The impacts of population mobility controls on the housing market: Evidence from the 2014 household registration reform in China

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Abstract

The household registration system (Hukou system) as a mobility control instrument in China largely restricts individuals' access to the social welfare system outside their hometown, making it costly and inconvenient for migrants to live long-term in host cities. Without local Hukou, most migrants will not consider purchasing houses where they work. In this paper, I study the effects of the 2014 Hukou reform on housing prices. Using apartment-complex-level housing data, I find that the implementation of the reform increased prices for lower-quality residential properties, while those of higher quality experienced negative shocks. This is consistent with findings in previous literature that more migrant workers move to places where controls are relaxed, spurring demand for basic housing.

Keywords: Hukou reform, housing, migration

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1 Introduction

The New Urbanization Plan, a major reform of China's household registration system in China, tremendously affects the migration pattern and urban development. First announced in 2014, it aimed to relax a population mobility control that had been in place for more than half a century. The goal was two-fold: 1) diminish the local rural urban divide and 2) attract more labor to cities that had high growth potentials by relaxing household registration restrictions. The government, worried the Plan might cause disruptions, conducted a pilot program prior to the national roll out. Qin and Wu [2022] recently identified a migration response to this policy shock wherein more migrants move to cities that have more relaxed regulations.

The sales and rental markets are both affected by the arrival and settlement of new migrants. The fluctuation in prices can lead to the redistribution of wealth and affect individuals decisions, such as school designation and employment status. I focus on the housing market responses to the reform. Exploring variation in the pilot status within cities and using monthly data at the apartment complex level in a difference-in-differences setup, this paper investigates how the housing market responds to Hukou reform. I find that within cities reform has a positive effect of 6% on housing sales price and about an 8% (marginally significant) positive effect on rental prices, which is consistent with the prediction that more demand for basic housing is generated by the new migrants. However, the effect on higher-end housing–which could be driven by the departure of potential higher-quality housing buyers–is much less positive. This contrast in the impacts on basic housing and high quality housing holds when controlling for treatment intensities, which I define as how much the change is precipitated by policy and what proportion of a city is affected geographically.

The potential sorting mechanism behind the heterogeneous housing price changes mirrors dynamics observed during the U.S. Great Migration. Defined in large part by the influx of black migrants from the rural South into the northern cities during World War II and the following decades, the white population fled from the central cities and moved to suburban areas in response. With every black migrant arriving in the city, 2.7

white residents left, according to the Boustan [2007]. Although the Chinese population is more homogeneous in racial composition, discrimination against migrants exists. It could be due to the increasingly crowded living environment, heterogeneity in the education attainment, or income disparities between the two population groups [Dorn and Zweimüller, 2021]. The consequences can be profound.

Similar concerns regarding regional disparities in the context of black white segregation also apply in the context of this paper. In China, many social welfare benefits and public services are locally financed. For example, since 2011, county governments have taken on most of the responsibilities of compulsory education, including financing, allocating resources, and school development [Zhao, 2009]. After financial decentralization, hospital revenues became less dependent on the local government subsidies, but the latter still accounts for about 10 % of revenues. The current social medical insurance has three networks with funds pooled at the county or prefecture levels [Meng et al., 2015]. Most primary care is provided by within counties. The departures of residents with certain characteristics in response to the adjustment of mobility restriction can lead to inequalities across regions.

Before relaxation, migrants in a city without local Hukou were discriminated against in the housing market. Moreover, they might have been ineligible to purchase housing units or apply for low-rent public housing, or they might have been charged a higher housing mortgage rate. That said, the benefits of relaxation are hard to ignore.

This paper contributes to the large literature that studies the impacts of the Hukou system–a means of population mobility control–on various social economic aspects and individuals' life decisions. The system creates inequality between rural and urban population, and between local residents and migrants. These inequalities are reflected in marriage [Qian et al., 2020], social identity [Afridi et al., 2015], rural urban labor market segregation [Meng, 2012, Ngai et al., 2018], wages [Liu and Kawata, 2022, Qin and Wu, 2022], unequal access to educational opportunities [Sieg et al., 2020, 2023] and other dimensions like rentals and purchases of residential properties [Li et al., 2017, Sun et al., 2017]. This study adds generally to the literature on internal migration, offering an additional example of local responses to migrant influxes, which recalls the black migration and white flight in the U.S. after World War II [Boustan, 2007, 2010, Boustan and Margo, 2013] and migrations across countries in Europe [Dorn and Zweimüller, 2021].

2 Institutional background

2.1 Household registration system

The household registration system, also known as the hukou system, has a long history in mainland China. It was first established in 1958 as a policy instrument to control internal migration. Under the system, every citizen is assigned at birth one of two Hukou types: an agricultural hukou or a non-agricultural hukou (sometimes referred to as an urban hukou). Also recorded on each hukou was the location where it was registered, information that determines whether one has access to social welfare benefits and public services provided locally. For example, if you are registered in Middlesex County in Massachusetts, it will be difficult for your children to enjoy the benefits of public education in Norfolk County, even if you live in Norfolk County. Given these restrictions, most people choose to buy houses in the county of their hukou. An individual can be considered local or non-local, depending on whether their hukou is in the area where a specific social benefit is provided. A person's hukou typically follows their parents' type, and the conversion is strictly regulated.

At the height of the hukou regulation, internal migration was rare because migrants were denied employment outside their hukou locality. Even after decades of evolution, the hukou continues to exist and determines individuals' access to local public services, such as education, medical insurance, housing and many other aspects of social and economic life. For example, migrants are discriminated against in the housing market. They might not be eligible to purchase housing properties, not qualify for low-rent public housing, face a higher housing mortgage rate, or lack the support of the housing provident fund. Moreover, they are not protected by other social welfare systems, such as medical insurance, which further impairs their ability to purchase residential properties in urban areas (Liao, 2020).

2.2 Administrative division in China

Figure 1a maps the boundaries of provinces. Each province can be further partitioned into prefectural cities. Take one coastal province, Zhejiang, as an example. Zhejing Province (in the middle of the east coast) contains 12 prefectural cities, each shaded in a different color (see Figure 1b). The prefectural city, Huzhou, at the top, can be separated into 5 county-level subdivisions (See Figure 1c). [1] Chinese prefectural cities are comparable to states in the U.S.–each constitutes a general partition of the country[2]–while Chinese counties are comparable to U.S. counties.

2.3 New urbanization plan and three rounds of pilots

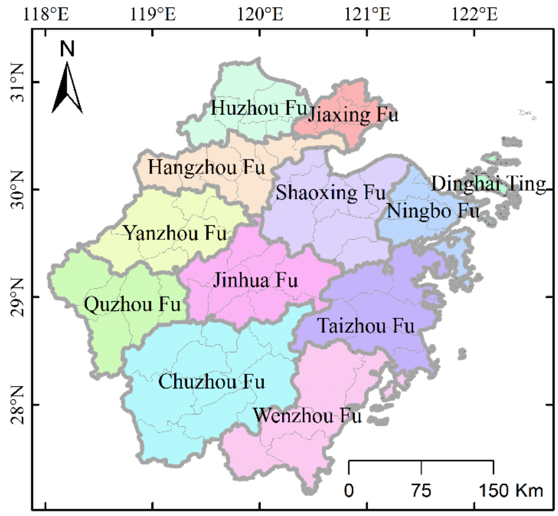
This paper focuses on the most recent large-scale hukou reform in China. Following the initial announcement in 2014, three rounds of pilots were conducted during the following two years in cities and counties across the nation. These pilot studies provide an opportunity to examine the short-term impacts of the hukou reform on the housing market. I use pilots as treatment units because they receive favorable fiscal support from both the central and provincial governments to implement the reform. Hence, I expect to see the effects of the reform more quickly in the pilot areas.

