

Huang, Bin; He, Xiaoyan; Xu, Lei; Zhu, Yu

Working Paper

Elite School Designation and House Prices - Quasi-experimental Evidence from Beijing, China

GLO Discussion Paper, No. 283

Provided in Cooperation with:

Global Labor Organization (GLO)

Suggested Citation: Huang, Bin; He, Xiaoyan; Xu, Lei; Zhu, Yu (2018) : Elite School Designation and House Prices - Quasi-experimental Evidence from Beijing, China, GLO Discussion Paper, No. 283, Global Labor Organization (GLO), Maastricht

This Version is available at:

<https://hdl.handle.net/10419/187513>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Elite School Designation and House Prices - Quasi-experimental Evidence from Beijing, China

Bin Huang^a, Xiaoyan He^a, Lei Xu^b and Yu Zhu^{c,d,e *}

^a: School of Public Administration, Nanjing University of Finance and Economics, China

^b: National Institute of Economic and Social Research, UK

^c: School of Business, University of Dundee, UK

^d: Global Labor Organization (GLO)

^e: IZA- Institute of Labor Economics

Abstract

We explore three recent comprehensive reforms which aim to equalize access to elite elementary schools in Beijing, to identify the causal effect of access to quality education on house prices. Using property transaction records from Beijing in 2013 and 2016, we construct a balanced panel of *residential complexes*, each of which linked to its designated primary school. Whereas the *multi-school dicing* reform involves randomly assigning previously ineligible pupils to key elementary schools through lotteries, the reform of *school federation led by elite schools* consolidates ordinary primary schools through alliance with elite schools. Moreover, an ordinary primary school can be promoted to key elementary school without involving neighbouring schools in surrounding residential complexes through a “*pure*” *re-designation* effect. We allow for systemic differences between the treated and non-treated *residential complexes* using the Matching Difference-in-Differences (MDID) approach. Our estimates indicate that the causal effect on house prices of being eligible to enrol in a municipal-level key primary school is about 5-7%, while the premium for being eligible for a less prestigious district-level key primary school is only about 1-3%.

Keywords: quality school designation, house price premium, Matching DID, China.

JEL code: R21 (Urban/Regional Economics: Housing Demand); I28 (Education: Government Policy); H44 (Publicly Provided Goods: Mixed Market)

* **Corresponding Author:** Yu Zhu, School of Business, University of Dundee, Dundee, DD1 4HN, UK. E-mail: yuzhu@dundee.ac.uk. We thank participants of the WPEG 2018 meeting in Sheffield and the 1st Renmin/GLO Conference on the Chinese Labour Market in Beijing, for helpful comments. This work has been supported by China’s National Social Science Foundation (Grant No: BFA140039). All errors remain our own.

“In Beijing’s overheated housing market, where schools go, money follows.”
– *Caixinglobal* (2017)

1. Introduction

It has been almost half a century since Oates’ seminal paper on the capitalization of local property taxes on house values (Oates (1969)). Since then, a growing number of studies have contributed to the literature on school quality capitalization under different contexts in terms of countries of study, school quality measures and methodological innovations, see Ross and Yinger (1999), Gibbons and Machin (2008), Black and Machin (2011) and Nguyen-Hoang and Yinger (2011) for reviews.

The phenomenon of steeply priced “school district houses (*xuequfang*)”, i.e. properties giving access to prestigious publicly funded schools, has consistently been one of the hottest topics in the Chinese media in recent years. According to one estate agent, in 2013 house prices in Beijing’s elite school districts were roughly 30 percent higher than in other districts on average (Xinhua 2016).

Using a panel data of *residential complexes* (school attendance/catchment zones), derived from comprehensive data on real estate transactions in Beijing in 2013 and 2016, we investigate how house prices react to the variation in quality of education offered by neighbouring publicly-funded elementary schools. To overcome the endogeneity of education quality, we exploit three recent comprehensive reforms which aim to equalize access to quality educational resources in compulsory education, to identify the causal effect of access to quality school on house prices. Whereas the ***multi-school dicing*** reform involves randomly assigning previously ineligible pupils to key elementary schools through lotteries, the ***school federation led by elite schools*** reform consolidates low quality schools through alliance with elite schools. However, an ordinary school can also be promoted to a key primary school directly without involving neighbouring schools, through a channel which we label as ***“pure” re-designation***.

We start off by estimating the spill-over effects of public education quality on house prices in Beijing, using the hedonic price model. The results indicate that, after controlling for housing and residential features, as well as neighbourhood and location characteristics, the mean house price in key primary school catchment areas is about 5% higher than that for ordinary primary school catchment areas in the Ordinary Least Squares specification. Moreover, the price premia for district-level key primary schools and municipal-level key primary schools catchment is not

statistically significant once we have a full set of controls.¹ Secondly, school attendance zone changes based on school district adjustment, multi-school dicing or pure school status upgrading have increased the premium of municipal key primary school catchment areas, but have no effect on district-level key primary schools in the fixed-effect specifications.

Furthermore, we allow for systemic differences between the treated and non-treated school attendance (catchment) zones using *Propensity Score Matching (PSM)* and account for the common trend in house price inflation using the *Difference-in-Differences (DID)* approach. Our *Matching DID (MDID)* estimates indicate that the causal effect on house prices of being eligible to enrol in a municipal-level key primary school is about 5-7%, while the premium for being eligible for a district-level key primary school is only about 1-3%. The price impacts of the three different channels are broadly comparable to each other.

Moreover, we find that the number of private primary schools within ten kilometres, higher service charges and more local amenities all have a significant positive impact on the average house price of school attendance zones. In contrast, mean floor area ratio, mean number of floors, mean floor area per flat, and the distances to the city centre, to the nearest top-grade hospital, and to the nearest subway station all have significant negative correlation with average house price of school attendance zones.

The remainder of the paper is structured as follows. Section 2 presents the background of the reforms in Beijing. Section 3 briefly reviews the relevant literature. Section 4 discusses the MDID methodology. Section 5 presents the data and the descriptive statistics. In Section 6, the empirical analyses are presented and discussed. Section 7 shows the sensitivity analysis. Finally, Section 8 concludes.

2. Background

A private housing market was not introduced in China until the early 1990s. Before that, most urban residents lived in housing units built and owned by their employers. After the reform, employees no longer received allocated housing and had to buy from a private housing market which had grown from strength to strength (Sato (2006), and Zhang and Yi (2017)). According to

¹ The price premia of key schools are significant before adding the full interaction between numbers of private schools and level of school.

Fang et al. (2015), the residential housing market as measured by residential house sales volume grew by about 15% per annum on average between 2002 and 2013.

