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Using Purchase Restrictions to Cool Housing Markets: A Within-Market Analysis*

Tsur Somerville[a,*], Long Wang[b] and Yang Yang[c]

[a]Sauder School of Business, The University of British Columbia. 2053 Main Mall, Vancouver, BC, V6T 1Z2, Canada.
[b]School of Entrepreneurship and Management, ShanghaiTech University. 393 Middle Huaxia Road, Shanghai, 201210, China
[c]CUHK Business School, The Chinese University of Hong Kong. 12 Chak Cheung Street, Hong Kong SAR, China

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Abstract

In response to worsening housing affordability resulting from rapid housing price appreciation, some governments have introduced taxes or restrictions to reduce demand for local residential real estate by non-residents. We study the effectiveness of these efforts using the restrictions imposed by local Chinese governments in 2010-11 on the number of residential properties an individual could purchase. We use sales by developers of newly constructed units and exploit within city variation in the imposition of these restrictions to identify their effect on prices and transaction volumes. In addition to these direct effects on demand, we also identify the effects of these policies on supply using data from local government auctions of land to developers. Our results suggest that in the short run, restrictions on non-owner-occupant purchases significantly reduce activity levels by approximately 40%, compared to areas without restrictions. These effects diminish with time. However, there are no relative changes post-policy introduction in housing price between restricted and unrestricted areas. The results operate via end-user demand and not through the land market and the subsequent supply response by developers, as there is no relative change in either the number of land auctions or prices paid for land between districts with and districts without purchase restrictions following their introduction.

I. Introduction

In response to dramatically worsening affordability resulting from house price inflation, local residents in some cities have targeted their ire at residential real estate purchases by non-residents. In these instances they blame external capital flows to residential real estate as a major cause of rising gap between local house prices and local incomes.[1] Their anger is supported by recent theoretical work: capital inflows by non-resident purchasers who acquire residential real estate for capital appreciation or as a store of wealth, as opposed to for their own regular occupancy or to earn income from rents from regular occupants, can cause local house prices and rents to reflect larger asset markets and diverge from local labour markets.[2] In response, governments in Australia, Canada, Hong Kong, Israel, and Singapore have all placed higher purchase taxes on non-resident buyers. The United Kingdom and New York have adjusted the tax treatment of non-resident owners. In Switzerland, the number of units available for investor purchase is restricted. And in the summer 2018, New Zealand introduced severe limits on foreign investment in residential real estate.

Similar concerns emerged in China in late 2010 and early 2011 in the midst of a nationwide housing boom that grew from expansive policies following the world financial crisis. In addition to concerns by local residents about housing affordability, the Chinese central government was worried about asset price bubbles.[3] Also, unlike the countries listed above, in China the main target was domestic rather than international flows. Beyond macro-prudential directives affecting mortgage interest rates, mortgage underwriting criteria, and access to mortgage credit, the Chinese government also directed municipalities to limit the number of properties a household could purchase. Targeting investors, these restrictions prevented buyers, depending on their residency status, from purchasing either a second or a third property.[4]

In this paper we test for the effectiveness of the purchase restrictions in calming the housing markets, lowering asset bubble risk, and improving affordability. Unlike the macro-prudential policies that were imposed on all significant cities and applied to all buyer types, the purchase restrictions were enacted with variation: some cities did not introduce restrictions at all, while others implemented restrictions that varied

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*Corresponding author
tsur.somerville@sauder.ubc.ca (T. Somerville); wanglong@shanghaitech.edu.cn (L. Wang); zoeyang@cuhk.edu.hk (Y. Yang)
ORCID(s): 0000-0002-2130-0454 (T. Somerville); 0000-0002-9721-3365 (L. Wang); 0000-0001-7511-2910 (Y. Yang)

[1]See Bryant, C. in *Bloomberg Opinions*, Aug 18, 1918 for a synopsis: https://www.bloomberg.com/view/articles/2018-08-20/can-t-afford-a-house-blame-the-apocalypse.

[2]See Favilukis and Van Nieuwerburgh (2017) for a model of a housing market with local residents and non-resident investors.

[3]Mufson, S. "In China, fear of a real estate bubble." *Washington Post*, Jan. 11, 2010. http://www.washingtonpost.com/wp-dyn/content/article/2010/01/10/AR2010011002767.html. And more recently Balding, C. "Why China Can't Fix Its Housing Bubble." *Bloomberg Opinions*, June 24, 2018. https://www.bloomberg.com/view/articles/2018-06-24/why-china-can-t-fix-its-housing-bubble.

[4]Much of this is not for rent. A study by the Southwestern University of Finance and Economics reported 20.9% of housing units are unoccupied in 2011. As reported in *Wall Street Journal*, June 11, 2014, https://www.wsj.com/articles/more-than-1-in-5-homes-in-chinese-cities-are-empty-survey-says-1402484499.

by district within the city. We exploit the within-city variation in the tightening policies of the in Chinese market to evaluate the success of restrictions on investor demand in cooling housing markets and moderating rapid house price appreciation. Our identification comes from the standard difference-in-differences (DiD) approach, where the variation we exploit is across districts within cities, between new development projects in those districts with purchase restrictions and those without.

The contributions of this paper to understanding the effects of government actions designed to calm housing market stem from both a better matching of data to the identification strategy and from a supply side estimation using data from local government land auctions. In comparison with other work, we are able to execute DiD tests at a fairly granular level of geography, obtaining identification from the differences in changes between different districts within a city for sales be developers of completed units to buyers in new development projects, with fixed effects for individual developments. This allows us to better circumvent the bias problems from non-random treatment, because of the endogeneity of housing market conditions and policy actions, that occur with the cross-country or inter-city analyses of housing market interventions in the extant literature. We argue that any unobserved patterns correlated with selection (imposing restrictions) are likely to be more acute across cities than within cities because the latter category shares the same housing and labour markets.[5] Our approach remains subject to problems with non-random treatment and inter-group differences because the allocation of district to treatment (restrictions) or non-treatment (no restrictions) is tied to the area's distance from the city centre. While we believe this to be a less acute violation of DiD assumptions, our findings survive a variety of robustness and falsification tests that reinforce our claim that our approach is more consistent with the necessary assumptions of non-randomness inherent in the DiD methodology.

The second aspect that sets this work apart from others in the area is that we are able to test for supply effects.[6] The land supply data yields results on local government and developer behaviour that serve to assess the price and volume effects in the apartment unit market. Data on land auctions enables us to test whether developers reduced their bids for land, consistent with an expectation of long-term decline in demand, or whether local governments reduced the volume of land and potential buildable floor area they introduced into the market, in which case local policy objectives would match the national priorities. Both allow us to ascertain more clearly whether the apartment market outcomes reflect changes in demand or whether supply side effects are also present.

We find that quantity restrictions have substantial immediate effects on transaction volume but no statistically different from zero effect on residential property prices. In the six months following the introduction of quantity restrictions, transaction volumes in the districts within a city that had purchase restrictions fell over 40% relative to volumes in unrestricted districts. Over time, this difference declines in magnitude, to 30% for a 12-month window and 24% for a two-year window. In contrast, the difference in the change in house prices pre-and post-policy between these two areas is not statistically different from zero over any period in our data. The results are robust across tests if we compare districts as a whole and if we use a border discontinuity approach and limit the sample to developments within a 3-km band on either side of the border between restricted and unrestricted districts. Falsification tests on using placebo dates and district boundaries for timing and location yield null results, supporting the argument that the quantity restrictions were responsible for the differences in transactions volumes. In comparison to our results, studies using cross-city panels find higher volume effects and find that residential real estate prices in cities with restrictions fall by over 10% when compared with cites without such restrictions. The results presented here suggest that some of this may reflect the non-random assignment of the restriction treatment.

The land supply tests on land auction prices and volume show no statistically-different-from-zero differential changes in the number of land auctions, buildable area "supplied", and the winning bids in these auctions between purchase-restricted and purchase-unrestricted districts between the pre- and post-restriction windows. Overall, developers did not change their bids for land in districts restricted post-policy introduction when compared with districts unrestricted, nor did local governments show any relative difference in the number of sites brought to auction. This is consistent with a no-supply response to the purchase restrictions.

We cannot unambiguously determine that the purchase restrictions in some districts lowered aggregate citywide activity. In using a difference in differences (DiD) methodology we identify relative changes, so it is possible that demand was just shifted from restricted districts to unrestricted district in exactly offsetting amounts. There is a net negative time effect, which normally would allow us to identify the aggregate effects. However, because citywide higher downpayment requirements and higher interest rates were imposed co-temporally with district specific purchase restrictions, we cannot rule out that the aggregate negative effect results from the credit restriction channel. Transactions fall in developments in restricted districts that are both better and worse substitutes (because of location) for developments in unrestricted areas so the aggregate decline does remain plausible.

There are a number of possible behavioural explanations for an outcome where in the absence of a shift in supply quantity falls but prices do not. If developers expect the restrictions to be loosened at some point in the not so distant

[5] Those Chinese cities not imposing restrictions are overwhelmingly of lower political and economic importance (nearly all major cities and provincial capitals imposed restrictions) suggesting problems with the assumption of random treatment in an inter-city analysis.