In February 2015, the first round of pilots were initiated. In November 2015, the second round of pilots started to roll out. In December 2016, the last round of pilots was announced. According to the plan, the reform would start to roll out nationwide between 2018 and 2020. While the exact progress of rollout must be checked by examining Hukou registration documents posted by local governments, for the sake of simplicity, it is reasonable to assume that the areas not listed in any of the three rounds were not

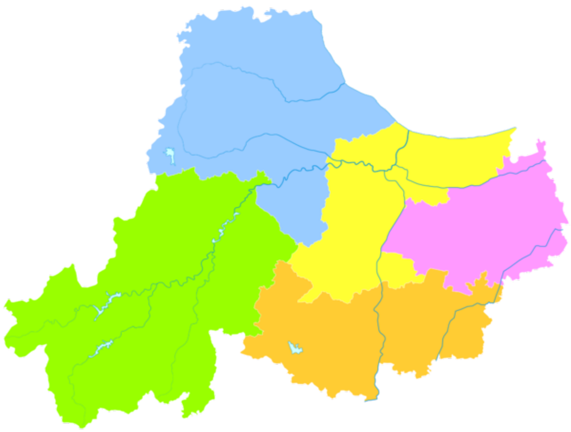
[1] While other administrative divisions exist, such as autonomous regions, municipalities, autonomous prefectures, districts, and so on, I categorize them on the basis of hukou regulations and city size into the most comparable group in this three-level division.

[2] They are comparable to some states in terms of area and population. For example, Huzhou city is 2,247 square miles in size–an area comparable to that of Delaware–and it has a population of 3,367,579, close to that of Connecticut.

(a) Provinces



(b) Prefectural city in Zhejiang Province



(c) Counties in Huzhou prefectural city, Zhejiang Province

Figure 1: The three-level administrative divisions in China

directly affected by the policy change before 2018.

The maps (Figure 5) show the geographic distributions of the pilot studies across the three rounds. Some pilots are not included in the housing data set, though. The index drawn on the maps is calculated at the city level. For example, city A consists of five counties, one of which is listed among the first round of pilot studies. The index, i.e., the fraction of counties treated within city A, is 0.2. City A falls into the category "$\leq$ 20% of counties treated". The categories in the legend do not cover the full range between 0 and 1 because there is no data point in the omitted range. [3] The pilot mostly samples from the southeastern half of China, which is roughly divided by the Heihe–Tengchong Line, within which approximately 96% of the country's population resides [4].

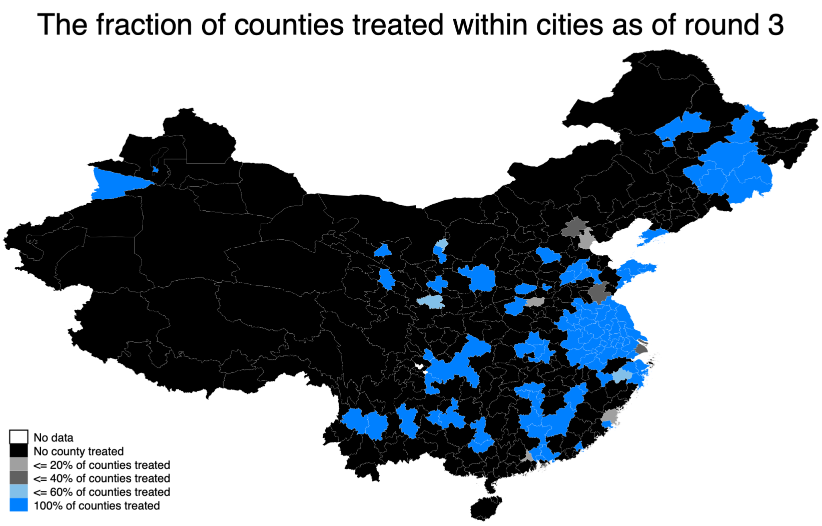


Figure 2: The geographical distribution of pilots

3 Data

In my analysis, I first examine two data sets from Xitai, a company that collects and compiles data from major online real estate transaction platforms. Table 1 and 2 present summary statistics. The first data set contains four outcome variables: sales prices, rental prices, and the two types of housing available on the market. Monthly data are

[3]These maps show all the listed pilots and are not limited to areas included in my data set on housing market.

[4]See Appendix Figure 5 for more details.

collected at apartment complex level. For each apartment complex, the data provide detailed information on features: geographic location, including latitude and longitude; green space; construction years; the total number of units; the number of parking lots; the floor area ratio (FAR); the type of buildings, such as villas and townhouses; the square footage ranges of each floor plan and the area compositions of different floor plans on sale and for rental. The data set covers residential units in 20 major prefectural cities from 2009 to 2018–a long window through which to study the effects of the 2014 reform on the housing market. The second data set also contains the four outcome variables, but here they are aggregate variables measured at the county level. The second data set covers a larger geographic area (70 cities) than the first.

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)	(5)
VARIABLES	N	mean	sd	min	max
Panel A. Policy-related					
$fraction_p$	4,958	0.479	0.442	0	1
$exclusion\_index_{pt}$	4,957	0.792	0.267	0.494	1.508
$\Delta exclusion\_index_{pt}$	4,957	0.142	0.254	-0.243	0.628
$apartment\_dist\_min_i$ (km)	4,957	191.5	103.4	28.19	515.9
Panel B. Apartment features					
unit sales price (k ¥/sqft)	4,364	1.821	1.653	0.208	17.86
unit rental price (k ¥/sqft)	3,206	0.003	0.002	0.001	0.025
units for sales	4957	20.113	30.931	0	390
units for rental	4957	11.402	20.056	0	222
green space (%)	4,896	34.83	8.276	10	80
FAR(floor_area_ratio)	4,894	2.650	1.468	0.130	13.85
construction year	4,875	2,010	6.743	1,980	2,018
parking lots	3,836	1,218	1,398	10	25,265
PUR (parking unit ratio)	3,823	1.037	1.229	0.008	23.95
villa available	4,958	0.129	0.335	0	1
townhouse available	4,958	0.048	0.215	0	1

Note: This table presents summary statistics for variables used in the regression analysis. Panel A shows policy-related variables, including the treatment fraction, exclusion index measures, and distance to nearest treated city. Panel B presents apartment complex features, including prices, unit counts, physical characteristics, and amenities. Numbers are rounded to the third decimal place.

4 Empirical Strategy

A key question when evaluating the effects of the Hukou reform is how to distinguish the treatment group from the control group. Qin and Wu [2022] define cities with an urban population above 5 million as the control group and those with an urban population below 5 million as the treatment group. They establish this division on the grounds that that the central government stated in its 2014 initial announcement that the overall stringency for mega cities with an urban population above 5 million should remain at the same level, while restrictions in cities with an urban population below 5 million should be relaxed to different degrees (see more details in Figure 4). In my housing data, most cities are large and fall into a single population category. They generally see a higher exclusion degree in Hukou regulation but the non-plot area increases more in the stringency compared to the pilot area. In this paper, I explore a setup for treatment and control groups that differs from the one employed by Qin and Wu [2022]. Granting Hukou more generously means integrating more into the local social welfare and public services system, which would require an upgrade of the system's capacity. For instance, when more migrants get local Hukou and reside in the city for long periods, more schools and housing supplies are needed. It takes time to build or expand these infrastructures [5]. According to the 2014 announcement, the reform was expected to be completed by 2020. With the fiscal support of both provincial and central government, I expect that pilot areas will be able to implement the reform more quickly. However, defining the treatment group on the basis of whether it was selected as a pilot could also raise concerns. Wang and Yang [2021] find that policy experiments in China are often positively selected for persuasive purposes. They also point out that local implementation and site selection can be affected by the career incentives of local leaders, even though the central government wants to achieve sample representativeness. Therefore, I next examine whether the selection of pilot programs is correlated with pre-reform housing prices.

[5] See Appendix Table 16. The housing price datasets used in this paper do not include small prefectural cities that have a population below 1 million.

4.1 Pilots as treatment groups

The pilot studies were carried out at a variety of administrative levels. Differences in their effects on housing prices depended on whether all or only part of the city was selected for the study. A whole city that was treated might offer strong incentives for migrants from another city to relocate. Among residents of the treated city, location preferences and restrictions on house purchases should remain largely preserved.

