How much are good elementary schools worth? Evidence from school acquisitions in Beijing*

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Abstract

We utilize government-sanctioned school acquisitions in Beijing to estimate individuals' willingness to pay for good elementary schools. The spatial and temporal variation in these acquisitions allows us to estimate a hedonic pricing model in the difference-in-difference framework. Comparing regular elementary schools that are acquired by good schools with those that are not, we find an average price premium of 7% for apartments in the catchment areas of acquired schools. We do not find strong evidence of free riding; in other words, individuals do not pay disproportionately more for smaller apartments to obtain the same enrollment privileges. Finally, we find heterogeneous price effects for different types of acquisitions, defined by their post-acquisition organizational structures. Across acquisition types, the difference in their price effects is significant in the short run (within the first three years post acquisition), but becomes insignificant in the long run (after three years).

Keywords: School acquisition, public schools, housing price, hedonic model, difference-in-difference.

JEL codes: H75, I28, R21

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1 Introduction

Individuals who value local public goods are willing to pay for them in the form of residential housing prices. In this paper, we empirically examine this willingness to pay for good elementary schools. To do so, we utilize government-sanctioned school acquisitions in Beijing as a quasi-natural experiment. A school acquisition is a merger between two public schools, one regarded as "good" and the other "regular", to reduce disparity in educational quality across schools. In this context, a regular elementary school being acquired by a good school acts as a treatment, while not being acquired serves as a control. Thus, by linking school acquisitions data to real estate transactions data, we can estimate a hedonic pricing model in the difference-in-difference (DID) framework. The treatment effect, i.e., the average price effect associated with school acquisitions, informs us of individuals' willingness to pay for good elementary schools.[1]

The capitalized value of school quality in the real estate market has received considerable attention in the economics literature. We contribute to the field in several important aspects.

First, in a number of western economies such as the United States, the United Kingdom, Canada, and Australia, local public schools are financed by local property taxes.[2] In that setting, causation can go in both directions: Better-quality public schools may lead to higher residential prices; at the same time, higher residential prices may generate higher property tax revenue, which can fund better-quality schools. As both the demand for and the supply of school quality are endogenous, to identify individuals' willingness to pay for good schools (a demand parameter), it is critical to control for supply side variations (Downes and Zabel 2002, Gibbons and Machin 2003, Cheshire and Shappard 2004, Bayer et al. 2007). In comparison, the funding for public schools in China comes from general tax revenue that is not directly linked to local property values (Zheng and Kahn 2008).[3] Therefore, our analysis has the notable advantage of avoiding reverse causation. In our setting, better-quality schools lead to higher residential prices, while higher residential prices have no direct impact on the funding, and hence the quality, of local public schools.

[1]Developed by Rosen (1974), hedonic pricing models have been used to estimate willingness to pay for a wide range of local amenities and disamenities, such as air quality (Kim et al. 2003, Chay and Greenstone 2005), water quality (Leggett and Bockstael 2000, Walsh et al. 2011), noise level (Day et al. 2007, Andersson et al. 2010), power plants (Davis 2011), shale gas developments (Muehlenbachs et al. 2015), and industrial plants (Currie et al. 2015), to name just a few.

[2]For example, Oates (1969) examines the relationship between local property taxes, public school expenditure, and property values, and finds evidence consistent with the Tiebout hypothesis—that is, individuals choose residential locations according to the provision of local public goods.

[3]While there are current policy debates on whether to enact a residential property tax, property taxes were never used to fund public schools in our sample period.

Second, following Black (1999), a large part of the literature has relied on the discontinuity created by administrative and/or geographic boundaries to control for unobserved heterogeneity. Residential properties on one side of a certain school boundary are matched to similar properties on the other side, and the average price difference is attributed to the difference in schools (Gibbons and Machin 2003, 2006, Fack and Grenet 2010, Gibbons et al. 2013, Chan et al. 2020). This method relies on the assumption that all unobserved characteristics of these properties are distributed smoothly across the boundary. If this assumption does not hold, unobservable differences across the boundary (e.g., neighborhood quality) will bias the estimate (Bayer et al. 2007, Clapp et al. 2008, Dhar and Ross 2012).[4] An alternative approach utilizes quasi-experimental variations in the data for identification, e.g., the opening of new charter schools (Andreyeva and Patrick 2017), school redistricting (Bogart and Cromwell 2000), school rezoning (Ries and Somerville 2010, Collins and Kaplan 2017), school relocation (Argawal et al. 2016), and the introduction of state-administered school ratings (Figlio and Lucas 2004). Our paper joins the latter group and uses government-sanctioned school acquisitions as a source of exogenous variation in school quality.[5] Both spatial and temporal variation in school acquisitions allows us to embed a hedonic pricing model in the DID framework, better controlling for unobserved heterogeneity than cross-sectional analysis alone.

A third advantage of our analysis arises from the fact that, in urban areas of China, residential housing consists almost exclusively of apartment units. Typically, a neighborhood ("xiao qu") is developed by a single real estate company and is comprised of similar styled multi-story buildings, with tens to hundreds of apartment units per building. Compared to single family homes, apartments in a given neighborhood are close substitutes for one another. In addition, as each elementary school has a designated catchment area consisting of multiple neighborhoods, we can establish a one-to-one mapping of neighborhoods onto their corresponding schools. This implies that neighborhood fixed effects are effective at capturing most of the unobserved heterogeneity in the empirical analysis.

We obtained confidential real estate transaction data from a large brokerage company in China, *Lianjia*, whose market share in Beijing is over 60%. Linking transaction data to school acquisitions, we find that school acquisitions lead to an average price premium of 7% for acquired schools, compared to those that are not. This price premium is both statistically and economically significant, translating into $280,000$ yuan on average, or over $40,000$ US dollars.[6] It is also robust to alternative model specifications.

[4]Increased school choice has also been shown to weaken the link between locally zoned schools and property values, see Schwartz et al. (2014), Chung (2015), and Machin and Salvanes (2016).

[5]In Section 2 we provide more details on this policy.

[6]During our sample period, the exchange rate between RMB and USD is 6–7 yuan to a dollar.

Following the public finance tradition, we also looked for evidence of free riding, i.e., whether individuals preferentially purchase smaller apartments to gain the same school enrollment privileges as those purchasing larger ones. If individuals are motivated by the free-riding incentive, we would expect to see a disproportionately high price premium for smaller apartments. We did not find such evidence. In fact, the price premium (in percentage terms) does not change with the size of the apartment. Thus, measured in absolute terms, the "price" for a seat in acquired schools is indeed smaller if one buys a smaller apartment. The fact that individuals do not compete away this benefit suggests that they are concerned about potential costs, i.e., inferior housing quality due to over-crowding if families with school-aged children were to live in smaller units. These two opposing forces appear to offset each other in the data.

Furthermore, we found heterogeneous price effects for different types of acquisitions defined by their post-acquisition organizational structures. First, we categorize acquisitions as fully or partially integrated, depending on whether the acquiring and the acquired schools operate as one entity afterward or as separate entities under the same name. Second, we categorize acquisitions as horizontal or vertical, depending on whether a regular elementary school is acquired by a good elementary school or a good middle school. In both instances, we find heterogeneous price effects. The *average* price premium is significantly larger for acquisitions that are fully rather than partially integrated, and significantly larger for acquisitions that are vertical rather than horizontal. However, these differences in price premiums are only significant in the short run (within the first three years post acquisition), and become insignificant in the long run. We interpret such intertemporal price effects, namely a significant difference in the transitional phase but convergence to a common steady state over time, as reflecting the different planning horizons faced by local government bureaucrats.

The rest of the paper is organized as follows. In Section 2 we provide institutional background about the public education system in China in general, and the specific educational reforms in Beijing in particular. We then present the econometric model and discuss our identification strategy in Section 3. Data used for the empirical analysis are described in Section 4, and estimation results are reported in Section 5. We conclude with a discussion of the results in Section 6. Tables referred to in the main text are placed in Appendix A. Supplementary analyses are described and corresponding results are reported in Appendix B.

2 Institutional background

After the founding of the People's Republic of China in 1949, the Chinese public education system was modeled after that of the former Soviet Unions. For primary and secondary education, limited resources were concentrated in a small number of “key” schools instead of being spread across all schools equally.[7] Compared to regular schools, the key schools enjoyed smaller classes, better teachers, better facilities, and more rigorous curricula. The goal of this system was to ensure a steady supply of academically prepared students for higher education, especially in fields deemed critical to the national interest.[8] At the same time, the rest of the schools were poorly funded. Since key schools were mostly located in urban rather than rural areas, the disparity in school quality was also more prominent in major cities such as Beijing.

Admissions to the key schools were traditionally merit based. That is, key schools would use entrance exams to assess the academic capability of their applicants and would select those with high test scores. Such competitive entrance exams marked every stage of the education process, from elementary schools to middle schools to high schools and, eventually, to colleges and universities.

Public education was heavily disrupted during the Cultural Revolution, but economic reform in China brought it back in focus. New policies significantly expanded the student base for public education, especially at the basic education level. In particular, the 1986 Compulsory Education Law (CEL) required all children to attend school for a minimum of nine years and stipulated that compulsory schooling should be tuition free. While these targets may have not yet been fully achieved in rural areas with limited fiscal capacity, they have been largely reached in urban areas such as Beijing.

Besides expanding the student base, new policies also aimed to offer more equal and better-quality education to all students, instead of favoring the high-ability ones at the expense of the rest. The 2006 amended Compulsory Education Law (ACEL) put an official end to the earlier system of key schools. It explicitly stipulated that local governments “shall promote balanced growth across schools, reduce the disparity in their funding and operational conditions, and not separate key schools from non-key schools, nor key classes from non-key classes within a school.” In Beijing, the implementation of this law brought a number of important changes to the administration of public education.