Beijing offers an excellent case study on the education reform and housing market of China. As the capital since the founding of the People's Republic in 1949 and the nation's political, cultural and educational centre, Beijing has not only the most developed housing market in the country but also arguably the best resources of education, in particular higher education. However, competition for access to the elite schools which traditionally has excellent track records of graduate enrolment into the country's best-known universities, is exceptionally fierce and starts well before the formal entry to the public education system.

The public schools dominate all stages of education in Beijing. In theory, access to the 9-year compulsory education is free and non-selective, and based on the principle of “*attending nearby schools*”, according to parental household registration (*hukou*) and house ownership (Feng and Lu (2013)).² This implies that securing an address in the catchment of the school district is a necessary if not sufficient condition to enrol one's kids into a so-called *key primary school (KPS)*.³

3. Literature

A large literature has been devoted to the effect of school quality on house prices, in general finding support to the Tiebout model which predicts residential sorting (Tiebout (1956)). Ross and Yinger (1999), Gibbons and Machin (2008), Black and Machin (2011) and Nguyen-Hoang and Yinger (2011) offer excellent reviews. While earlier studies are largely descriptive, recent ones strive to uncover the causal relationship using quasi-experimental framework. This is extremely important for policy designs.

Traditional hedonic pricing model estimates of the school quality effect are likely to suffer from omitted variable bias or endogeneity problems. Black (1999) first applies the *regression discontinuity design (RDD)* using administrative boundaries, also known as the *boundary discontinuity design (BDD)* approach, in an attempt to remove time-invariant unobserved neighbourhood fixed-effects which are correlated with school quality. Fack and Grenet (2010) and Gibbons et al. (2013) further develop the RDD approach using matching. Compared to the OLS baselines, they all find a smaller capitalization effect, at below 4% for a one standard deviation

² *Hukou* is effectively a household registration system in China which intends to reserve access to education, health care, employment and welfare to the holders of local *hukou*, see Wang (2005).

³ You (2006) provides an excellent review of the *key school system* in basic education in China.

increase in test scores.

To the best of our knowledge, Feng and Lu (2013) is the only causal study of the effect of school quality on house prices in China published in English. Using a DID approach, they find that the re-designation of a previously ordinary high school to a specific high-quality school status increases the house price in its residential area by 6.9% in Shanghai. However, to the extent that school designation policy by the municipal government is not entirely exogenous, e.g. due to concerns for equal access across geographical areas (e.g. districts), one cannot rule out the possibility of endogeneity bias in the DID estimates.

4. Methodology

This study employs a quasi-experimental research design to examine three recent educational policy reforms in Beijing which aim to widen access to quality education for all. Conventional multivariate regression analysis is unlikely to uncover the true causal effect of the treatment due to omitted variable bias and endogeneity or self-selection in the treatment (see e.g. Rubin (1974) and Blundell and Diaz (2009)).

To the extent that the treatment status is randomly assigned, a conventional DID estimator would suffice to uncover the true causal effect with the help of a well-defined control group which is assumed to share the common trend. Following the literature, we choose the semi-log specification:

$$\ln price_{it} = \beta_0 + \beta_1 keysch_i + \sum \beta_i X_i + \varepsilon_{it}$$

where $\ln price_{it}$ is the logarithm of mean house price of residential complex i in year t , $keysch$ is a dummy of the designated primary school (alternatively we use two dummies to distinguish between district and municipal-level key schools), X_i 's are control variables, ε_{it} is the error term, and β_0 , β_1 , and β_i 's are coefficients.

However, there are good reasons to believe that the assignment of the treatment status by policy makers in our case is non-random. For example, the government might encourage the creation of *school federations* of non-KPS's in certain areas led by existing elite schools to improve the access to elite education geographically.⁴ In other words, the *non-ignorable treatment assignment assumption* required for unbiased DID estimates is not satisfied. To deal with this issue, we will

⁴ In a leading Chinese education journal, Ha and Yu (2017) present evidence on the price premium of previously non-key primary school catchment areas which were integrated into school federations led by elite schools in Beijing. They find a modest 1.2% effect on average. While they attempt to apply two-way fixed effect and boundary discontinuity design, they do not account for the non-random assignment of the reformed schools.

use *Propensity Score Matching (PSM)* to achieve data balance such that DID can yield unbiased estimates on the matched data. In practice, we will use two alternative matching strategies to ensure that there are no systemic differences between the treatment and control groups (Guo and Fraser (2010)). The strategies are defined by propensity scores estimation using logistic regressions method with either Mahalanobis distance (Strategy 1) or nearest neighbour within caliper (Strategy 2).

4.1 Channels

During our sample period of 2013-2016, there were three possible ways in which the designated primary school of a *residential complex* (RC) could change from an ordinary primary school to a key primary school (KPS). Apart from *multi-school dicing* and *school federation*, a school can also be directly upgraded to KPS by consistently meeting quality thresholds through “*pure*” *re-designation*, a change which does not involve any neighbouring schools. However, these three channels can affect the house price differently due to the different nature of the distribution of resources.

The multi-school dicing reform reduces education inequality by distributing the educational sources by lotteries. However, people who are risk averse may not be willing to buy a property which cannot guarantee their children a place at a KPS. On the other hand, the school federation redistributes the education resources throughout all the schools in the alliance. In reality, people may doubt how much resources would be redistributed from the leading elite school to the low-quality schools. This in turn will affect their willingness to pay for the property. Moreover, as for those schools which are upgraded to key schools (through “*pure*” *re-designation*), if parents value the actual “quality” rather than the relabelling, they might perceive the newly upgraded school less favourably than the pre-existing key schools, leading to differential price premia.

Besides, it turns out that having more independent schools in the surrounding areas (within 10km radius) have a statistically significant positive effect. We are concerned that the effect might be endogenous, and importantly, could affect the estimates of the effect of the reforms. Therefore, we allow for the interaction of number of independent schools with the key variables of interest in the regressions.

5. Data

This paper is based on a balanced panel of residential complexes (*xiaoqu*) in the 12 urban districts in Beijing in 2013 and 2016.⁵ An RC is the urban equivalent of a village and serves as the most fundamental organization unit for the urban population in China. Each RC has its own neighbourhood or residents' committee. In Chinese megacities like Beijing, an RC usually contains hundreds of condominiums in medium or high-rise buildings within well-defined boundaries of one designated publicly funded primary school where the kids are enrolled (Zhang and Yi (2017)).

Using the half million or so actual transaction records of second-hand properties from the two leading property websites *Fang.com* (<http://www.fang.com/>) and *Lianjia.com* (<https://www.lianjia.com/>),⁶ we derive the mean transaction prices and key characteristics for RCs for 2013 and 2016 respectively. Using Google Maps, we also construct the distance of each RC to the city centre proxied by the Central Business District (CBD), the nearest subway station, the nearest top-grade hospital and the number of independent schools within a 10-kilometre radius. The designated schools are identified from the school's admission policies available online for the relevant years. The grade of the school and the policy regime it belongs to are derived from the websites of the school itself and relevant District Education Authorities.