However, if only a small part of a city was listed as a pilot, its pull on migrants residing in other cities might not be strong. Meanwhile, it is reasonable to expect the gap between housing prices of pilot-study versus non-pilot-study areas within a city to change in response to this policy shock. To avoid mixing heterogeneous effects through multiple channels, I estimate separately the effects for fully treated cities and those for partially treated cities.

In China, an individual registered in a household in a city typically is allowed to purchase a residential unit anywhere in that city. Thus, household registration reforms in one or more counties in a city should have spillover effects in housing markets in the other counties that comprise the same city. However, because some social welfare benefits are linked to household registrations and offered at the county level, moving from one county to another within a city can be inconvenient. For instance, public medical insurance usually does not cover medical costs incurred outside the individual's home county. Similarly, low-cost mandatory education is offered in schools only within the county of one's household registration. Every county is a relatively large administrative unit. Currently, the whole nation consists of 2851 county-level divisions. Cixi, a coastal county in Ningbo City, Zhejiang Province, covers 525 square miles and in 2020 was home to 1.83 million residents. In comparison, Boston covers a land area of 48.4 square miles and has a population of 675,647. The commuting cost is high if you reside in one county and work or attend school in another. Thus, it is not common for an individual to purchase a residential property in a county other than the one in which most of his or her daily activities take place. Most migrants obtain household registration through employment. Hence, it is reasonable to expect that in counties in which household registration is relatively relaxed, the demand for housing will be greater than in untreated counties.

Equation 1 estimates the average effect on the sales prices of residential units in a county listed as a pilot study. Using city-year fixed effects, I exploit the variation in treatment statuses across counties within individual cities. The sample for this regression includes cities that do not include any pilots and cities that are partially treated. Being a partially treated city means that, over time, at least one but not all counties within the city will be listed as a pilot. As noted above, I believe the dynamics of fully treated cities differ from that of partially treated cities, and, thus, I do not include fully treated cities in this regression.

$$\begin{aligned} ln(price)_{icpt} = \beta_0 + \beta_1 pilot_c \times post_y + \boldsymbol{\beta_2'} \boldsymbol{F_i} \times pilot_c \times post_y + \boldsymbol{\beta_3'} \boldsymbol{F_i} \times post_y \\ + \boldsymbol{\beta_4'} \boldsymbol{FM_i} \times pilot_c \times post_y + \boldsymbol{\beta_5'} \boldsymbol{FM_i} \times post_y + \alpha_i + \gamma_{py} + \epsilon_{icpt} \end{aligned} \tag{1}$$

$ln(price)_{icpt}$ is the log of average sales prices for apartment complex i in county c, prefectural city, p in month t. α_i is the apartment complex fixed effect. γ_{py} is the city-year fixed effect. $post_y$ is a dummy variable that equals 1 if it is year 2014, when the initial announcement of New Urbanization Plan is released, or later. Since three rounds of pilots were announced close in time, I explore the standard DiD setup first. Later, I set the treatment time to correspond exactly with the announcement of the round of pilot studies, leading to a staggered DiD setup. To avoid the negative weighting problems in staggered DiD, I estimate the treatment effects for pilot areas announced in each round using never-treated areas as the control group. $pilot_c$ is a group indicator; in this regression for partially treated cities, it varies at the county level. For all apartments located in counties that are listed in some round of pilot, $pilot_c$ equals 1; otherwise, it is 0. F_i is a vector of standardized apartment features, including green space, the floor area ratio (FAR), the number of parking lots per unit (PUR), the latest construction year, whether the compartment complex has villas or townhouses, the fraction of one bedroom units among all the units on sale, and the fraction among all rental units of one bedroom rental units. If information in a complex is missing for some features, 0s are assigned for the standardized feature variables, which means I assign sample averages to them. I include a vector of missing value indicators, FM_i for each feature. Some interactions are omitted in the regression model due the presence of fixed effects. For example, the main terms of apartment complex features are not included because they are constant across time and, thus, are controlled for by the apartment complex fixed effect. Similarly, the main term of $pilot_c$ is skipped because an apartment complex can belong to either a pilot area or non-pilot area. Once the apartment complex is fixed, there will be no variation in the data across pilot versus non-pilot groups. I cluster the standard errors at the county level.

I also estimate a variant of the model by replacing all the continuous feature variables by dummy indicators, which equal 1 if the features have a value above the sample mean and are 0 otherwise. I assign 0 to missing features. For these observations the corresponding missing value indicator is 1.

I also explore the effects of a county being listed as a pilot study on rental prices, the number of residential units on sale, and the number of posted rental units at the apartment complex level under the framework of Equation 1. Renting and owning an apartment can substitute for one another. Under a more welcoming household registration policy, it is possible that more migrants would switch from tenancy to ownership, driving up sales prices and dampening rental prices. However, even the substitution between ownership and renting exists, the rental prices could still go up if there is enough new demand for housing due to an influx of new migrants. The comparison across the changes in these outcome variables could help reveal the underlying dynamics to some extent.

With Equation 2, I estimate the effect on housing prices when cities are fully listed as pilots. I first use the sample with information from the apartment-complex level, but now I select cities listed as pilots and cities not listed at all. I exclude partly treated cities included in Equation 1. Specifications are also modified. I replace the city-year fixed effect in Equation 1 with the year fixed effect, η_t, because I am no longer exploring variation in the treatment statuses within prefectural cities. Standard errors are clustered at the city level. Then I estimate the equation using the data set with county-level information for 70 cities. The effects on sales price and rental price are nonsignificant.

$$ln(price)_{ipt} = \beta_0 + \beta_1 pilot_p * post + \alpha_i + \eta_t + \epsilon_{ipt} \tag{2}$$

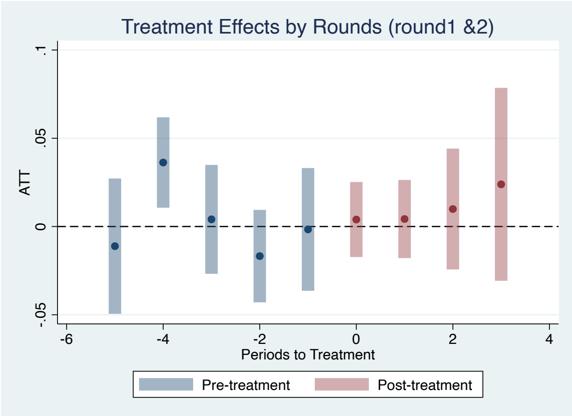
When I use the data set with information from the county level, all the specifications remain the same except that I replace the apartment fixed effects with city fixed effects. This is so because in this data set there is no apartment-complex-level information. Because few cities are fully listed a pilot, I focus for the remainder of this paper on the results from exploring variations in the treatment statuses for partially treated cities [6]

5 Results

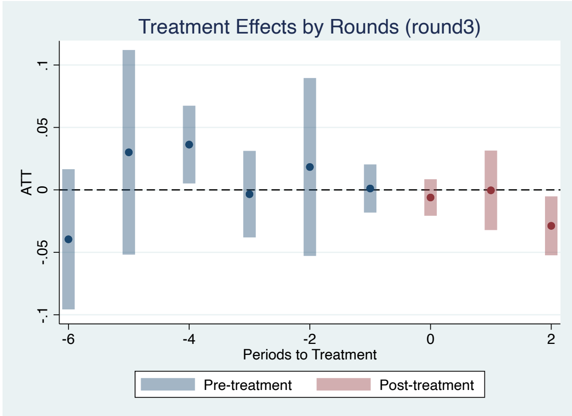
5.1 Heterogeneous effects on sales prices, rental prices, sales and rental volumes

Before showing the results from estimating Equation 1, I examine the overall effects without controlling for the apartment features (Columns (1) and (2) in Table 3 and Columns (1) and (2) in Table 4). This is true whether I use a single treatment time (because the three rounds were announced close in time) or use different treatment times–both round 1 and round 2 were announced in 2015, and round 3 (small sample compared to the first two rounds) in 2016. Figure 3 summarizes the results using the Callaway and Sant'Anna estimator with no apartment feature controls. Because round 1 and round 2 were announced in the same year, the estimator treats them as the same group. The overall effects on the housing market are not significant. Being listed as a pilot county does not have a clear overall impact on the sales prices, rental prices, sales, or rental volumes. However, after I control for a few apartment features, the results suggest that the pilot program has heterogeneous effects across housing units of different qualities (Table 3 and 4). Controlling for standardized continuous measures of apartment complex features and associated interactions terms, the coefficient of the main interaction term of

[6]See Appendix Table 12 and Table 13 for the results from comparing fully treated cities and non-treated cities.



