DOI: 10.1111/1540-6229.12452

ORIGINAL ARTICLE

WILEY

The heterogeneous impact of house purchase limits policy on housing prices: Comparison between elite and non-elite school district houses

Yongbin Huang | **Hai Hong**

School of Economics, Anhui University, Hefei, Anhui, China

Correspondence
Huang Yongbin, Anhui University, Qingyuan Campus, 111 Jiulong Road, Hefei 230601, Anhui, China.
Email: nomand1122@hotmail.com

Abstract

The effectiveness of housing purchase limits policies has aroused heated debate, yet few discussed its impact on educational capitalization. We examine the heterogeneous effect of housing purchase limits policy on the price of elite school district houses (ESDH) and non-elite school district houses. By exploiting second-hand houses data in Hefei China, we find that the price of ESDH has increased greatly after the limitation, compared with non-elite school districts. Further we discover that the ESDH have lower depreciation risks, and their price is higher in neighborhoods with smaller dwelling area. Our finding indicates that the limits policy may have exacerbated the educational capitalization.

KEYWORDS

housing purchase restrictions, elite school district housing, educational capitalization

1 | INTRODUCTION

The effectiveness of the housing purchase limits policy (hereafter, the limits policy or limitations) in China has long been a controversial topic. Massive studies have discussed its impact on housing prices with the cross-municipal data (Hui & Wang, 2014; Li et al., 2017; Lu et al., 2021; Wu & Li, 2018; Yan et al., 2022). Yet few, if any, shed light on its heterogeneous impact with the neighborhood data. Families who purchase houses take the neighborhood environment, the location, the education quality, into accountd. Among these attributes, education quality is of great importance. Scholars have pointed out that the education quality is capitalized into housing prices (Oates, 1969; Black, 1999; Fack & Grenet, 2010; Chan et al., 2020), but few examined the effect of limits policy on educational capitalization. In this article, we explore the unintended effect of the limits policy on the price of elite school district houses (ESDH) and non-elite school district houses (NSDH).

Education quality of school district is regarded as one of the most important factors affecting the housing price. As Oates (1969) used the Tiebout model to study the effect of educational resources on housing prices, numerous studies have found that families tend to buy houses in school districts with better educational resources, promoting housing prices in the neighborhoods (Black, 1999; Fack & Grenet, 2010; Peng et al., 2021; Collins & Kaplan, 2022). Traditional Confucian culture values education, and parents expect their children to get ahead when they are young. As purchasing ESDH are one of the few ways to get access to prestigious education resources, the limits policy may not effectively inhibit the price rise of ESDH. Three reasons could stand for such argument. First, the demand elasticity for ESDH is relatively lower, and such demand may not be changed by the limits policy. Second, the speculative demand has been generally curbed by limits policy (Sun et al., 2017; Li et al., 2017), inducing potential risks of devaluation on estate market. However, the price of ESDH is relevant to education quality, which may survive from devaluation risks. Third, the limitations may result in changes of buyers' purchasing strategies. Affordable small-sized ESDH may become more popular, as buyers can run a full payment to avoid liquidity constraints imposed by the limitations.

Based on the tracking data of secondhand houses from 2016 to 2020 in Hefei City, we examine the impact of the limits policy on the price of ESDH and NSDH using a difference-in-differences (DID) approach. We define the headquarter schools with branches as the elite schools and compares the differences in the price of ESDH and NSDH, before and after the limits policy. Our results of by-period specification indicate that there is no significant pre-trend in price changes between ESDH and NSDH prior to the implementation of limits policy. Compared to NSDH, the price of ESDH grows more rapidly after the limitations. We identified a 4.8% increase in price of ESDH after the limits policy, which is roughly equivalent to 54,546 RMB increase in total price, a figure that as twice as the per capita disposable income in 2021. Our DID identification remains significant after a series of robustness and placebo tests, such as including additional fixed effects, substituting the definition of elite schools, excluding school federations, coarsened exact matching (CEM) method, using housing quality as a confounder and rents as a placebo.

Our mechanism analysis shows that the transactions of ESDH have lower deprecation risks, which may be the results of rigid educational investment demand. Besides, we found that affordable small-sized ESDH become more popular in post-policy periods, despite their shabby and unlivable environment. It implies that families may prefer luxury shabby houses at the expense of living conditions to gain access to good quality education. Our finding suggests that the housing purchase limits policy could not curb educational investment demand and may even exacerbate the price differentiation between ESDH and NSDH, deteriorating education equity to some extent.

We contribute to the studies of educational capitalization of elite schools. Economists have discovered that the enrollment qualification attached to elite schools can bring about additional premium, which leads to the price differentiation between ESDH and NSDH (Oates, 1969; Black, 1999; Fack & Grenet, 2010; Collins & Kaplan, 2022). Previous education economics studies have exerted efforts on the impact of educational policies on housing prices, such as interdistrict choice programs (Brunner et al., 2012; Avery & Pathak, 2021), school district designation (Huang et al., 2020), admission policy (Zhu et al., 2023), and school quality disclosure (Figlio & Lucas, 2004; Fiva & Kirkebøen, 2011; Haisken-Denew et al., 2018; Kuroda, 2022). We discuss the issue from the perspective of regulatory policies, which shed new light on how regulatory policy exacerbates educational capitalization.

Our article also contributes to the literature of the limits policies. Our contribution is twofold. First, although many previous quasi-experimental designs investigate the effects of limits policy on housing prices (Hui & Wang, 2014; Li et al., 2017; Wu & Li, 2018; Lu et al., 2021), these plausibly exogeneous limitations might suffer from the threat of endogeneity, because limitations are more likely be triggered in cities with higher housing price increase. In contrast, by comparing the heterogeneous effect of limits policy on ESDH and NSDH, our specification implies that the status of adoption is irrelevant to the implementation of the policy, as it is not aimed explicitly for ESDH. Therefore, the endogenous problem in our setting is relatively mild, and we can simply impose a weaker assumption that there ought to be no anticipatory effect. Second, we exploit a novel neighborhood-level data to study the impact of the limits policy within a specific city to exclude confounding factors. Numerous studies have evaluated the effect of limits policy with the cross-municipal data (Hui & Wang, 2014; Li et al., 2017; Wu & Li, 2018; Lu et al., 2021; Yan et al., 2022), which may be challenged by unobserved heterogeneity. The limits policies are varied across cities, for example, targeting investors differences. These limitations prevented buyers, depending on their residency status (Hukou), from purchasing either a second or third property (Somerville et al., 2020). In this regard, our work is mostly related to Sun et al. (2017), Jia et al. (2018), and Li et al. (2020). They explore these effects with micro level data of a specific city. In our setting, we focus on the heterogeneous effect of the limitations with respect to housing education attributes.

The remainder of this article is organized as follows. Section 2 briefly reviews the relevant institutional background. In Section 3, we introduce our dataset and identification strategy. Section 4 reports the baseline results of the impact of the limits policy on the housing price, and the mechanism of how the limitations changed the housing purchasing strategies. Section 5 concludes.

2 | BACKGROUND

2.1 | Housing purchase limits policies

China real estate markets have developed rapid since housing markets reform in 1998. By 2010, the housing prices in 35 large and medium-sized cities increased by an annual average of 9.7%. 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 about housing affordability, the Chinese central government was worried about asset price bubbles. The State Council issued the Ten National Rules (effective on April 17, 2010) that first explicitly proposed limitations on housing purchasing. In 2014, many cities have deregulated the limitations, and housing prices surged again. In 2016, Beijing unexpectedly proposed the limits policy before the National Days. Within 10 days, Hefei, Suzhou, Hangzhou, and other cities urgently launched a new round of housing purchase limitation policy.

The housing purchase limits policy come into effect through two ways. The first is macro-prudential directives that affect mortgage interest rates. Researchers discovered that the directives, including loan-to-value policies and down payment ratios, impose liquidity constraints to households and force them to adjust their credit levels, thereby curbing the rise of housing prices

(Armstrong et al., 2019; Laufer & Tzur-llan, 2021; Acharya et al., 2022). The second is purchase quantity limitations that limit the number of properties an individual could purchase (Somerville et al., 2020). Studies indicated that the limitations would cause resources misallocation among consumers (Glaeser & Luttmer, 2003; Lu et al., 2021). Some reckon that the limits policy can inhibit the housing prices (Du & Zhang, 2015; Li et al., 2017; Wu & Li, 2018; Yan et al., 2022), whereas others believe that the impact only exists in the short run (Jia et al., 2018; Chen et al., 2018; Somerville et al., 2020).

On October 2, 2016, the Hefei Municipal Government issued a purchasing limits policy. As for the quantity limits, the policy stipulates that nonlocal buyers with the certification of social insurance payment for 1 year are allowed to purchase one unit, whereas locals can buy an additional unit. As for the loan limits, the minimum down payment ratio for the first unit is 30%, it raises to 40% for the second unit if previous loan has been closed, and 50% if loan has not yet been closed. Households need to run full payment if their previous loans are not closed, whereas they attempt to purchase a third unit. The limits policies launched in Hefei are similar to most of the other cities, which makes it a good representative to evaluate the policy effects.