[6] We use buildable area, which is equal to the product of maximum allowed floor area ratio and land area, as our quantity measure of the supply from a given land auction rather than land area as it better reflects the potential number of apartments that could result from the auctioned land.

future and their holding costs are low, then they would have little incentive to reduce prices rather than wait until demand recovers once restrictions are lifted. A related explanation is loss aversion behaviour by developers. The strength in the land auction market following the demand restrictions is more consistent with this first explanation than the loss aversion story. More generally, this outcome does suggest that in the presence of real estate demand booms, policies aimed at restricting demand may not be successful in the short to medium run at addressing affordability challenges, but can dramatically reduce market activity.

The remainder of the paper is structured as follows. Section 2 provides a brief summary of Chinese housing policies, focusing on the mix of measures introduced by the government in 2010 and 2011 with the objective of cooling down Chinese residential real estate markets. Section 3 is a review of the literature on policies to slow housing markets, covering both macro-prudential regulation and quantity restrictions, both in China and elsewhere. In Section 4, we describe the data used here as well as the identification strategy to test for effects. Finally, in Section 5, we present the results for price and volume effects at different levels of geography along with falsification and placebo tests for robustness. Section 6 concludes the paper.

II. Chinese Housing Market Interventions

The introduction of measures to cool the Chinese housing market in 2010 and early 2011 followed a period of intense growth in the Chinese housing market. In the wake of the world financial crisis, China pursued a program of stimulus led by an almost $US 600 billion investment program announced in Nov 2008[7]. Some pointed to this stimulus and the associated increase in liquidity as driving the subsequent estate boom: Wu, Deng, and Liu (2014) estimate real land prices in key Chinese cities rose by a factor of five between 2004 and 2012. Media reports describe high investment flows with investors owning apartments as pure stores of wealth: some estimated up to 30 percent of new apartments being purchased and left vacant[8]. The conditions of rapid price appreciation, surging investment volume, and high rates of new construction following the post crisis stimulus eventually led the Chinese central government to introduce measures intended to rein in the housing market. The State Council issued two directives, the Ten National Rule (effective on April 17, 2010) and the Eight National Rules (effective on January 26, 2011). These included changes to the Housing Provident Fund (HPF) underwriting to lower the maximum loan to value (LTV) ratio for purchases of residential property greater than 90 m^2.[9] Minimum down payments on loans from commercial banks were increased and minimum interest rates for mortgages were also raised. Finally, steps were taken to limit investment in residential real estate directly by restricting the number of properties an individual could purchase. While these were directives from the centre, the implementation decision was left to lower levels of government, where provincial governments forward the messages from the central government to the municipal and lower level governments. It was then up to a local government's discretion to customize these policies and determine the time-line based on local economic conditions.

The implementation of these policies had considerable variation by jurisdiction. First, even the financing policies, which were imposed in all cities, varied by implementation date: May 1, 2010 in Beijing to March 31, 2011 in Hefei.[10] In contrast to the changes to LTV rules and interest rates, purchase restrictions, which limited purchases based on *hukou*, a person's official city residency status, were not uniformly imposed. For instance, Guangzhou allowed those with *hukou* to purchase an additional unit, but forbad any purchases by non-residents; Shanghai allowed both to purchase just one additional unit; and many other cities limited residents to two units and non-residents to one. In addition, not all cities imposed purchase restrictions, and of those that did so, some did not impose them uniformly throughout all the districts in the municipality or county. It is the latter group, cities that imposed restrictions on some districts but not on others, that we use for our analysis. We exploit the differences between these groups before and after the imposition of restrictions in a standard DiD identification strategy.

For reasons given in the data section below, we use data on sales by developers of new units from four cities: Chengdu, Guangzhou, Hefei, and Qingdao. These cities provide us with variation by city type, date of implementation of purchase restrictions, and within-city geography. Guangzhou is a Tier 1 city, the others are Tier 2.[11] The municipal governments introduced the policies at different times between October 2010 and March 2011. In all four cities there are at least two districts without quantity restrictions on resident and non-resident buyers. The details, introduction timing, and district allocation of the quantity restriction for each city are provided in Appendix Table A-1. The restrictions, both citywide financing restrictions and the purchase restrictions by district, are the same in all four cities. The only variation across cities we observe is the date of implementation.

[7] 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

[8] 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.

[9] The HPF is a mandatory savings plan for employees of government, state owned enterprises, and some private businesses. Withdrawals are allowed only for the purchase of owner-occupied real estate and is a buyer's lowest cost financing, but typically need to be supplemented by bank financing. Studies and summaries of the HPF include Tang and Coulson (2017), Xu (2017), Yang and Chen (2014), Yeung and Howes (2006).

[10] Our larger sample of cities is limited to 126 of the largest or economically most important Chinese cities. These include all major cities and provincial capitals.

[11] For a discussion of Chinese city tiers see https://www.chinacheckup.com/blogs/articles/china-city-tiers. Traditionally rankings ranged from Tier 1-4, but rankings are not entirely consistent; for instance, comparing YiCai Rising Lab, and the *South China Morning Post*. However, consistently the largest 4-5 economies are in Tier 1 and the next group of major economic centres and nearly all provincial capitals are in the next lower tier. SCMP has 5 cities in Tier 1, 60 in Tier 2, 137 in Tier 3, and 411 in Tier 4

III. Literature Review

The root issue this paper addresses is the effect on local real estate of "external" capital flows, studying the effects of policies to curb these flows. Theoretical models of foreign demand (Chao and Yu, 2015; Tai, Hu, Chao, and Wang, 2017) or non-residents more generally (Favilukis and Van Nieuwerburgh, 2017) demonstrate how these inflows into local housing markets worsen affordability by raising house prices more than incomes rise.[12] In contrast, empirical studies using cross-country panels of capital flows (as measured by the current account deficit) and house prices are mixed: Aizenman and Jinjarak (2009) and Sa, Towbin, and Wieladek (2011) find a positive correlation between the capital account and house price inflation, but Jinjarak and Sheffrin (2011) do not. Greater success in demonstrating the relationship between capital inflows and house price inflation has come using city level and within city data. Papers such as Cvijanovic, Spaenjers, et al. (2015), Sá (2016), Badarinza and Ramadorai (2018), and Pavlov and Somerville (2017) all find evidence linking capital inflows to residential real estate to higher local house prices.

Our paper examines the effectiveness of housing market intervention that via direct restrictions rather unlike macro-prudential policies that target a broader credit channel mechanism. The Chinese purchase restrictions targeted domestic investment, with tighter constraints on non-local investment demand. As such, they are similar in objective to policies in other countries that have attempted to reduce non-resident demand through taxation. Using Chinese data is attractive for assessing polices that restrict housing demand because of the variation in their implementation across geography.

There are a number of papers in English that study the effects of the Chinese purchase restrictions.[13] Almost all of these work use a panel of cities and a DiD methodology, comparing cities with and without restrictions, before and after the introduction of the restrictions. For these papers, the challenge is the non-randomness in the application of the treatment. Individual city governments decided whether or not to apply the restrictions: those that did so are overwhelming the bigger, more economically important, faster growing cities in China. Though not an official hierarchy, Chinese cities are typically ranked in tiers, where the designation reflects a mixture of their economic and political importance. Using the South China Morning Post- 29 of 30 Tier 1 and Tier 2 cities had purchase restrictions, and only among the 137 Tier 3 cities are there substantial numbers of cities that did not impose restrictions.[14] Therefore the DiD treatment is essentially comparing treated more "important" cities with untreated less important ones.

A number of the inter-city panels analyses of the restrictions have used different strategies to deal with this identification problem. Cao, Huang, and Lai (2015) include pre-trend variables and follow the two-stage approach of Donald and Lang (2007) to the DiD estimation. They find purchase restrictions associated with an 18% decline in prices and a 60% decline in sales volume in the four quarters following the introduction of purchase restriction policies in restricted versus unrestricted cities. To address the problem of unobserved difference across cities, Yan and Ouyang (2018) use propensity score matching to define more limited, but better matching treatment and control groups. They end up with four cities that have restrictions matched to four control cities based on per capita GDP and population. Their regressions have limited explanatory power. However, the difference in mean differences appears to be a 20% relative decline in the prices in restricted cities. Similarly, Du and Zhang (2015) construct a replica of Beijing based on smaller lower status cities that did not adopt purchase restrictions during the period May 2010 and Nov 2011. They compare Beijing price appreciation with its replica and find that price appreciation in Beijing was 7.5 percentage points lower than predicted by the replica after the introduction of the restrictions. In general, the inter-city analysis of Chinese purchase restrictions finds larger effects on prices than the macro-prudential policy studies, but in all cases declines in volume are much larger than the declines in prices.[15]

The literature on non-resident and foreign buyer restrictions and taxes outside of China is very sparse. Hilber and Schoni (2016) study the January 2013 Swiss restrictions that banned the construction of new second homes in select Swiss municipalities.[16] Using a DiD methodology, they find that the ban resulted in increases in the price of second homes (less future supply) but lowered the prices of primary homes in affected areas by 12% (negative economic impact).