We exclude RCs with too few transactions in either of the two years, or with missing values on key variables. To ensure our results are not driven by outliers in the outcome measure of mean real price per square metre (in RMB yuan), we also drop the top and bottom 1% of the mean price distribution. Moreover, we also realize that a handful of district-level KPS were promoted to municipal-level KPS during our sample period. Since our interest is to estimate the effect of promoting an ordinary school to a KPS, it is natural to drop those KPS districts which have experienced further upgrading. The final sample is a balanced panel of 2,314 RCs observed in both 2013 and 2016.⁷

Table 1A shows the frequencies of RCs by whether their designated primary school has changed from ordinary to key school status over the sample period, and if yes, by the treatment types. Of the 2314 RCs, 222 (9.6%) RCs have experienced change in the school status over the sample period, while 2,092 (90.4%) RCs remain the same.

⁵ The remaining 4 districts where data is unavailable are all rural suburbs, and far away from the Central Business District (CBD).

⁶ Jointly they cover virtually all "used (second-hand)-property" transactions in Beijing.

⁷ We also drop one *defier*, i.e. a RC which is in the catchment of a key primary school in 2013 but not in 2016.

Table 1A: Residential complexes by treatment types, initial sample

Change from ordinary to key school during 2013-16:	No School federation		School federation		Total
	No Multi-school dicing	Multi-school dicing	No Multi-school dicing	Multi-school dicing	
No change (Control)	1,809	59	218	6	2,092
Change (treatment)	130	29	58	5	222
Total	1,939	88	276	11	2,314

While these two categories would naturally constitute the treatment and the control group for our analysis, there are a few complications. In order to make our identification transparent and consistent, we exclude any RCs which have undergone multi-school dicing or school federation reforms in 2013 already (yet are still classified as ordinary primary school catchment) from the control group. In order to attribute the effect of the treatment to the 3 possible channels, we also exclude the 5 RCs which experienced multi-school dicing and school federation simultaneously from the treatment group. So, the final control group consists of 1,809 RCs while the final treatment group consists of 217 RCs, with 29 RCs undertaken multi-school dicing, 59 RCs undertaken school federation and the remaining 130 RCs accounted for by a “pure” re-designation, shown in Table 1B.

Table 1B: Residential complexes by treatment types, final analytical sample

Treatment types	None	Multi-school dicing	School federation	Pure re-designation	Total
Control	1,809				1,809
Treatment		29	58	130	217
Total	1,809	29	58	130	2,026

Table 2 presents the descriptive statistics for the analytical sample before and after the policy change year. All house prices are converted to constant 2013 prices using the Consumer Price Index (CPI) for Beijing. The mean house price in Beijing grows from 37709 RMB yuan (USD 6129) in 2013, to 50652 yuan (USD 8060) in 2016, an increase of 34.3% in real terms over 3 years.⁸ The extent of house price appreciation is consistent with Zhang and Yi (2017), who show that prices of newly-built houses increase by 15–24% for different quantiles in Beijing between January 2013 and December 2013 alone. Over the 3-year sample period, 10.7% of RCs

⁸ The year-end exchange rates between USD and CNY are 6.152, 6.158, 6.284 and 6.643 for 2013, 2014, 2015 and 2016 respectively. We use the mean of 6.31 over the period to derive the USD equivalents.

experienced a positive change in the status of the designated primary school, with 1.4, 2.9 and 6.4 percentage points of these accounted for by multi-school dicing, school federation and pure-resignation respectively. While 35.3% of all residential complexes are in the school district (SD) of a Key primary school in 2013, two thirds of which are district-level KPS, the share of elite SDs grows to 46.0% in 2016, with increases in both the district-level and municipal-level key schools.

Table 2: Descriptive Statistics, Analytical Sample

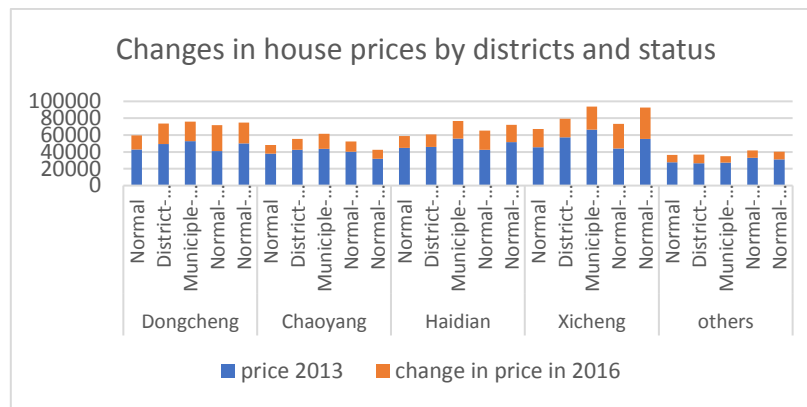
	2013	2016
Price per m ² (dependent variable)	37709	50652
<i>School characteristics</i>		
School District (SD) Change	0	0.107
Multi-school dicing	0	0.014
School federation	0	0.029
Pure re-designation	0	0.064
Key Primary School	0.353	0.460
District-level Key Primary School	0.237	0.314
Municipal-level Key Primary School	0.116	0.146
<i>Control variables</i>		
# independent schools (within 10km)	7.282	
Greening rate	0.329	
Mean floor area ratio	2.585	
Service charges	1.586	
# floors	12.204	
Mean floor area per flat	86.128	
Years since construction	14.566	17.566
# Local amenities (Banks, stores etc.)	3.995	
Distance to City Centre	12.227	
Distance to nearest top-grade hospital	2.429	
Distance to nearest subway station (km)	1.019	
Observation	2,026	2,026

All control variables except for years since construction are time-invariant. There are on average 7.3 independent schools within a 10km radius of the RC. The mean greening rate of 0.329 indicates that the green areas account for almost one-third of the land surface of the residential complex. The floor area ratio is the ratio of total construction area to the land area. The average service charge is 1.586 RMB yuan (0.27 USD) a month per square metre. The mean number of floors is 12.2, reflecting the fact that is Beijing is very densely populated metropolis. The mean floor area per flat is 86.1m², while the average years since construction is 14.6 in 2013. The average number of amenities including stores, post-offices, banks and leisure facilities is 4. The straight-line distances to the city centre and the nearest top-grade hospital are 12.2 and 2.4 km's respectively, while the distance to the nearest subway station is only 1.0 km.

6. Empirical Results

Table 3 presents the pooled OLS and FE (fixed-effect) estimates as well as the corresponding DID estimates, without and with the breakdown of the elite schools into district or municipal-level. These will form the benchmark against which the MDID results are compared. Note that in all specifications we include district dummies and full interaction of number of independent schools with the variable of interest. This is important, given that Figure 1 suggests that there is significant heterogeneity in the initial house prices by transition status and districts.