(a) overall effects: round 1,2 vs never treated



(b) overall effects: round 3 vs never treated

Figure 3: Overall effects without apartment feature controls.

Notes: The figure was generated by Callaway and Sant'Anna (2021) estimator with no apartment feature controls. Each period stands for a year. Round 1 and round round 2 were announced in the same year; round 3 was announced in the following year. Round 3 pilot had far fewer data points than pilot 1(110,041) and pilot 2 (20,004).

the treatment dummy, *pilot*, and time dummy, *post*, suggests an average 6% increase in the sales prices attributable to the pilot conduction, setting all the features to 0s, which correspond to the average values in the sample before standardization. The estimate remains roughly the same when I switch from continuous controls to binary controls for apartment features.

To examine the effects on high-quality and basic housing separately, I assume a higher portion of green space in the complex, more parking lots per unit (higher PUR), having a high floor area ratio, recent construction, and having villas or townhouses offered in the complex, all of which are indicators of high-quality complexes. Although most of these variables are unambiguous, having a high floor area ratio requires an explanation. In China, many high-rise residential buildings in urban areas were constructed in recent years in response to rapid population growth. These modern skyscrapers make the most of the limited land resources in cities and often feature high-standard amenities and maintenance. In contrast, old residential buildings, built when land resources were abundant, tend to be low-rise, are less well maintained, and offer fewer amenities. The difference between the policy effects on apartment complexes of higher quality and housing considered more basic would be as large as about 17%, significant at the 0 .05 significance level. Given th at the supply of housing is relatively inelastic over the short term, these results hint at the possibility that basic housing is facing a surge in demand in response to the reform, while the demand for high-end housing embraces a negative shock.

Rents also increase by about 8% when all apartment features are set to average levels, although this increase is only marginally significant (Column (6) Table 3). This is consistent with the findings of Qin and Wu [2022] that more migrants are attracted to cities where Hukou regulations are relaxed. More migrants move to the pilot counties, creating new demand in the rental market and possibly in the sales market, too. The numbers of new listings for sales and rentals do not change significantly, which suggests the change in the prices might be mainly due to changes in demand.

Table 3: Effects on Housing Prices and Rents

	No Controls		Binary Controls		Continuous Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Ln(price)	Ln(rent)	Ln(price)	Ln(rent)	Ln(price)	Ln(rent)
pilot * post	-0.012	0.046	0.055*	0.083	0.058**	0.081*
	(0.026)	(0.030)	(0.030)	(0.062)	(0.027)	(0.049)
Constant	0.457***	-5.825***	0.482***	-5.813***	0.464***	-5.800***
	(0.003)	(0.003)	(0.016)	(0.017)	(0.008)	(0.012)
Observations	149,339	122,122	149,339	122,122	149,339	122,122
R^2	0.972	0.907	0.972	0.907	0.972	0.907
Apartment feature controls	NO	NO	Binary	Binary	Continuous	Continuous
apartment complex FE	YES	YES	YES	YES	YES	YES
city-year FE	YES	YES	YES	YES	YES	YES
cluster	141	137	141	137	141	137
mean of dep var	0.455	-5.820	0.455	-5.820	0.455	-5.820

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table, which presents the effects of the pilot program on housing prices and rents, explores variation in pilot program statuses within cities, across three specifications. Columns (1)-(2) show results without apartment feature controls. Columns (3)-(6) control for the proper interactions between standardized apartment complex features between the pilot status indicator (*pilot*) and the time dummy (*post*), including FAR (floor area ratio), PUR (parking lots per unit), last construction year, indicator for having villas or townhouses in the complex, fraction of one bedroom units among all units for sale, the fraction of one bedroom units for rent among all rental units, and green space areas. Columns (3)-(4) include binary apartment feature controls (above/below mean indicators), and columns (5)-(6) use continuous standardized apartment feature controls. The dependent variables are log housing prices and log rents. All specifications include apartment complex fixed effects and city-year fixed effects. Standard errors are clustered at the county level (counties are contained within and partition cities).

Table 4: Effects on Housing Market Activity

	No Controls		Binary Controls		Continuous Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	# sales unit	# rental unit	# sales unit	# rental unit	# sales unit	# rental unit
pilot * post	3.800	-0.862	-17.846***	-2.785	-1.071	-1.060
	(3.492)	(3.196)	(6.297)	(7.565)	(3.857)	(3.017)
Constant	23.737***	16.203***	22.779***	11.235***	26.999***	18.002***
	(0.449)	(0.411)	(2.321)	(2.864)	(1.158)	(1.030)
Observations	164,463	164,463	164,463	164,463	164,463	164,463
R^2	0.475	0.589	0.483	0.596	0.482	0.595
Apartment feature controls	NO	NO	Binary	Binary	Continuous	Continuous
apartment complex FE	YES	YES	YES	YES	YES	YES
city-year FE	YES	YES	YES	YES	YES	YES
cluster	141	141	141	141	141	141
mean of dep var	24.23	16.09	24.23	16.09	24.23	16.09

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table, which presents the effects of the pilot program on housing market activity, explores variation in pilot statuses within cities, across three specifications. Columns (1)-(2) show results without apartment feature controls. Columns (3)-(6) control for the interactions between standardized apartment complex features and both the pilot status indicator (*pilot*) and time dummy (*post*). These features include FAR (floor area ratio), PUR (parking lots per unit), last construction year, an indicator for having villas or townhouses in the complex, the fraction of one-bedroom units among all units for sale, the fraction of one-bedroom units for rental among all rental units, and green space area. Columns (3)-(4) include binary apartment feature controls (above/below mean indicators), and columns (5)-(6) use continuous standardized apartment feature controls. The dependent variables are the number of sales units and rental units transacted. All specifications include apartment complex fixed effects and city-year fixed effects. Standard errors are clustered at the county level (counties are contained within and partition cities).

5.2 Treatment intensity: Does the fraction of treated counties within a city matter?

For those cities that are partially listed as pilots, the treatment effects might vary based on how much area or population the regulation has changed. Geographically, it could be represented by the fraction of counties conducting pilot experiments within a prefectural city. I estimate the main regression again except that I use a continuous measure of treatment intensity, $intensity^f$, defined as the fraction of pilot counties within each city if a county is listed as a pilot and 0 if not. If a larger fraction of counties within a city proposes more welcoming policies, migrants might view that city as presenting more opportunities to settle down. If the synergy effect that attracts migrants is stronger as more counties undergo reform, we would expect a larger effect if the fraction of pilot counties is higher (i.e., a positive β_1 in Equation 3). Meanwhile, if potential buyers of better quality housing have a strong distaste for living or owning properties alongside these new residents, better-quality housing prices should be more negatively affected as more migrants are attracted to the area by friendlier policies. I also estimate the regression using three other outcome variables: ln(rent), the number of sales units, and the number of rental units.

$$\begin{aligned} ln(price)_{icpt} = \beta_0 + \beta_1 intensity_c^f \times post_y + \boldsymbol{\beta_2'} \boldsymbol{F_i} \times intensity_c^f \times post_y + \boldsymbol{\beta_3'} \boldsymbol{F_i} \times post_y \\ + \boldsymbol{\beta_4'} \boldsymbol{FM_i} \times intensity_c^f \times post_y + \boldsymbol{\beta_5'} \boldsymbol{FM_i} \times post_y + \alpha_i + \gamma_{py} + \epsilon_{icpt} \end{aligned} \tag{3}$$

In Table 5 Column (3), for apartment complexes with all features at the sample average level and without offering townhouses or villas, the pilot treatment has a positive effect on promoting the sales prices. It ranges from 1.6% (0.174∗0.09) to 8.7 % (0.174∗0.09) for the lowest intensity of 0.09 and for highest intensity of around 0.5. The estimated range is between 2.0 % and 10% using binary controls for apartment features (Column (1)). They are roughly comparable. Similar to the results from the main regression, higher-quality residential units experienced a more negative price shock[7]. I define "high quality" housing as having all continuous desirable apartment features set to the 75th percentile of the sample—including green space area, floor area ratio (FAR), the number of parking lots per unit (PUR), and the latest construction year—with the dummy for townhouses or villas set to 1, and indicators for basic housing characteristics—including the fraction of one-bedroom units among all units on sale and the fraction of one-bedroom units for rental among all rental units—set to the 25th percentile of the sample. The definition for "basic housing" is that all continuous features are set to 0, which corresponds to the means before standardization, and the dummy for townhouses or villas is also set to 0. Table 6 reports the difference in the policy effects. If the intensity is set to be 1

[7]See Appendix Table 14 for effects on housing market activities.