Our paper differs from the papers above because we address the challenge with non-random treatment by using within city variation rather than comparing restricted and non-restricted cities. We use detailed data that provide housing market measures at the project level. This allows us to take advan-

[12] In Chao and Yu (2015) the welfare implications depend on how taxes on foreign buyers are used, while in Favilukis and Van Nieuwerburgh (2017) the results are sensitive to non-resident versus local preferences for location.

[13] There is also a literature in Chinese academic journals that studies these policies. Liu (2013) and Wang and Huang (2014) 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 (2014) suggest that the purchase quota policy may reduce housing prices, but at an insignificant magnitude.

[14] Using YiCai Global's Rising Lab recent 2017 breakdown, which has six tiers, all of the four Tier 1 cities (Beijing, Guangzhou, Shanghai, Shenzhen) had purchase restrictions, as did 13 of 15 Tier 1A cities (and the two that did not are outliers in size, Chongqing, or a questionable inclusion, Donguan), 27 of 30 Tier 2 cities had purchase restrictions, and only among Tier 3 cities are there many cities without restrictions (only 10 of 70 Tier 3 cities had purchase restrictions).

[15] Sun, Zheng, Geltner, and Wang (2017), estimate the effects of purchase restrictions using data from a single city, Beijing. 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 and 51-77% in transaction volumes post-policy constraints.

[16] The policy applied to areas where second homes made up more than 20% of the housing stock, effectively resort areas. Hilber and Schoni note, the jurisdictions with more homeowners, more second homes, that are closer to a ski resort were most opposed to the initiative.

tage of the differences in the imposition of purchase restrictions within cities, both generally and in very local variation between restricted and unrestricted districts of cities. Within a city, treatment and non-treatment areas share the same general housing and labour markets, as well as the same local economy, therefore we have treatment and control groups (different districts within a single city) that should be more similar in conditions than is the case for the inter-city variation in the work cited above. We find much smaller and not statistically different than zero price effects and smaller declines in transaction volumes than do the papers that use variation across cities.

The other way our work differs from previous efforts is that we are able to make some traction on supply effects. The second part of our paper is an analysis of the effects of the restrictions on land sales to developers by local governments. This provides us with an indirect method to assess the effects of the restrictions on the supply function. We know of no other analyses of these types of restrictions on investor demand for residential real estate that address supply-side factors. Overall, we get no price effects and smaller volume effects than previous work. From the analysis of the land supply auctions to developers we show that the declines in transaction activity does not result from supply reductions.

IV. Data and Identification

4.1. 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. Transaction data are all for new units sold by developers to end buyers reported at the city, project, and deeds levels, where the first two are aggregate data and the last are individual transactions. We use monthly project level aggregate data from CREIS for sales by developers to individual buyers of newly constructed apartment (condominium) units. The data cover 49,525 projects in 126 cities from as early as 2005. Data is not uniformly available: often in earlier periods the aggregate data is available but individual transaction data are not, or their coverage is incomplete. We are interested in the variation in purchase restrictions within a city by district limits. this restricts us to nine of the cities in these data. And of these nine cities, we have sufficient pre-2011 data for only four as prior to 2011 the CREIS data is very sparse, with no observations for most cities. We are left with data for 2,014 projects in Chengdu, Guangzhou, Hefei, and Qingdao.

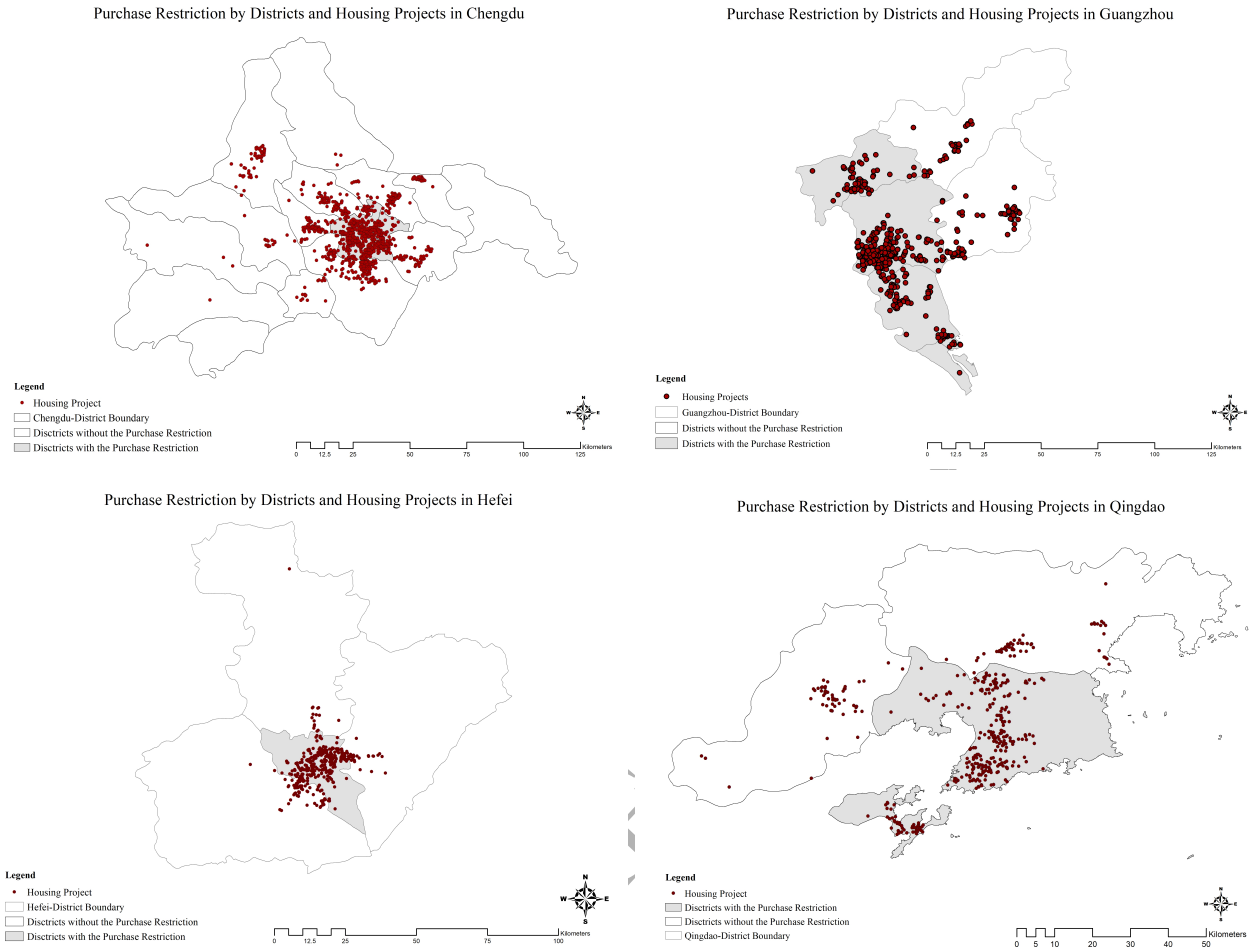
In the data we have different before and after policy introduction periods. As we describe above, the purchase restrictions were introduced on different dates in the four cities: Guangzhou introduced policies on October 15, 2010; Qingdao on January 31, 2011; Chengdu on February 15, 2011; and Hefei on March 31, 2011. For project transaction volumes, we have observations from Oct 2009 on for all districts, but for mean project prices, only from six months prior to the restriction introduction. We drop October 2010 for Guangzhou and February 2010 for Chengdu from the data because the policies were implemented mid-month and we have only monthly aggregations. In the basic set of regressions we will use a six-month pre-restriction window (before) with varying post-restriction windows.

Following the directives from the central government, these municipalities imposed restrictions on the more central and core urban districts. Buyers of properties in more distant suburban districts were not subject to the purchase restrictions. Strictly this violates the random treatment requirement for DiD estimation, though less so than with city level data. We address this in the estimation. Figure 1 shows the distribution of projects across city districts for the four cities, differentiating between purchase-restricted and -unrestricted districts. In contrast, the changes in housing finance rules, both higher interest rates, down payment requirements, and limits on HPF loans applied in all areas.

In the data, each observation is a project's summary statistics for a given month of sales of individual new housing units. Of these, we use the average unit price (Chinese Yuan per m^2), total units sold in the project that month, and average unit size (m^2). We provide these summary statistics for the project aggregates in the four cities in Table 1 for the -6-month/+12-month window. The data are broken down between purchase-restricted (Panel B) and purchase-unrestricted (Panel C) districts, and for periods before and after the imposition of the restrictions. Mean monthly sales volumes per project are lower in the restricted districts than in the unrestricted districts both before and after the implementation of restrictions. Consistent with urban models, prices are higher and average unit sizes are lower in more central restricted districts than in the unrestricted suburban districts, both before and after the implementation of restrictions. Unit prices rise in both types of districts after the imposition of purchase restrictions, and sales volumes fall in both district types. The latter finding is consistent with a decline in demand following the introduction of lending restrictions, higher interest rates, and higher down payment requirements that affect all districts in these cities and were introduced at the same time. However, the rise in prices over the period is not consistent with that explanation. The same patterns occur in city-specific descriptive statistics, which are available in the Online Appendix Table OL-1.