2.2 | Compulsory education in Hefei

China's nationwide Compulsory Education Law was officially gone into effect on July 1, 1986. The law had several important features (China Ministry of Education 1986). First, 9 years of education became compulsory. Second, all children who have reached the age of 6 shall enroll in schools and receive their compulsory education. In this way, children were supposed to complete their compulsory education when they reached 15 years old, including primary and secondary education. Third, the children are enrolled in schools that should be adjacent to their home. Fourth, education responsibility was decentralized to lower levels of governments. Therefore, local government delineates the attendance zones of schools.

Hefei has implemented proximity enrollment policy from 1986. Since 2011, the authority requires the domicile address of children should be the same as the address of their legal guardian's real estate certificate. The policy implies households need to purchase a house to be eligible for admission, and different neighborhoods imply different school and education qualities. The admission consists of both the elementary school and the secondary school admission. In 2016, Hefei abolished the entry threshold for key high schools, and key high school admission quotas of secondary schools were allocated more equally. It greatly incentivized household to purchase school district houses from elementary school onward, in order to enroll prestigious schools.

Hefei's prestigious school should be traced back to the key elementary schools listing published by the Anhui Educational Bureau in the 1970s. Such schools mainly locate in the oldest town. As the city expands rapidly, the suburban areas agglomerate newcomers, where schools are insufficient. The vast gap of education quality between the old town and suburban area makes the municipal government relocate to the key elementary schools toward suburban area or shut down the key elementary schools in the old town. For instance, the prestigious Suzhou Road elementary school was shut down in 2000s, and Yonghong Road elementary school was relocated to the suburban area. Thus, the antique key elementary schools listing may lose reputation to attract children enroll.

Besides the relocation of key elementary schools, a more common approach is to set up branches of existing schools. These existing schools may locate in the old town or the suburban areas. In old town, Nanmen elementary school, Tunxi Road elementary school, Normal elementary school, Heping Road elementary school, and so on established branch schools in suburban area around 2010. However, in suburban areas, some good schools were also establishing branch schools, such as Wanghu elementary school, Shuguang elementary school, Weigang elementary school, Anjuyuan elementary school, Mengyuan elementary school, and so on.

By September 2020, there were 225 elementary schools within the jurisdiction of Hefei, including 11 private elementary schools and 214 public elementary schools. In public schools, there are 34 headquarter schools with branch schools, accounting for 15.87% of all elementary school. We defined the headquarter schools with branches as the ESDH in our specification, as will be discussed in the following section.

3 | DATA AND IDENTIFICATION

3.1 | Data

The data are mostly obtained from *Anjuke* website, one of the biggest real estate agent websites in China. As most of the elite schools locate in the old town or built suburban area, and few neighborhoods are newly constructed, secondhand houses are more suitable for analysis. We crawled the data in March 2016, June 2016, September 2016, and December 2016. From 2017 to 2020, we crawled the September data annually.[1] The raw data are cross-sectional, we use the neighborhood's name to match for each cross section and construct a balanced panel data with 1852 neighborhoods of 8 time period.

We first evaluate the representativeness of our sample. Figure 1 compares the housing price's change rates of our sample and the NBS monthly data. The two lines share a consistent, and nearly identical time trend: They both grow rapidly between June and September in 2016 and followed by a sharp reduction after the limitation. The parsimonious comparisons imply that our data have a representativeness.

In addition to the housing price of the neighborhoods (*price*), we also crawl the median rental price (*rent*), and several physical attributes of the neighborhood, such as the built year of the neighborhoods (*build_year*), the average greening rate (*green*), the average floor area ratio (*floor_ratio*), and the average property price (*property*). We use ArcGIS to calculate locational attributes of neighborhoods and obtain the distance to the nearest metro station (*metro_dis*), to tertiary hospital (*hospi_dis*), to landscape park (*park_dis*), and the distance to the city center (*CBD_dis*). We also calculate the number of private schools within 5 km of each neighborhood (*priv_num*). Most of the variables are time-invariant, we thus use a more flexible specification in baseline regressions, which interacted these variables with the time fixed effect.

Our explanatory variable is the housing price for ESDH and NSDH. Identifying elite school districts is an important part in the research of school district housing. Previous studies use student

[1] We have crawled September data from 2017 to 2020, and our data may have nonrandom sampling problems. September is the traditional sale season for China real estate market, and NSDH will have more transactions this month. In contrast, ESDH transactions are concentrated in the first half of the year, and prices stabilize after students enter school in September. Therefore, the price difference between ESDH and NESDH will narrow in September; thus, estimation will not bias upwardly. In addition, we further obtained the monthly price ratio of Hefei secondhand houses based on December 2015 from China National Bureau of Statistics in January 2016 and December 2020. We find that consistent with other months, house prices of September maintain a stable upward trend (Figure A1), indicating that our sampling is not seriously biased.

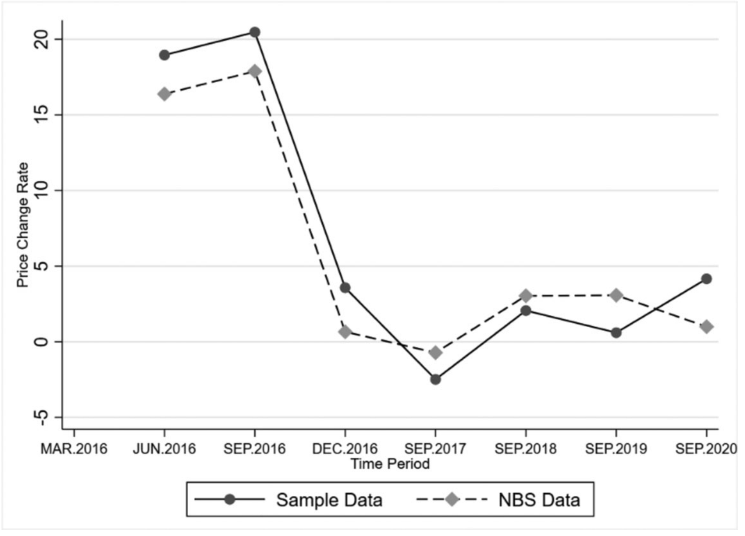


FIGURE 1 Comparison between sample data and NBS data. *Notes*: The NBS (China National Bureau of Statistics) data are collected from the average change rates of housing price in 70 major cities

grades to evaluate the quality of school education (Black, 1999; Fack & Grenet, 2010; Andrabi et al., 2017; Barrera-Osorio et al., 2020). Although students' grades are not publicly released in China, studies mostly use historical reputation or the parents questionnaires as a criterion for elite schools (Wen et al., 2017; Huang et al., 2020; Peng et al., 2021). However, such measures may lead to sample selection bias. First, the antique key elementary schools listing may lose reputation to attract children enroll and may ignore newly developed schools. Second, the questionnaires or comments for parents on school quality depend on the sampling efficiency. Good grades students' parents may comment positive on school's quality, while the bad comment negatively. Since 2010, Hefei established the branch schools to supply the high-quality educational resources. We thus define that the headquarter elementary schools with branches built as elite schools. There are 34 headquarter schools as elite schools. The rest, either the branch schools or other elementary schools, are defined as non-elite schools.

The Hefei Education Bureau announces the annual Compulsory Education Enrollment Program at later June. The program delineates school district, and the corresponding neighborhoods enroll respective schools. We drew school district boundaries on ArcGIS software and overlaid the boundary information with the address of each neighborhood. We then cross-validate the information with the crawled data of neighborhood's attendance school. Finally, we divided the sample into two types: ESDH and NSDH. There were no cases of NSDH turning into ESDH, either no cases of ESDH back into NSDH in the sample period. There are no elite school district boundaries changes due to openings or relocations of schools in the sample, so the school location is predetermined. Moreover, the increase supply of education resources, such as opening and relocation of schools, may reduce the housing premiums of ESDH (Sun & Lin, 2020), which will not induce upward bias in our specification.

Table 1 provides descriptive statistics of ESDH and NSDH. We separately calculate the difference in characteristics between ESDH and NSDH, before and after the limitation. The simple comparison reveals several patterns of our data. First, the average price and change rates of ESDH are higher. The average price of ESDH increased 28.4% after the limits policy, whereas NSDH

TABLE 1 Descriptive statistics of variables by school districts.