(Table 6 A.2.), the price growth rate of the selected high-quality housing could be 32.5 percentage points lower than that of basic housing. This estimate could be exaggerated due to the projection in treatment intensity. In Panel A.3 and Panel A.4, I set the fraction to its maximum and minimum in the sample, with the growth rate estimated to be 16 percentage points and 3 percentage points lower, respectively [8].

[8] The estimates using binary controls are quite similar (Panel B). See appendix Table 15

Table 5: Treatment Intensity Effects on Housing Prices and Rents

	Binary Controls		Continuous Controls	
	(1)	(2)	(3)	(4)
VARIABLES	Ln(price)	Ln(rent)	Ln(price)	Ln(rent)
pilot * fraction * post	0.198**	0.178	0.174*	0.207
	(0.096)	(0.185)	(0.090)	(0.164)
Constant	0.481***	-5.811***	0.463***	-5.801***
	(0.017)	(0.018)	(0.009)	(0.014)
Observations	149,339	122,122	149,339	122,122
R^2	0.972	0.907	0.972	0.907
Apartment feature controls	Binary	Binary	Continuous	Continuous
apartment complex FE	YES	YES	YES	YES
city-year FE	YES	YES	YES	YES
cluster	141	137	141	137
mean of dep var	0.455	-5.820	0.455	-5.820

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table presents the treatment intensity effects of the pilot program on housing prices and rents across two specifications. The key variable of interest is *pilot * fraction * post*, where *fraction* represents the fraction of counties listed as pilots within a city where a county c is located, serving as a measure for treatment intensity. Columns (1)-(2) include binary apartment feature controls (above/below mean indicators), while columns (3)-(4) use continuous standardized apartment feature controls. Apartment features include FAR (floor area ratio), PUR (parking lots per unit), the latest construction year, an indicator for having villas or townhouses in the complex, fraction of one bedroom units among all units for sale, the fraction of one bedroom units for rental among all rental units, and green space area. The dependent variables are log housing prices and log rents. All specifications include apartment complex fixed effects and city-year fixed effects. Standard errors are clustered at the county level.

Table 6: Heterogeneous Effects with Continuous Features

	(1)	(2)	(3)	(4)
VARIABLES	ln(price)	ln(rent)	# sales unit	# rental unit
A.1. Basic pilot effect (high quality - basic)				
Difference	-0.106**	-0.062	7.215	-4.264
	(0.047)	(0.066)	(5.969)	(6.034)
A.2. Intensity effect, fraction = 1 (high quality - basic)				
Difference	-0.325**	-0.191	18.321	-14.842
	(0.126)	(0.217)	(17.755)	(16.206)
A.3. Intensity effect, fraction = 0.5 (high quality - basic)				
Difference	-0.162**	-0.095	9.161	-7.421
	(0.063)	(0.109)	(8.877)	(8.103)
A.4. Intensity effect, fraction = 0.09 (high quality - basic)				
Difference	-0.030**	-0.017	1.666	-1.349
	(0.011)	(0.020)	(1.614)	(1.473)
Observations	149,339	122,122	164,463	164,463

*** p<0.01, ** p<0.05, * p<0.1

Note: This table presents heterogeneous effects using continuous apartment feature controls. Standard errors in the parentheses. The "high quality" definition includes apartment features at the 75th percentile: green space, the floor area ratio (FAR), parking lots per unit (PUR), the latest construction year, the townhouse/villa indicator = 1, and the basic housing indicators (the fractions of one bedroom units for sale and for rent) at the 25th percentile. The "basic" definition sets all continuous features to 0 (corresponding to the sample means before standardization) and the townhouse/villa indicator to 0. Row A.1 shows the basic pilot effect from the main specification. Rows A.2-A.4 show intensity effects at different treatment fractions (1.0, 0.5, and 0.09, respectively).

5.3 Treatment intensity: measuring the policy changes

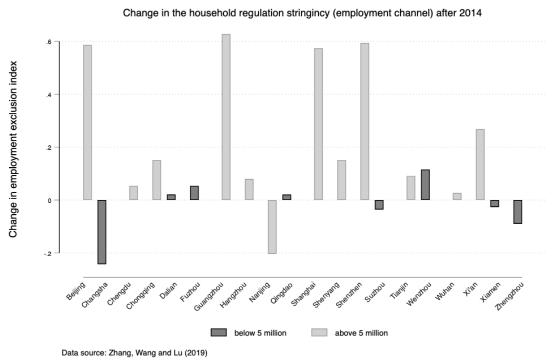
Similarly, I estimate Equation 4 with a different measure for treatment intensity . The variable $intensity_c^e$ is the interaction between the change in employment-related Hukou granting policy after and before 2014 in prefectural city p and the pilot status of county c. Given that most cities experience a slight increase in the index during the post period, the result is in line with previous observations that the policy has a positive effect on prices for basic housings (Table 7). The high-quality housing is estimated to have a less positive shock, although the result is not significant in this setting, which could be caused by imprecise measurement of policy changes (Table 8).

$$\begin{aligned} ln(price)_{icpt} = \beta_0 + \beta_1 intensity_c^e \times post_y + \boldsymbol{\beta_2'} \boldsymbol{F_i} \times intensity_c^e \times post_y + \boldsymbol{\beta_3'} \boldsymbol{F_i} \times post_y \\ + \boldsymbol{\beta_4'} \boldsymbol{FM_i} \times intensity_c^e \times post_y + \boldsymbol{\beta_5'} \boldsymbol{FM_i} \times post_y + \alpha_i + \gamma_{py} + \epsilon_{icpt} \end{aligned} \tag{4}$$

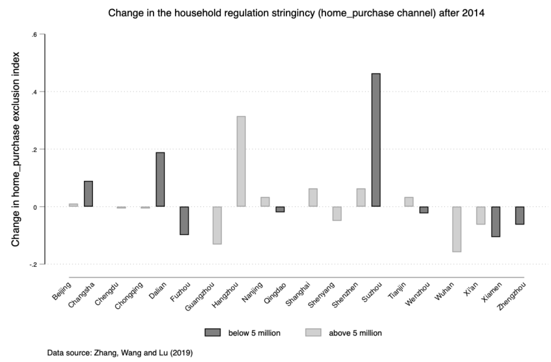
Migrants can obtain household registration (Hukou) in their hosting city mainly through four channels: investment, house purchase, high-end employment, and ordinary employment. Other channels through direct relatives and special contributions exist, but their regulations either lack variation across cities or target a small number of migrants. Qin and Wu [2022] find that the difficulty of obtaining a Hukou in cities with urban populations below 5 million through ordinary employment significantly decreases compared to that of mega cities with urban populations above 5 million in the period between 2014 and 2016. However, they do not observe a significant drop in stringency in other channels.