CREIS also supplies land transaction information in China from information published by the central, provincial, and local governments on individual land auctions. The dataset includes characteristics such as transaction price, transaction date, listing date, reserve price of an auction, size and location of land parcel, maximum building area per unit of land or floor space ratio (FSR), land type (residential, commercial, industrial, mixed, and others), transaction type (negotiation [*xieyi*], English auction[*paimai*], two-stage auction [*guapai*], sealed bid auction[*zhaobiao*]), and buyer name (both firm and individual). We use land auction data for Chengdu, Guangzhou, Hefei, and Qingdao, the same cities as above. These are sales of land through auctions from local govern-

Figure 1: Purchase Restrictions by District and Geographic Distribution of Housing Projects



Notes: This figure presents the the geographical distribution of housing projects across city districts for the four cities. The areas of shaded regions correspond to the districts subject to the purchase restrictions. The dots represent the housing projects.

ments to developers, and are thus distinct from the transactions based data described above. The summary statistics for these auctions are reported in Table 2. As with unit sales, land prices are higher in the core restricted districts and prices rise in both regions over the period under study, from 6 months prior (before) to 12 months following (after) the introduction of the purchase restriction policies. Average land area and buildable area per land auction are lower in both regions post-restrictions. The geographic distribution of these auctions as well as descriptive statistics by city are available in the Online Appendix Figure OL-1 and Table OL-2.

4.2. Specification

If the government interventions were successful, then we would expect to see both a decline in transaction volume as investors reduced purchases, and a decline in prices in response to the inward shift in demand. We hope to exploit within-market differences, where the restrictions in some areas result in a larger drop in demand in those areas, when compared with areas in the city that did not restrict demand. Between the pre- and post-restriction periods, the areas with restriction should have relative declines in both prices and volumes. The price effect, though depends, on the extent of downward price rigidity. In particular, if developers are not under pressure from lenders to liquidate unsold properties, and they believe that restrictions are temporary, it may be more profitable to not reduce prices and wait out the decline in demand, i.e. their reservation price does not fall.

The baseline regression specifications for the DiD analysis. The first is the standard treatment where we have a dummy variable $After_t$ that takes on the value of one in the months after the introduction of purchase restrictions. Projects in districts where purchase restrictions are or will be imposed have the value of one for the dummy variable $Treat_i$. The DiD effect is captured in the interaction of these two in $Treat_i * After_t$. Formally:

$$y_{i,t} = \alpha + \beta_1 * Treat_i * After_t + \beta_2 * After_t + \beta_3 * X_{i,t} + \mu_i + \delta_t + \epsilon_{i,t} \quad (1)$$

Table 1
Summary Statistics - Housing Projects (-6/+12 months)

Sample	Before Restriction		After Restriction	
Statistics	Mean	S.D	Mean	S.D
Panel A: All Districts				
Avg. Price/m2	8,200.41	5,219.12	9,356.20	6,478.49
Total Transactions	40.26	63.34	25.98	46.81
Avg. Unit Size(m2)	110.69	47.53	106.65	47.36
Observations	3,303		7,465	
Panel B: Restricted Districts				
Avg. Price/m2	9,378.54	5,720.57	11,257.92	7,393.86
Total Transactions	37.92	62.95	22.90	44.90
Avg. Unit Size(m2)	106.99	45.85	103.37	45.35
Observations	2,296		4,563	
Panel C: Unrestricted Districts				
Avg. Price/m2	5,514.23	2,088.27	6,366.01	2,717.28
Total Transactions	45.59	63.95	30.81	49.28
Avg. Unit Size(m2)	119.12	50.16	111.81	49.94
Observations	1,007		2,902	

Notes: This table reports the project aggregates in four cities (Chengdu, Guangzhou, Hefei, and Qingdao). The data are broken down between purchase restricted and unrestricted districts, and for 6 months before and 12 months after the imposition of the restrictions.

where $y_{i,t}$ is one of the outcome variables (price or transactions) for project i in year-month t. $X_{i,t}$ are other time and project specific control variables. μ_i refers to the project fixed effects, capturing the unobserved mean variations across projects. δ_t is the set of year-month fixed effects. The estimated coefficient of interest for the DiD effect is β_1. The dummy variable $Treat_i$ does not enter on its own because it is subsumed in the project fixed effects μ_i. Policy implementation dates vary, so $After_t$ is not perfectly co-linear with the year-month fixed effects δ_t.

We use two alternative specifications. Both address the effects the innate differences between the treatment and non-treatment districts might have on the DiD coefficients of interest in ways not covered by Specification (1), which assumes no systematic time-varying pre-treatment differences.

The first allows for non-parallel trends in the data in the period prior to the treatment (introduction of restrictions). We do this by allowing the pre-treatment (months before purchase restrictions were applied) mean effect for the projects in districts where there will be restrictions to vary from that of the non-treatment districts. Formally, we interact $Treat_i$ (the treatment fixed effect) with $Before'_t$, which has the value of one for the second three months of the six month pre-restriction period and zero otherwise. Other elements are similar to specification (1):

$$y_{i,t} = \alpha + \beta_1 * Treat_i * Before'_t + \beta_2 * Treat_i * After_t + \beta_3 * After_t + \beta_4 * X_{i,t} + \mu_i + \delta_t + \epsilon_{i,t} \quad (2)$$

Here β_1 will show the sign and statistical significance of a pre-treatment trend difference for projects in districts with purchase restrictions that would cause specification (1) to violate the no-parallel-trends assumption. The estimated coefficient of interest for the DiD effect is β_2. The magnitude of the difference is the difference between β_2 and β_1.

The second alternative imposes a functional form to address trend differences in the dependent variable before and after the introduction in restrictions across the two groups of districts. Here, we allow for different trends between the restricted and non-restricted areas, through $Trend$ and $Treat * Trend$, and then the difference in differences above and beyond this through a third interaction $Treat * Trend * After$. The full specification is:

$$y_{i,t} = \alpha + \beta_1 * Trend + \beta_2 * Treat_i * Trend + \beta_3 * Treat_i * Trend * After_t + \beta_4 * After_t + \beta_5 * X_{i,t} + \mu_i + \lambda_{month} + \epsilon_{i,t} \quad (3)$$

The variables have the same meaning noted above, except that λ_{month} is a month fixed effect for seasonality. The sign of the DiD effect is captured by β_3. There is not a simple parameter or combination of parameters to identify the magnitude of the effects of the restrictions because they depend on the values for $Trend$. We will estimate the changes for each

Table 2
Summary Statistics: Land Auctions (-6/+12 months)

Sample	Before Restriction		After Restriction	
Statistics	Mean	S.D	Mean	S.D
Panel A: All Districts				
Avg. Price/m2 of Buildable	1,636.91	1,964.69	1,654.94	4,042.24
Land Area	46,481.29	46,070.92	39,918.21	40,160.87
Buildable Area	110,199.19	137,017.04	98,243.17	121,477.48
Distance to CBD	31.89	22.26	32.07	21.58
Auction Type	0.51	0.50	0.49	0.50
FSR	2.43	1.39	2.42	1.41
Observations	413		537	
Panel B: Restricted Districts				
Avg. Price/m2 of Buildable	2,765.66	2,217.75	2,758.29	6,659.87
Land Area	45,703.78	46,786.53	40,001.56	41,512.22
Buildable Area	126,513.11	169,296.45	91,087.20	88,064.52
Distance to CBD	22.22	12.31	26.49	15.84
Auction Type	0.45	0.50	0.50	0.50
FSR	2.93	1.53	2.63	1.42
Observations	119		182	
Panel C: Unrestricted Districts				
Avg. Price/m2 of Buildable	1,180.04	1,649.26	1,089.28	1,056.17
Land Area	46,796.00	45,854.86	39,875.47	39,509.68
Buildable Area	103,595.93	121,259.45	101,911.86	135,419.57
Distance to CBD	35.81	24.13	34.93	23.51
Auction Type	0.53	0.50	0.49	0.50
FSR	2.23	1.27	2.32	1.40
Observations	294		355	

Notes: This table reports the district-month level summary statistics for land transactions in districts with and without the purchase restrictions in four cities (Chengdu, Guangzhou, Hefei, and Qingdao), 6 months before and 12 months after the implementation of the restrictions.

of the windows using the mid-trend value for each before and after window period. As with the two previous specifications stand alone values for $Treat$ are subsumed in the project fixed effects.