Figure 1. House prices by districts and reform status



Column 1 shows that the regression adjusted price premium of access to a key primary school is 5.3%. When we distinguish between district and municipal-level key primary schools in column 2, we find that the price premium for the more prestigious municipal-level key school is only marginally higher than its district-level counterpart, at 6.7% and 5.5% respectively. Note however that the former is statistically insignificant, presumably reflecting the relatively lower share in the sample. Columns 3 and 4 report the corresponding FE estimates which rely on RCs with re-designation of schools for identification. Whereas there is still a positively significant premium for municipal-level KPS of 9.6%, there is no statistically significant effect for district-level KPS. Therefore, the price premium of an elite school is driven by the change to a municipal-level key school. The last two columns of Table 3 present the DID estimates. This time the re-designation as an elite school is only significant for change to a district-level key school, with a modest return of 4.6%. Note that the estimated time effect is remarkably consistent across all specifications, within the range of 0.275-0.290. These correspond to an increase in real house prices of approximately 31.7%-33.6% over three years.⁹

⁹ A slope coefficient of b in a log specification can be interpreted as an approximately $100 \cdot b$ percent increase. The exact percentage change is given by $100 \cdot (\exp(b) - 1)$ where \exp is the exponential function.

Table 3: Effect of multi-school dicing on price premium (OLS, FE, DID)

	OLS		FE		DID	
	(1)	(2)	(3)	(4)	(5)	(6)
KPS	0.0533*** (0.0181)		0.0184* (0.0105)		0.0410** (0.0181)	
After-reform (2016)	0.284*** (0.00389)	0.285*** (0.00385)	0.290*** (0.00337)	0.290*** (0.00337)	0.275*** (0.00499)	0.276*** (0.00497)
KPS*After-reform					0.0183** (0.00719)	
District KPS (DKPS)		0.0545*** (0.0189)		-0.0120 (0.0115)		0.0458** (0.0189)
Municipal KPS (MKPS)		0.0670 (0.0520)		0.0961*** (0.0187)		0.0480 (0.0525)
DKPS *After-reform						0.0159** (0.00799)
MKPS *After-reform						0.0344*** (0.00943)
Greening rate	0.205*** (0.0598)	0.285*** (0.00385)			0.206*** (0.0599)	0.276*** (0.00497)
Mean floor area ratio	-0.00824*** (0.00289)	0.218*** (0.0582)			-0.00824*** (0.00290)	0.219*** (0.0583)
Service charges	0.0318*** (0.00541)	-0.00773*** (0.00277)			0.0318*** (0.00541)	-0.00773*** (0.00277)
# Local amenities	0.101*** (0.0125)	0.0313*** (0.00538)			0.101*** (0.0128)	0.0312*** (0.00539)
Distance to City Centre	-0.0167*** (0.000698)	0.103*** (0.0182)			-0.0167*** (0.000697)	0.105*** (0.0188)
Dist. to nearest top-grade hospital	-0.0226*** (0.00293)	-0.0165*** (0.000690)			-0.0227*** (0.00293)	-0.0165*** (0.000689)
Dist. to nearest subway station	-0.0319*** (0.00548)	-0.0233*** (0.00288)			-0.0318*** (0.00548)	-0.0233*** (0.00289)
Dist. to nearest subway station sq.	0.00401*** (0.000403)	-0.0316*** (0.00536)			0.00401*** (0.000402)	-0.0315*** (0.00536)
# independent schools (within 10km)	0.0131*** (0.00170)	0.0136*** (0.00168)			0.0130*** (0.00170)	0.0136*** (0.00168)
# independent schools * KPS	0.00254 (0.00219)					
# independent schools *DKPS		-0.00184 (0.00229)			0.00265 (0.00219)	-0.00175 (0.00229)
#independent schools * MKPS		0.0109* (0.00577)				0.0110* (0.00576)
Mean floor area per flat	-0.00074*** (0.00021)	-0.00078*** (0.00021)			-0.00074*** (0.00021)	-0.00077*** (0.00021)
# floors	-0.00191** (0.000773)	-0.00179** (0.000759)			-0.00191** (0.000773)	-0.00179** (0.000760)
Years since construction	1.99e-05 (0.000674)	-0.000503 (0.000659)			2.04e-05 (0.000674)	-0.000493 (0.000659)
Chaoyang District	-0.143*** (0.0168)	-0.117*** (0.0164)			-0.143*** (0.0168)	-0.117*** (0.0164)
Haidian District	0.0851*** (0.0185)	0.0806*** (0.0175)			0.0855*** (0.0185)	0.0815*** (0.0175)
Xicheng District	0.142*** (0.0201)	0.140*** (0.0188)			0.141*** (0.0201)	0.139*** (0.0188)
Other districts	-0.258*** (0.0176)	-0.240*** (0.0172)			-0.258*** (0.0175)	-0.240*** (0.0172)
Observations (RC-waves)	4,052	4,052	4,052	4,052	4,052	4,052
R-squared	0.797	0.806	0.805	0.807	0.797	0.806

Note: Robust SE in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% respectively. DKPS and MKPS indicate district and municipal-level key primary schools. Omitted district is Dongcheng District.

Table 4 shows the FE results by each channel. In each subgroup, we only compare the relevant treatment group to the common control group which is not affected by any of the reforms. The first two columns show the effect of multi school-dicing, which is a modest 4.9% and only significant at the 10% level. When we distinguish between district and municipal KPS, only the latter has a significant price premium of 8.7%. The next two columns show that there is a 13.7% increase in price premium for school federation, but only if it involves a municipal KPS. The last two columns include the results for the “pure” re-designation effect, which is statistically insignificant overall. However, when we distinguish between the two tiers of elite schools, being re-designated as the more prestigious municipal KPS carries a significant 6.5% price premium while being upgraded to a district-level KPS has no significant effect. Presumably this difference partly reflects the difference in the teaching quality and resources of the two types of schools.

Table 4: Fixed Effect by subgroups

	Multi-school dicing		School federation		“pure” re-designation	
	(1)	(2)	(3)	(4)	(5)	(6)
KPS	0.0487*		0.0381		0.00279	
	(0.0272)		(0.0236)		(0.0118)	
After-reform (2016)	0.290***	0.290***	0.290***	0.290***	0.290***	0.290***
	(0.00337)	(0.00337)	(0.00337)	(0.00337)	(0.00337)	(0.00337)
District KPS		0.0256		-0.0266		-0.0136
		(0.0357)		(0.0276)		(0.0126)
Municipal KPS		0.0865**		0.137***		0.0654**
		(0.0383)		(0.0324)		(0.0255)
Observations	3,676	3,676	3,734	3,734	3,878	3,878
R-squared	0.804	0.804	0.802	0.804	0.806	0.806

Note: Robust standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% respectively. Same controls as in Table 3.