I use index data constructed by Zhang et al. [2019]. They construct indices to measure the stringency of Hukou regulation in two periods: 2000 to 2013; and 2014 to 2016. Each index is based on regulations for obtaining local urban Hukou through a specific channel. Information on regulations is extracted from Hukou policy documents at prefectural, provincial, and national levels. A higher value of a specific index means greater stringency in granting a local urban Hukou through that channel. For example, academic qualifications, years of employment, years of residence in the hosting city, and years of contribution to the local social security (insurance) network are often considered in evaluating Hukou grants via ordinary employment. Zhang et al. [2019] assign higher scores to more stringent requirements and synthesize all dimensions into a single index. For instance, if a city relaxes its requirements for academic qualifications and years of employment after the reform while keeping other requirements unchanged, we would expect a lower index value for the city, although the magnitude of the change is less interpretable. Another limitation of the index data is that it only calculates averages in the two periods for each city, so I rely on the strong assumption that changes between the two values are driven by the reforms in the pilot counties.

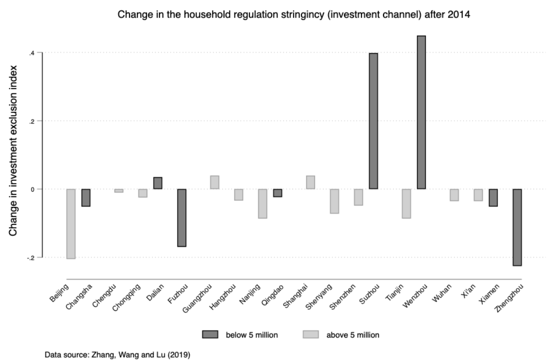
Figure 4 summarizes changes in the stringency of obtaining a local Hukou through each channel. Each bar represents the difference between the average stringency level for 2014-2016 and the average stringency level for 2000-2013 for each city in the housing price dataset. From this figure, we can see that large cities with urban populations above 5 million in the sample–Beijing, Guangzhou, Shanghai, and Shenzhen–have witnessed increased stringency for the ordinary employment channel. In contrast, cities like Changsha, Zhengzhou, Nanjing, Suzhou, and Xiamen have reduced the stringency in this channel.



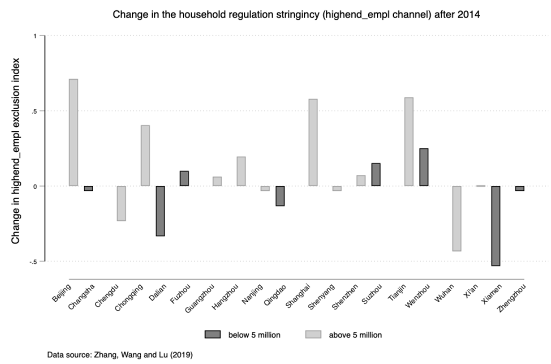
(a) Ordinary employment channel



(b) Home purchase channel



(c) Investment channel



(d) High-end employment channel

Figure 4: These figures display changes in household registration stringency after 2014 through four main channels. The change for each channel is calculated as the difference between the index during Period 2 and the index during Period 1. The index data are constructed by Zhang et al. [2019]. Each index is based on regulations for obtaining local urban Hukou through that specific channel. Information on regulations is extracted from Hukou policy documents at prefectural, provincial, and national levels. A higher value of a specific index means greater stringency in granting a local urban Hukou through that channel.

Table 7: Treatment Intensity: Policy Changes Effects

	(1)	(2)	(3)	(4)
VARIABLES	Ln(price)	Ln(rent)	# sales unit	# rental unit
pilot * change in exclusion * post	0.112*	0.182	-9.816	-4.881
	(0.060)	(0.113)	(8.189)	(7.815)
Constant	0.468***	-5.796***	27.143***	17.906***
	(0.008)	(0.010)	(1.010)	(0.962)
Observations	149,339	122,122	164,463	164,463
R^2	0.972	0.907	0.482	0.595
Apartment feature controls	Continuous	Continuous	Continuous	Continuous
apartment complex FE	YES	YES	YES	YES
city-year FE	YES	YES	YES	YES
cluster	141	137	141	141
mean of dep var	0.455	-5.820	24.23	16.09

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table presents the effects of treatment intensity measured by policy changes measured using a stringency index, exploring variation in treatment status within cities. The key variable of interest is *pilot * change in exclusion * post*, which captures how changes in exclusion policies within pilot areas affect housing outcomes in the post-treatment period. The *change in exclusion* variable measures the intensity of policy implementation across different pilot locations. All specifications include continuous apartment feature controls (green space, FAR, PUR, construction year, villa/townhouse indicator, and one-bedroom unit fractions) and their interactions with the treatment variables. The dependent variables are log housing prices, log rents, the number of sales units, and the number of rental units transacted. All specifications include apartment complex fixed effects and city-year fixed effects. Standard errors are clustered at the county level.

5.4 Treatment intensity: measuring spillover effects

Given that my sample of apartment complexes only contains a subset of major cities, one might worry that proximity to the nearest pilot areas that are smaller than themselves in urban population size which relax Hukou requirements by a larger degree, might affect the policy effect observed for the cities in my sample. To alleviate this concern, I calculate the distance from each apartment complex in my sample to the nearest small city with an urban population below 1 million that is either partially listed as a pilot or fully listed as a pilot. The motivation is that being closer to such an area would reduce the positive effect of the policy in attracting migrants.

$$
\begin{aligned}
ln(price)_{icpt} = \beta_0 + \beta_1 distance_i \times pilot_c \times post_y + \boldsymbol{\beta_2'} \boldsymbol{F_i} \times distance_i \times pilot_c \times post_y \\
+ \boldsymbol{\beta_3'} \boldsymbol{F_i} \times post_y + \boldsymbol{\beta_4'} \boldsymbol{FM_i} \times distance_i \times pilot_c \times post_y + \boldsymbol{\beta_5'} \boldsymbol{FM_i} \times post_y \\
+ \alpha_i + \gamma_{py} + \epsilon_{icpt}
\end{aligned} \tag{5}
$$

I estimate the model shown in Equation 5. $distance_i$ varies across apartment complexes. The estimated effect for basic housing in Table 9 is 0.0003. Given that the sample mean distance is about 227 km, the average effect would be 0.0681 (6.81 %). The results also predict that being closer to a city with a larger degree of relaxation would mitigate the positive effect in cities with relatively more moderate policy change. The difference between the effects on basic housing and better-quality housing is also stark. The sales price of the latter increases less than that of basic housing by about 10.4 percentage points given the average distance (Table 10).

Table 2: Summary Statistics: Unit Composition

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
Panel C. Units for sales (area %)					
1B	4,889	0.091	0.168	0.000	1
2B	4,889	0.328	0.236	0.000	1
3B	4,889	0.405	0.235	0.000	1
4B	4,889	0.132	0.181	0.000	1
5B	4,889	0.037	0.117	0.000	1
6B	4,889	0.006	0.041	0.000	0.813
Panel D. Units for sales (imputed fractions)					
1B	4,889	0.125	0.203	0	1
2B	4,889	0.358	0.239	0	1
3B	4,889	0.376	0.239	0	1
4B	4,889	0.108	0.167	0	1
5B	4,889	0.029	0.102	0	1
6B	4,889	0.005	0.033	0	0.767
Panel E. Units for rental (area %)					
1B	4,715	0.169	0.241	0	1
2B	4,715	0.374	0.255	0	1
3B	4,715	0.354	0.259	0	1
4B	4,715	0.081	0.156	0	1
5B	4,715	0.019	0.090	0	1
6B	4,715	0.003	0.028	0	0.828
Panel F. Units for rental (imputed fractions)					
1B	4,715	0.219	0.287	0	1
2B	4,715	0.386	0.264	0	1
3B	4,715	0.314	0.264	0	1
4B	4,715	0.064	0.146	0	1
5B	4,715	0.015	0.082	0	1
6B	4,715	0.002	0.023	0	0.847

Note: This table presents summary statistics for unit composition variables. Panels C and E show area percentages of bedroom types (1B to 6B) for sales and rental units. Panels D and F present corresponding imputed fractions. 7B and 8B categories are omitted due to minimal observations.