First, at the nine-year compulsory schooling stage, there are no longer official designations of key schools or merit-based admissions. Schools are prohibited from using entrance exams to select students. Instead, they can only enroll students

[7]Focusing on the case of Beijing, Sui (2012) provides excellent archival evidence of the design and operation of this system in the early years (1949–1966) up to the Cultural Revolution. Wang (2015) provides an extensive review of the history of the system of key schools in China, see `http://www.hprc.org.cn/gsyj/yjjg/zggsyjxh_1/gsnhlw_1/d14jgsxsnh/201512/t20151229_4132803.html`, accessed on May 27, 2019.

[8]These well-trained, high-skill individuals accounted for only a tiny fraction of the working population, but they played an outsized role in the early development of the nation.

based on the "proximity principle." For elementary schools (grades 1–6), each school has a designated catchment area consisting of multiple (not necessarily contiguous) neighborhoods, and all children with legal residence ("hukou") in the catchment area are free to enroll. Schools are also prohibited from charging any enrollment fees to students who are not otherwise eligible under the proximity principle. Thus, the only way parents can influence the enrollment eligibility of their children in a given elementary school is through their choice of residential locations in the corresponding neighborhoods.[9]

Second, and most importantly for our analysis, local governments in Beijing have promoted school acquisitions as a means to reduce disparity in educational quality. Regarding primary and secondary educational institutions, the Board of Education at the district government level ("qu jiao wei") has direct authority to mandate structural changes such as school acquisitions, and the Education Commission at the municipal government level ("shi jiao wei") has broad oversight over the standards for both the setup and operation of schools. For a school acquisition decision, the Board of Education issues an administrative order specifying which good (historically "key") school is to acquire which regular (non-key) school, and whether the two schools are to become one legal entity or remain separate entities post acquisition. Such decisions are purely bureaucratic and do not involve any public consultation, and their announcements are not anticipated by the general public.[10]

One may wonder why local governments prefer school acquisitions as a means to reduce educational inequality, when there are more straight-forward measures such as increasing funding for regular schools to match that of good schools. The answer may be multi-faceted. On the one hand, increasing funding for regular schools without corresponding funding cuts to good schools can be fiscally challenging, as the Board of Education has a fixed budget to allocate across schools. On the other hand, redistributing funding from good to regular schools may generate unwanted public controversy, as it creates ostensible "winners" and "losers" as a result. In comparison, school acquisitions create an opaque channel through which educational resources may flow across schools. While the public appears generally hopeful that acquisitions will improve the equality of acquired schools, there is little

[9]For middle schools (grades 7–9), instead of individual school catchment area, several schools are designated as a bloc with a collective catchment area. All students within this collective area are randomly assigned to one of the middle schools by computer generated lotteries. So unlike elementary school, residential location choices do not guarantee the enrollment eligibility in a given middle school.

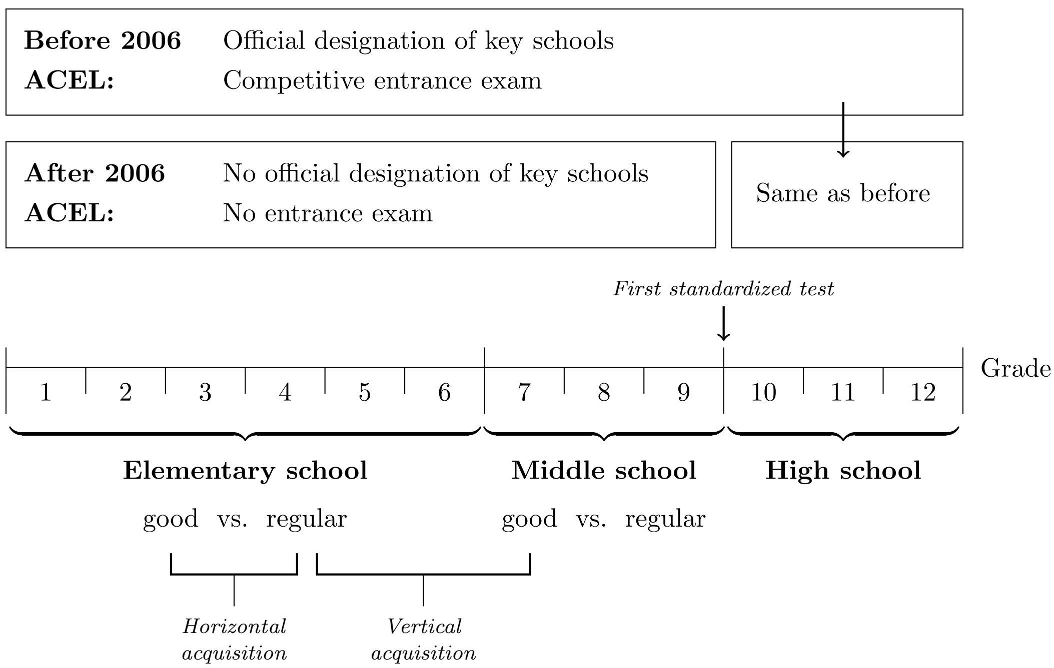
[10]Although only anecdotal evidence, there are ample news reports where parents are surprised by an announcement and feel elated to learn that their school is the one chosen to be acquired. For example, see `http://finance.people.com.cn/n/2014/0329/c1004-24770033.html`, `http://roll.sohu.com/20141210/n406836892.shtml`, and `http://theory.people.com.cn/n/2015/0601/c40531-27083685.html`, last accessed on September 14, 2020.

concern about its impact on the acquiring schools. As a result, there is very little public pushback against these reforms, making them a preferred policy instrument for local governments.

Not all school acquisitions follow the same template. They can be categorized as either fully or partially integrated, depending on the organizational structures of schools post acquisition. In a fully integrated acquisition, the acquiring school and the acquired school become one legal entity and operate as one school, i.e., there is one school principal, a common pool of teachers, a common pool of students (combined from the two previously separate catchment areas), and a unified school budget. Regarding their physical spaces, typically the campus of one school is used for all classes of certain grades (e.g., grades 1–3), while the campus of the other school is used for all classes of the remaining grades (e.g., grades 4–6). In this case, educational resources are fully equalized between the two schools. In contrast, in a partially integrated acquisition, the two schools maintain a certain degree of autonomy and operate as separate legal entities under the same name, i.e., there are two school principals, separate pools of teachers, separate pools of students (each from its own catchment area), and two distinct school budgets. The acquiring school is tasked with implementing a number of measures to help improve the educational quality of the acquired school, including sharing its management standards and best practices, facilitating teacher exchange, sharing its professional development program for teachers, and sharing its curriculum-related resources. However, there is no guarantee that educational resources will be fully equalized between the two schools.

School acquisitions can also be categorized as horizontal or vertical depending on whether the acquiring school is an elementary or a middle school. Traditionally, elementary schools (grades 1–6) are stand-alone primary educational institutions, while middle schools (grades 7–9) and high schools (grades 10–12) are the two divisions of secondary educational institutions. This creates a physical transition when students graduate from elementary school to attend middle school, i.e., they go to a new school, are assigned to a new class, and encounter a new set of teachers and classmates. To minimize such transitional disruptions, local governments also experiment with the creation of integrated nine-year schools that cover the entire length of compulsory education. Furthermore, nine-year schools eliminate the uncertainty associated with the lottery for middle school assignment, with students in the catchment area enjoying guaranteed enrollment eligibility for all nine years (grades 1–9) instead of only six years (grades 1–6). Thus, from the perspective of a regular elementary school, a horizontal acquisition is when it is acquired by a good elementary school, and a vertical acquisition is when it is acquired by a good middle school, resulting in potentially different benefits. These changes are summarized in Figure 1 below.

Figure 1: Primary and secondary educational institutions



We would like to understand how acquisition decisions are made by local governments; specifically, we want to know how schools are selected and why certain acquisition types are chosen. However, despite an extensive search, we cannot find any documentation on the internal decision-making process, possibly a result of local governments' attempt to minimize public scrutiny and accountability. Lacking document evidence, we will rely on empirical evidence to show that, from the perspective of regular elementary schools, acquisition decisions (which are acquired and which are not) constitute exogenous policy shocks because they appear random and unanticipated in the data.[11]

Ideally, we would also like to link school acquisitions to direct measures of school quality, such as educational expenditure per student, teacher quality, class size, and student (value-added) test scores. Unfortunately, government agencies do no release school-level data to the public, even when they are internally available.[12]

[11] We will also show that from the perspective of good elementary schools, these acquisition decisions (which are to acquire other schools) cannot be viewed as exogenous policy shocks. See Appendix B for details.

[12] As the amended Compulsory Education Law bans public schools from using test scores to rank students, government agencies do not have standardized test scores for students in elementary and middle schools. The first standardized test that students encounter is the high school entrance exam after they have completed compulsory schooling.

As the second-best choice, we rely on historical quality differences between key and non-key schools (school reputation) as a proxy for the current school quality. A potential downside of using this indirect measure is that we cannot distinguish the reputation effect from the schooling quality effect, after a regular school is acquired by a good school.

3 The Empirical Approach

For our empirical analysis, we embed a hedonic pricing model in the DID framework. This utilizes variations in school acquisitions both across neighborhoods and over time periods for identification.

3.1 Average treatment effect

As a starting point, we estimate the following *average treatment effect* model:

$$p_{int} = \alpha_n + \beta_t + \gamma A_{nt} + \theta X_{int} + \epsilon_{int}. \quad (1)$$

The dependent variable p_{int} is the log of the price for apartment unit i in neighborhood n sold in period t (defined as year-month intervals). On the right hand side, α_n is the neighborhood fixed effect and β_t is the year-month fixed effect, allowing for linear time trend as a special case. The variable A_{nt} is the acquisition dummy: for a regular elementary school whose catchment area includes neighborhood n, the dummy takes the value 1 if this school has been acquired by a good school in period t, and 0 otherwise. X_{int} is a vector of control variables that captures the physical characteristics of the apartment unit, such as its size, the number of bedrooms and bathrooms, and the age of the building. The residual term is ϵ_{int}. Our parameter of interest is γ.