Table 5 describes DID results in subgroups with similar setting as for Table 4. Compared to the OLS and FE results, the magnitudes of DID estimates for municipal key primary schools are smaller, at around 4% for all subgroups, but still statistically significant at the 1% level. For district key primary schools, the price premia are just 2% and only statistically significant for school federation and pure re-designation.

Table 6 shows the post-matching balancing test results for the main sample, for each of the 2 matching strategies employed. Due to the common support restriction, the matched sample is reduced by approximately 58% and 21% for Strategy I and II respectively, compared to the unmatched sample used in Table 3. For both strategies, none of the variance ratios are statistically significant at the 5% level post-matching.

Figures 2 and 3 compare the kernel densities of propensity score before and after matching, for each of the 2 matching strategies used. They show that the matching has been successful, for both strategies.

Table 5: DID by subgroups

	Multi-school dicing		School federation		“pure” re-designation	
	(1)	(2)	(3)	(4)	(5)	(6)
KPS	0.0480** (0.0190)		0.0400** (0.0194)		0.0533*** (0.0196)	
After-reform (2016)	0.277*** (0.00498)	0.279*** (0.00498)	0.277*** (0.00497)	0.278*** (0.00497)	0.278*** (0.00498)	0.280*** (0.00497)
KPS*After-reform	0.0181*** (0.00694)		0.0282*** (0.00682)		0.0272*** (0.00669)	
District KPS		0.0361* (0.0192)		0.0327* (0.0195)		0.0413** (0.0197)
Municipal KPS		0.154*** (0.0233)		0.150*** (0.0239)		0.161*** (0.0239)
District KPS*After-reform		0.0130 (0.00792)		0.0181** (0.00789)		0.0196** (0.00783)
Municipal KPS*After-reform		0.0334*** (0.00915)		0.0463*** (0.00894)		0.0402*** (0.00865)
Observations	3,676	3,676	3,734	3,734	3,878	3,878
R-squared	0.714	0.738	0.712	0.737	0.709	0.735

Note: Robust standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% respectively. Same controls as in Table 3.

Table 6: Post-matching balancing tests

	Unmatched		
	Treatment Mean	Control Mean	Variance Ratio
Service charges	1.65	1.53	1.12*
Total floor	12.49	12.01	0.98
Distance to City Centre	10.75	13.24	0.58*
Distance to nearest top-grade hospital	1.94	2.76	0.53*
Distance to nearest subway station	0.81	1.16	0.59*
Observations		4,052	

	Matched					
	Strategy 1			Strategy 2		
	Treatment Mean	Control Mean	Variance Ratio	Treatment Mean	Control Mean	Variance Ratio
Service charges	1.58	1.58	0.92	1.66	1.60	0.99
Total floor	12.72	12.94	0.97	12.51	12.90	0.93
Distance to City Centre	10.63	10.39	1.10	10.91	10.90	0.89*
Distance to nearest top-grade hospital	1.94	1.94	0.97	1.97	2.02	1.00
Distance to nearest subway station	0.74	0.72	1.04	0.80	0.79	1.05
Observations		1,684			3,172	

Figure 2: Comparison of kernel density of propensity scores before and after matching, school changing, Strategy 1 (Logit & Mahalanobis Metric)

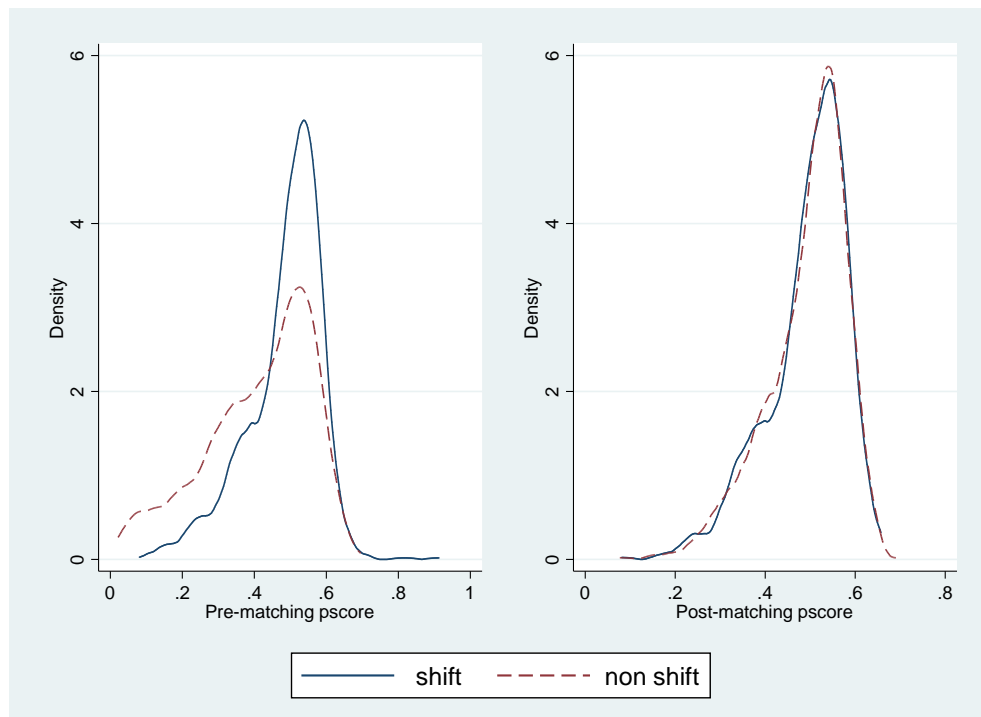


Figure 3: Comparison of kernel density of propensity scores before and after matching, school changing, Strategy 2 (Logit+Neighbour+DID)