Table 8: Heterogeneous effects on housing units: policy change

VARIABLES	(1) ln(price)	(2) ln(rent)	(3) # sales unit	(4) # rental unit
Difference (high quality - basic)	-0.017	-0.035	2.231	-2.894
	(0.023)	(0.028)	(2.446)	(2.594)
Observations	149,339	122,122	164,463	164,463

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1 The definition used for "High quality" in the testing is having desirable apartment features, including green space, floor area ratio (FAR), the number of parking lots per unit (PUR), the latest construction year, at the 75th percentile in the sample, the dummy for townhouses or villas being 1, and indicators for basic housing, including the fraction of one bedroom units among all the units on sale and the fraction of one bedroom units for rental among all rental units, at 25th percentile. The definition for "basic" is that all the continuous features set to 0, which correspond to the means before standardization and the dummy for townhouses or villas also set to 0. The change in restriction index is set to be around 0.23.

Table 9: Treatment Intensity: Distance to Nearest Treated City

VARIABLES	(1) Ln(price)	(2) # sales unit	(3) # rental unit	(4) # rental unit
pilot * distance min * post	0.0003**	-0.003	-0.006	-0.006
	(0.000)	(0.017)	(0.012)	(0.012)
Constant	0.463***	26.949***	18.134***	18.134***
	(0.009)	(1.203)	(1.049)	(1.049)
Observations	149,339	164,463	164,463	164,463
R^2	0.972	0.482	0.595	0.595
Apartment feature controls	Continuous	Continuous	Continuous	Continuous
apartment complex FE	YES	YES	YES	YES
city-year FE	YES	YES	YES	YES
cluster	141	141	141	141
mean of dep var	0.455	24.23	16.09	16.09
mean of distance (km)	226.6	229.5	229.5	229.5

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table presents the effects of treatment intensity measured by distance to the nearest treated city, exploring the variation in treatment status within cities. The key variable of interest is *pilot * distance min * post*, which captures how proximity to other treated cities affects housing outcomes in pilot areas during the post-treatment period. Distance is measured in kilometers, with an average distance of approximately 227-230 km across specifications. All specifications include continuous apartment feature controls (green space, FAR, PUR, construction year, villa/townhouse indicator, and one bedroom unit fractions) and their interactions with the distance and treatment variables. The dependent variables are log housing prices, number of sales units, and number of rental units transacted. All specifications include apartment complex fixed effects and city-year fixed effects. Standard errors are clustered at the county level.

Table 10: Heterogeneous effects on housing units: distance

VARIABLES	(1) ln(price)	(2) ln(rent)	(3) # sales unit	(4) # rental unit
Difference (high quality - basic)	-0.104**	6.132	-4.638	-4.638
	(0.047)	(6.061)	(5.757)	(5.757)
Observations	149,339	164,463	164,463	164,463

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1 The definition used for "High quality" in the testing is having desirable apartment features, including green space, floor area ratio (FAR), the number of parking lots per unit (PUR), the latest construction year, at the 75th percentile in the sample, the dummy for townhouses or villas being 1, and indicators for basic housing, including the fraction of one bedroom units among all the units on sale and the fraction of one bedroom units for rental among all rental units, at 25th percentile. The definition for "basic" is that all the continuous features set to 0, which correspond to the means before standardization and the dummy for townhouses or villas also set to 0. The distance is set to be the sample mean for the regression, varying from 226.6 km to 229.5 km depending on the outcome variable.

6 Conclusion

Evidence found in this paper suggests that the pilot program on Hukou relaxation has heterogeneous effects on high-quality housing versus basic housing. For average housing units, pilot implementation could potentially contribute to a 6% increase in the sales prices. The supply of new housing units is relatively inelastic. In addition to the null effects on the volume of rental and sales units discussed earlier, land sales for residential construction after 2014 in large cities do not change significantly (Table 16. The change in prices could mostly be attributed to the changes in demand. These housing units are usually the choices for new settlers in urban areas. In line with previous literature (e.g. [Qin and Wu, 2022]) which documents that more migrants were attracted to cities after the reform where Hukou policies were adjusted, the increased prices for basic housing units could reflect the arrival of this population and their demand for housing. Rents are also estimated to increase by about 8 %. This further suggests that the increase in sales prices is not purely driven by incumbent migrants in the city substituting from renting to purchasing housing units.

However, the policy has almost opposite effects on apartment complexes of higher quality. They could have experienced a much less positive price shock. The difference can be as large as 17 %. This could be driven by locals leaving the area, or fewer investments overall in higher-quality housing units. Local residents or potential investors might be concerned that the influx of new migrants would overcrowd the area and lead to a decline in the quality of public services and social welfare benefits.

Appendix

Table 11: Pilot prefectural cities in the housing price data set

	Round 1	Round 2	Round 3
Full city	Nanjing(P5)[1], Dalian(P4), Guangzhou(P5), Wuhan(P5), Suzhou(P4), Changsha(P4), Qingdao(P4)		
Part of city	Beijing (0.056)(P5), Zhengzhou(0.1)(P4), Wenzhou (0.1) (P4), Chongqing(0.237)(P5)	Beijing (0.167)(P5), Zhengzhou(0.1)(P4), Wenzhou (0.1) (P4), Chongqing(0.289)(P5), Shanghai(0.056)(P5)	Beijing (0.333)(P5), Zhengzhou(0.1)(P4), Wenzhou (0.1) (P4), Chongqing(0.368)(P5), Shanghai(0.222)(P5), Tianjing(0.111)(P5), Fuzhou(0.091)(P3)
Not listed	Shanghai (P5), Tianjing(P5), Chengdu(P5), Hangzhou(P5), Shenyang(P5), Shenzhen(P5), Xi'an(P5), Xiamen (P4), Fuzhou(P3)	Tianjing(P5), Chengdu(P5), Hangzhou(P5), Shenyang(P5), Shenzhen(P5), Xi'an(P5), Xiamen (P4), Fuzhou(P3)	Chengdu(P5), Hangzhou(P5), Shenyang(P5), Shenzhen(P5), Xi'an(P5), Xiamen (P4)

[1] This table summarizes cities included in the housing data set used by this paper and their pilot status. P5 stands for urban population above 5 million, P4 for urban population between 3 million and 5 million, P3 for urban population between 1 million and 3 million, P2 for urban population between 0.5 million and 1 million, P1 for urban population below 0.5 million.

Table 12: Effects of Conducting Pilot in the Whole City

VARIABLES	(1) Ln(price)	(2) Ln(rent)	(3) # rental unit	(4) # sales unit
pilot * post	-0.093	-0.001	0.292	5.581
	(0.164)	(0.039)	(2.102)	(4.034)
Constant	0.269***	-5.958***	16.388***	21.593***
	(0.076)	(0.018)	(0.977)	(1.875)
Observations	163,021	138,283	180,367	180,367
R^2	0.606	0.347	0.030	0.045
City FE	YES	YES	YES	YES
year FE	YES	YES	YES	YES
cluster(city)	13	13	13	13
mean of dep var	0.225	-5.959	16.52	24.19

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table presents the effects of conducting the pilot program at the whole city level. The key variable of interest is *pilot * post*, which captures the city-level treatment effect in the post-implementation period. The dependent variables are log housing prices, log rents, number of rental units, and number of sales units. All specifications include city fixed effects and year fixed effects. Standard errors are clustered at the city level. This regression uses the part of Xitai data set that contains apartment complex level information and covers 20 cities.

Table 13: Effects of Conducting Pilot in the Whole City

	(1)	(2)	(3)	(4)
VARIABLES	Ln(price)	Ln(rent)	# rental unit	# sales unit
pilot * post	0.007	-0.017	57.436	165.170
	(0.043)	(0.030)	(160.378)	(149.679)
Constant	-0.600***	-6.559***	1,277.178***	1,530.862***
	(0.014)	(0.010)	(50.290)	(46.936)
Observations	63,603	61,597	65,321	65,321
R^2	0.669	0.525	0.293	0.308
City FE	YES	YES	YES	YES
year FE	YES	YES	YES	YES
cluster(city)	61	61	61	61
mean of dep var	-0.598	-6.565	1295	1583

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table presents the effects of conducting the pilot program at the whole city level. The key variable of interest is *pilot * post*, which captures the city-level treatment effect in the post-implementation period. The dependent variables are log housing prices, log rents, number of rental units, and number of sales units. All specifications include city fixed effects and year fixed effects. Standard errors are clustered at the city level. This regression uses the part of Xitai data set that contains county level information and covers 61 cities.This regression uses a broader dataset covering 61 cities.