	Before limitations			After limitations		
	ESDH	NSDH	Mean Diff	ESDH	NSDH	Mean diff
Price	13,629.6	11,731.1	1888.252***	17,500.7	14,328.4	3462.527***
Rent	2013.85	1646.67	367.183***	2096.29	1529.41	566.882***
Property	0.480	0.680	−0.201***	0.490	0.680	−0.195***
floor_ratio	2.530	2.340	0.190***	2.530	2.340	0.190***
build_year	2001	2004	−3.587***	2001	2004	−3.476***
Green	0.590	0.530	0.0680	0.650	0.510	0.134*
metro_dis	3.040	3.950	−0.915***	1.240	2.090	−0.853***
park_dis	0.950	1.320	−0.369***	0.950	1.330	−0.377***
hospi_dis	1.320	2.010	−0.685***	1.350	2.010	−0.666***
priv_num	1.770	1.660	0.114***	1.730	1.670	−0.058**
CBD_dis	3.420	5.510	−2.093***	3.460	5.540	−2.085***
Observations	1077	4479	–	1927	7333	–

Abbreviations: ESDH, elite school district houses; NSDH, non-elite school district houses.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

increases 22.1%. Second, ESDH have location advantages. They are closer to public goods than NSDH, such as metro stations, parks, and hospitals. Third, ESDH are generally older. They built around 2001, which is 3 years older than NSDH. Fourth, the rents of ESDH are slightly higher than NSDH, which may due to their location advantages. The discrepancies in housing characteristics between ESDH and NSDH are distinct. By including these variables, our specification allows us to compare the neighborhoods with similar housing attributes, which can provide more convincing estimates.

3.2 | Identification strategy

Our main specification strategy follows the standard DID design, which compare the relative change in the housing prices before and after the limitations, between ESDH and NSDH. The difference between our specification and the standard DD strategy is that both types of houses are affected by the limitations. Moreover, once the intensity and the direction of the impact varied, we can evaluate the heterogeneous impact of the limitations on the price of ESDH and NSDH. Such empirical design should be examined cautiously to exclude confounding factors. We therefore exploit a boundary discontinuity regression design to detect the comparability of different housing types.

3.2.1 | Boundary discontinuity regression

Economists have traditionally relied on hedonic models to estimate the impact of school performance on housing prices. Black (1999) pointed that the traditional hedonic model will produce upwardly biased estimates if the error term includes unobservable neighborhood attributes that are correlated with school's education quality and have an independent effect on housing prices. To circumvent the problem, Black's estimation strategy consists in focusing exclusively on the set of neighborhoods that locates in the vicinity of a school district boundary.

Although Black solved the endogeneity problems that undermine the traditional hedonic model, her school district boundary fixed effect specifications embodied several restrictions. First, the unobserved attributes of neighborhood related to housing prices are restricted to be uncorrelated across common school district boundaries. In other words, the neighborhoods across the boundaries are randomly assigned, the school district delineation would only affect the housing prices and has no impact on other explanatory variables. Second, the comparison of neighborhoods located on both sides of a common school district boundary does not take the distance from boundaries into account.

To overcome the restrictions of boundary fixed effect model, we adopt a boundary discontinuity design that restricts the neighborhood comparison within specific vicinity from boundaries. The model is defined as follows:

$$lnprice_{ijt} = \beta kschool_{ij} + \sum_t X_i' \lambda_t \gamma + \delta boundary_{ij} + \sum_t \lambda_t + \sum_j \phi_j + \varepsilon_{ijt} \tag{1}$$

where $lnprice_{ijt}$ is the natural logarithm of the average transaction prices of secondhand houses in neighborhood i of school district j in time period t. $kschool_{ij}$ is a dummy variable that equals 1 if school j that neighborhood i enrolls is the elite school; otherwise, it takes the value of 0. $\sum_t X_i' \lambda_t$ is the interaction term of control variables and time fixed effect. $\sum_t \lambda_t$ and $\sum_j \phi_j$ are time fixed effects and school fixed effect. ε_{ijt} denotes the error term. The $boundary_{ij}$ in model (1) is a quadratic polynomial, which represent the distance of a neighborhood i to the boundary of its enrolling school district j. Neighborhood centroids are used to locate neighborhoods, and these centroids were generated by Baidu map API.

Here we use the rental data, in replace of the housing price, as a placebo to exclude unobserved confounding factors. In China, only buyers can enjoy the school enrollment qualification, for which tenants are excluded from. The rental price is uncorrelated with school enrollment and can reflect unobserved physical characteristics of neighborhood across the boundaries. If the neighborhoods across the boundaries are randomly assigned, the rents should vary little at the boundary threshold. Model (1) is applied to verify whether the price and rents would vary significantly at the boundaries of 300 and 500 m bandwidths.

3.2.2 | Difference-in-differences

The unprecedented implementation of the limits policy is not expected by house buyers, and the policy is not intended to curb the transactions of ESDH, which provides evidence that the introduction of the limitations is rather exogeneous. We evaluate the impact of the limits policy on the price of school district houses with a DID approach, and the specific model setting is as follows:

$$lnprice_{ijt} = \beta kschool_{ij} \times restric_t + \sum_t X_i' I_t \gamma + \delta boundary_{ij} + \sum_t I_t + \sum_j \phi_j + \varepsilon_{ijt} \tag{2}$$

The variable of interest is the interaction term $kschool_{ij} \times restic_t$, where $kschool_{ij}$ is a dummy variable indicating the elite schools. $restric_t$ is also a dummy variable which takes the value of 1 after the policy implementation (i.e., after September 2016). We also include the variable $boundary_{ij}$ in Equation (2), which would effectively restrict the comparison within the neighborhoods that have the same distance to the school district boundaries, between ESDH and NSDH. In all specifications, standard errors are clustered at the school level. Other variables share the same definition with model (1). The coefficient of interest in Equation (2) is β, which captures the relative treatment effect of limits policy on the price of ESDH.

We can also examine the link between the limits policy and price of school district houses period by period. In this way, we can examine whether there were already different trends for ESDH and NSDH before the limits policy. The specification is as follows:

$$lnprice_{ijt} = \sum_t \beta_t kschool_{ij} \times I_t + \sum_t X_i' I_t \gamma + \delta boundary_{ij} + \sum_t I_t + \sum_j \phi_j + \varepsilon_{ijt} \quad (3)$$

All variables in Equation (3) share the same definition with Equation (2), the only difference between these two specifications is that we interact $kschool_{ij}$ with the time fixed effect, rather than the post-policy indicator $restirc_t$. The period of March in 2016 is left as a comparison.

4 | EMPIRICAL RESULTS

4.1 | Identification of boundary discontinuity

We first use the boundary discontinuity regression to compare the differences in rents and housing prices within 300 and 500 m bandwidth across the school district boundaries to inspect for unobserved confounding factors. For the boundary discontinuity estimation, the regression discontinuity plots can give more intuitive comparison. In Figure 2, parts (a) and (b) plot the housing prices and rents across the school district boundaries within 300 m bandwidth, whereas parts (c) and (d) plot the housing prices and rents across the school district boundaries within 500 m bandwidth. Reassuringly, we find no evidence that the rent varies distinctly at the boundaries of 300 or 500 m bandwidth, which implies that the houses' physical characteristics across the boundaries should be identical. However, the prices leap significantly at the boundaries, as shown in Figure 2b,d, implying that the NSDH within the boundary bandwidth could be served as an appropriate counterfactual for ESDH.To exclude potential sparse observations influence, we conduct sample winsorsize at 5% percent and rerun the RD specification(Figure A2), which implies our RD results are robust.

4.2 | Baseline regression

We now estimate our main estimating Equation (2), reported in Table 2. We report the results of full sample, sample within bandwidth of 300 and 500 m. The table reports two specification for each sample. The first specification, which we only control for the time and school fixed effect, is reported in columns (1), (3), and (5). The second specification, which we add full set of controls for housing characteristics and the distance to the boundary, is reported in columns (2), (4), and (6).