All of the specifications suffer from a possible problem that we identify only relative changes between treated and untreated (purchase restricted and unrestricted) districts. So, a negative estimated coefficient cannot differentiate between total declines and a redistribution of demand from one area to another. Tables 1 and 2 show transactions declined in both restricted and unrestricted districts after the introduction of restrictions. However, this occurred along with financing restrictions that applied globally in these cities. Thus we cannot separate out the pure absolute effect of the purchase restrictions. Formally, for each city there is a magnitude of a negative effect of the financing restrictions on transactions in both areas that is sufficiently large that the purchase restrictions had no aggregate effect, but merely transferred sales from one area to another. However, the similar pattern of declines in mean values between entire districts and those areas that are closer substitutes (comparing restricted and unrestricted areas) from the summary statistics for projects in the districts (Table 1) and those in 3km bands along the border between restricted and unrestricted districts (Online Appendix Table OL-3) are consistent with an actual decline.

V. Results

5.1. Apartment Sales

These empirical estimates of the relative effect of purchase restrictions use monthly residential development project level data for prices and sales volumes of apartment unit sales by month. In Table 3, we test the difference in individual development project mean log apartment prices per m^2 of floor area across districts before and after the introduction of purchase restrictions. All else being equal, one would expect higher relative demand in unrestricted areas post policy introduction that did not limit demand. This could result from larger declines in restricted areas or because demand shifted

Table 3
District Level DiD - Prices

Dependent Variable	*ln(average transaction price)*					
Time Window	-6m to 6m			-6m to 12m		
Model Specification	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Before		0.009			0.016	
		(0.016)			(0.017)	
Treat*After	0.010	0.017		0.018	0.034	
	(0.024)	(0.026)		(0.026)	(0.029)	
Treat*Trend			-0.002			0.003
			(0.004)			(0.003)
Treat*Trend*After			0.001			-0.000
			(0.001)			(0.001)
After	0.002		-0.002	0.011		0.047**
	(0.023)		(0.019)	(0.023)		(0.020)
Trend			0.015***			0.007***
			(0.003)			(0.002)
ln(size)	0.039	0.039	0.039	0.081*	0.081*	0.083*
	(0.055)	(0.055)	(0.055)	(0.045)	(0.045)	(0.045)
Observations	6,779	6,779	6,779	10,768	10,768	10,768
R-squared	0.883	0.883	0.883	0.849	0.849	0.848
Year-Month FE	Yes	Yes	No	Yes	Yes	No
Project FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	No	No	Yes

Notes: This table shows the difference in individual development project mean log apartment prices per square meter across districts 6 months before and 6-12 months after the introduction of purchase restrictions. $Trend$ starts at 1 for Oct 2008. Standard errors are clustered at the district level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

from restricted to unrestricted areas. Either way, with any inelasticity in supply, we would expect to see prices rise in the unrestricted areas relative to the districts with purchase restrictions.

All regressions in Table 3 use a six-month pre-treatment window, which varies slightly by city as shown in Appendix Table A-1. The first three regressions in Table 3 use a six-month post-purchase restriction policy implementation post window; the second three use a 12-month post window. Within each window length group, the regressions are ordered by specification. This table structure will be used for most of the tests.

The DiD effect for specifications (1) and (2) is the coefficient on $Treat * After$. For specification (3), it is the estimated coefficient on $Treat * Trend * After$. The calculation of the magnitude of the effects varies by specification. For specification (1), it is the estimated coefficient on $Treat_i * After_t$; for specification (2), the difference between $Treat_i * After_t$ and $Treat_i * Before'_t$; and for specification (3), specific trend start and end values were applied to the benchmark pre- and post-periods and then multiplied by $Trend_t$, $Treat_i * Trend_t$, and $Treat_i * Trend_t * After_t$.[17]. There is no statistically significant difference in the change in prices post-restriction implementation date between development projects in regions with purchase restrictions and those without. The point estimates are also small in magnitude, ranging from 1.0% to 3.4% across specifications. We examined longer windows of 18 and 24 months post-restriction policy introduction and found similar results, i.e. no difference in demand, or strongly downward sticky prices in the restricted districts pre- and post- relative to unrestricted districts (the table is available in the Online Appendix Table OL-4).

In contrast to the effect on prices, there are qualitatively large and statistically-different-from-zero effects of the purchase restrictions on differences in transaction volumes across the two types of districts. Table 4 shows that transaction volumes drop off much more after the introduction of restrictions in projects in the districts with purchase restrictions than in those without: falling by 42-51% in the six-month window. As the analysis window lengthens, the size of the effect declines, to a 30-36% decline for the 12-month window. These changes are statistically different from zero across all three specifications. We also tested for longer windows, finding negative and statistically different than zero DiD effects, though with smaller point estimates (24-26% decline in transactions) than in the shorter 6 and 12 month post-policy windows. These results are available in the Online Appendix Table OL-5.

Though the price regressions did not suggest a differential effect from the presence of restrictions, the effect is very strong and clear on the volume of property purchases. The

[17] We use window mid-point trend values, where Trend=1 for Oct 2008. For + 6 month the values are 25 and 31; for +/- 12 months they are 25 and 37. Strictly we calculate the trend effect pre and post for both groups and take the difference in the differences.

Table 4
District Level DiD - Transactions

Dependent Variable	*ln(number of transactions)*					
Time Window	-6m to 6m			-6m to 12m		
Model Specification	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Before		-0.108			-0.070	
		(0.083)			(0.087)	
Treat*After	-0.423***	-0.618***		-0.299***	-0.431***	
	(0.102)	(0.090)		(0.095)	(0.093)	
Treat*Trend			-0.005			0.049***
			(0.024)			(0.017)
Treat*Trend*After			-0.012*			-0.022***
			(0.007)			(0.006)
After	-0.286*		-0.534***	-0.217*		-0.070
	(0.146)		(0.168)	(0.120)		(0.143)
Trend			0.016			-0.068***
			(0.017)			(0.011)
Observations	6,779	6,779	6,779	10,768	10,768	10,768
R-squared	0.666	0.666	0.655	0.619	0.618	0.606
Year-Month FE	Yes	Yes	No	Yes	Yes	No
Project FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	No	No	Yes

Notes: This table shows the difference in individual development project mean log transaction volume across districts 6 months before and 6-12 months after the introduction of purchase restrictions. *Trend* starts at 1 for Oct 2008. Standard errors are clustered at the district level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

absence of a price effect could occur because of downward sticky prices or a significant inward shift in supply. Below in the land auction regressions we test for evidence of the latter mechanism. We also ran the log price and transactions regressions individually for each city (the results are included in the Online Appendix Table OL-6 and Table OL-7). The patterns in the results are identical for all cities to that found in the aggregate results presented here, though with variation in the point estimates across cities.

A key concern in these data is that there are difference between central (restricted) and suburban (unrestricted) districts that generate the observed results rather than the purchase restriction treatment. To address this, and test for robustness beyond different specifications, we conduct border discontinuity regressions using the DiD methodology. Here we limit the sample to projects within 3km of the border between districts where purchase restrictions are imposed and those where they are not. While the districts are different, this should reduce the variation in unobserved location characteristics between projects in the restricted vs. unrestricted districts by excluding those that are particularly close (in the restricted districts) or far away (in the unrestricted districts) from the urban centre. This should also serve as a better application of the random assignment assumption than in the district level regressions above. Figures showing the location of these projects and tables with the descriptive statistics of their monthly unit sales are included on the Online Appendix Figure OL-2. In limiting the sample to projects within 3km of the border between restricted an unrestricted districts we should exclude developments that are the least alike: the highest priced core urban areas developments and the least expensive, most distant suburban developments.

In Tables 5 and 6, we present the same regressions as those shown in Tables 3 and 4 but using the more focused border discontinuity sample. The differences between the border and entire district samples vary slightly by specification, but the patterns and general magnitudes are consistent across both samples. The overall results for prices are again qualitatively small and not statistically different from zero. For transactions, again we find large and statistically-different- from-zero declines in transaction volume in the 3km band in the restricted districts relative to those in the same width band just across the district border. The effects are a little larger than in the district analysis, with declines all approximately 48-51% across specifications, falling to declines of 31-37% for the 12-month windows. This suggests more demand switching in these areas compared to the districts overall as the areas should be closer substitutes that are the larger areas. Longer windows yield results consistent with smaller aggregate declines in volume with window length, as with the district regressions above.