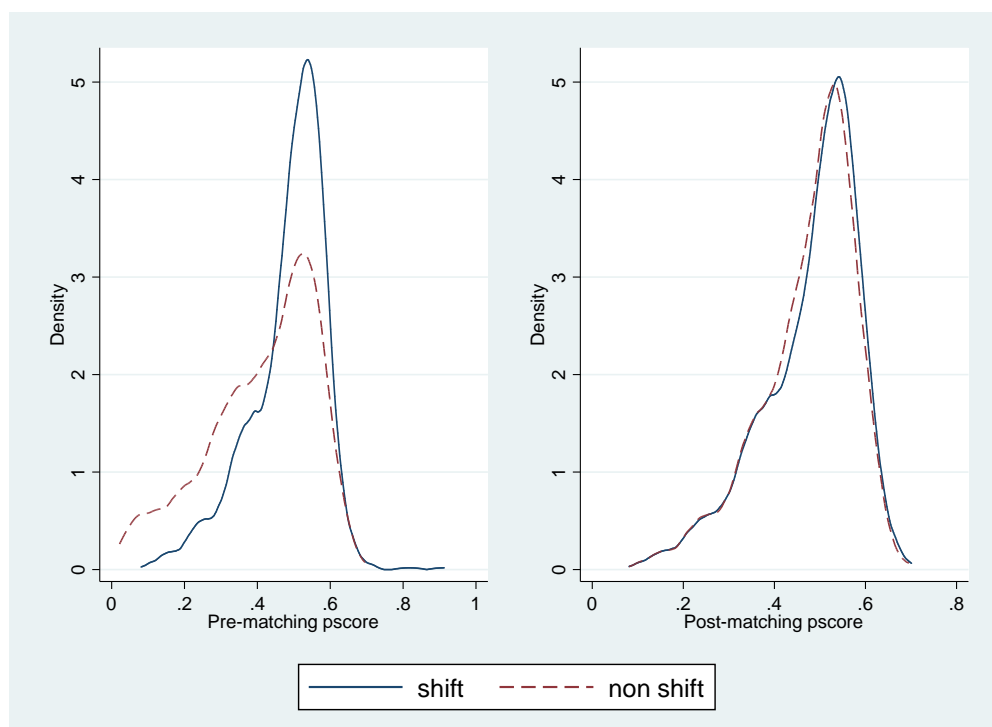


Table 7: Matching Difference-in-differences (MDID) Estimates, Alternative specifications

	Strategy 1		Strategy 2	
	All			
SD Change	0.023** (0.009)	-	0.029*** (0.007)	-
SD Change to District-level Key	-	0.018* (0.011)	-	0.023** (0.008)
SD Change to Municipal-level Key	-	0.038** (0.012)	-	0.043*** (0.009)
R ²	0.77	0.78	0.76	0.77
Observations	1,684		3,172	

	Strategy 1		Strategy 2	
	Multi-school dicing			
SD Change	0.032*** (0.009)	-	0.041*** (0.007)	-
SD Change to District-level Key	-	0.021* (0.011)	-	0.033*** (0.008)
SD Change to Municipal-level Key	-	0.050*** (0.012)	-	0.054*** (0.009)
R ²	0.78	0.79	0.77	0.78
Observations	1,524		2,964	

	School federation			
SD Change	0.033*** (0.009)	-	0.044*** (0.007)	-
SD Change to District-level Key	-	0.024** (0.011)	-	0.034*** (0.008)
SD Change to Municipal-level Key	-	0.050*** (0.011)	-	0.061*** (0.008)
R ²	0.78	0.79	0.77	0.78
Observations	1,472		3,020	

	“pure” re-designation			
SD Change	0.022** (0.010)	-	0.029*** (0.007)	-
SD Change to District-level Key	-	0.016 (0.011)	-	0.023*** (0.008)
SD Change to Municipal-level Key	-	0.039*** (0.012)	-	0.047*** (0.009)
R ²	0.77	0.78	0.77	0.78
Observations	1,640		3,364	

Note: ***, ** and * indicate statistical significance at the 1%, 5% and 10% respectively. Control variables include all regressors in Table 3, plus dummies for districts, and the full interaction between numbers of independent schools and the level of school.

Table 7 shows the MDID estimates for the pooled sample and by different channels. The MDID results are more statistically significant compared to the simple DID estimates, regardless of the matching strategy chosen. With strategy 2, all results are statistically

significant at the 5% level. Depending on the specific treatment, there is an around 5-6% and 2-3% increase in the price after changing to municipal and district KPS respectively. And in general, the magnitudes are also larger than the standard DID results, which might be due to the failure of the critical common trend assumption for DID.

7. Robustness checks

In this section we will undertake a number of robustness checks to ensure our findings are insensitive to the exclusion of all key primary schools from the control group, and to the number of independent schools in the surrounding areas. We will also investigate potential heterogeneous treatment effects with respect to the age and average number of floors of the residential complex, and to the distance to the CBD.¹⁰

Table A1 in the Appendix present descriptive statistics by transition status over the sample period. The patterns indicate that the RCs which have experienced upgrading of the designated primary schools are unlikely to be randomly selected, as they tend to be closer to the CBD and the flats are smaller on average.

7.1. Excluding all key primary schools from the control group

Recall that our control group includes all primary schools which have not experienced a status change over our sample period, regardless of their key school status at the beginning of the period. One might be concerned that while multi-school dicing and school federation reforms increase the attractiveness of the previously non-key schools, they might have an opposite effect on the pre-existing key schools involved, through perhaps a dilution of resources.¹¹ We deal with this issue by reanalysing the sample after excluding all key primary schools from the control group.

Compared to Table 4, we can see that the FE coefficients of the treatment variables in Table 8 remain statistically significant, while the magnitudes of the variables become 2-3 percentage points larger. This indicates that if anything, our main results in Table 7 are lower-bounds. To the extent that the quality of the pre-existing key primary schools might deteriorate, the MDID

¹⁰ Table A1 in the Appendix present descriptive statistics by transition status over the sample period. The patterns indicate that the RCs which have experienced upgrading of the designated primary schools are unlikely to be randomly selected.

¹¹ Multi-school dicing reforms are normally implemented in such a way that only surplus places at the elite school concerned are allocated to nearby non-key school districts. This implies no one loses out and the enrolment lottery only applies to the latter group (references?).

estimates using a control group which include existing key primary schools could be too conservative.

Table 8. Robustness checks with respect to the exclusion of pre-existing key primary schools, FE

	Full sample	Multi-school dicing	School federation	“Pure” re-designation
District KPS	0.0059 (0.0135)	0.0497 (0.0360)	0.0072 (0.0297)	0.0090 (0.0131)
Municipal KPS	0.0870*** (0.0253)	0.111*** (0.0386)	0.163*** (0.0326)	0.0894*** (0.0267)
After-reform (2016)	0.269*** (0.00509)	0.266*** (0.00513)	0.263*** (0.00503)	0.267*** (0.00492)
Observations	1,876	1,540	1,620	1,940
R-squared	0.791	0.788	0.788	0.788

Note: ***, ** and * indicate statistical significance at the 1%, 5% and 10% respectively. Control variables include all regressors in Table 3, plus dummies for districts, and the full interaction between numbers of independent schools and the level of school.

7.2. Age and average number of floors of the residential complex, and distance to the CBD

Table 9 checks the robustness of the MDID Strategy 2 results of Table 7, i.e. Logistic regression with nearest neighbour within caliper, with respect to age and average number of floors of the residential complex, and distance to the CBD. Given that both matching strategies appear to fit the data equally well, we prefer Strategy 2 which preserves a much larger proportion of the original sample. The first two columns compares RCs with years since construction below or above the median. The next two columns present the results by the average floors of districts. The last two columns present the results on the basis of distance to CBD.