3.2 Identification

Since school acquisitions are mandated by local governments instead of being negotiated by schools themselves, they are arguably a source of exogenous policy shock. After controlling for the fixed effects (α_n and β_t) and observed heterogeneity (X_{int}), for neighborhood n that had a regular school initially but was acquired by a good school in period t, the observed price difference $p(A_{nt}=1)-p(A_{n,t-1}=0)$ captures the effect of the better quality school, as well as other unobserved factors that result in price changes between period $t-1$ and t. The counter-factual $p(A_{nt}=0) - p(A_{n,t-1}=0)$, namely the price difference that would have been if neighborhood n had not experienced the school acquisition, is not observed. Instead, it can be approximated by that of another neighborhood m that did not experience school acquisition in either period $t-1$ or t. For this neighborhood, the observed price difference $p(A_{mt}=0)-p(A_{m,t-1}=0)$ captures the impact of other unobserved factors, while the school quality remains unchanged. By netting out the difference across the two periods, the remaining difference can be solely attributed to the school acquisition. Thus, the parameter γ is identified by the difference in differences

$$\left[\,p(A_{nt}\!=\!1)-p(A_{n,t-1}\!=\!0)\,\right]\;-\;\left[\,p(A_{mt}\!=\!0)-p(A_{m,t-1}\!=\!0)\,\right].$$

The identification of γ relies critically on the "common trend" assumption, that is, but for the treatment (school acquisition), the pattern of price changes would have been the same between the treated group and the control group. To determine whether such a "common trend" assumption holds in the data, we compare the price patterns *before* school acquisitions (i.e., $A_{nt}=0$) between two groups: the treated group consisting of all neighborhoods that were later treated ($g_n=1$ if $A_{nt}=1$ for some $t\geq\hat{t}$), and the control group consisting of all neighborhoods that were never treated ($g_n=0$ if $A_{nt}=0$ for all t). Moreover, we aggregate over the seasonal variations of prices (captured by the year-month fixed effect β_t) to focus on the difference in the annual trends (captured by year T). In particular, we estimate the following *pre-treatment* model:

$$p_{int}\;=\;\alpha_n\;+\;\beta_t\;+\;\delta(g_n\cdot T)\;+\;\theta X_{int}\;+\;\epsilon_{int}. \tag{2}$$

Note that the group dummy g_n is perfectly collinear with the neighborhood dummies α_n, and the year variable T is perfectly collinear with the year-month dummies β_t, so neither can be separately estimated. On the other hand, their interaction term $g_n\cdot T$ allows for potentially different time trends between the treated and the control group, where a significant δ would indicate a violation of the common trend assumption.

Note that, in this analysis, we focus only on neighborhoods that had regular schools initially, some of which were later acquired (treated) while others were not (control). We intentionally exclude neighborhoods that have always had good schools. These neighborhoods could be considered as "always treated" in our framework ($A_{nt}=1$ for all t), but including them in our current DID analysis is problematic. Since educational resources are meant to flow from acquiring schools to acquired schools, we expect school acquisitions to have an impact on the acquiring schools, potentially in the opposite direction of the impact on the acquired schools. Thus, while these "always treated" neighborhoods may experience price changes as a result of school acquisitions, these changes are not captured in our acquisition dummy A_{nt} as defined above. Including these neighborhoods in the estimation may potentially bias our result.

Nonetheless, as a supplement (see Appendix B for details), we separately consider a mirroring DID analysis using neighborhoods that have always had good schools. This allows us to examine the resource dilution effect of school acquisition, where treated schools are good schools that acquired regular schools, and control schools are good schools that did not. The validity of such DID analysis also relies on the common trend assumption, now tested among good schools. We show that the common trend assumption fails to hold in the pre-treatment analysis for good schools, suggesting potential selection biases. Accordingly, the DID result cannot be taken at face value as representing the unbiased resource dilution effect on the acquiring schools.

3.3 Heterogeneous treatment effects

Going beyond the average treatment effect, we also explore potentially heterogeneous price effects along several dimensions.

First, we divide the before-and-after two-period specification of the acquisition dummy A_{nt} into a sequence of year dummies $A_{n,T+k}$ indexed by $k = \pm 1, \ \pm 2, \ \pm 3, \ \ldots,$ representing consecutive annual intervals relative to the acquisition date. Each of these dummies has a separate parameter γ_k, thus allowing potentially different estimates for the years not only after but also before the acquisitions.

$$p_{int} \ = \ \alpha_n \ + \ \beta_t \ + \ \Sigma_k \, \gamma_k A_{n,T+k} \ + \ \theta X_{int} \ + \ \epsilon_{int}. \tag{3}$$

This event analysis serves two purposes. First, the coefficients for the years before the acquisition event (i.e., negative values of k) can be viewed as a falsification test, namely assuming that the treatment (being acquired) had happened *before* it actually took place. Significant estimates of these coefficients would cast doubt on our identification, as they represent price effects where none is expected. On the other hand, estimates of these coefficients will lend additional support to the common trend assumption. Second, the coefficients for the years after the acquisition event (i.e., positive values of k) will reveal the intertemporal pattern of the price effects. According to Goodman-Bacon (2018), if the treatment effects vary strongly over time, the DID estimate, as a weighted average of all pairwise DID estimates, could provide a misleading summary. So understanding the intertemporal pattern of the post-acquisition price effects is particularly important given the staggered treatment timing (acquisition date) in our setting.

Second, following the public finance literature, we also look for evidence of free riding: that is, whether individuals preferentially purchase smaller (and hence cheaper) apartment units to gain the same school enrollment privileges as those purchasing larger units. If parents are motivated by the free-riding incentive, we should see a disproportionately high price impact of school acquisitions on smaller apartments. We estimate the following model to examine whether the price effect differs by the size of the apartment:

$$p_{int} \;=\; \alpha_n \;+\; \beta_t \;+\; \Sigma_b \, \gamma_b A_{nt} * I(unit\ i \in bin\ b) \;+\; \theta X_{int} \;+\; \epsilon_{int}, \tag{4}$$

where $I(unit\ i \in bin\ b)$ is the indicator variable that equals one if the size of apartment unit i falls within one of the subgroups indexed by b. We are interested in the relationship between the coefficients γ_b, each measuring a price premium for a particular subgroup of apartment sizes. If γ_b is significantly larger for small apartments relative to large ones, this can be interpreted as evidence of free riding.

Third, as discussed in Section 2, different types of acquisitions may confer different benefits on the acquired schools. Recall that educational resources are fully equalized if the acquisition is fully integrated, but not necessarily so if it is partially integrated. To detect their difference, we estimate the following model:

$$p_{int} \;=\; \alpha_n \;+\; \beta_t \;+\; \gamma_1 A1_{nt} \;+\; \gamma_2 A2_{nt} \;+\; \theta X_{int} \;+\; \epsilon_{int}, \tag{5}$$

where $A1_{nt}$ and $A2_{nt}$ are dummy variables for fully integrated and partially integrated acquisitions respectively. The parameters γ_1 and γ_2 will then inform us whether there is a difference and how large it is.

Similarly, the benefit of horizontal acquisitions may be a direct quality improvement for the acquired schools, while the benefit of vertical acquisitions can be more indirect through guaranteed enrollment in good middle schools instead of random assignment. To detect their difference, we re-estimate (5) using $A1_{nt}$ and $A2_{nt}$ to denote horizontal and vertical acquisitions respectively.

4 Data

For our empirical analysis, we compile data from two separate sources, one regarding real estate transactions and the other regarding schools.

4.1 Real estate transactions data

Since the hedonic pricing model relies on the assumption of a competitive market, where both sellers and buyers take the implicit price for each of the housing characteristics as given, we focus on the real estate *resale* market in Beijing. In the resale market, existing housing units (apartments) are transacted; since most sellers are individual home owners, they have little market power. This is quite different from the primary market where newly constructed housing units are transacted and each developer, which supplies a large number of apartments, has significant market power.

The confidential real estate resale data in Beijing come from Lianjia (formerly called Homelink), a large real estate brokerage company founded in 2001. For transactions completed through its listing platform, Lianjia has recorded relevant information in a centralized database, which started as a pilot program in early 2009 and then as a full roll-out in late 2011. Through the end of 2018, Lianjia has recorded $590,137$ sales in total.[13]

For each completed transaction, Lianjia provides us the following information. First, we have the transaction information, namely the sale date, the sale price, and the name and address of the neighborhood in which the apartment unit is located. For privacy protection, Lianjia does not give us the precise address of the apartment itself, i.e., the unit number or the building number. This prevents us from matching transactions as repeated sales of the same unit. Nevertheless, the name and address of the neighborhood allows us to match the real estate transaction data with the corresponding school information, as described in the next subsection. Furthermore, we also know certain physical characteristics of the apartment unit, including its size, the number of bedrooms and bathrooms, and the age of the building itself. These physical characteristics allow us to control for observed heterogeneity.

Finally, Lianjia records detailed land usage rights and the ownership title category for each apartment unit. As urban land is owned by the state, strict regulations are in place to govern the development of land parcels. In particular, the usufruct rights of land for residential purposes typically carry a duration of 70 years, and those carrying shorter durations (e.g., 40 or 50 years) confer no school enrollment privileges. Similarly, there are different ownership title categories for the apartment units, where those facing minimal resale restrictions are designated "commodity" housing units ("shang pin fang"), while those facing various additional resale restrictions are designated "economy" housing units ("jing ji shi yong fang") or "limited price" housing units ("xian jia fang"), etc. Such information on property rights allows us to select a relatively homogeneous sample for empirical analysis.