Table 14: Treatment Intensity Effects on Housing Market Activity

	Binary Controls		Continuous Controls	
	(1)	(2)	(3)	(4)
VARIABLES	# sales unit	# rental unit	# sales unit	# rental unit
pilot * fraction * post	-52.671**	-13.776	1.140	-7.544
	(21.932)	(19.949)	(11.891)	(9.459)
Constant	22.965***	11.923***	26.641***	18.287***
	(2.340)	(2.895)	(1.278)	(1.084)
Observations	164,463	164,463	164,463	164,463
R^2	0.483	0.597	0.482	0.595
Apartment feature controls	Binary	Binary	Continuous	Continuous
apartment complex FE	YES	YES	YES	YES
city-year FE	YES	YES	YES	YES
cluster	141	141	141	141
mean of dep var	24.23	16.09	24.23	16.09

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table presents the treatment intensity effects of the pilot program on housing market activity across two specifications. The key variable of interest is *pilot * fraction * post*, where *fraction* represents the fraction of counties being listed as pilots within a city where a county c is located, serving as a measure for treatment intensity. Columns (1)-(2) include binary apartment feature controls (above/below mean indicators), and columns (3)-(4) use continuous standardized apartment feature controls. The apartment features include FAR (floor area ratio), PUR (parking lots per unit), last construction year, indicator for having villas or townhouses in the complex, fraction of one bedroom units among all units on sale, fraction of one bedroom units for rental among all rental units, green space area. The dependent variables are log housing prices and log rents. All specifications include apartment complex fixed effects and city-year fixed effects. Standard errors are clustered at the county level.

Table 15: Heterogeneous Effects with Binary Features

	(1)	(2)	(3)	(4)
VARIABLES	ln(price)	ln(rent)	# sales unit	# rental unit
B.1. Basic pilot effect (above mean - below mean)				
Difference	-0.130**	-0.157*	34.407**	6.058
	(0.065)	(0.084)	(15.310)	(15.846)
B.2. Intensity effect, fraction = 1 (above mean - below mean)				
Difference	-0.332**	-0.292	109.485**	29.251
	(0.156)	(0.288)	(48.615)	(39.570)
B.3. Intensity effect, fraction = 0.5 (above mean - below mean)				
Difference	-0.166**	-0.146	54.742**	14.626
	(0.078)	(0.144)	(24.307)	(19.785)
B.4. Intensity effect, fraction = 0.09 (above mean - below mean)				
Difference	-0.030**	-0.027	9.953**	2.659
	(0.014)	(0.026)	(4.420)	(3.597)
Observations	149,339	122,122	164,463	164,463

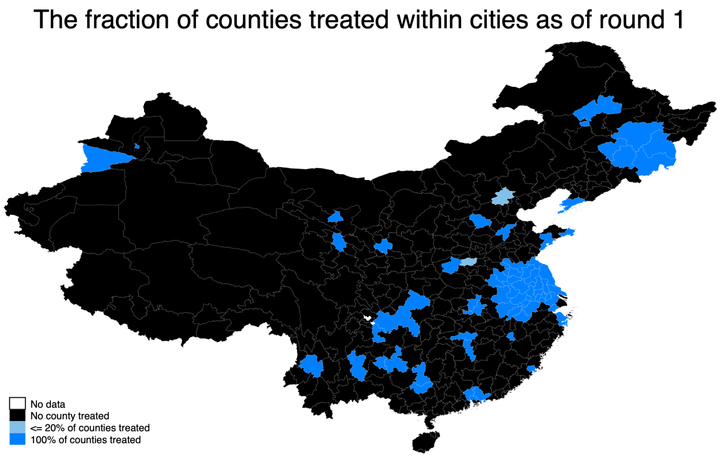
*** p<0.01, ** p<0.05, * p<0.1

Note: This table presents heterogeneous effects using binary apartment feature controls (above/below mean indicators). Standard errors in parentheses. The comparison is between apartments with above-mean versus below-mean characteristics across all relevant features (green space, FAR, PUR, construction year, townhouse/villa presence, and one bedroom unit fractions). Row B.1 shows the basic pilot effect from the main specification with binary controls. Rows B.2-B.4 show intensity effects at different treatment fractions (1.0, 0.5, and 0.09 respectively) comparing above-mean to below-mean apartment characteristics.

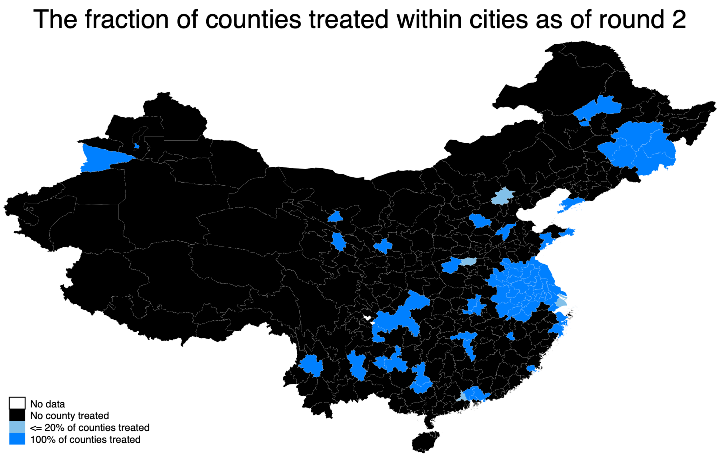
Table 16: Land transactions for residential purposes involve larger volumes after 2015

VARIABLES	(1) construction area	(2) building area
post	-1,173	-28,296
	(9,877)	(23,184)
pop0.5-1 ∗ post	21,399*	74,225***
	(11,819)	(28,042)
pop1-3 ∗ post	10,884	37,752
	(11,044)	(25,927)
pop3-5 ∗ post	4,638	23,157
	(11,963)	(28,080)
pop5above∗ post	-3,930	-29,626
	(12,494)	(29,327)
Constant	29,365***	69,866***
	(1,827)	(4,341)
City fixed effects	Yes	Yes
Observations	1,456	1,445
R-squared	0.294	0.315

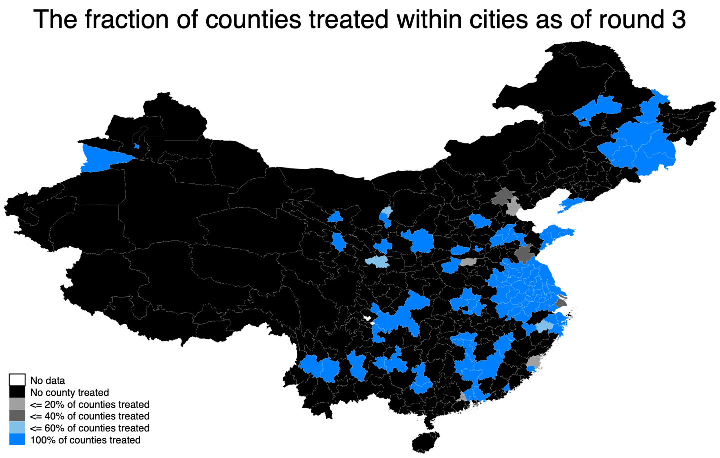
Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. pop0.5-1, pop1-3, pop3-5, pop5above stands for urban population between 0.5 million and 1 million, between 1 million and 3 million, between 3 million and 5 million and above 5 million, respectively. The base category are cities with urban population below 0.5 million.



(a) The first round of pilots



(b) The second round of pilots



(c) The third round of pilots

Figure 5: The geographical distribution of pilots

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