Table 9 suggests that the elite school designation effect on house prices are more pronounced for RCs which are newer (i.e. with below median years since construction), closer to the city centre, and more densely populated (above median number of floors). However, as in Table 7, the effect of municipal KPS is always larger than that of district KPS, with the exception of RC with above median distance to the CBD, in which case both estimates are statistically insignificant.

Tables 10-12 repeat Table 9, but focus on the treatment effect of multi-school dicing, school federation, and pure “re-designation”, respectively. The results turn out to be highly robust to that of Table 9, indicating no significant differences across the 3 channels.

Table 9. Robustness w.r.t. age and average number of floors of the residential complex and distance to the CBD, all RCs

	MDID					
	Years since construction		Distance to the CBD		Average number of floors	
	Below median	Above median	Below median	Above median	Below median	Above median
District KPS	-0.0331 (0.0397)	0.0267 (0.0295)	0.126*** (0.0329)	0.0407 (0.0256)	-0.0436 (0.0283)	0.121*** (0.0363)
Municipal KPS	0.110 (0.0799)	-0.0549 (0.0939)	0.153** (0.0766)	0.0300 (0.0820)	-0.0730 (0.0703)	0.185* (0.104)
After-reform (2016)	0.240*** (0.008)	0.275*** (0.008)	0.248*** (0.0067)	0.293*** (0.0096)	0.268*** (0.0082)	0.257*** (0.0080)
SD Change to District-level Key	0.0330*** (0.0117)	0.0311** (0.0126)	0.0118 (0.0119)	0.0194 (0.0132)	0.0310** (0.0136)	0.0273** (0.0118)
SD Change to Municipal-level Key	0.0659*** (0.0115)	0.0357** (0.0181)	0.0818*** (0.0122)	-0.0037 (0.0157)	0.031*** (0.0132)	0.0560*** (0.0151)
Observations	1,478	1,694	1,586	1,586	1,586	1,586
R-squared	0.812	0.679	0.748	0.716	0.802	0.668

Note: ***, ** and * indicate statistical significance at the 1%, 5% and 10% respectively. The observations are matched based on the Nearest Neighbourhood Matching. MDID estimates by subgroups. Control variables as in Table 3.

Table 10. Robustness w.r.t. age and average number of floors of the residential complex and distance to the CBD, Multi-school dicing

VARIABLES	MDID					
	Years since construction		Distance to the CBD		Average number of floors	
	Below median	Above median	Below median	Above median	Below median	Above median
District KPS	0.0450 (0.0303)	0.0953*** (0.0277)	0.149*** (0.0360)	0.0541** (0.0233)	0.0272 (0.0261)	0.128*** (0.0332)
Municipal KPS	0.208*** (0.0787)	0.0478 (0.0961)	0.0949 (0.0758)	0.135 (0.0823)	0.0388 (0.0720)	0.229** (0.103)
After-reform (2016)	0.241*** (0.0076)	0.285*** (0.0085)	0.245*** (0.0066)	0.303*** (0.0095)	0.271*** (0.0083)	0.254*** (0.0079)
SD Change to District-level Key	0.0355*** (0.0106)	0.0303** (0.0120)	0.0439*** (0.0108)	0.0174 (0.0124)	0.0257** (0.0128)	0.0389*** (0.0107)
SD Change to Municipal-level Key	0.0779*** (0.0107)	0.0309* (0.0165)	0.0904*** (0.0113)	0.0120 (0.0148)	0.0402*** (0.0121)	0.0653*** (0.0142)
Observations	1,382	1,582	1,482	1,482	1,482	1,482
R-squared	0.861	0.741	0.762	0.774	0.845	0.711

Notes: ***, ** and * indicate statistical significance at the 1%, 5% and 10% respectively. The observations are matched based on the Nearest Neighbourhood Matching. MDID estimates by subgroups. Control variables as in Table 3.

Table 11. Robustness w.r.t. age and average number of floors of the residential complex and distance to the CBD, School federation

	MDID					
	Years since construction		Distance to the CBD		Average number of floors	
	Below median	Above median	Below median	Above median	Below median	Above median
District KPS	0.0291 (0.0292)	0.118*** (0.0260)	0.138*** (0.0372)	0.0709*** (0.0220)	0.0391 (0.0257)	0.129*** (0.0321)
Municipal KPS	0.177** (0.0761)	0.103 (0.0954)	0.0830 (0.0759)	0.209*** (0.0790)	0.0840 (0.0708)	0.229** (0.103)
After-reform (2016)	0.235*** (0.0076)	0.284*** (0.0080)	0.245*** (0.0066)	0.294*** (0.0089)	0.264*** (0.0081)	0.255*** (0.0077)
SD Change to District-level Key	0.0444*** (0.0107)	0.0265** (0.0122)	0.0412*** (0.0110)	0.0194 (0.0123)	0.0277** (0.0126)	0.0393*** (0.0109)
SD Change to Municipal-level Key	0.0860*** (0.0112)	0.0468*** (0.0175)	0.100** (0.0112)	0.0177 (0.0146)	0.0503*** (0.0121)	0.0748*** (0.0144)
Observations	1,430	1,590	1,510	1,510	1,510	1,510
R-squared	0.859	0.744	0.762	0.775	0.848	0.714

Notes: ***, ** and * indicate statistical significance at the 1%, 5% and 10% respectively. The observations are matched based on the Nearest Neighbourhood Matching. MDID estimates by subgroups. Control variables as in Table 3.

Table 12. Robustness w.r.t. age and average number of floors of the residential complex and distance to the CBD, “Pure” re-designation

VARIABLES	MDID					
	Years since construction		Distance to the CBD		Average number of floors	
	Below median	Above median	Below median	Above median	Below median	Above median
District KPS	0.0104 (0.0304)	0.113*** (0.0254)	0.131*** (0.0324)	0.0659*** (0.0228)	0.0167 (0.0247)	0.137*** (0.0307)
Municipal KPS	0.185** (0.0770)	0.0938 (0.0974)	0.121 (0.0735)	0.215** (0.0849)	0.0718 (0.0729)	0.250*** (0.0951)
After-reform (2016)	0.237*** (0.0078)	0.287*** (0.0077)	0.246*** (0.0067)	0.300*** (0.0086)	0.272*** (0.0080)	0.258*** (0.0076)
SD Change to District-level Key	0.0291** (0.0113)	0.0200* (0.0117)	0.0167 (0.0111)	0.00963 (0.0126)	0.0127 (0.0128)	0.0322*** (0.0108)
SD Change to Municipal-level Key	0.0689*** (0.0114)	0.0296* (0.0170)	0.0868*** (0.0111)	-0.00212 (0.0155)	0.0290** (0.0127)	0.0603*** (0.0143)
Observations	1,576	1,788	1,682	1,682	1,682	1,682
R-squared	0.845	0.749	0.747	0.766	0.838	0.716

Notes: ***, ** and * indicate statistical significance at the 1%, 5% and 10% respectively. The observations are matched based on the Nearest Neighbourhood Matching. MDID estimates by subgroups. Control variables as in Table 3.