In all these regressions using specification (2), for both prices and volumes, we regularly reject a unique pre-trend for districts where subsequently the municipal government will impose purchase restrictions. The coefficient β_4 on $Treat_i * Before'_t$ identifies the percentage mean difference between prices or transaction volumes for projects in the districts that have purchase restrictions imposed 3-6 months vs. 0-3 months prior to the restriction implementation. This estimated coefficient is never statistically different from zero and is typ-

Table 5
3km Border Band DiD - Prices

Dependent Variable	*ln(average transaction price)*					
Time Window	-6m to 6m			-6m to 12m		
Model Specification	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Before		-0.001			0.014	
		(0.020)			(0.023)	
Treat*After	0.003	0.012		0.017	0.035	
	(0.022)	(0.028)		(0.023)	(0.029)	
Treat*Trend			-0.004			0.004
			(0.004)			(0.004)
Treat*Trend*After			0.001			-0.001
			(0.001)			(0.001)
After	0.036		0.020	0.040		0.077***
	(0.027)		(0.030)	(0.025)		(0.028)
Trend			0.013***			0.005*
			(0.003)			(0.003)
ln(size)	0.017	0.016	0.018	0.067	0.066	0.067
	(0.063)	(0.062)	(0.063)	(0.053)	(0.053)	(0.052)
Observations	4,431	4,431	4,431	7,130	7,130	7,130
R-squared	0.855	0.855	0.855	0.811	0.811	0.811
Year-Month FE	Yes	Yes	No	Yes	Yes	No
Project FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	No	No	Yes

Notes: This table presents the same regression as those shown in Table 3, using a sample that contains projects within 3km of the border between districts where purchase restrictions are imposed and those where they are not. The dependent variable is log average transaction price. *Trend* starts at 1 for Oct 2008. Standard errors are clustered at the district 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 6
3km Border Band DID - Transactions

Dependent Variable	*ln(number of transactions)*					
Time Window	-6m to 6m			-6m to 12m		
Model Specification	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Before		-0.109			-0.054	
		(0.094)			(0.099)	
Treat*After	-0.478***	-0.619***		-0.308**	-0.422***	
	(0.134)	(0.133)		(0.127)	(0.136)	
Treat*Trend			-0.012			0.077***
			(0.037)			(0.022)
Treat*Trend*After			-0.012**			-0.031***
			(0.005)			(0.007)
After	-0.247		-0.382	-0.309*		-0.149
	(0.160)		(0.235)	(0.165)		(0.195)
Trend			0.001			-0.064***
			(0.025)			(0.017)
Observations	4,431	4,431	4,431	7,130	7,130	7,130
R-squared	0.668	0.668	0.666	0.618	0.617	0.608
Year-Month FE	Yes	Yes	No	Yes	Yes	No
Project FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	No	No	Yes

Notes: This table presents the same regression as those shown in Table 4, using a sample that contains projects within 3km of the border between districts where purchase restrictions are imposed and those where they are not. The dependent variable is log transaction volume. *Trend* starts at 1 for Oct 2008. Standard errors are clustered at the district level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

ically small in magnitude. As a result, in subsequent apartment sales tables, we will just present specification (1). For the land sales data, we will re-test these three specifications.

5.2. Robustness Tests

The DiD specifications used above ideally have a random allocation of the treatment across observations. This does not strictly hold here because treatment is geographic and systematic: inner districts have restrictions and outer, more suburban districts do not. Our results may be compromised if there is a geographically correlated effect that occurs with the purchase restrictions or it is the geography of the treatment that matters more than the treatment itself. For instance,if restrictions have a larger effect on more expensive units, which are those in the inner districts. So the financing restrictions, which were imposed on all cities and all districts at the same time as the purchase restrictions but did not hold for units less than or equal to 90 m^2 in size. The cost of financing restrictions, might bind more for more expensive units. In the standard monocentric urban model, both unit prices and sizes will vary parametrically with distance from the city centre. To address whether our findings reflect a cotemporal other effect or the purchase restrictions, we conduct a number of tests to determine whether the results above are robust to changes in variation of geography and timing.

Our primary test for the validity of the results are two falsification tests, one in geography and one in time. The first is a border discontinuity DiD placebo test, where we create an artificial district boundary within districts without purchase restrictions. This addresses whether the difference we see in the DiD regressions above reflects purchase restrictions, or just a more general effect related to distance from the city centre, since the purchase-restricted areas are closest to the urban core. For instance, the higher down payment requirements from the macro-prudential policies introduced during the same time period as the purchase restrictions could depress demand more for higher priced properties, which are more likely to be located in purchase restriction, urban core areas. If price falls continuously with distance, then this would show up again in this placebo test. The second placebo test examines whether the DiD effects above just reflect more general time patterns, either as part of the real estate cycle that affected districts deferentially. We proxy the introduction of the purchase restrictions as occurring separately as prior and subsequent to when they actually occurred, and have both pre- and post- periods lie entirely in the period either prior to or post the actual introduction of restrictions.

The geographic tests create a placebo purchase restriction and 3km band in districts that do not actually have restrictions. One half of this band lies 0-3km from the border with the actual purchase restriction districts, but is entirely in non-restricted districts. This is compared with the second half of the band that lies 3-6km from the border. The falsification test is that demand fell more for more expensive areas, as defined by proximity to, so we assign the placebo treatment effect to the 0-3 km band area, which is closer to the city centre. Appendix Table A-2 shows the results of this geographic falsification test for specification (1). There is no statistically significant difference in the changes in prices or transaction volume between the two areas pre-and post-restriction dates. As well, the point estimates are small. This is consistent with the results we present above being the result of the difference in restrictions across districts, and not a continuous effect of the differences in proximity to the city centre.

The time falsification tests are presented in Appendix Table A-3 again just using specification (1). Because of limited price data more than six months prior to the restriction imposition dates we are constrained to testing transaction volumes alone. Regressions (1) and (2) have a placebo restriction assigned to a period prior to the actual restrictions: 0-6 months prior for regression (1) and 0-10 months for regression (2), where both pre-and post-restriction periods occur before the actual introduction of restriction. For regressions (3)-(5) both periods occur after the actual introduction, so the placebo is assigning a no-treatment where one actually existed. Within these groups, the regressions differ by window length. The results are consistent: the estimated coefficient on the interaction $Treat_i * After_t$ is consistently not statistically different from zero. The effects are thus tied to the policy interventions by local governments and not a more general time effect.

The second class of robustness tests addresses the concern that macro-prudential lending restrictions that have geographic variation in their effects cause our results. For instance, because the financing restrictions effect units differently by price, which varies by distance from the CBD, as do the imposition of the purchase restrictions. To test and control for the possibility of geographically correlated changes, we conduct two types of tests. The first introduces distance controls in the form of distance to the CBD for the district sample and distance to the treatment/no-treatment district border for the 3km border sample, both on their own and interacted with time. The second segments the sample by property size, using only transactions of properties less than $90m^2$ in size, as the financing restrictions only applied to units above this size. For brevity and not to distract from the main focus of the paper the empirical results of these final three tests are presented in the Online Appendix Table OL-8, Table OL-9, and Table OL-10.

Introducing controls for the distance to the CBD or the distance to the border between restricted and unrestricted districts does not change our results on the relative effect of purchase restrictions on development project transaction volumes. The DiD effects from purchase restrictions are declines of 22-36% in transaction volumes for post-policy windows of 6-12 months. Transaction volume increase with distance from the CBD, but the slop of this gradient does not change post-policy introduction. Distance to the border, in the 3km sample, does not affect transaction volume, either generally or after. A direct comparison of the results with those in Tables 6 and 7 is not possible because including the distance measures requires excluding the development

project fixed effects. Even so, the results support are primary findings that purchase restrictions reduce transaction volume, and that these effects are substantially smaller in intra-city analysis than in inter-city analysis.

If we limit the sample to units less than $90m^2$ in size we continue to get negative DiD treatment effects on development project transaction volumes for the purchase restrictions treatment. However, none of the estimated coefficients are significantly different than zero. These results are not directly comparable because we use a smaller data sample with higher variance: projects where we have transactions of smaller units that can be aggregated to the project level. This limits us to only some projects from two cites: Qingdao and Hefei. The project volume figures are built up from projects where we have individual unit transactions, which results in a smaller set of projects (approximately 55% smaller) yielding larger standard errors. More importantly, the expected effect on small units is not clear. The absence of financing restrictions might increase demand for these units, which are more common in central purchase-restricted districts. Alternatively, for investors seeking to purchase multiple units the cost of capital became lower for the purchase one large unit than two smaller units of the same total purchase price.

5.3. Land Supply - Government Auctions

The analysis in the previous section evaluates the outcomes in the market for completed apartment (condominium) units. We cannot necessarily distinguish which part of the identified changes comes from the effect on buyer demand because of purchase restrictions, and which part may come from a supply response by developers. For instance, the observed drop in volume without a decline in prices is consistent with a shift inwards in both demand and supply curves. It is also consistent with a drop in demand but no drop in developer reservation price, because the profit maximizing strategy is to wait until the policies are reversed rather than sell at a discount.

To shed light on supply side effects, we perform the same DiD tests on the land supply market. In Chinese cities, local governments determine land supply through the auction of lands they designate as available to developers, which is an important source of local government revenue. The short-run quantity effects of these auctions should reflect local governments' decisions to bring land to the market, while price effects are determined by the bids of developers, given the current and expected future supply of land as well as and expected conditions in the apartment market when they expect to sell units developed on the land that is for auction. Thus, we interpret the quantity effects, number of land auctions and total buildable area, as reflecting government land supply, and the price effects developer demand given this supply.[18] No decline in land supply, but a decline in prices, suggests that there is an inward shift in developer supply. Alternatively, no decline in land auction bids is more consistent with developers waiting out the policy without lowering prices. This reflects future supply and not the number of units offered for sale by developers from their completed buildings (inventory).

Estimates of the differential effect of purchase restrictions on the price bid for land in restricted and unrestricted districts price effects in the land auctions are shown in Table 7. The dependent variable is the log of price paid for land per buildable m^2, the land component of potential buildable area. The fact that the coefficients on the DiD measures are not statistically different from zero rejects the hypothesis that land auction prices changed deferentially in response to the variation in purchase restrictions on apartment buyers. The point estimates suggest a moderate decline of between 2% and 11%, but the standard errors are quite large to have any confidence in these magnitudes or price direction. The results imply that given the supply of land for development, developers did not lower their bids further, or raise them less, in the districts with restrictions. This has two possible explanations: either the supply of auctions dropped enough to keep prices stable, or developers were confident that there would not be a major long-term effect on profitability from the purchase restrictions, as land bids reflect sales of units at least two years further on in the development process.

To test for land supply effects by local governments, we test two different measures of quantity in land supply. The first is the number of land auctions that occurred, and the second is the total buildable area that could result from development on the auctioned land. The number of auctions is aggregate counts per district-month, or the totals in a district in a given month, so our observation count is much lower than in the other analyses here. For buildable area per auction, we use individual land auctions as the unit of observation and are assessing the variation over time and across districts in the amount of buildable area offered in any given land auction.

Table 8 shows the relative change in the number of land auctions between restricted and unrestricted districts over the period before and after the implementation of the purchase restrictions. There is no apparent change, since the raw magnitudes of the point estimates are all below a 4% change and the standard errors are large relative to the point estimates. Table 9 presents the building area regressions. Here too, we cannot reject the hypothesis that following the restrictions, buildable area per auction did not decline in restricted areas, compared with non-restricted areas. However, the point estimates of declines are large in magnitude, though offset by the very sizable standard errors. Falsification tests like those applied to the project data and presented in Appendix Tables 2 and 3 do not yield any statistically-different-from-zero results.

The land supply regressions do not indicate an inward shift in supply in response to the purchase restrictions. Land prices, the number of parcels auctioned, and the buildable potential did not change in any differential way between districts with restrictions and those without. This pattern is con-

[18] Total potential supply of apartment space in the auctioned land is calculated as the total auction land area times the maximum allowed floor space ratio (FSR), which is the ratio of built area to land area used in land use regulations.

Table 7
Land Supply Analysis: Price (Transaction Level)

Dependent Variable	*ln(transaction price/buildable land area)*					
Time Window	-6m to 6m			-6m to 12m		
Model Specification	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Before		0.032			-0.053	
		(0.212)			(0.206)	
Treat*After	-0.051	-0.020		-0.067	-0.107	
	(0.168)	(0.244)		(0.175)	(0.221)	
Treat*Trend			-0.074*			-0.011
			(0.039)			(0.021)
Treat*Trend*After			0.009			-0.000
			(0.008)			(0.007)
After	0.059		-0.086	-0.034		0.049
	(0.202)		(0.212)	(0.176)		(0.157)
Trend			0.033			-0.003
			(0.035)			(0.015)
Auction Type	-0.104	-0.103	-0.093	-0.149	-0.150	-0.126
	(0.172)	(0.172)	(0.168)	(0.132)	(0.132)	(0.126)
ln(size)	-0.127***	-0.127***	-0.122***	-0.103***	-0.103***	-0.099***
	(0.044)	(0.045)	(0.045)	(0.036)	(0.036)	(0.036)
FSR	-0.219***	-0.218***	-0.225***	-0.186***	-0.187***	-0.190***
	(0.040)	(0.041)	(0.038)	(0.035)	(0.035)	(0.038)
ln(distance to CBD)	-0.212**	-0.213**	-0.204**	-0.229**	-0.228**	-0.244**
	(0.100)	(0.102)	(0.097)	(0.095)	(0.096)	(0.101)
Observations	673	673	673	950	950	950
R-squared	0.660	0.660	0.662	0.599	0.599	0.589
Year-Month FE	Yes	Yes	No	Yes	Yes	No
Land Type FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	No	No	Yes

Notes: This table presents the price effects in the land auctions in response to the purchase restriction 6 months before and 6-12 months after the introduction of purchase restrictions. The dependent variable is the log of price per buildable area. The $Trend$ is a time trend variable equals 1 for Oct 2008. Standard errors are clustered at the district level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

sistent with developers who see the government policies as temporary, to be reversed after some period. As a result, with no drop in the offered land in restricted areas compared to unrestricted areas, there were no differential changes in bids. Such a response is also consistent with prices of completed units remaining unchanged in the face of less buyer demand.

VI. Conclusion

In this paper, we look at the effects of restrictions on investor purchases of residential properties on housing market outcomes. Typically, such restrictions were introduced in China and other countries as differential taxes designed to address worsen affordability by calming overheated housing markets. Here, we try to measure the effectiveness of the Chinese policies in achieving these objectives. Other studies of China's restrictions policy have relied on cross-city panels using a methodology that assumes policy treatment to be random. Where we differ is that we use variation in the implementation of these policies within cities, which represents a step forward towards a cleaner test. While we do not fully escape the non-randomness problem since restrictions are imposed in central districts and not in suburban districts, our results survive both robustness and falsification tests. Additionally, we use data from government land auctions to compare outcomes in the end-user apartment market with conditions in the land input market. These comparisons indicate that our observed results come from the shift in demand by apartment purchasers, and not through changes in developer or government land seller actions.

Uniformly, we find that districts within a city where there were restrictions on purchases of apartments had significantly greater declines in transaction volume than did districts without these restrictions following their introduction. At the same time, there were no differential changes in transaction prices. The declines in volume were large, in excess of 40% in the first six month following the implementation of restrictions, but this number declined to less than 30% after 12 months. The supply regressions also show no differential decline in land bids and in the number of land auctions over this

Table 8
Land Supply Analysis: Quantity (District Month Level)

Dependent Variable	*ln(number of transactions)*					
Time Window	-6m to 6m			-6m to 12m		
Model Specification	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Before		-0.065			0.015	
		(0.150)			(0.143)	
Treat*After	0.038	-0.033		0.025	-0.002	
	(0.201)	(0.215)		(0.163)	(0.153)	
Treat*Trend			-0.089			0.004
			(0.070)			(0.034)
Treat*Trend*After			0.019			0.000
			(0.017)			(0.010)
After	-0.076		-0.346	-0.155		-0.063
	(0.286)		(0.417)	(0.248)		(0.236)
Trend			0.066			-0.006
			(0.059)			(0.018)
Observations	222	222	222	341	341	341
R-squared	0.570	0.570	0.559	0.515	0.514	0.474
Year-Month FE	Yes	Yes	No	Yes	Yes	No
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	No	No	Yes

Notes: This table presents the price effects in the land auctions in response to the purchase restriction 6 months before and 6-12 months after the introduction of purchase restrictions. The dependent variable is the log transaction volume. The $Trend$ is a time trend variable equals 1 for Oct 2008. Standard errors are clustered at the district 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 9
Land Supply Analysis: Buildable Area (Transaction Level)

Dependent Variable	*ln(buildable land area)*					
Time Window	-6m to 6m			-6m to 12m		
Model Specification	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Before		-0.101			-0.097	
		(0.412)			(0.363)	
Treat*After	-0.409	-0.469		-0.352	-0.434	
	(0.328)	(0.467)		(0.276)	(0.366)	
Treat*Trend			0.087			-0.100*
			(0.094)			(0.058)
Treat*Trend*After			-0.014			0.026
			(0.021)			(0.019)
After	0.044		0.086	-0.120		-0.383
	(0.382)		(0.455)	(0.305)		(0.347)
Trend			-0.022			0.027
			(0.077)			(0.034)
Auction Type	0.189	0.187	0.158	0.168	0.167	0.197
	(0.188)	(0.187)	(0.195)	(0.157)	(0.155)	(0.162)
ln(distance to CBD)	-0.066	-0.065	-0.071	-0.150	-0.149	-0.164
	(0.105)	(0.103)	(0.101)	(0.100)	(0.100)	(0.105)
Observations	673	673	673	950	950	950
R-squared	0.534	0.534	0.527	0.457	0.457	0.443
Year-Month FE	Yes	Yes	No	Yes	Yes	No
Land Type FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	No	No	Yes

Notes: This table presents the price effects in the land auctions in response to the purchase restriction 6 months before and 6-12 months after the introduction of purchase restrictions. The dependent variable is the log buildable area. The $Trend$ is a time trend variable equals 1 for Oct 2008. Standard errors are clustered at the district level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

period across the restricted and unrestricted districts of the four cities we study. Together, the results suggest that while buyers were affected by the restrictions, developers did not drop prices and behaved in a manner consistent with an expectation that the restrictions were temporary and that waiting to sell would be more profitable than dropping prices.

Our within-city sample produces results different from those of researchers who have studied these restrictions across cities. Our decline in volumes result at least 10 percentage points lower, and, most striking, while other reseachers find price declines in cities with restrictions of up to 16% compared to those without, we find no differential change in prices across districts.

Unfortunately, the simultaneous introduction of the purchase restrictions and at the same time as the financing restrictions prevents us from cleanly determining the aggregate effect of the purchase restrictions. Overall transactions are down in both restricted and unrestricted areas after the introduction of purchase restriction policy. However, for each city the effect of the financing restrictions is sufficiently large and applies equally to both areas such that the purchase restriction effects estimated here could just represent a shift of demand from restricted to unrestricted areas that exactly offsets, so that there is no aggregate effect of restrictions. We cannot rule this out. However, raw transaction volumes declined more in purchase restricted areas, both in sub-areas that were closer substitutes for units in unrestricted areas and those that were not. That even transaction volumes in those areas closer to the CBD fell more than those in the more distant parts of unrestricted districts units that might not be ready substitutes after the purchase restrictions were introduced supports the claim that declines were absolute, not just relative.

The stated objective of the restrictive policies was to tame high and accelerating house prices and calm markets. We find little evidence that the purchase restrictions resulted in price declines, though market activity clearly declined. This downward sticky response is consistent with a number of possible mechanisms (Stein, 1995; Clayton, Miller, and Peng, 2010). For instance, sellers might expect the policies to be temporary and therefore choose not to offer units for sale, or not to lower their reservation prices. Alternatively, the response is consistent with loss aversion. Whatever the explanation for the behaviour, this research reflects other studies on macro-prudential policies finding that although policies to restrict demand when housing markets are hot have strong dampening effects on market volumes, their ability to reverse problems of high house prices and address affordability are limited in the short to medium run.

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Appendix Table A-1: Regulation details/dates by city for financing purchase restrictions

City	Date	Policy	Purchase Restriction		Financing Constraint
			Local Residents	Non-local Residents	Required Down Payment
Guangzhou	October 15, 2010	No. 1311 [2010] of the Municipal Government of Guangzhou: *Notice of the State Council on Resolutely Curbing the Soaring of Housing Prices in Some Cities* (http://www.chinaacc.com/new/63_74_201010/19le156320208.shtml)	A local household can purchase one *additional* housing unit if the household only has *one* housing unit; Households are *not* allowed to purchase the *third* housing unit if the household already has *two* housing units.	A non-resident who can provide local tax payment proof or proof of social insurance payment for a period of one year or longer is allowed to purchase *one* additional housing unit; A non-resident who can *not* provide local tax payment proof or proof of social insurance payment for a period of one year or longer is *not* allowed to purchase any housing units.	For families purchasing the *first* housing units for the owner's own use with a construction floor area of 90 sq.m. or more, the down payment of loan shall not be less than *30%* of the total price; for families who purchase a *second* unit with mortgage, the down payment of loan shall not be less than *50%*; Loans will *not* be issued to the *third* housing unit.
			The purchase restriction only applied to housing units located in the following *nine* districts: Tianhe, Fanyu, Baiyun, Haizhu, Huadu, Liwan, Luogang, Yuexiu, and Huangpu. Conghua and Nansha Districts are *not* subject to the purchase restriction.		The financing constraint applied to housing transactions in *all* eleven districts in Guangzhou.
Chengdu	February 15, 2011	No. 5 & 7 [2011] of the Chengdu Municipal Commission of Housing and Rural and Urban Construction: *Notice on Further Improving Regulation of the Real Estate Market* (http://fgj.chengdu.gov.cn/cdsfgj/gfwj/2011-02/16/content_3ec509e62f134d189e6dd00139ff9c33.shtml)	A local household can purchase one *additional* housing unit if the household only has *one* housing unit; Households are *not* allowed to purchase the *third* housing unit if the household already has *two* housing units.	A non-resident who can provide local tax payment proof or proof of social insurance payment for a period of one year or longer is allowed to purchase *one* additional housing unit; A non-resident who can *not* provide local tax payment proof or proof of social insurance payment for a period of one year or longer is *not* allowed to purchase any housing units.	For families purchasing the *first* housing units for the owner's own use with a construction floor area of 90 sq.m. or more, the down payment of loan shall not be less than *30%* of the total price; for families who purchase a *second* unit with mortgage, the down payment of loan shall not be less than *60%;* Loans will *not* be issued to the *third* housing unit.
			The purchase restriction only applied to housing units located in the following *six* districts: Chenghua, Wuhou, Jinniu, Jinjiang, Qingyang, and Gaoxin. Xindu, Wenjiang, Qingbaijiang, Longquanyi, Pi, and Dujianyan districts are *not* subject to the purchase restriction.		The financing constraint applied to housing transactions in *all* twelve districts/counties in Chengdu.
Qingdao	January 31, 2011	No. 8 [2011] of the Municipal Government of Qingdao: *Notice on Further Improving Regulation of the Real Estate Market* (http://www.qddongbu.com/news_detail/newsId=269.html)	A local household can purchase one *additional* housing unit if the household only has *one* housing unit; Households are *not* allowed to purchase the *third* housing unit if the household already has *two* housing units.	A non-resident who can provide local tax payment proof or proof of social insurance payment for a period of one year or longer is allowed to purchase *one* additional housing unit; A non-resident who can *not* provide local tax payment proof or proof of social insurance payment for a period of one year or longer is *not* allowed to purchase any housing units.	For families purchasing the *first* housing units for the owner's own use with a construction floor area of 90 sq.m. or more, the down payment of loan shall not be less than *30%* of the total price; for families who purchase a *second* unit with mortgage, the down payment of loan shall not be less than *60%;* Loans will *not* be issued to the *third* housing unit.
			The purchase restriction only applied to housing units located in the following *seven* districts: Sifang, Shinan, Chengyang, Laoshan, Shibei, Licang, and Huangdao. Jimo, Pingdu, Jiaonan, Jiaozhou, and Laixi districts are *not* subject to the purchase restriction.		The financing constraint applied to housing transactions in *all* twelve districts in Qingdao.
Hefei	March 31, 2011	No. 6 [2011] of the Municipal Government of Qingdao: *Notice on Further Improving Regulation of the Real Estate Market* (http://www.ahaxfz.com/display.asp?id=405)	A local household can purchase one *additional* housing unit if the household only has *one* housing unit; Households are *not* allowed to purchase the *third* housing unit if the household already has *two* housing units.	A non-resident who can provide local tax payment proof or proof of social insurance payment for a period of one year or longer is allowed to purchase *one* additional housing unit; A non-resident who can *not* provide local tax payment proof or proof of social insurance payment for a period of one year or longer is *not* allowed to purchase any housing units.	For families purchasing the *first* housing units for the owner's own use with a construction floor area of 90 sq.m. or more, the down payment of loan shall not be less than *30%* of the total price; for families who purchase a *second* unit with mortgage, the down payment of loan shall not be less than *60%;* Loans will *not* be issued to the *third* housing unit.
			The purchase restriction only applied to housing units located in the following *four* districts: Baohe, Luyang, Yaohai, and Shushan. Beicheng, Zhengwu, Xinzhan, Jinkai, Gaoxin, and Binhu New districts are *not* subject to the purchase restriction.		The financing constraint applied to housing transactions in *all* ten districts in Hefei.

Appendix Table A-2: Falsification Tests: Placebo 3 km Border

Dependent Variable	*ln(price)*		*ln(volume)*	
Time Window	-6m to 6m	-6m to 12m	-6m to 6m	-6m to 12m
Model Specification	(1)	(2)	(3)	(4)
Treat*After	-0.029	-0.028	0.058	-0.001
	(0.025)	(0.032)	(0.148)	(0.150)
After	0.057**	0.065**	-0.683***	-0.658***
	(0.022)	(0.024)	(0.110)	(0.141)
ln(size)	0.004	0.027		
	(0.075)	(0.053)		
Observations	3,388	5,239	3,388	5,239
R-squared	0.854	0.807	0.652	0.606
Year-Month FE	Yes	Yes	Yes	Yes
Project FE	Yes	Yes	Yes	Yes

Notes: This table reports the results of geographic falsification test using specification (1). The sample includes a 3km band in districts that do not have restrictions. The treatment group includes housing projects lie 0-3km from the border in the non-restricted district. The control group contains housing projects lie 3-6km from the border in the non-restricted district. Standard errors are clustered at the district level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

Appendix Table A-3: Falsification Tests: Placebo Restriction Date (Quantity Only)

Dependent Variable	*ln(number of transactions)*				
Time Window	(-12 to -6m) vs (-6 to 0m)	(-20 to -10m) vs (-10 to 0m)	(0 to 6m) vs (6 to 12m)	(0 to 10m) vs (10 to 20m)	(0 to 12m) vs (12 to 24m)
Model Specification	(1)	(2)	(3)	(4)	(5)
Treat*After	0.084	-0.159	0.124	0.117	0.080
	(0.203)	(0.280)	(0.083)	(0.076)	(0.073)
After	0.073	-0.048	-0.066	-0.208**	-0.285***
	(0.186)	(0.234)	(0.094)	(0.097)	(0.098)
Observations	5,866	7,554	7,465	12,701	16,120
R-squared	0.617	0.492	0.652	0.594	0.569
Year-Month FE	YES	YES	YES	YES	YES
Project FE	YES	YES	YES	YES	YES

Notes: This table reports the results of time falsification test using specification (1). The dependent variable is log transaction volume. Model 1 to 5 have a placebo restriction assigned to a random month, and the regressions differ by window length. Standard errors are clustered at the district level and are shown in parentheses under the estimated coefficients. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.