18m to 18m	(4) -24m to 24m
Treat*After	**-0.372*** **	**-0.218** **	**-0.177***	**-0.158***
	(0.103)	**(0.102)**	**(0.098)**	**(0.093)**
After	-0.011	-0.190*	-0.425***	-0.557***
	(0.125)	(0.107)	(0.099)	(0.082)
ln(size)	-0.629***	-0.601***	-0.477***	-0.434***
	(0.100)	(0.076)	(0.052)	(0.059)
Time Trend	-0.046***	-0.055***	-0.033***	-0.019***
	(0.008)	(0.006)	(0.005)	(0.003)
Constant	33.720***	39.290***	25.266***	16.797***
	(5.018)	(4.020)	(2.914)	(1.772)
Observations	8,545	12,514	17,403	22,190
R-squared	0.627	0.580	0.538	0.511
Month FE	YES	YES	YES	YES
Project FE	YES	YES	YES	YES

Notes: *Treat* is a dummy taking a value 1 if a district is subject to a purchase restriction during full sample period and 0 otherwise. *After* is a dummy taking a value 1 after the policy of purchase restriction is enacted, and 0 otherwise. Standard errors are clustered at the project level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

The concern remains even with the within city comparisons across districts that the non-random designation of which districts are subject to purchase restrictions and unobserved differences in trends invalidate the difference in differences methodology used above. As a more refined test we conduct border regressions, limiting the sample to projects within 3km of the border between districts where purchase restrictions are imposed and those where they are not. These results are presented in Tables 8 and 9 and follow the same template with the same controls as those the compare for the entire districts. The overall results are extremely similar, though the magnitude of difference between the changes is slightly reduced, reflecting better controls of

unobserved changes correlated with district designation. While price levels in both purchase restricted and unrestricted districts are higher after the introduction of restrictions than before (see Table 8).[12] Finally, again we find strong consistent evidence of policy effects on transaction volumes (Table 9). The short run effects are very strong, with transactions in projects in the areas of districts with purchase restrictions that were within 3km of districts without purchase, being 40% lower than nearby projects in districts without these restrictions compared with the difference prior to the introduction on the purchase restrictions. This holds even in the presence of large overall declines in the shared difference.

Table 8 – 3km Border DiD – Project Level Data
Dependent Variable: ln(Price)

VARIABLES	(1) -6m to 6m	(2) -12m to 12m	(3) -18m to 18m	(4) -24m to 24m
Treat*After	**-0.011**	**0.005**	**0.007**	**0.006**
	(0.013)	**(0.013)**	**(0.013)**	**(0.013)**
After	0.055***	0.079***	0.117***	0.138***
	(0.015)	(0.015)	(0.012)	(0.012)
ln(size)	0.056**	0.077***	0.078***	0.113***
	(0.027)	(0.022)	(0.017)	(0.014)
Time Trend	0.007***	0.004***	0.001*	-0.001***
	(0.001)	(0.001)	(0.001)	(0.000)
Constant	4.385***	5.976***	7.897***	9.111***
	(0.805)	(0.689)	(0.328)	(0.246)
Observations	5,387	8,085	11,331	14,205
R-squared	0.858	0.811	0.803	0.797
Month FE	YES	YES	YES	YES
Project FE	YES	YES	YES	YES

Notes: *Treat* is a dummy taking a value 1 if a project's district has a purchase restriction anywhere during full sample period and 0 otherwise. *After* is a dummy taking a value 1 after the policy of purchase restriction is enacted, and 0 otherwise. Standard errors are clustered at the project level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively

[12] The implied annual price appreciation rates over the window periods ranges from 7-11%

Table 9 – 3km Border DiD – Project Level Data
Dependent Variable: ln_volume

VARIABLES	(1) -6m to 6m	(2) -12m to 12m	(3) -18m to 18m	(4) -24m to 24m
Treat*After	**-0.398*** **	**-0.273*** **	**-0.224*** **	**-0.179*** **
	(0.067)	**(0.061)**	**(0.058)**	**(0.058)**
After	0.021	-0.302***	-0.523***	-0.612***
	(0.093)	(0.073)	(0.057)	(0.055)
ln(size)	-0.579***	-0.597***	-0.517***	-0.518***
	(0.078)	(0.059)	(0.045)	(0.039)
Time Trend	-0.045***	-0.044***	-0.025***	-0.016***
	(0.008)	(0.005)	(0.002)	(0.002)
Constant	33.479***	32.635***	20.792***	15.436***
	(4.972)	(2.966)	(1.459)	(1.088)
Observations	5,387	8,085	11,331	14,205
R-squared	0.631	0.581	0.540	0.506
Month FE	YES	YES	YES	YES
Project FE	YES	YES	YES	YES