8. Concluding Remarks

This paper examines the effect of access to quality education on house prices, by exploring recent comprehensive educational reforms which aim to equalize access to elite elementary schools in Beijing, China. While the *multi-school dicing* reform involves randomly assigning previously ineligible pupils to key elementary schools through lotteries, the reform of *school federation led by elite schools* consolidates low quality schools through alliance with elite

schools. Moreover, an ordinary primary school in a certain residential complex can be upgraded to key elementary school without involving neighbouring schools in surrounding residential complexes through a “*pure*” *re-designation* effect.

Using the Matching Difference-in-Differences (MDID) approach, we identify the causal effect of being eligible to enrol in elite primary schools on house prices while allowing for underlying systemic differences between the treated and non-treated school districts. Our estimates suggest that the price premium of being eligible to enrol in a municipal-level key primary school is about 5-6%, while the premium for being eligible for a district-level key primary school is about 2-3%, with only the former statistically significant consistently. The three different channels have similar effects but with slightly different magnitudes. School districts which have undertaken school federation reforms are likely to experience slightly higher increase in prices. The magnitude of these results are in line with the limited causal evidence on the price premium of quality school access in China currently available.

Our findings are robust to the use of alternative matching strategies and to possible interaction effects of the reforms with the number of independent schools in surrounding areas. We also find that excluding all pre-existing key primary schools from the control group will, if anything, increase the elite school designation effect by 2-3 percentage points. To the extent that multi-school dicing and school federation might lead to dilution of resources of the existing key primary schools, our estimates should be interpreted as a lower bound effect. Moreover, the elite school designation effect on house prices are found to be more pronounced for RCs which are newer, more densely populated and closer to the city centre, holding all other factors constant.

One limitation of our study is that we do not have measures of the probability of getting into a key school under multi-school dicing or the exact formation of the school federation led by an elite school. Having such variation would allow us to discriminate between treatments of various intensity.

Nevertheless, our findings have important policy implications. Although both the multi-school dicing and the school federation reforms aim to equalize education opportunities for all, they are shown to have the unintended consequences of pushing up house prices that are already out of reach for people on average earnings in this metropolis. Future educational policy reforms would benefit from careful evaluations of similar programmes implemented in different contexts and possibly randomized controlled pilot studies.

References

- Black, S.E. (1999) Do better schools matter? Parental valuation of elementary education. *Quarterly Journal of Economics*, 114(2), 577-599.
- Black, S.E. and Machin, S. (2011) Housing valuations of school performance. *Handbooks in Economics*, 3, 485-519.
- Blundell, R. and Dias, M.C. (2009) Alternative Approaches to Evaluation in Empirical Microeconomics. *Journal of Human Resources* 44(3), 565-640.
- Caixinglobal (2017). Residency rules for school enrolment drive up property prices near Beijing's best institutions. Accessed 30th May 2018. <https://www.caixinglobal.com/2017-03-17/101067331.html>.
- Fang, H., Gu, Q., Xiong, W., and Zhou, L.-A. (2015). Demystifying the Chinese housing boom. *National Bureau of Economic Research Working Paper* 21112.
- Fack, G. and Grenet, J. (2010). When do better schools raise housing prices? Evidence from Paris public and private schools. *Journal of Public Economics* 94(1-2), 59-77.
- Feng, H. and Lu, M. (2013). School quality and housing prices: Empirical evidence from a natural experiment in Shanghai, China. *Journal of Housing Economics* 22, 291-307.
- Gibbons, S. and Machin, S. (2008) Valuing school quality, better transport and lower crime: Evidence from house prices. *Oxford Review of Economic Policy* 24(1), 99-199.
- Gibbons, S., Machin, S. and Silva, O. (2013). Valuing school quality using boundary discontinuities. *Journal of Urban Economics*, 75(3), 15-28.
- Guo, S., and Fraser, M. W. (2010). *Propensity score analysis: statistical methods and applications*. Sage.
- Ha, W. and Yu, R. (2017) What is the value of school reforms: An estimation of the “Elite School District Property” effect based on comprehensive reforms of compulsory education in Beijing (in Chinese). *Peking University Education Review* 15(3), 134-151.
- Nguyen-Hoang, P. and Yinger, J. (2011). The capitalization of school quality into house prices: A review. *Journal of Housing Economics* 20, 30-48.
- 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 (6), 957-971.

- Ross, S., and Yinger, J. (1999). Sorting and voting a review of the literature on urban public finance, In: Cheshire, P. and Mills, E.S. (Eds), *Handbook of Urban and Regional Economics*, vol. 3. North Holland, 2001-2060.
- Rubin, D. B. (1974). Estimating causal effects of treatments in randomized and nonrandomized studies. *Journal of Educational Psychology*, 66(1), 688-701.
- Xinhua (2016) Xinhua Insight: Housing – the new price of education. 10th March 2016. Accessed 30th May 2018. http://www.xinhuanet.com/english/2016-03/10/c_135175890.htm.
- Sato, H. (2006) Housing inequality and housing poverty in urban China in the late 1990s. *China Economic Review* 17, 37-80.
- Tiebout, C.M. (1956) A pure theory of local expenditures. *Journal of Political Economy*, 64(5), 416-424.
- Wang, F.L. (2005) *Organizing through Division and Exclusion: China's Hukou System*. Stanford, CA: Stanford University Press.
- You, Y. (2006) A deep reflection on the “key school system” in basic education in China. *Jiaoyu Xuebao* (Journal of Education Studies), 2(2): 36–42 (in Chinese).
- Zhang, L. and Yi, Y. (2017) Quantile house price indices in Beijing. *Regional Science and Urban Economics* 63, 85-96.

Appendix:

Table A1. Descriptive statistics, by status change

Status 2013-2016	Distance to city centre (km)	Average area(m ²)	Year of completion
Normal in both years	13.62	86.83	1999
District-level KPS in both years	11.87	88.20	1998
Municipal-level KPS in both years	9.10	82.23	1994
Normal to District-level KPS	9.68	84.23	1997
Normal to municipal-level KPS	8.55	77.05	1998
Total	12.22	86.12	1998

Note: The residual “other” district contains the districts of Fengtai, Daxing, Fangshan, Changping, Shijingshan, Tongzhou, Mentougou and Shunyi.