The estimates of Equation (2) reveal a significant positive effect of the limits policy on the ESDH. According to our estimate in column (2), compared to NSDH, the introduction of the limits policy increases the price of ESDH by 4.8% relatively. Even in our most stringent specification in

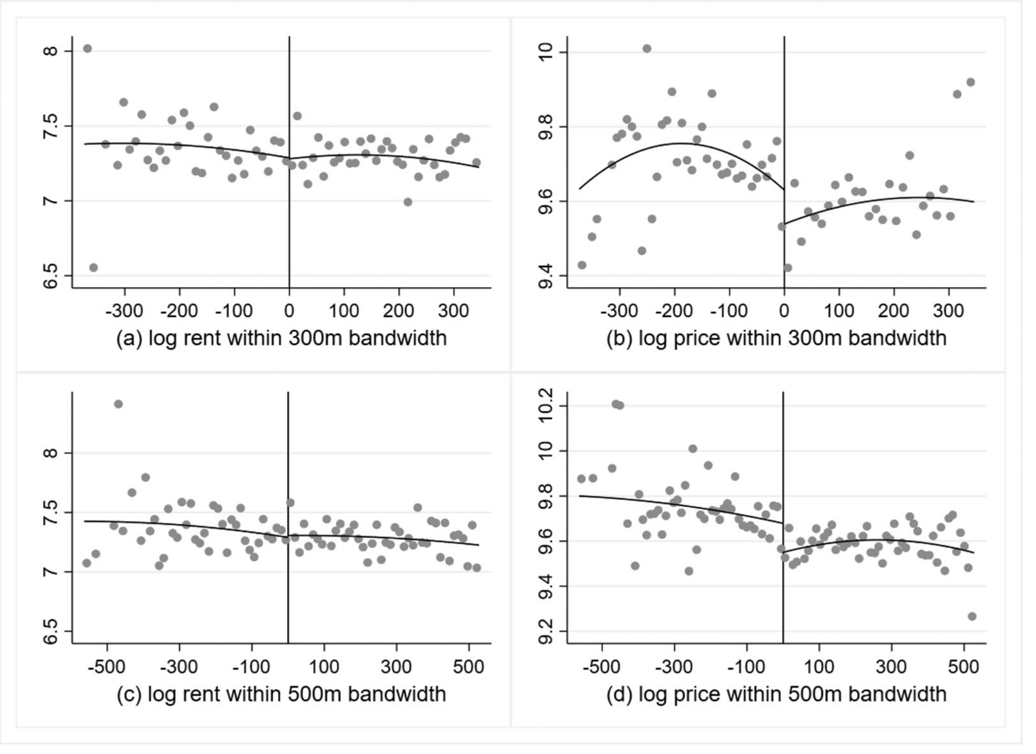


FIGURE 2 Plots of rent and price against boundary. *Notes*: The figure depicts the single dimension RD graphs. The *x*-axis denotes the distance against the elite school district boundaries, where negative number refers to the treat group (elite school district houses [ESDH]). The markers and capped spikes represent the estimators and 95% confidence intervals. Parts (a) and (b) plot the housing prices and rents across the school district boundaries within 300 m bandwidth, whereas parts (c) and (d) plot the housing prices and rents across the school district boundaries within 500 m bandwidth. The observations of log rent and log price within 300 m (or 500 m) bandwidth are 2730 (3664) and 4753 (6328)

TABLE 2 Baseline estimates.

Dependent variable: lnprice	(1)	(2)	(3)	(4)	(5)	(6)
Sample	**Full sample**	**Full sample**	**300 m**	**300 m**	**500 m**	**500 m**
kschool × *restric*	0.051***	0.048***	0.040***	0.028**	0.046***	0.034**
	(0.016)	(0.015)	(0.015)	(0.012)	(0.016)	(0.013)
Baseline controls	No	Yes	No	Yes	No	Yes
(Interacted with time FE)						
Boundary	No	Yes	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,816	14,816	5432	5432	7232	7232
R^2	0.667	0.703	0.642	0.682	0.649	0.686

Note: Standard errors in parentheses are clustered at the school level.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

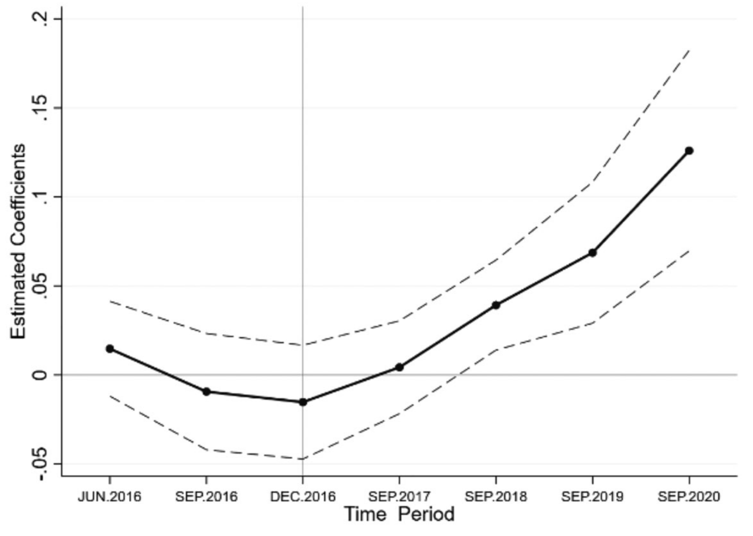


FIGURE 3 Dynamic effects of the limits policy on housing prices of elite school district houses (ESDH) and non-elite school district houses (NSDH). *Notes*: The figure depicts the estimated coefficients of the by-period specification and their 95% confidence intervals of full sample with control variables

column (4), which only applies the variation of the boundary threshold within 300 m bandwidth, the limits policy still leads to an increase in the price of ESDH by 2.8%. To illustrate the magnitude of our estimates, the average of ESDH and NSDH is given 15,783 and 14,024 RMB in September 2016. For conciseness, if we assume that the NSDH price remains unchanged, the estimates of Table 2 would indicate ESDH price increases ranging from 442 to 805 RMB. Given the average housing area of ESDH is 72 m^2, the limits policy would cause total housing prices increase ranging from 31,824 to 57,960 RMB, whereas Hefei's per capita disposable income in 2021 is only 28,200 RMB.

Based on Nunn and Qian (2011), we further perform a simple calculation that measures how the relative increase in log price of ESDH between 2017 and 2020 can be explained by the introduction of the limits policy. According to our data, the average price of ESDH increased 12.6% relatively. Using the baseline estimates reported in column (2) of Table 2, the counterfactual of relative increase would have been 62% ([12.6–4.8]/12.6) of the observed increase if the limits policy had not been introduced. In other words, the introduction of the limits policy explains 38% of the observed increase in price differentiation.

4.3 | By-period specification

One of the threats of our identification strategy is the anticipatory effect. If buyers are aware of the arrival of limits policy prior to its implementation, then our estimates will be biased by such anticipation. We construct Figure 3 to identify such threat, where we plot the point estimates(Table A1) from Equation (3) and their 95% confidence intervals.

It shows that the trends in the price of ESDH and NSDH are nearly identical before the implementation, which indicates no anticipatory effect. Even if there is, the anticipatory effect would only bias our estimated results downward. None of the estimated coefficients before the limits policy are significant, and all of them are close to 0. This result suggests that the increasing educational capitalization probably occurred due to the implementation of the limits policy rather than systematic indigenous differences between neighborhoods.

Recent econometrics literature suggests that the power of pretest is too low to detect the potential violation of the parallel trend assumption (Roth, 2022), and such problems would exacerbate in research designs that have fewer pretreatment periods. Our research design only has three pretreatment periods, and the parallel trend assumption may possibly be violated even if the pretest detects no substantial violation in the pre-trend. For instance, the price of ESDH grows faster than NSDH in posttreatment periods, whereas the limits policy exerts no effect on price differentiation, then the baseline estimation may be biased upwardly. We adopt two approaches to diagnose potential violations of parallel trends.

First, we apply the changes-in-changes (CIC) methods developed by Athey and Imbens (2006) to estimate the quantile treatment effects nonparametrically. CIC allows the two groups to differ in the distribution of unobservable characteristics in arbitrary ways as long as the distribution is time-invariant within each group. Therefore, CIC can overcome the potential violation of parallel trend assumption, which cannot be specified in the canonical DID designs. We follow the two-step procedure suggested by Athey and Imbens (2006) to estimate the distribution of the treatment effect. We display the estimated quantile treatment effect and the bootstrapped standard errors in Figure A3. We find that the limits policy exhibits a significant positive effect on the educational capitalization. This finding is consistent with our standard DID estimation results.

Second, we employ the synthetic differences-in-differences (SDID) (Clarke et al., 2023; Arkhangelsky et al., 2021) estimator to reestimate the baseline results. Compared with the canonical DID model, SDID calculates optimal weights that balance the pre-trends between the ESDH and NSDH, and trends before and after the limits policy within the ESDH; thus, we can reduce the potential bias and improve the precision of the baseline estimations. We present the estimates in Figure A4. Again, the pattern revealed by the synthetic-DID is consistent with the baseline estimation. After the limitation, the price of NSDH remains virtually unchanged, whereas the price of ESDH remains higher and shows a slow upward trend. It is this segregated trend that leads to enlarged educational capitalization.

Taken together, we highlight that both methods represent a generalization of the standard DID method by flexibly allowing for systematic differences between the ESDH and NSDH. The results consistently show that our findings are not subject to the potential violations of parallel trends.

4.4 | Robustness

4.4.1 | Inclusion of additional fixed effects

Hefei City governs seven districts, each have different policies for school district houses. Moreover, our estimated effect may be biased by the district-specific factors. To address such concerns, we include school district fixed effect (in replace of the single school fixed effect), district-time fixed effect, and school-time fixed effect (in replace of the single time fixed effect) in our baseline specification. The inclusion of these fixed effects can rule out the unobserved variation in housing price by any school or at any locality, the impact of district-specific policies, and unobserved trend in schools' educational quality. To further corroborate our estimations, we only include samples within the 300 m bandwidth across the boundaries, which is the most stringent specification. The estimated results are reported in Table 3, columns (1) and (2). As shown in the table, our results remain robust to the inclusion of additional fixed effects. In both specifications, the esti-

TABLE 3 Summary of robustness checks I.