Notes: *Treat* is a dummy taking a value 1 if a project's district has a purchase restriction anywhere during full sample period and 0 otherwise. *After* is a dummy taking a value 1 after the policy of purchase restriction is enacted, and 0 otherwise. Standard errors are clustered at the project level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively

In the difference in differences tests of the effect of purchase restrictions on transaction it is consistently the case both that volume falls overall after the mix of financing and purchase restrictions are put in place, and that this drop is greater for cities or districts within cities that impose purchase restrictions. Uniformly the shorter windows result in larger purchase restriction effects relative to the overall change, but for the longer 18 and 24 month windows the overall decline is larger than the pure purchase restriction difference effect. We understand the overall change to reflect the broader financing controls, such as the increased interest rates and higher underwriting standards, on both borrowing from the Housing Provident Fund and Conventional Bank Lenders as these more traditional macro-prudential policies were

introduced in cities of all Tiers. One possible interpretation is that purchase restrictions have a strong short-run effects that weaken with time, while the effect of are not as pronounced in the very short-run, but matter increasingly with time.

7. Conclusion

In this paper, we look at the effects of macroprudential policies on housing market outcomes to measure their effectiveness at cooling housing markets. Unlike most previous work, we are not limited to a within country time series or cross-country panel. The use of data across cities is something we share with other studies of China's cooling policies and a few other papers fro other countries. Where we differ from the existing work is that we are not limited to inferences based on differences across cities, which may themselves be quite different from each other with ample for violation of difference in differences assumptions. Instead, we are able to take advantage of differences in the implementation of residential property purchase restrictions in China within cities in the timing and application of these restrictions for our identification. We obtain fairly consistent results, that restrictions had minimal effects on prices but large effects on volumes. As well, the effects seemed to be short lived, as longer-run tests find either lower or no effects on aggregate market outcomes. These findings suggest that while macroprudential policies can have strong dampening effects on market activity, their ability to reverse problems of high house prices and address affordability are limited at best.

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Appendix Table A-1 – City Characteristics

City	City Tier	Purchase Restriction	Population	Per GDP	Price per m2
Shanghai	1	Yes	14,200,000	131,331.50	14,657.17
Beijing	1	Yes	12,800,000	124,455.70	17,518.88
Guangzhou	1	Yes	8,159,525	154,025.80	12,817.45
Shenzhen	1	Yes	2,552,140	388,083.80	19,021.10
Chongqing	2	No	33,200,000	29,190.35	6,027.06
Sanya	2	Yes	570,275	44,646.67	22,375.02
Nanjing	2	Yes	6,322,980	83,376.98	11,054.28
Nanning	2	Yes	7,043,900	26,407.26	7,591.63
Nanchang	2	Yes	5,030,950	48,306.05	6,758.30
Xiamen	2	Yes	1,860,340	128,695.60	11,722.21
Hefei	2	Yes	6,229,020	54,629.63	6,079.47
Haerbin	2	Yes	9,931,220	41,569.36	5,882.89
Dalian	2	Yes	5,882,800	103,201.50	10,938.93
Tianjin	2	Yes	9,933,625	115,485.80	9,224.58
Taiyuan	2	Yes	3,544,400	44,999.54	6,159.11
Chengdu	2	Yes	11,500,000	50,157.75	7,977.40
Wuxi	2	Yes	4,684,940	141,906.60	7,594.53
Wuhan	2	Yes	8,303,075	75,074.86	6,206.48
Shenyang	2	Yes	7,194,200	71,532.67	5,407.02
Haikou	2	Yes	1,724,075	41,336.63	8,473.20
Wenzhou	2	Yes	7,943,440	41,361.41	15,828.62
Fuzhou	2	Yes	6,449,720	48,947.62	12,699.67
Suzhou	2	Yes	6,430,020	163,465.10	9,887.70
Xi'an	2	Yes	7,918,280	48,083.93	6,322.92
Guiyang	2	Yes	3,727,150	34,206.38	4,689.11
Zhengzhou	2	Yes	4,700,800	77,179.00	6,089.87
Changchun	2	Yes	7,573,200	45,398.77	5,478.02
Changsha	2	Yes	6,538,175	77,544.14	5,323.90
Qingdao	2	Yes	7,581,200	80,637.72	7,547.05
Dongguan	3	No	1,814,320	236,014.10	8,120.13
Zhongshan	3	No	1,522,767	159,063.50	5,512.94
Baotou	3	No	2,364,050	84,135.33	4,341.17
Nantong	3	No	7,644,340	52,225.21	9,591.64
Tangshan	3	No	7,354,320	62,802.31	6,222.23
Huizhou	3	No	3,426,400	73,630.66	6,189.27
Shantou	3	No	5,200,850	23,393.71	6,010.95
Luzhou	3	No	5,032,220	17,357.77	3,500.75
Luoyang	3	No	6,710,110	34,610.23	3,837.28
Jining	3	No	8,470,800	37,651.70	4,116.10
Huaibei	3	No	2,195,600	21,023.87	3,872.67
Huai'an	3	No	5,328,600	14,310.32	3,937.63
Wuhu	3	No	3,157,022	45,933.35	5,201.46
Putian	3	No	3,213,776	23,769.74	6,362.33
Fuzhou	3	Yes	3,710,560	151,101.40	7,462.37
Xuzhou	3	Yes	9,749,333	33,958.24	4,833.42
Shaoxing	3	Yes	4,388,775	64,893.94	11,277.68