4.2 Data on schools and school acquisitions

Since the late 2000s, local governments in Beijing have stopped officially designating key elementary or middle schools. Nonetheless, an unofficial list of "good schools" has been widely known and circulated among interested parties, in particular parents. While the format of this unofficial list may be different across websites, its content stays essentially the same across platforms and over time, and largely coincides with the list of historically key schools. As a starting point,

[13] Lianjia is the leading platform in the resale market during this period, and accounts for over 60% of all completed transactions in Beijing.

we have downloaded this unofficial list, with the good schools listed by each of the sixteen districts in Beijing.[14] By exclusion, we regard any school that is not included in this list as a regular school.

To track school acquisitions over time, we compile the list of official announcements from the website `http://www.ysxiao.cn` (literally translated as "from kindergartens to elementary schools"). As an information hub, this website provides detailed information on various aspects of elementary schools in Beijing and is very popular among parents. In particular, it publishes real-time announcements regarding school acquisitions. We have collected all such announcements from this website, and recorded information on the announcement date, the acquiring school, the acquired school, and the nature of each acquisition (fully or partially integrated, horizontal or vertical).

As the central link between schools and neighborhoods, we have also collected all available admissions guides from the same website `http://www.ysxiao.cn`. Most of these admissions guides are for sought-after schools, i.e., good schools as well as regular schools after they are acquired by good schools.[15] For each school, the admissions guide explicitly lists its designated catchment area, spelling out the name and the address of each included neighborhood. Except for a few instances, the catchment areas for the schools are stable over the years. We then manually match each school to its corresponding neighborhoods, linking school acquisitions data to real estate transaction data.

4.3 Sample selection and summary statistics

After having merged the school data with the real estate transactions data, we take several steps to select the final sample for our empirical analysis.

First, even though we have data for all sixteen districts in Beijing, we focus only on the four core districts ("Dongcheng," "Xicheng," "Chaoyang," and "Haidian") that comprised the city of Beijing before its amalgamation with what used to be the outer rural counties. This sample restriction is necessary because core districts differ from outer districts in important aspects. In our school data, historically and officially designated key schools are only present in the four urban districts but not the rural counties; therefore, good schools in the outer districts are not comparable

[14] `http://www.sohu.com/a/108411984_256157`, last accessed on June 11, 2019.

[15] Schools are not required by law to publish their admissions guides online. Instead, for each upcoming academic year, a school typically posts its admissions guide in paper form at both the school entrance and the entrance to each neighborhood within its catchment area. Such posted admissions guides are then collected by this website, some submitted by parents and others photographed by its own staff. Note that these admissions guides do not cover all schools. In particular, since there is little parental interest outside the catchment area of a regular school, the website has very few admissions guides for regular schools that are not acquired by good schools.

to those in the core districts. Moreover, in our real estate data, observations in the core districts are overwhelmingly resale transactions for existing housing units. On the other hand, sales of newly constructed housing units frequently occur in the outer districts, where new land parcels continue to be developed. Therefore, to ensure comparability of observations we focus on the four core districts, which account for 52% (306, 999) of all (590, 137) observations.[16] For these districts, good real estate transaction data coverage begins in September 2011. Thus, our sample period is from September 2011 through December 2018, leaving us 306, 520 observations.[17]

Second, for comparability, we restrict our sample to observations of real estate with 70-year usufruct land rights and an ownership title designation as a "commodity" housing unit.[18] This excludes 12% of the remaining sample, leaving us with 268, 675 observations. Furthermore, as we cannot directly distinguish arms-length transactions from sales among related parties, in order to minimize potential bias resulting from the latter we also exclude transactions with very low unit prices. We set the cutoff for exclusion to 5, 000 yuan per square meter (the mean price in our sample is 55, 200 yuan per square meter, with a standard deviation of 23, 600).[19] This removes 321 observations, leaving a remaining sample of 268, 354. Finally, in a small number (336) of observations, there are missing values in one or more of the three control variables (age of the building, number of bedrooms, and number of bathrooms). Removing these observations with missing values, we end up with a final sample of 268, 018 observations.[20]

We divide this final sample into two sub-samples, one consisting of all neighborhoods that are in the catchment areas of regular elementary schools at the beginning of the sample period, and the other consisting of all neighborhoods that are in the catchment areas of good elementary schools. As discussed in Section 3.2, we use only the first sub-sample for our main analysis, estimating the price effect on regular schools that are later acquired relative to those not acquired. This sub-sample accounts for 60% of the full sample, or 161, 145 observations.[21] This

[16] In fact, we find that the pretreatment common trend assumption fails to hold in the outer twelve districts. This suggests that the DID method is not suitable for that sub-sample.

[17] In the core districts, observations prior to September 2011 account for less than 0.2% of total observations. Our main results are robust to alternative starting months for the sample period, ranging from July 2011 to January 2012.

[18] A small fraction of the transactions, accounting for 0.16% of the sample, involves resale of designated parking spots. As parking spots do not confer school enrollment privileges, we also exclude these transactions from our analysis.

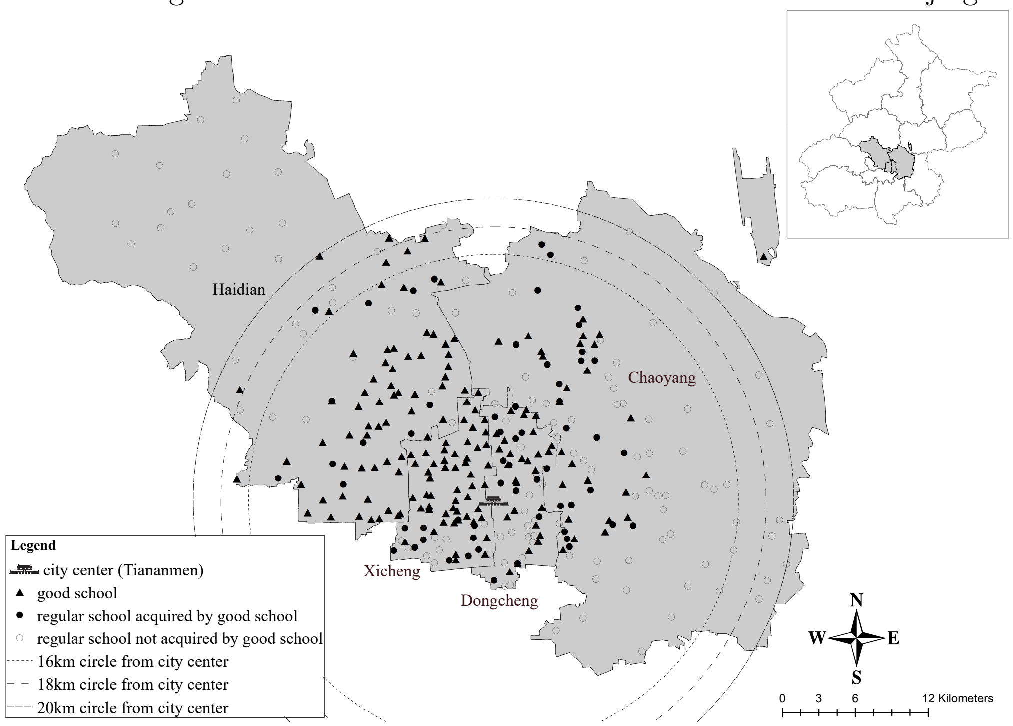
[19] We experimented with alternative cutoff levels ranging from 1, 000 to 10, 000 yuan per square meter, and the main results are robust.

[20] Our main results are robust if dummy variables are used to include the observations with missing values in the estimation.

[21] We use the second sub-sample for supplementary analysis in Appendix B, estimating the price effect on good schools that later acquire regular schools relative to those that do not. That

sub-sample spans 87 months and four core districts, consisting of 127 communities and 1,880 neighborhoods. Figure 2 shows the spatial locations of all schools within the sample, in particular, 57 regular schools acquired by good schools (the treated group) and 120 regular schools not acquired (the control group). Table 1 shows the temporal variations of school acquisitions by year, with the majority of acquisitions taking place in 2014–2015. Table 2 reports the summary statistics.[22]

Figure 2: School locations in the four core districts of Beijing



5 Estimation results

As both the school quality indicator (the acquisition variable) and the sales price (the outcome variable) are serially correlated, the DID approach overestimates the significance of the policy impact unless the clustered error structure is properly corrected for (Bertrand et al. 2004). Here, all reported standard errors are clustered at the neighborhood level.

sub-sample accounts for 40% of the full sample, or 106,873 observations. The take-away of that mirroring DID analysis is that the common trend assumption fails to hold in the pre-treatment analysis, thus suggesting potential selection bias.

[22]A tiny fraction of the sample has no private bathroom. This is not a measurement error. Instead, such apartments tend to be in older buildings where multiple units on the same floor share one common bathroom. Dropping these observations does not affect our results.

5.1 Average treatment effect

Table 3 reports the estimated average treatment effect model (1) when regular schools are acquired by good schools. In columns 1–6, we estimate the same model specification using increasingly restrictive samples, i.e., the number of observations (bottom of the table) decreases. Similarly, even though only the key parameter of interest (γ associated with the acquisition dummy A_{nt}) is reported in each estimation, the four panels A–D estimate the model (1) using different specifications.[23]

In Panel A, we estimate (1) using very flexible controls (X_{int}) to account for a wide range of possible non-linear relationships, including third degree polynomials of the apartment size, third degree polynomials of the building age, the full set of dummies of the number of bedrooms, and the full set of dummies of the number of bathrooms. This is what we refer to as the main specification. Together with the neighborhood fixed effects (α_n) and year-month fixed effects (β_t) as described in (1), this is our preferred model.

This model is estimated using all observations (column 1), excluding observations with the apartment size over 140 square meters (column 2),[24] and further excluding observations in neighborhoods whose school quality change is due to redistricting instead of school acquisition (column 3). Furthermore, as can be seen in Figure 2, the outer regions of the four core districts experience no school acquisition. One may be concerned that their inclusion in the estimation as the control group could potentially bias the result. So, in columns 4–6, we further exclude neighborhoods that are more than 20 kilometers away from the city center (column 4), more than 18 kilometers away (column 5), and more than 16 kilometers away (column 6). At the expense of losing observations, the control group and the treated group become arguably more homogeneous when we move from column 1 to column 6. The main result is both qualitatively and quantitatively stable. After regular schools are acquired by good schools, apartments in catchment areas of the acquired schools sell for significantly higher prices on average. The average price premium due to school acquisitions ranges from 6.7% to 7.5% across the estimation samples.

For sensitivity check, we also consider alternative model specifications. In Panel B we re-estimate (1) using parsimonious controls that only allow for either log-linear or linear relationships, including the logarithm of the apartment size, the logarithm of the building age, the number of bedrooms, and the number of bathrooms. This is what we refer to as the parsimonious specification. Together

[23]To save space, we do not report the coefficients on the control variables. These results are available upon request.

[24]In Beijing, different rules and regulations apply to housing units at or below 140 square meters (regarded as "regular" housing units) versus those above 140 square meters (regarded as "luxury" housing units), e.g., there are different requirements for the holding period to qualify for capital gains tax exemption, different requirements for the mortgage down payment, etc.

with the neighborhood fixed effects and year-month fixed effects, this parsimonious model estimates the average price effect of school acquisitions to be from 6.6% to 7.5% across the estimation samples, virtually identical to those in Panel A. Thus, our results are not driven by the particular functional form of the controls.

In Panel C we re-estimate (1) using no controls other than the neighborhood fixed effects and year-month fixed effects. The estimated average price premium ranges from 6.7% to 7.5%, which is again virtually identical to those in Panel A. This suggests that our results are not driven by changes in apartment stock with different characteristics.

Finally, in Panel D, we re-estimate the main specification with *community* instead of neighborhood fixed effects. On average each community consists of about fifteen neighborhoods, thus representing a much coarser grid and involving much fewer parameters. The estimated average price premium ranges from 6.2% to 6.8% across the estimation samples, qualitatively similar to the main results.

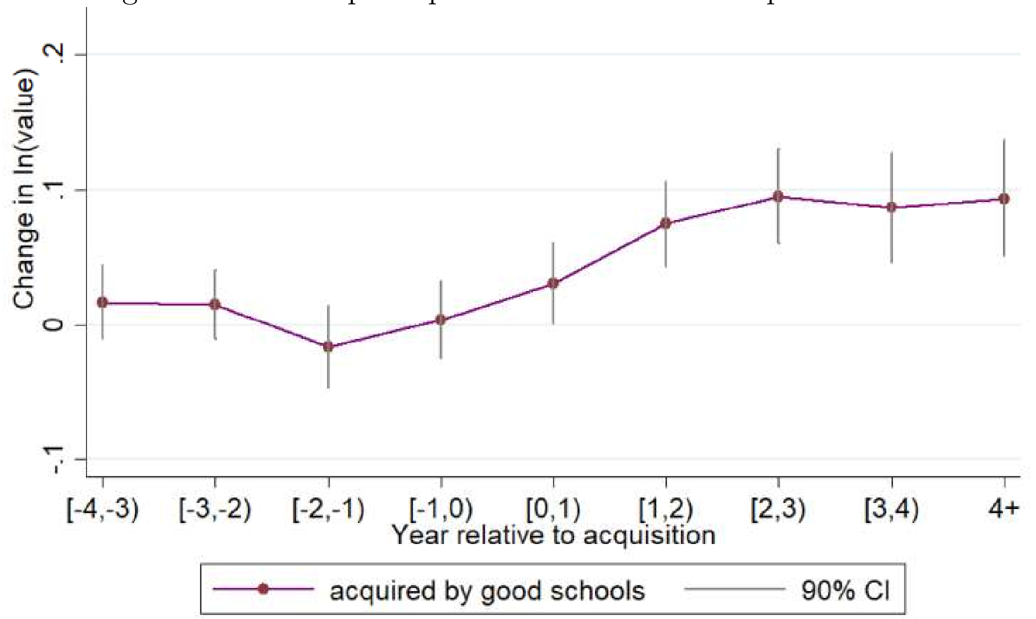
Overall, across model specifications and estimation samples, the average price effect of school acquisitions remains qualitatively and quantitatively stable at about 7%. Since an average apartment sells for 4 million yuan, this translates into a price premium of $280,000$ yuan (or over $40,000$ US dollars), which is both statistically and economically significant.

5.2 Pretreatment price trends

The validity of our DID method depends critically on the common trend assumption. Here we estimate (2) to examine whether any significant difference exists in the price trends between the treated group and the control group prior to the treatment. Table 4 reports the coefficient δ for the interaction term $g_n \cdot T$, estimated using our preferred model, namely, the main specification (flexible controls) with the neighborhood fixed effects and year-month fixed effects. Note that the structure of Table 4 mimics that of Table 3; however, since the pre-treatment sample excludes all post-acquisition observations, the sample size in each column is smaller in Table 4 than that in Table 3.

The results support the common trend assumption. Comparing regular schools that are later acquired by good schools with those that are not acquired, we find no significant difference in the annual price trends before each acquisition takes place. That is, up to the date of the announced school acquisition, the price behavior in the catchment areas of the treated schools is not distinguishable from the price behavior in the catchment areas of the control schools. Thus, from the perspective of the real estate resale market, the assignment of the treatment (i.e., which regular school is to be acquired) is both random and unanticipated, and the control schools serve as a good counter-factual for the treated schools.

Figure 3: Intertemporal price effects of school acquisitions



5.3 Heterogeneous effects

For the event analysis, we estimate a sequence of the annual price differences between the treated schools and the control schools. More specifically, using the period of more than four years prior to acquisition as the default group, we separately estimate the price effects for the period three to four years prior, two to three years prior, one to two years prior, and so forth, all the way up to four years or more post acquisition. Table 5 reports the sequence of parameters γ_k as described in (3), estimated using our preferred model, and the structure of Table 5 again mimics that of Table 3. For visual illustration, in Figure 3 we graph the point estimates and the corresponding 90% confidence intervals reported in column 1 of Table 5. Similar graphs can be produced from the estimation results reported in other columns. As a falsification test, it is reassuring to see that all coefficients for the periods prior to acquisition are insignificant. This corroborates our pre-treatment analysis and lends additional support to the common trend assumption. Regarding the post-acquisition periods, while the price effect within the first year is relatively small and only marginally significant, the price effects for all periods after the first year are highly significant at 1% level. Moreover, except for the first year, the treatment effects do not appear to vary strongly over time.

Next, to detect free riding, we estimate the price effects of school acquisitions by differently sized apartments. More specifically, using large sized apartments (over 90 square meters) as the default group, we separately estimate the price effects for small (up to 50 square meters) and medium-sized (50–90 square meters) apartments. Panel A of Table 6 reports the potentially heterogeneous price effects g_b as described in (4). We do not find any significant difference in the price effects by differently sized apartments.[25] This result indicates that the free-riding incentive does not play a significant role in individuals' purchasing decisions, which we attribute to two causes. On the supply side, the size of an apartment is predetermined and cannot be subdivided into smaller units, so the opportunity to free ride is limited by the available supply of small units.[26] On the demand side, since it is very costly for households to buy more than one apartment (e.g., one for schooling and the other for living), the potential benefit of free riding is counter-balanced by the cost of crowded living conditions. Overall, we find no evidence that school acquisitions lead to disproportionately high price increases on small or medium sized apartments.

Furthermore, we estimate the price effects for different types of acquisitions, as described in (5). Panel B of Table 6 reports the estimated price effects for fully integrated versus partially integrated acquisitions, with an added test for the statistical significance of the difference between the two. We find that while both types of acquisitions lead to significantly positive price effects, the price premium is larger for fully integrated acquisitions ($9.5 - -10.0\%$) than for partially integrated acquisitions ($5.1 - -5.9\%$), and the difference between them is significant at the 5% level. As expected, regular schools benefit more in fully integrated acquisitions, since educational resources are fully equalized between the acquiring and the acquired schools.

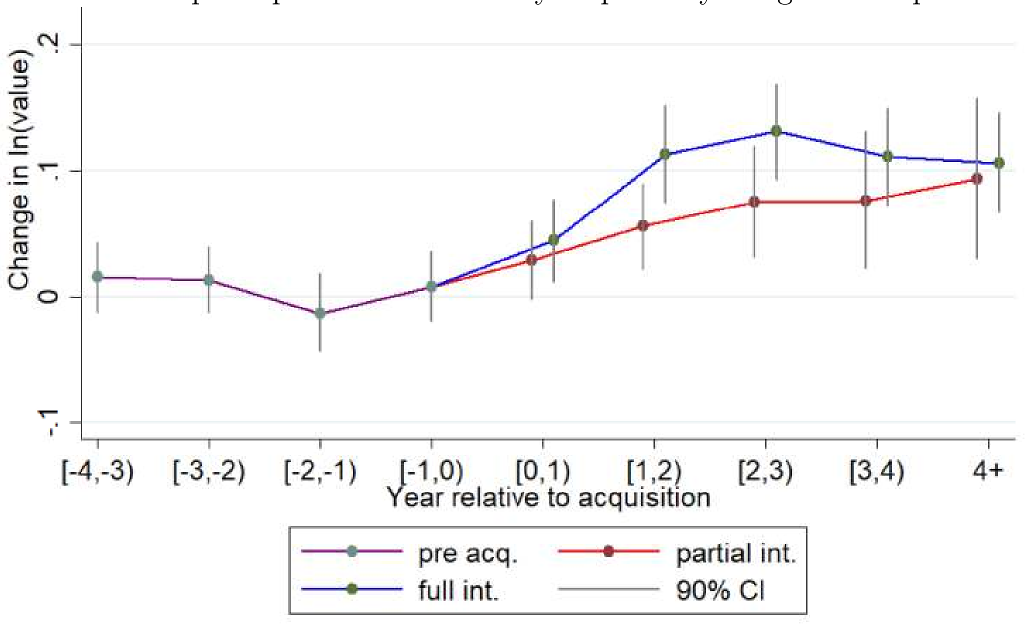
Panel C of Table 6 reports the estimated price effects for horizontal versus vertical acquisitions and includes a test for the statistical significance of the difference between the two. We find that while both horizontal and vertical acquisitions lead to highly significant price premiums, the price effect is smaller for horizontal acquisitions ($5.5 - -6.4\%$) than for vertical acquisitions ($9.6 - -10.1\%$), and the difference is significant at least at the 10% level.

Given the significantly different price effects for different types of acquisitions, it is natural to ask *why* local governments decide that certain acquisitions would be fully integrated while others would be only partially integrated, and why certain acquisitions would be horizontal while others would be vertical. Even though we cannot find any document evidence about the government's internal decision making process, our empirical analysis sheds some light on this issue. In particular, we extend the event analysis approach to account for the heterogeneous price effects

[25] We have also directly interacted the variable of apartment size with the school acquisition dummy, instead of dividing the apartment size into subgroups. The coefficient on the interaction term is also insignificant.

[26] In our sample, only 16% of observations are units at or below 50 square meters in size, 4% are at or below 40 square meters, and a mere 0.6% are at or below 30 square meters.

Figure 4: Intertemporal price effects for fully v. partially integrated acquisitions

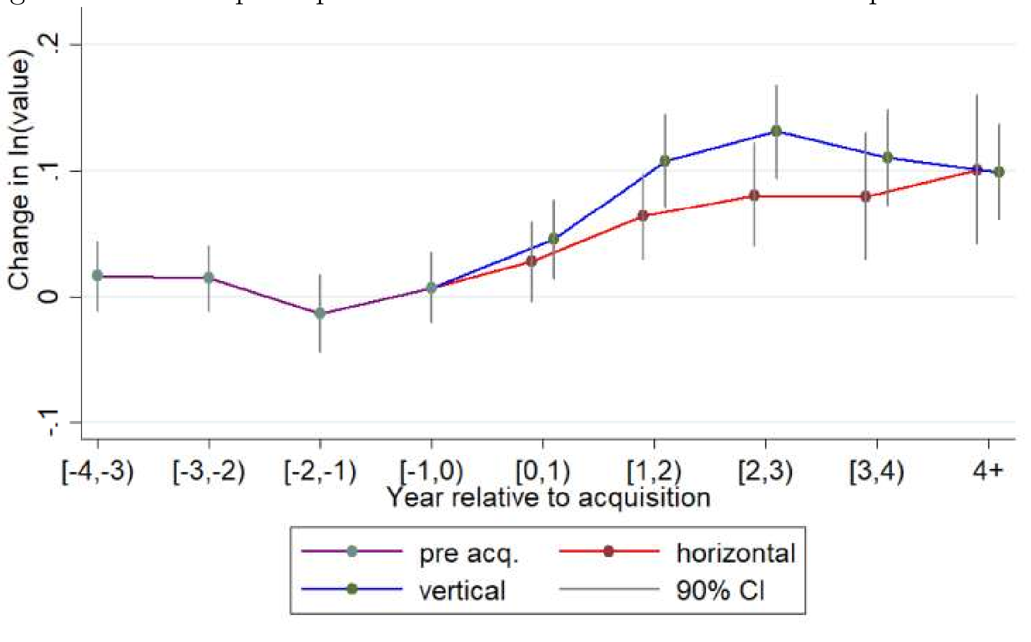


of different types of acquisitions. Table 7 reports the intertemporal price effects for fully versus partially integrated acquisitions.

For visual illustration, in Figure 4 we also graph the point estimates and the corresponding 90% confidence intervals reported in column 1 of Table 7, where two point estimates for the same post-acquisition period are offset slightly to avoid potentially overlapping confidence intervals. Similar graphs can be produced from the estimation results reported in other columns. It is interesting to see the price dynamics for the two types of acquisitions. Within the first year post acquisition, both price effects are small, and their difference is not significantly different from zero (each confidence interval covers the other point estimate). However, in the second and third year post acquisition, while partially integrated acquisitions exhibit slow and steady price increases, fully integrated acquisitions exhibit a sharp jump (and even over-shooting). As a result, the gap between the two price effects widens and becomes significant. Nevertheless, such a significant difference does not persist. In the fourth year post acquisition and afterward, partially integrated acquisitions continue to generate slow and steady price increases, while fully integrated acquisitions result in slight price decreases from the over-shooting peak. As a result, the gap between the two price effects narrows and becomes insignificant.

Similarly, Table 8 reports the intertemporal price effects for horizontal versus vertical acquisitions. For visual illustration, in Figure 5 we also graph the point estimates and the corresponding 90% confidence intervals reported in column 1 of Table 8. The price dynamics for the two types of acquisitions exhibit similar pat-

Figure 5: Intertemporal price effects for horizontal v. vertical acquisitions



terns. Within the first year post acquisition, both price effects are small and their difference is insignificant. In the second and third year post acquisition, while horizontal acquisitions result in slow and steady price increases, vertical acquisitions result in a sharp jump, so the gap between the two widens and becomes significant. In the fourth year post acquisition and afterward, horizontal acquisitions continue to generate slow and steady price increases, while vertical acquisitions result in slight price decreases from the over-shooting peak. The gap between the two narrows and becomes insignificant.

Overall, different types of acquisitions appear to converge to a common "steady state" price effect at about 10%, despite their different rates of convergence. Thus, it is plausible that local governments choose different types of acquisitions because the bureaucrats in charge face different planning horizons, i.e., some want to see immediate effects (in two or three years) while others can wait longer. Regardless of differences in the transitional phase, the long-run outcomes of all types of acquisitions appear indistinguishable from one another.

6 Conclusion

As an investment into the human capital of their children, parents are willing to pay for better quality public schools in the form of higher residential housing prices. In this paper, we take advantage of a quasi-experimental policy in Beijing—

government-sanctioned school acquisitions—to estimate individuals' willingness to pay for better schools. Using a hedonic pricing model embedded in the DID framework, we find that the acquisition of a regular school by a good school increases the average price of apartments within its catchment area by 7%, relative to apartments in areas with schools that are not acquired. We find no evidence of free riding; that is, the apartment size does not influence the price premium (in percentage terms) of school acquisitions. On the other hand, we do find differential price effects for different types of acquisitions: on average, the price effect is larger for fully integrated than for partially integrated acquisitions, and larger for vertical than for horizontal acquisitions. However, these differences are only significant in the first three years post acquisition, and become insignificant in the long run.

These findings deserve some discussion in a broader policy context. First, we do not have conclusive answer to the flip side of our question, namely whether there is a price penalty on good schools that acquire regular schools. In Appendix B. we conduct a mirroring DID analysis on good elementary schools and find no significant price penalty on the acquiring schools. However, as the common trend assumption fails to hold in the pre-treatment analysis in that context, we cannot interpret the DID result at face value due to potential selection biases. A complete understanding of the welfare effects of government-sanctioned school acquisitions will depend on a convincing identification of the price effect on the acquiring schools, where the absence of a significant price penalty would suggest that school acquisitions do not diminish the quality of the acquiring schools and hence increase total welfare, whereas a large penalty would suggest that the welfare effect is largely redistributive.

Second, despite their different price effects in the short run, different types of acquisitions seem equally capable of achieving the stated policy objective to reduce educational inequality in the long run. Within the first three years post acquisition, acquired schools appear to benefit more from fully integrated rather than from partially integrated acquisitions, and more from vertical rather than from horizontal acquisitions. However, after the first three years, acquired schools appear to benefit equally regardless of the particular acquisition type. Overall, the difference during the transitional phase and the convergence to a common "steady state" are indicative of the planning horizon faced by different bureaucrats. While the impatient ones may opt for acquisition types with results that are more immediate (e.g., fully integrated acquisitions or vertical acquisitions) but also more organizationally disruptive, the patient ones may opt for acquisition types with results that are more gradual (e.g., partially integrated acquisitions or horizontal acquisitions) but less disruptive.

Finally, to curb the considerable price premiums for apartments in the catchment area of good schools, some local governments have begun to change the rules that govern enrollment eligibility. For example, in Haidian (one of the four core districts in our sample), starting on January 1, 2019 residence in a given school's catchment area no longer guarantees enrollment eligibility in that school. Instead, similar to the rules used for middle schools, multiple elementary schools are designated as a bloc with a collective catchment area, and all students within this collective area are randomly assigned to one of the schools by lottery. This reform would equalize educational resources *ex ante*, even though there will still be educational inequality *ex post*. In contrast, government-sanctioned school acquisitions aim to reduce educational inequality both *ex ante* and *ex post*. With additional data, real estate price effects under both regimes could be compared, with the result shedding light on parents' risk attitudes regarding educational inequality.

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Appendix A.

Table 1: No. of regular schools acquired in the four core districts of Beijing

Year	Acquired	Full integration	Partial integration	Horizontal	Vertical
2011[a]	1	1	0	1	0
2012	8	1	7	7	1
2013	1	1	0	0	1
2014	26	20	6[b]	14	12
2015	13	4	9[c]	11	2
2016	6	2[b]	4	5	1
2017	1	1[c]	0	1	0
2018	1	0	1	1	0
Total	57	30	27	40	17

Notes: a. Data for 2011 starts in September, 2011.
b. A school was first acquired in 2014 through a partial integration and later changed in 2016 to full integration. It is included in the 2014 but not the 2016 count.
c. A school was first acquired in 2015 through a partial integration and later changed in 2017 to full integration. It is included in the 2015 but not the 2017 count.

Table 2: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min.	Max.
value (,000)	161,145	4,004	2,638	100	181,300
price (,000)	161,145	49.8	19.8	5	156
age	161,145	20.5	12.8	1	78
size (m^2)	161,145	81.5	38.9	7.4	1,745.5
No. bdrms	161,145	1.94	0.77	0	9
No. bthrms	161,145	1.19	0.44	0	9
acquired	161,145	0.134	0.341	0	1
full int.	161,145	0.054	0.226	0	1
partial int.	161,145	0.080	0.271	0	1
horizontal	161,145	0.089	0.285	0	1
vertical	161,145	0.045	0.207	0	1

Table 3: Average price effect when regular schools are acquired by good schools

ln(value)	(1)	(2)	(3)	(4)	(5)	(6)
A. Main specification, neighborhood fixed effects						
Acquired	0.067***	0.069***	0.069***	0.070***	0.070***	0.075***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
Adj. R-sq	0.947	0.942	0.942	0.942	0.942	0.941
B. Parsimonious specification, neighborhood fixed effects						
Acquired	0.066***	0.068***	0.068***	0.069***	0.069***	0.075***
	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Adj. R-sq	0.949	0.941	0.941	0.941	0.941	0.940
C. No controls, neighborhood fixed effects						
Acquired	0.067***	0.068***	0.068***	0.068***	0.069***	0.075***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
Adj. R-sq	0.746	0.732	0.732	0.732	0.731	0.729
D. Main specification, community fixed effects						
Acquired	0.065***	0.068***	0.068***	0.068***	0.068***	0.062***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Adj. R-sq	0.905	0.901	0.901	0.901	0.902	0.900
No. neighborhoods	1,880	1,787	1,784	1,763	1,714	1,625
Observations	161,145	148,889	148,852	147,444	144,730	131,676

Notes: 1. Year-month fixed effects are included in all regressions.
2. The main specification includes third degree polynomials of the apartment size, third degree polynomials of the building age, the full set of dummies on the number of bedrooms, and the full set of dummies on the number of bathrooms as controls. The parsimonious specification includes ln(size), ln(age), number of bedrooms, and number of bathrooms as controls.
3. For estimation, column 1 uses all observations, column 2 excludes observations with the apartment size over 140 square meters, column 3 further excludes neighborhoods whose school quality change is due to redistricting instead of school acquisition, and columns 4–6 further exclude neighborhoods that are more than 20, 18, and 16 kilometers away from the city center respectively.
4. Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table 4: Pre-treatment analysis: difference in annual trends

ln(value)	(1)	(2)	(3)	(4)	(5)	(6)
Treated group · T	0.004	0.004	0.004	0.003	0.002	0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Adj. R-sq	0.949	0.944	0.944	0.944	0.944	0.943
No. neighborhoods	1,880	1,781	1,778	1,757	1,708	1,619
Observations	139,573	128,638	128,622	127,261	124,595	112,524

Notes: 1. Year-month fixed effects are included in all regressions.
2. Reported results are for the main specification with neighborhood fixed effects, including third degree polynomials of the apartment size, third degree polynomials of the building age, the full set of dummies on the number of bedrooms, and the full set of dummies on the number of bathrooms as controls.
3. For estimation, column 1 uses all observations, column 2 excludes observations with the apartment size over 140 square meters, column 3 further excludes neighborhoods whose school quality change is due to redistricting instead of school acquisition, and columns 4–6 further exclude neighborhoods that are more than 20, 18, and 16 kilometers away from the city center respectively.
4. Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table 5: Intertemporal price effects of school acquisitions

ln(value)	(1)	(2)	(3)	(4)	(5)	(6)
3-4 years prior	0.017	0.019	0.019	0.019	0.015	0.019
	(0.017)	(0.017)	(0.017)	(0.017)	(0.028)	(0.028)
2-3 years prior	0.015	0.019	0.019	0.020	0.015	0.019
	(0.016)	(0.016)	(0.016)	(0.016)	(0.027)	(0.028)
1-2 years prior	–0.016	–0.017	–0.016	–0.016	–0.025	–0.021
	(0.018)	(0.019)	(0.019)	(0.019)	(0.029)	(0.030)
0-1 year prior	0.004	0.004	0.004	0.004	–0.003	0.002
	(0.017)	(0.018)	(0.018)	(0.018)	(0.028)	(0.029)
0-1 year post	0.030*	0.033*	0.033*	0.034*	0.026	0.034
	(0.018)	(0.019)	(0.019)	(0.019)	(0.029)	(0.030)
1-2 years post	0.074***	0.078***	0.078***	0.079***	0.072**	0.081***
	(0.019)	(0.020)	(0.020)	(0.020)	(0.029)	(0.030)
2-3 years post	0.095***	0.097***	0.097***	0.098***	0.091***	0.104***
	(0.021)	(0.022)	(0.022)	(0.022)	(0.031)	(0.031)
3-4 years post	0.086***	0.088***	0.088***	0.088***	0.082**	0.093***
	(0.024)	(0.025)	(0.025)	(0.025)	(0.033)	(0.034)
4+ years post	0.094***	0.097***	0.097***	0.097***	0.091***	0.101***
	(0.026)	(0.027)	(0.027)	(0.027)	(0.034)	(0.036)

Notes: 1. Year-month fixed effects are included in all regressions.
2. Reported results are for the main specification with neighborhood fixed effects, including third degree polynomials of the apartment size, third degree polynomials of the building age, the full set of dummies on the number of bedrooms, and the full set of dummies on the number of bathrooms as controls.
3. For estimation, column 1 uses all observations, column 2 excludes observations with the apartment size over 140 square meters, column 3 further excludes neighborhoods whose school quality change is due to redistricting instead of school acquisition, and columns 4–6 further exclude neighborhoods that are more than 20, 18, and 16 kilometers away from the city center respectively.
4. Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table 6: Heterogeneous price effects

ln(value)	(1)	(2)	(3)	(4)	(5)	(6)
A. Apartment size						
Acquired	0.062***	0.074***	0.075***	0.074***	0.074***	0.078***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
Acquired*($size \leq 50$)	–0.002	0.014	0.014	0.015	0.016	0.018
	(0.019)	(0.017)	(0.017)	(0.017)	(0.017)	(0.018)
Acquired*($50 < size \leq 90$)	0.010	–0.015	–0.015	–0.015	–0.015	–0.013
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)
B. Fully vs. partially integrated acquisitions						
Full int.	0.096***	0.095***	0.096***	0.096***	0.096***	0.100***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Partial int.	0.051***	0.054***	0.054***	0.054***	0.054***	0.059***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)
difference	–0.045**	–0.042**	–0.042**	–0.042**	–0.042**	–0.041**
	(0.019)	(0.020)	(0.020)	(0.020)	(0.020)	(0.021)
C. Horizontal vs. vertical acquisitions						
Horizontal acq.	0.055***	0.058***	0.058***	0.058***	0.059***	0.064***
	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)
Vertical acq.	0.097***	0.096***	0.096***	0.096***	0.096***	0.101***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
difference	0.041**	0.038**	0.038**	0.038**	0.038**	0.037*
	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)

Notes: 1. Year-month fixed effects are included in all regressions.
2. Reported results are for the main specification with neighborhood fixed effects, including third degree polynomials of the apartment size, third degree polynomials of the building age, the full set of dummies on the number of bedrooms, and the full set of dummies on the number of bathrooms as controls.
3. For estimation, column 1 uses all observations, column 2 excludes observations with the apartment size over 140 square meters, column 3 further excludes neighborhoods whose school quality change is due to redistricting instead of school acquisition, and columns 4–6 further exclude neighborhoods that are more than 20, 18, and 16 kilometers away from the city center respectively.
4. Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table 7: Intertemporal price effects for fully vs. partially integrated acquisitions

ln(value)	(1)	(2)	(3)	(4)	(5)	(6)
3-4 years prior	0.015	0.018	0.018	0.018	0.014	0.017
	(0.017)	(0.017)	(0.017)	(0.017)	(0.028)	(0.028)
2-3 years prior	0.013	0.018	0.017	0.018	0.013	0.017
	(0.016)	(0.016)	(0.016)	(0.016)	(0.027)	(0.027)
1-2 years prior	–0.013	–0.014	–0.013	–0.014	–0.022	–0.018
	(0.019)	(0.020)	(0.020)	(0.020)	(0.029)	(0.030)
0-1 year prior	0.007	0.007	0.007	0.007	–0.000	0.006
	(0.017)	(0.018)	(0.018)	(0.018)	(0.028)	(0.029)
0-1 year post partial int.	0.029	0.033*	0.033*	0.033*	0.026	0.033
	(0.019)	(0.019)	(0.019)	(0.019)	(0.029)	(0.030)
1-2 years post partial int.	0.056***	0.060***	0.060***	0.060***	0.053*	0.063**
	(0.020)	(0.021)	(0.021)	(0.021)	(0.030)	(0.031)
2-3 years post partial int.	0.075***	0.078***	0.078***	0.079***	0.073**	0.086**
	(0.026)	(0.027)	(0.027)	(0.027)	(0.035)	(0.035)
3-4 years post partial int.	0.076**	0.078**	0.078**	0.078**	0.072*	0.083**
	(0.033)	(0.034)	(0.034)	(0.034)	(0.040)	(0.042)
4+ years post partial int.	0.094**	0.098**	0.098**	0.099**	0.092**	0.106**
	(0.038)	(0.040)	(0.040)	(0.040)	(0.045)	(0.051)
0-1 year post full int.	0.044**	0.044**	0.044**	0.044**	0.036	0.044
	(0.020)	(0.021)	(0.021)	(0.021)	(0.030)	(0.031)
1-2 years post full int.	0.113***	0.114***	0.115***	0.115***	0.108***	0.116***
	(0.023)	(0.024)	(0.024)	(0.024)	(0.032)	(0.033)
2-3 years post full int.	0.131***	0.131***	0.131***	0.131***	0.124***	0.135***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.032)	(0.033)
3-4 years post full int.	0.111***	0.111***	0.112***	0.111***	0.105***	0.115***
	(0.023)	(0.024)	(0.024)	(0.024)	(0.032)	(0.033)
4+ years post full int.	0.106***	0.105***	0.106***	0.106***	0.099***	0.110***
	(0.024)	(0.025)	(0.025)	(0.025)	(0.033)	(0.034)

Notes: 1. Year-month fixed effects are included in all regressions.
2. Reported results are for the main specification with neighborhood fixed effects, including third degree polynomials of the apartment size, third degree polynomials of the building age, the full set of dummies on the number of bedrooms, and the full set of dummies on the number of bathrooms as controls.
3. For estimation, column 1 uses all observations, column 2 excludes observations with the apartment size over 140 square meters, column 3 further excludes neighborhoods whose school quality change is due to redistricting instead of school acquisition, and columns 4–6 further exclude neighborhoods that are more than 20, 18, and 16 kilometers away from the city center respectively.
4. Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table 8: Intertemporal price effects for horizontal vs. vertical acquisitions

ln(value)	(1)	(2)	(3)	(4)	(5)	(6)
3-4 years prior	0.016	0.019	0.019	0.018	0.015	0.018
	(0.017)	(0.017)	(0.017)	(0.017)	(0.028)	(0.028)
2-3 years prior	0.014	0.019	0.019	0.020	0.015	0.019
	(0.016)	(0.016)	(0.016)	(0.016)	(0.027)	(0.028)
1-2 years prior	–0.014	–0.014	–0.014	–0.014	–0.023	–0.018
	(0.019)	(0.019)	(0.019)	(0.019)	(0.029)	(0.030)
0-1 year prior	0.007	0.007	0.007	0.006	–0.000	0.005
	(0.017)	(0.018)	(0.018)	(0.018)	(0.028)	(0.029)
0-1 year post hor. acq.	0.027	0.031	0.031	0.031	0.024	0.032
	(0.019)	(0.019)	(0.019)	(0.019)	(0.029)	(0.030)
1-2 years post hor. acq.	0.063***	0.067***	0.068***	0.068***	0.061**	0.071**
	(0.020)	(0.021)	(0.021)	(0.021)	(0.030)	(0.031)
2-3 years post hor. acq.	0.081***	0.084***	0.084***	0.085***	0.078**	0.091***
	(0.025)	(0.025)	(0.025)	(0.025)	(0.033)	(0.034)
3-4 years post hor. acq.	0.079***	0.082***	0.082***	0.082***	0.076**	0.086**
	(0.031)	(0.032)	(0.032)	(0.032)	(0.038)	(0.040)
4+ years post hor. acq.	0.101***	0.107***	0.107***	0.107***	0.101**	0.116**
	(0.036)	(0.037)	(0.037)	(0.037)	(0.043)	(0.048)
0-1 year post ver. acq.	0.045**	0.045**	0.045**	0.046**	0.038	0.046
	(0.019)	(0.020)	(0.020)	(0.020)	(0.029)	(0.030)
1-2 years post ver. acq.	0.108***	0.110***	0.110***	0.110***	0.103***	0.111***
	(0.022)	(0.023)	(0.023)	(0.023)	(0.032)	(0.032)
2-3 years post ver. acq.	0.131***	0.131***	0.131***	0.131***	0.125***	0.135***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.032)	(0.033)
3-4 years post ver. acq.	0.110***	0.110***	0.110***	0.110***	0.103***	0.114***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.032)	(0.033)
4+ years post ver. acq.	0.099***	0.096***	0.097***	0.096***	0.090***	0.101***
	(0.023)	(0.024)	(0.024)	(0.024)	(0.033)	(0.033)

Notes: 1. Year-month fixed effects are included in all regressions.
2. Reported results are for the main specification with neighborhood fixed effects, including third degree polynomials of the apartment size, third degree polynomials of the building age, the full set of dummies on the number of bedrooms, and the full set of dummies on the number of bathrooms as controls.
3. For estimation, column 1 uses all observations, column 2 excludes observations with the apartment size over 140 square meters, column 3 further excludes neighborhoods whose school quality change is due to redistricting instead of school acquisition, and columns 4–6 further exclude neighborhoods that are more than 20, 18, and 16 kilometers away from the city center respectively.
4. Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Appendix B.

In this Appendix, we consider a mirroring DID analysis using neighborhoods that have always had good elementary schools. More specifically, we estimate a model similar to (1), but the acquisition dummy A_{nt} is defined from the acquiring school's perspective. More specifically, for a good elementary school whose catchment area includes neighborhood n, the dummy takes the value 1 if this school has acquired a regular school in period t, and 0 otherwise. Thus, the treated schools are good schools that have acquired regular schools during the sample period, and control schools are good schools that have not. This DID model allows us to examine the resource dilution effect, if any, on the acquiring schools.

The validity of such DID analysis also relies on the common trend assumption, now tested among good schools. We estimate the pre-treatment model similar to (2), where the treated group $g_n = 1$ consists of all good elementary schools that later acquired regular schools, and the control group $g_n = 0$ consists of all good elementary schools that did not acquire regular schools. Again the coefficient δ associated with the interaction term $g_n \cdot T$ captures the difference in the annual price trends between the two groups *before* each acquisition takes place. A significant estimate of δ would violate the common trend assumption and hence suggest potential selection bias.

Recall that in the last paragraph of our sample selection process (Section 4.3), we divide the final sample into two sub-samples: one corresponding to neighborhoods with regular elementary schools at the beginning of the sample period (60% of the observations), and the other corresponding to neighborhoods with good elementary schools (40% of the observations). The first sub-sample is used for our main analysis. Here the second sub-sample is used for the mirroring DID analysis, with the summary statistics reported in Table B1. Note that the average apartment price is about a quarter higher here than in the first sub-sample, reflecting both the difference in school quality and other unobserved neighborhood heterogeneity.

The average treatment effect is reported in Table B2. Parallel to the structure of Table 3 in the main text, we estimate the model using both the main specification and the parsimonious specification (corresponding to Panels A and B), together with the neighborhoods fixed effects and year-month fixed effects. For each model specification, column 1 is estimated using all observations, and column 2 excludes apartments larger than 140 square meters.[27] The coefficient of interest is insignificant across model specifications and estimation samples. Taken at face value, the result seems to suggest that good schools do not suffer any significant price penalty after they acquire regular schools.

[27] Further sample exclusions of redistricted neighborhoods and neighborhoods farther away from the city center (corresponding to columns 3–6) are not applicable here.

However, the validity of the DID result depends on the common trend assumption, which fails to hold in this case. The pre-treatment analysis result is reported in Table B3, which follows the structure of Table B2. We find that for the treated group, the annual price trend is 2% lower than for the control group *before* local governments mandate them to acquire regular schools. This trend difference is both statistically and economically significant, translating into roughly $100,000$ yuan less in appreciation in value every year when an average apartment sells for five million yuan.

The significant difference in the annual trends between the treated group and the control group clearly violates the common trend assumption. It could arise as a result of local governments' selection bias. If "less popular" good schools are systematically chosen to acquire regular schools, this could contribute to the lower annual trends in the pre-treatment analysis. Moreover, as the control group would give an upward biased estimate of the counter-factual outcome for the treated group, the DID result would be downward biased. Alternatively, the trend difference could arise if individuals somehow anticipate that these good schools would acquire regular schools later, and their pre-treatment prices had already factored in the anticipated resource dilution effect before the announcements of these acquisitions. The anticipatory price change could also contribute to the lower annual trends in the pre-treatment analysis, but the DID result would be upward biased. Without further evidence we cannot pinpoint the exact reason for the non-parallel trends. Since different reasons would imply opposite biases, we caution against interpreting the DID result at face value.

Table B1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min.	Max.
value (,000)	106,873	4,944	3,137	110	57,000
price (,000)	106,873	63.6	26.2	5	150
age	106,873	25.4	14.8	1	113
size (m^2)	106,873	79.2	37.8	7.8	640
No. bdrms	106,873	2.02	0.77	0	9
No. bthrms	106,873	1.16	0.42	0	7
acquiring	106,873	0.10	0.30	0	1

Table B2: Average price effect when good schools acquire regular schools

	Main specification		Parsimonious specification	
ln(value)	(1)	(2)	(1)	(2)
Acquiring	–0.012	–0.012	–0.010	–0.011
	(0.008)	(0.008)	(0.008)	(0.008)
Adj. R-sq	0.940	0.931	0.940	0.930
No. neighborhoods	1,692	1,631	1,692	1,631
Observations	106,873	98,991	106,873	98,991

Notes: 1. Year-month fixed effects and neighborhood fixed effects are included in all regressions.
2. The main specification includes third degree polynomials of the apartment size, third degree polynomials of the building age, the full set of dummies on the number of bedrooms, and the full set of dummies on the number of bathrooms as controls. The parsimonious specification includes ln(size), ln(age), number of bedrooms, and number of bathrooms as controls.
3. For estimation, column 1 uses all observations, and column 2 excludes observations with the apartment size over 140 square meters.
4. Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table B3: Pre-treatment analysis: difference in annual trends

	Main specification		Parsimonious specification	
ln(value)	(1)	(2)	(1)	(2)
Treated group · T	–0.019**	–0.018*	–0.017**	–0.017*
	(0.009)	(0.009)	(0.009)	(0.009)
Adj. R-sq	0.939	0.931	0.939	0.930
No. neighborhoods	1,645	1,582	1,645	1,582
Observations	96,222	89,438	96,222	89,438

Notes: 1. Year-month fixed effects and neighborhood fixed effects are included in all regressions.
2. The main specification includes third degree polynomials of the apartment size, third degree polynomials of the building age, the full set of dummies on the number of bedrooms, and the full set of dummies on the number of bathrooms as controls. The parsimonious specification includes ln(size), ln(age), number of bedrooms, and number of bathrooms as controls.
3. For estimation, column 1 uses all observations, and column 2 excludes observations with the apartment size over 140 square meters.
4. Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.