Dep. Var.: lnprice	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Sample: 300 m	**Additional fixed effects**		**Alternative definition**		**Exclude school federation**	
kschool × *restric*	0.028**	0.023**			0.101***	0.090*
	(0.013)	(0.012)			(0.009)	(0.050)
reputation × *restric*			0.051***	0.039**		
			(0.018)	(0.016)		
Baseline controls (Interacted with time FE)	Yes	Yes	No	Yes	No	Yes
Boundary	Yes	Yes	No	Yes	No	Yes
Time fixed effect	–	–	Yes	Yes	Yes	Yes
School fixed effect	–	–	Yes	Yes	Yes	Yes
School district fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
District-time fixed effect	Yes	–	–	–	–	–
School-time fixed effect	–	Yes	–	–	–	–
Observations	5432	5432	7232	7232	1600	1600
R^2	0.688	0.695	0.695	0.702	0.597	0.669

Note: Standard errors in parentheses are clustered at the school level.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

mated coefficients remain significant at 5% level, indicating that our baseline specification is not confounded by the unobserved factors in district-time, school district, and school-time level.

4.4.2 | Alternative definition for the elite schools

In our baseline specification, we define headquarter schools with branches as elite schools. Such definition is distinct from previous studies, which commonly use the definition of reputational schools. To verify the robustness of our research, we replace the variable *kschool* in our baseline specification with a dummy variable *reputation*, which indicates the reputational schools.[2] The structural of the specification is nearly identical to Equation (2). The only difference is that the estimated coefficient β now represent the relative price change for reputational schools aroused by the limits policy. The estimated results are reported in Table 3, columns (3) and (4). The point estimates for *reputaion* × *restric* remain stable, suggesting that our estimated impact of the limits policy is robust to an alternative definition of elite school. However, we find that the estimated coefficients are slightly higher than baseline specifications. It may be due to the fact that nearly half of the headquarter schools with branches are reputational schools. In other word, half of the headquarter schools we identified as elite schools are not reputational schools, thus our baseline estimations actually capture the lower-bound impact of the limits policy.

[2] The reputational schools are defined according to the document released by Department of Education of Anhui province in 1970s, which specifies the list of key schools in Hefei City. We exploit the document and define the key schools in 1970s as the reputational schools in present.

TABLE 4 Summary of robustness checks II.

Dep. Var.: lnprice/lnrent Sample: 300 m	(1) Coarsened exact matching	(2)	(3) Test housing quality	(4)	(5) Placebo test using rents	(6)
kschool × *restric*	0.042***	0.041*	0.044**	0.041**	−0.019	−0.010
	(0.015)	(0.022)	(0.018)	(0.019)	(0.045)	(0.044)
Baseline controls (Interacted with time FE)	Yes	Yes	Yes	Yes	No	Yes
Boundary	Yes	Yes	Yes	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4292	1532	2172	2571	4753	4753
R^2	0.629	0.653	0.181	0.228	0.181	0.228

Note: Standard errors in parentheses are clustered at the school level.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

4.4.3 | Exclusion of the school federations

The school federations are defined as officially established education groups, consist of the headquarter schools and their branch schools. It is an initiative to expand the coverage of good quality education, and may impact the housing prices in two aspects. First, school federation may increase the price of headquarters school district through the enrollment effect and prestige effect (Huang et al., 2019). Second, school federation may reduce the price of headquarters school district through sharing the teachers and other teaching resources, which narrows the education quality between headquarters and branches. To exclude the confounding of school federations, we excluded the school-federation observations that were established during the sample periods.

The estimated results are reported in Table 3, columns (5) and (6). Our identification yields enormous estimations, which is approximately three times larger than the baseline specifications. It may be the result of huge sample loss. Nearly two thirds of sample are dropped in the regressions; thus, these estimations are restricted to those headquarters who have already established their branches or federations before the limits policy. Those earlier birds may inherently have better education quality than the others, so these specifications may capture the upper bound of the impact of the limits policy.

4.4.4 | Coarsened exact matching

Another potential concern is that our results may result from nonrandom sample selection against school district boundaries. To ensure that the treated and untreated counties are comparable, we adopt the CEM method to refine the control group. We first coarsen each control variable by setting 5 equally sized bins, and 4292 neighborhood-year observations matched. We reestimate the baseline specification with the matched sample and report the results in column (1) of Table 4. The estimated magnitude of the matched sample is even larger than the baseline results, indicating that the different characteristics between ESDH and NSDH may bias our results downward.

In column (2) of Table 4, we further coarsen the controls by setting 10 equally sized bins, and the matched sample shrinks to 1532 neighborhood-year observations. Reassuringly, the point estimates are nearly identical to that in column (7), but with a larger standard error. Together, the results suggest that our baseline results are unlikely driven by the difference in housing characteristics.

4.4.5 | Housing quality as a confounder

One may suspect that our results are mostly driven by the demand for housing quality, rather than the demand for ESDH. Results in Table 1 indicate that ESDH is closer to public goods, such are subway stations and parks, implying that ESDH may have location advantages over NSDH. Such difference may bias our baseline estimations even if we have included the controls interacted with full set of period dummies in our specifications.

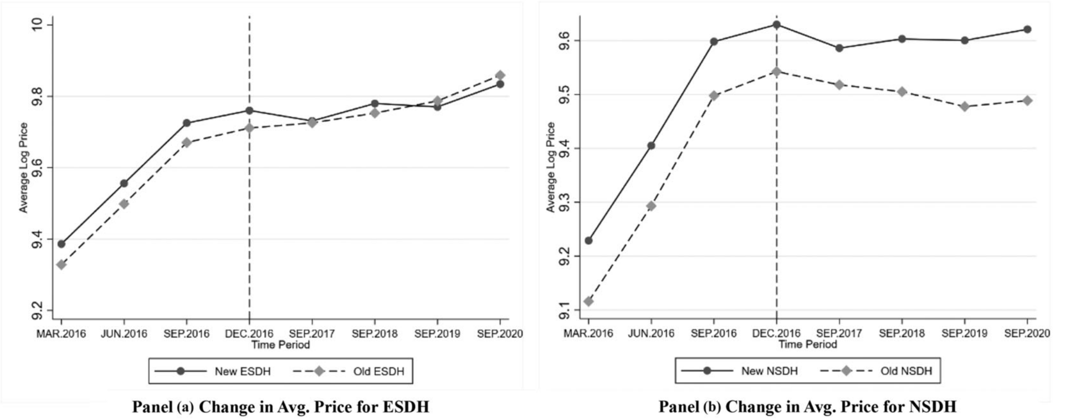
To clarify this concern, we use housing rent as a proxy for the housing quality of neighborhoods. Specifically, we divide the sample into a higher and a lower rent group, based on the mean of the rent. The estimated results for higher and lower rent groups are reported in column (3) to (4) of Table 4, respectively. We find that the effect of limits policy is similar across higher and lower rent neighborhood, and the differences between the estimated coefficients in two groups are −0.0032, with a p-value of 0.360, indicating that our results are less likely driven by the confound of housing quality.

4.4.6 | Using rents as a placebo

As we adopt the school boundary as a "discontinuity," our empirical strategy shares the same advantages and disadvantages of RD designs. On the one hand, the plausibly exogenous school boundary provides sufficient exogeneity for specification. On the other hand, our estimates may also suffer from the omitted variable bias. To rule out such concern, we use rents as a placebo to check whether the difference in rents has changed after the limits policy between ESDH and NSDH. As renters are not eligible to enroll in elite schools, the rents should not change significantly before and after the limits policy, between ESDH and NSDH. We, therefore, reestimate the baseline regression with the dependent variable replaced by the natural logarithm of housing rent. The results are reported in columns (5) and (6) of Table 4. The estimated results are close to zero with larger standard errors, indicating that there is no significant change in the rents after the limits policy, thus reassuring our concerns of the omitted variable bias.

4.5 | Potential explanations

The above discussions revealed that the limits policy could enlarge educational capitalization between ESDH and NSDH, but we are still unaware of the underlying mechanism. In this section, we try to identify potential channels through which the limits policy may enlarge educational capitalization.



Panel (a) Change in Avg. Price for ESDH **Panel (b) Change in Avg. Price for NSDH**

FIGURE 4 Comparison of elite and non-elite, new and old houses *NOTES. Part A plots the average price for new ESDH and old ESDH, and part B plots the average price for new NSDH and old NSDH.*

4.5.1 | Depreciation risks differences

Previous studies have shown that the limits policy reduces housing prices (Li et al., 2017; Wu & Li, 2018; Yan et al., 2022), inducing potential depreciation risks. In comparison with newly built houses, old houses may intrinsically have higher depreciation rates for their poorer living conditions (Bokhari & Geltner, 2018). Old houses can divide into old ESDH and old NSDH according to the school district types. The old ESDH enroll in elite schools, which may survive depreciation risks due to the unchanged education investment demand before and after the limits policy. Thus, the depreciation risk is particularly salient for old NSDH, and market participants are more unwilling to buy old NSDH which has higher depreciation risks, thereby causing enlarged educational capitalization between ESDH and NSDH after the limits policy.

Before presenting formal estimations, we first give graphic evidence in Figure 4, which indicates several patterns of our data.[3] First, no pre-trend is detected in both figures. For both ESDH and NSDH, the prices between new and old houses seemingly followed a parallel trend before the limits policy. Second, old ESDH prices continue to grow even after the limitation (Figure 4A), suggesting that the old ESDH are preferred by market participants after the limits policy. Third, we find that the old NSDH have witnessed a significant price decrease (Figure 4B), which indicates that old NSDH are strongly affected by the depreciation risks in periods ex-post the limitation.

To formally test whether the depreciation risks differences drive our results, we construct the following equation:

$$\begin{aligned} lnprice_{ijt} &= \beta_1 kschool_{ij} \times restric_t \times old_i + \beta_2 kschool_{ij} \times restric_t \\ &+ \beta_3 restric_t \times old_i + \textstyle\sum_t X_i' I_t \gamma + \delta boundary_{ij} + \sum_t I_t + \sum_j \phi_j + \varepsilon_{ijt} \end{aligned} \tag{4}$$

where we generate a variable old_i to denote whether the neighborhood is built before 2000 (the year 2000 is also included), which is more likely to suffer from the depreciation risks. The triple

[3] We use the practice of providing mortgage loans in China banks as a grouping standard. Out of avoiding the risks of messy accounting, banks are unwilling to provide mortgage loans for old houses. In Hefei City, banks require that the sum of residence time and loan term for secondhand houses should not exceed 30 years when providing mortgage loans, and the age of secondhand houses should not exceed 15 years. If we set the year 2016 as the beginning year of applying for mortgage loans, then houses built before 2000 are much more difficult to get mortgage loans.

TABLE 5 Summary of mechanisms.

Dependent variable: lnprice Sample: 300 m	(1) Depreciation risks	(2) Small houses preference
kschool × *restric* × *old*	0.059**	
	(0.027)	
kschool × *restric* × shabby		0.075***
		(0.024)
kschool × *restric*	0.017	0.001
	(0.011)	(0.013)
restric × *old*	−0.021	
	(0.016)	
restric × shabby		−0.045**
		(0.017)
Baseline controls (Interacted with time FE)	Yes	Yes
Boundary	Yes	Yes
Time fixed effect	Yes	Yes
School fixed effect	Yes	Yes
Observations	5432	5432
R^2	0.676	0.681

Note: Standard errors in parentheses are clustered at the school level.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

interaction term *kschool* × *restric* × *old* captures the price relative change between new and old, ESDH and NSDH, before and after the limitation. All other variables are the same as the previous specifications, except for the control variable set where the built year of the neighborhoods (build_year) excluded.

The estimated results are reported in column (1) of Table 5. We find that the estimated results for the triple interaction term are positive and significant, implying that old ESDH witness faster price growth. The estimated coefficient for the variable *restric* × *old* is negative, though noisily estimated, implying that the price of old NSDH is lower after the limits policy, compared with the new NSDH. These estimated results further validate our assumptions that the enlarged educational capitalization is mainly driven by the higher depreciation risks of old NSDH, which is saliently affected by the limits policy.

4.5.2 | Small houses preference

One remaining question is that whether families' preference for ESDH has changed after the limitation. Compared with other ESDH, houses with smaller areas have lower total cost. Under the liquidity constraints, will families prefer to buy houses with smaller areas? In 2016, the per capita residential area in Hefei was 23.6 m^2, whereas the average residential areas for a traditional family of three were about 70 m^2. We obtain the data of the main dwelling areas of neighborhoods from the Hefei Property Market Information website run by the Hefei housing security and real estate bureau.

To identify whether the price of small-area ESDH increases faster after the limits policy, we follow the same specification in Equation (4), replacing the variable *old* with a newly generated variable *shabby*, which is also a dummy indicating whether the dwelling area of the neighborhood is below 70 m^2 or not. We report the estimated results in column (2) of Table 5. We find a large magnitude and high significance of the triple difference term, and the estimated coefficient of $kschool \times restric$ loses any interpretable significance, implying that our baseline results are mostly driven by these small houses. Taking the estimated results for the variable together, we can calculate the total effect of the limits policy on the smaller ESDH, which is 0.03 (with p-value of 0.032), indicating that the limits policy increases the price of smaller ESDH. The estimated results further validate our assumptions that after the limits policy, families prefer to buy ESDH with smaller dwelling areas, for such houses have lower total costs. The results also suggest that families would sacrifice their living needs to acquire an enrollment qualification into elite schools.

5 | CONCLUSIONS

In this article, we analyze the heterogenous effects of house purchase limits policies on the housing price between ESDH and NSDH using a DID approach. Exploiting data of secondhand houses in Hefei, we discover consistent evidences with previous findings that the limits policy can effectively limit the growth rates of housing prices. Nonetheless, such inhibition effect generates systematic differences between ESDH and NSDH. Our study contributes to the further understandings of the economic impact of the limits policy and raises the questions for policymakers to take a more comprehensive consideration of the potential impact of the limits policy on the educational equity. The limitation policy may lead to unintended price differentiation between ESDH and NSDH. Therefore, high-income families would seize and monopoly the premium educational resources through competitive bidding.

Previous studies on school district houses indicated that the price differentiation between ESDH and NSDH is due to the scarcity of premium educational resources. To promote for educational equity, previous policies highlight expanding the supply of high-quality education, which are deemed as effective way to narrow downward the price differentiation between ESDH and NSDH (Sun & Lin, 2020; Huang et al., 2020). However, these reforms require the premise of housing property, which leads purchasing ESDH be the most reliable way to obtain premium education resources.

Our study reveals that the limits policy is ineffective in inhibiting the purchasing ESDH, which is induced by strong educational investment needs. Families can never ignore the education issues for their children and turn to pursue ESDH with smaller dwelling areas. The needs for living now give way to the need for educating, even if the house is shabby and crowded, families are still willing to purchase these luxury shabby houses, just for an enrollment qualification of elite schools. Future policy should unbundle house property rights from school enrollment rights, such as equal rights for home tenants and owners, or multi-dicing for the same neighborhood. Figures A1, A2. Table A1

ACKNOWLEDGEMENTS

We thank the editor, Professor McMillen, Dr. Daniel, and two anonymous referees for their valuable comments and suggestions. All errors are our own.

ORCID

Yongbin Huang https://orcid.org/0000-0001-9500-8424
Hai Hong https://orcid.org/0009-0008-8657-3948

REFERENCES

Acharya, V. V., Bergant, K., Crosignani, M., Eisert, T., & Mccann, F. (2022). The anatomy of the transmission of macroprudential policies. *The Journal of Finance*, *77*, 2533–2575. https://doi.org/10.1111/jofi.13170

Andrabi, T., Das, J., & Khwaja, A. I. (2017). Report cards: The impact of providing school and child test scores on educational markets. *American Economic Review*, *107*, 1535–1563. https://doi.org/10.1257/aer.20140774

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., & Wager, S. (2021). Synthetic difference-in-differences. *American Economic Review*, *111*, 4088–4118. https://doi.org/10.1257/aer.20190159

Armstrong, J., Skilling, H., & Yao, F. (2019). Loan-to-value ratio restrictions and house prices: Micro evidence from New Zealand. *Journal of Housing Economics*, *44*, 88–98. https://doi.org/10.1016/j.jhe.2019.02.002

Athey, S., & Imbens, G. W. (2006). Identification and inference in nonlinear difference-in-differences models. *Econometrica*, *74*, 431–497. https://doi.org/10.1111/j.1468-0262.2006.00668.x

Avery, C., & Pathak, P. A. (2021). The distributional consequences of public school choice. *American Economic Review*, *111*, 129–152. https://doi.org/10.1257/aer.20151147

Barrera-Osorio, F., Gonzalez, K., Lagos, F., & Deming, D. J. (2020). Providing performance information in education: An experimental evaluation in Colombia. *Journal of Public Economics*, *186*, 104185. https://doi.org/10.1016/j.jpubeco.2020.104185

Black, S. E. (1999). Do better schools matter? Parental valuation of elementary education. *The Quarterly Journal of Economics*, *114*, 577–599. https://doi.org/10.1162/003355399556070

Bokhari, S., & Geltner, D. (2018). Characteristics of depreciation in commercial and multifamily property: An investment perspective: Characteristics of depreciation in commercial and multifamily property. *Real Estate Economics*, *46*, 745–782. https://doi.org/10.1111/1540-6229.12156

Brunner, E. J., Cho, S.-W., & Reback, R. (2012). Mobility, housing markets, and schools: Estimating the effects of inter-district choice programs. *Journal of Public Economics*, *96*, 604–614. https://doi.org/10.1016/j.jpubeco.2012.04.002

Chan, J., Fang, X., Wang, Z., Zai, X., & Zhang, Q. (2020). Valuing primary schools in urban China. *Journal of Urban Economics*, *115*, 103183. https://doi.org/10.1016/j.jue.2019.103183

Chen, J., Hui, E. C.-M., Seiler, M. J., & Zhang, H. (2018). Household tenure choice and housing price volatility under a binding home-purchase limit policy constraint. *Journal of Housing Economics*, *41*, 124–134. https://doi.org/10.1016/j.jhe.2018.06.004

Clarke, D., Pailañir, D., Athey, S., & Imbens, G. (2023). *Synthetic difference in differences estimation*. http://arxiv.org/abs/2301.11859

Collins, C. A., & Kaplan, E. K. (2022). Demand for school quality and local district administration. *Economics of Education Review*, *88*, 102252. https://doi.org/10.1016/j.econedurev.2022.102252

Du, Z., & Zhang, L. (2015). Home-purchase restriction, property tax and housing price in China: A counterfactual analysis. *Journal of Econometrics*, *188*, 558–568. https://doi.org/10.1016/j.jeconom.2015.03.018

Fack, G., & Grenet, J. (2010). When do better schools raise housing prices? Evidence from Paris public and private schools. *Journal of Public Economics*, *94*, 59–77. https://doi.org/10.1016/j.jpubeco.2009.10.009

Figlio, D. N., & Lucas, M. E. (2004). What's in a grade? School report cards and the housing market. *American Economic Review*, *94*, 591–604. https://doi.org/10.1257/0002828041464489

Fiva, J. H., & Kirkebøen, L. J. (2011). Information shocks and the dynamics of the housing market. *Scandinavian Journal of Economics*, *113*, 525–552. https://doi.org/10.1111/j.1467-9442.2011.01651.x

Glaeser, E. L., & Luttmer, E. F. P. (2003). The misallocation of housing under rent control. *American Economic Review*, *93*, 1027–1046. https://doi.org/10.1257/000282803769206188

Haisken-DeNew, J., Hasan, S., Jha, N., & Sinning, M. (2018). Unawareness and selective disclosure: The effect of school quality information on property prices. *Journal of Economic Behavior & Organization*, *145*, 449–464. https://doi.org/10.1016/j.jebo.2017.11.015

Huang, B., He, X., Xu, L., & Zhu, Y. (2020). Elite school designation and housing prices-quasi-experimental evidence from Beijing, China. *Journal of Housing Economics*, *50*, 101730. https://doi.org/10.1016/j.jhe.2020.101730

Huang, B., Yun, R., & Fan, F. (2019). The premium effect of elite schools and their affiliated schools: Enrollment effect and prestige effect. *Peking University Education Review*, *17*, 138–159+188. https://doi.org/10.19512/j.cnki.issn2096-2088.2022.02.002

Hui, E. C. M., & Wang, Z. (2014). Price anomalies and effectiveness of macro control policies: Evidence from Chinese housing markets. *Land Use Policy*, *39*, 96–109. https://doi.org/10.1016/j.landusepol.2014.04.003

Jia, S., Wang, Y., & Fan, G.-Z. (2018). Home-purchase limits and housing prices: Evidence from China. *The Journal of Real Estate Finance and Economics*, *56*, 386–409. https://doi.org/10.1007/s11146-017-9615-2

Kuroda, Y. (2022). What does the disclosure of school quality information bring? The effect through the housing market. *Journal of Regional Science*, *62*, 125–149. https://doi.org/10.1111/jors.12557

Laufer, S., & Tzur-Ilan, N. (2021). The effect of LTV-based risk weights on house prices: Evidence from an Israeli macroprudential policy. *Journal of Urban Economics*, *124*, 103349. https://doi.org/10.1016/j.jue.2021.103349

Li, V. J., Cheng, A. W. W., & Cheong, T. S. (2017). Home purchase restriction and housing price: A distribution dynamics analysis. *Regional Science and Urban Economics*, *67*, 1–10. https://doi.org/10.1016/j.regsciurbeco.2017.08.002

Li, Y., Zhu, D., Zhao, J., Zheng, X., & Zhang, L. (2020). Effect of the housing purchase restriction policy on the real estate market: Evidence from a typical suburb of Beijing, China. *Land Use Policy*, *94*, 104528. https://doi.org/10.1016/j.landusepol.2020.104528

Lu, Z., Zhang, S., & Hong, J. (2021). Examining the impact of home purchase restrictions on China's housing market. *China Economic Review*, *67*, 101620. https://doi.org/10.1016/j.chieco.2021.101620

Nunn, N., & Qian, N. (2011). The potato's contribution to population and urbanization: Evidence from a historical experiment. *The Quarterly Journal of Economics*, *126*, 593–650. https://doi.org/10.1093/qje/qjr009

Oates, W. E. (1969). The effects of property taxes and local public spending on property values: An empirical study of tax capitalization and the Tiebout hypothesis. *Journal of Political Economy*, *77*, 957–971. https://doi.org/10.1086/259584

Peng, Y., Tian, C., & Wen, H. (2021). How does school district adjustment affect housing prices: An empirical investigation from Hangzhou, China. *China Economic Review*, *69*, 101683. https://doi.org/10.1016/j.chieco.2021.101683

Roth, J. (2022). Pretest with caution: Event-study estimates after testing for parallel trends. *American Economic Review: Insights*, *4*, 305–322. https://doi.org/10.1257/aeri.20210236

Somerville, T., Wang, L., & Yang, Y. (2020). Using purchase restrictions to cool housing markets: A within-market analysis. *Journal of Urban Economics*, *115*, 103189. https://doi.org/10.1016/j.jue.2019.103189

Sun, W., & Lin, J. (2020). Does school supply reduce school district housing price premium? Evidence from newly-built primary schools in Beijing. *China Economics Quarterly*, *19*, 499–520. https://doi.org/10.13821/j.cnki.ceq.2020.01.06

Sun, W., Zheng, S., Geltner, D. M., & Wang, R. (2017). The housing market effects of local home purchase restrictions: Evidence from Beijing. *J Real Estate Finan Econ*, *55*, 288–312. https://doi.org/10.1007/s11146-016-9586-8

Wen, H., Xiao, Y., & Zhang, L. (2017). School district, education quality, and housing price: Evidence from a natural experiment in Hangzhou, China. *Cities*, *66*, 72–80. https://doi.org/10.1016/j.cities.2017.03.008

Wu, Y., & Li, Y. (2018). Impact of government intervention in the housing market: Evidence from the housing purchase restriction policy in China. *Applied Economics*, *50*, 691–705. https://doi.org/10.1080/00036846.2017.1340569

Yan, H., Wu, J., & Shao, Z. (2022). Do home purchase limits help? Government policy and housing prices in China. *Journal of Urban Affairs*, *44*, 1101–1116. https://doi.org/10.1080/07352166.2020.1773836

Zhu, P., Zhang, Y., & Wang, J. (2023). Canceling the admission priority of private schools enlarges housing price gap in public school districts: Evidence from Shanghai's new admission policy. *Real Estate Economics*, *51*, 49–67. https://doi.org/10.1111/1540-6229.12403

How to cite this article: Yongbin, H., & Hong, H. (2023). The heterogeneous impact of house purchase limits policy on housing prices: Comparison between elite and non-elite school district houses. *Real Estate Economics*, *51*, 1055–1078. https://doi.org/10.1111/1540-6229.12452

APPENDIX

TABLE A1 Results of by-periods specification.

Dependent variable: lnprice	(1) Full sample	(2) Full sample	(3) 300 m	(4) 300 m	(5) 500 m	(6) 500 m
$kschool \times I_{-2}$	0.008	0.015	0.024	0.030	0.016	0.023
	(0.014)	(0.013)	(0.021)	(0.018)	(0.018)	(0.016)
$kschool \times I_{-1}$	−0.018	−0.009	0.002	0.002	−0.006	−0.004
	(0.017)	(0.017)	(0.024)	(0.021)	(0.021)	(0.018)
$kschool \times I_0$	−0.016	−0.015	0.003	0.005	−0.003	−0.001
	(0.016)	(0.016)	(0.023)	(0.021)	(0.020)	(0.018)
$kschool \times I_1$	0.019	0.004	0.015	−0.001	0.017	−0.001
	(0.014)	(0.013)	(0.018)	(0.017)	(0.016)	(0.015)
$kschool \times I_2$	0.040**	0.039***	0.038***	0.022*	0.041***	0.026**
	(0.015)	(0.013)	(0.014)	(0.012)	(0.015)	(0.013)
$kschool \times I_3$	0.065***	0.069***	0.066***	0.055***	0.068***	0.058***
	(0.022)	(0.020)	(0.024)	(0.019)	(0.023)	(0.019)
$kschool \times I_4$	0.118***	0.126***	0.112***	0.100***	0.113***	0.102***
	(0.030)	(0.029)	(0.033)	(0.026)	(0.032)	(0.028)
Baseline controls (interacted with time FE)	No	Yes	No	Yes	No	Yes
Boundary	No	Yes	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,816	14,816	5432	5432	7232	7232
R^2	0.670	0.705	0.645	0.682	0.653	0.688

Notes: Standard errors in parentheses are obtained using bootstrap procedures.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

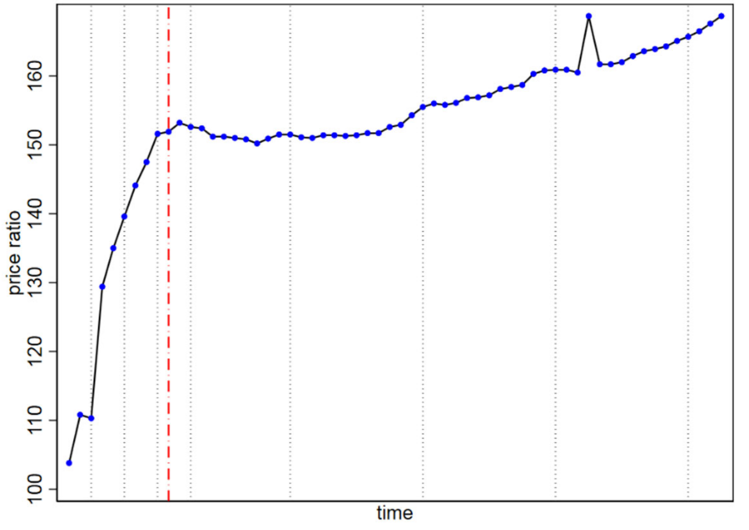


FIGURE A1 Monthly price ratios of Hefei secondhand houses. *Notes*: The monthly price ratio data of Hefei secondhand houses during January 2016 and December 2020 is collected from China National Bureau of Statistics. All these ration data are calculated on the basis of 2015. The red-dash-dot line represents October 2016 when the limits policy enacts, and the black-dot lines represent the data-crawled time. [Color figure can be viewed at wileyonlinelibrary.com]

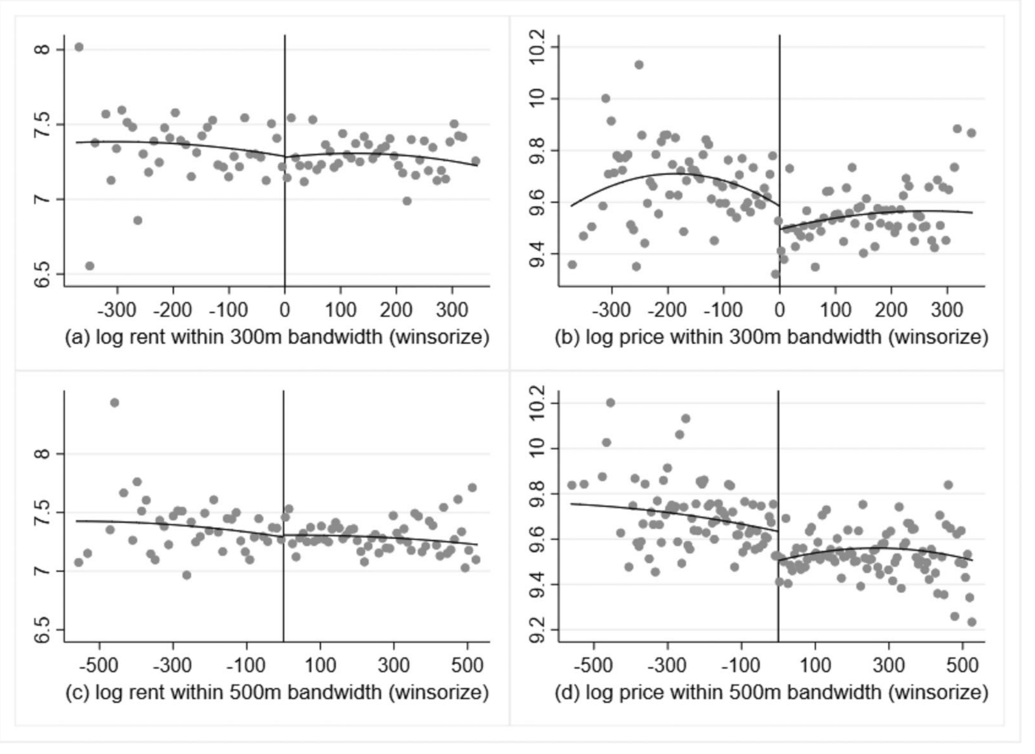
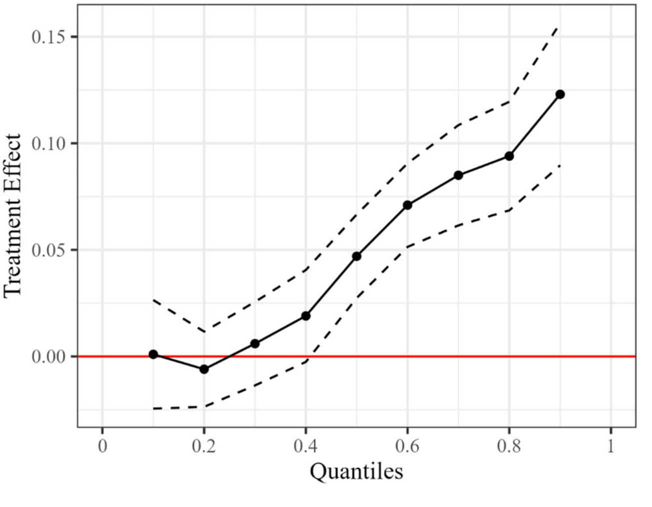


FIGURE A2 Plots of rent and price against boundary with the winsorized sample. *Notes*: The figure depicts the single dimension RD graphs. The *x*-axis denotes the distance against the elite school district boundaries, where negative number refers to the treat group (elite school district houses [ESDH]). The markers and capped spikes represent the estimators and 95% confidence intervals. The observations of log rent, and log price within 300 m (or 500 m) bandwidth is 2730 (3664) and 4753 (6328).

FIGURE A3 Changes-in-changes estimations. *Notes*. The figure depicts the quantile treatment effects on the distribution estimated using the changes-in-changes method. The solid line represents the point estimates, whereas the dashed lines represent the bootstrapped 95% confidence intervals. The estimation is based on full sample with control variables, **school fixed effects and year fixed effects are included as well. [Color figure can be viewed at wileyonlinelibrary.com]



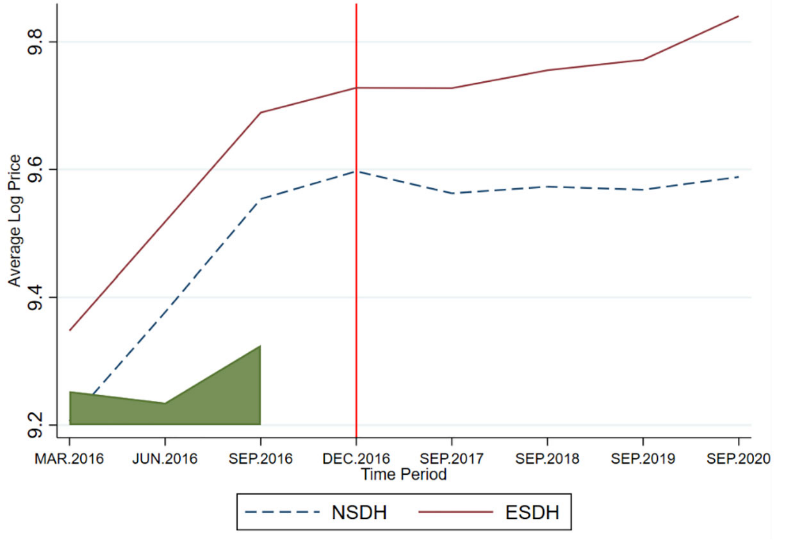


FIGURE A4 Synthetic differences-in-differences. *Note*. The figure depicts the trends in housing price over time for elite school district houses (ESDH) and non-elite school district houses (NSDH), with the weights used to average pretreatment time periods at the bottom of the figure. [Color figure can be viewed at wileyonlinelibrary.com]