Figure A-1. Locations of 46 Chinese Cities

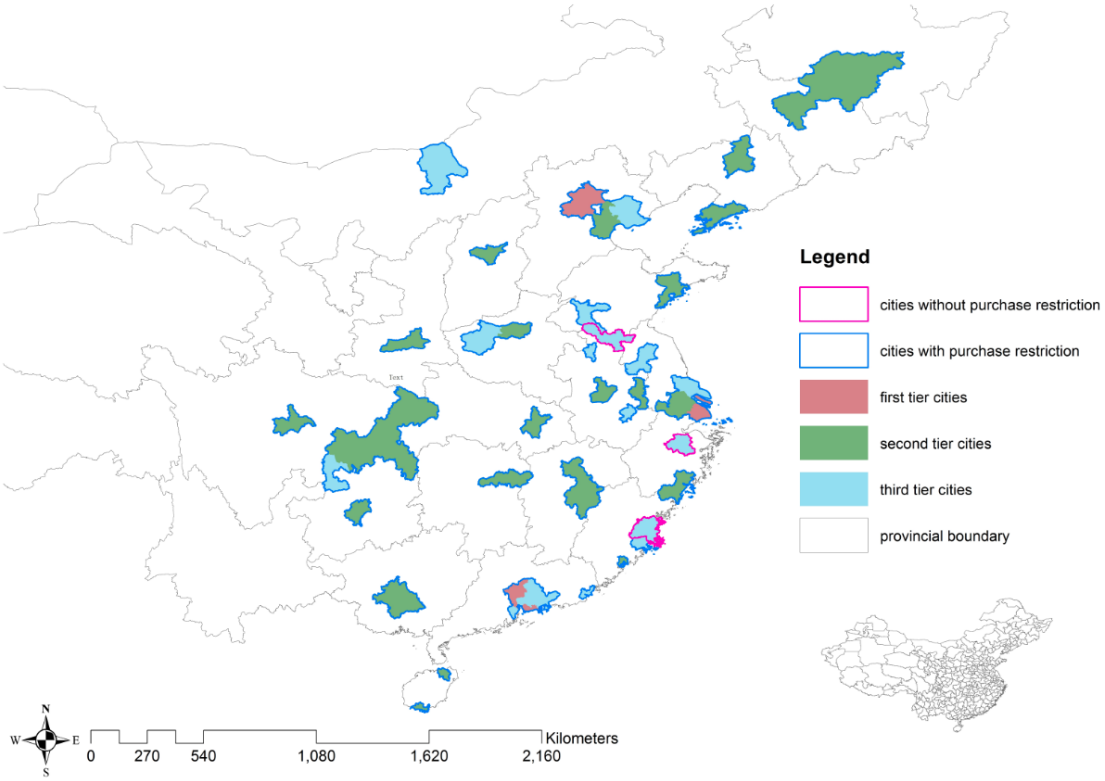

Table A-2 Probability of Purchase Restriction
Logit Model at City-Level in the 3rd tier cities

VARIABLES	(1)	(2)	(3)
Ln(city size)	4.381	3.545	4.483
	(3.234)	(2.677)	(3.013)
Ln(Per GDP)	-0.372	0.752	0.792
	(1.285)	(0.976)	(0.942)
Ln(price)	4.764		
	(3.686)		
Ln(unit sales)		-0.140	
		(0.835)	
Price Change			-7.096
			(6.686)
Constant	-74.126	-36.762	-45.941
	(45.833)	(24.803)	(28.513)
Observations	17	17	17

Notes: Standard errors are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively

Table A-3 Probability of Purchase Restriction
Logit Model at District Level in Four Cities

VARIABLES	(1)	(2)	(3)
Ln(district size)	-0.178	-1.056***	-0.951***
	(0.462)	(0.382)	(0.359)
Ln(price)	9.369***		
	(3.437)		
Ln(unit sales)		0.874	
		(0.997)	
Price Change			-187.004
			(491.184)
Constant	-81.284***	1.345	4.070**
	(30.832)	(3.561)	(1.898)
Observations	49	49	49

Notes: Standard errors are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively