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Valuing Elementary Schools: Evidence from Public School Acquisitions in Beijing

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Abstract

We utilize government-sanctioned public school acquisitions in Beijing to estimate individuals' willingness to pay for enrollment eligibility in sought-after 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 sought-after schools to those that are not, we find an average price increase of 7% for apartments in the catchment areas of acquired schools. This percentage increase is both statistically and economically significant, and it does not vary by the size of the apartment. We also find heterogeneous price effects for different types of acquisitions, defined by their post-acquisition organizational structures, but these differences are not statistically significant.

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 enrollment eligibility in sought-after elementary schools. To do so, we utilize government-sanctioned school acquisitions in Beijing as a quasi-natural experiment. An acquisition is a merger between two public schools: one is highly regarded and sought-after among parents, colloquially known as a "good" school, and the other is a "regular" school. (In this paper, we will use the terms sought-after schools and good schools interchangeably.) Local governments promote such acquisitions 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 transaction data, we can estimate a hedonic pricing model in the difference-in-difference (DID) framework. The treatment effect, i.e., the average price increase due to school acquisitions, informs us of individuals' willingness to pay for enrollment eligibility in sought-after 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 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 advantage of avoiding reverse causation. In our

[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 al2015), to name just a few.

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

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

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.

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 for identification. Both the spatial and the temporal variations in school acquisitions allow 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.

At the same time, we also acknowledge the limitation of our data. We do not have data to directly measure school quality itself. Instead, we rely on the reputation label, i.e., whether a school is highly regarded and sought-after among parents (known as "good") or not, as an indirect measure. As a result, we cannot separate the school quality effect from the school reputation effect, both of which may change when regular schools are acquired by the sought-after schools.

[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).

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 increase of 7% for regular schools acquired by good schools, compared to those that are not acquired. This price premium is both statistically and economically significant, translating into 280,000 Chinese Yuan (CNY) on average, or over 40,000 U.S. Dollars (USD).[5] It is also robust to alternative model specifications.

To put this in context, our finding is broadly consistent with what the literature has found. Depending on what measures are used for school quality or school reputation, there is a wide range of estimates in the literature. On the one hand, when test scores are used to measure school quality, one standard deviation increase in school performance is found to increase housing prices by 1–4% (e.g., Black 1999, Fack and Grenet 2010, Ries and Somerville 2010, Collins and Kaplan 2017). On the other hand, other measures for school reputation have been found to generate much larger price effects. For example, Figlio and Lucas (2004) find that schools receiving a government rating of "A" have a 19.5% increase in house prices relative to those receiving a rating of "B", and this effect is above and beyond the price effect due to test score differences across schools. Similarly, Chan et al (2020) find a 14% price premium for tournament superstar schools, i.e., schools performing above the 90th percentile in highly competitive tournaments (instead of standardized tests). Our estimate of a 7% price effect falls within this range and is consistent with school acquisitions improving both the quality and the reputation of acquired schools.

Besides estimating the average price effect, we also investigate whether there are heterogeneous price effects. We find that the price increase (in percentage terms) does not vary by apartment size, so individuals can obtain the same enrollment privilege for less (in absolute terms) if they buy smaller apartments. The presence of such benefits in the data implies that there must also be potential costs associated with buying smaller apartments, e.g., crowded living conditions if families with school-aged children were to live in smaller units.

Furthermore, we check for 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 economically meaningful differences: the price increase is larger for fully versus partially integrated acquisitions (9.6% vs. 5.2%), and larger for vertical versus horizon-

[5] During our sample period, the exchange rate is 6–7 CNY for 1 USD.

tal acquisitions (9.6% vs. 5.6%). However, these differences are not statistically significant.

The rest of the paper is organized as follows. In Section 2 we provide an institutional background on 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. Supplementary analyses are described and corresponding results are reported in the Appendix.

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.[6] 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.[7] 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 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 school to middle school to high school and, eventually, to college and university.

Public education was heavily disrupted during the Cultural Revolution, but came back in focus with the economic reform in China. 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

[6]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.

[7]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.

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 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 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.[8]

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. The administrative order takes effect immediately, either on the same day or the next day of the announcement.[9] Overall, these decisions

[8]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 enrollment eligibility for a given middle school.

[9]For our empirical analysis, we use the announcement date as the acquisition date, even though logistically it may take some time for the schools to complete the acquisition process.

are made bureaucratically and do not involve any public consultation,[10] and their announcements are not anticipated by the general public.[11]

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 quality of acquired schools, there is little concern about its impact on the acquiring schools. Similarly, school principals generally welcome such acquisitions, because as public servants, satisfactory implementation of these administrative orders can enhance their career advancement within the public school system. Overall, 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

[10] Lacking an understanding of the government's decision-making process, we cannot fully rule out the potential for endogeneity regarding how two schools are chosen for an acquisition. Instead, we rely on empirical evidence to test whether acquisition decisions exhibit selection biases.

[11] 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 for acquisition. 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.

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.

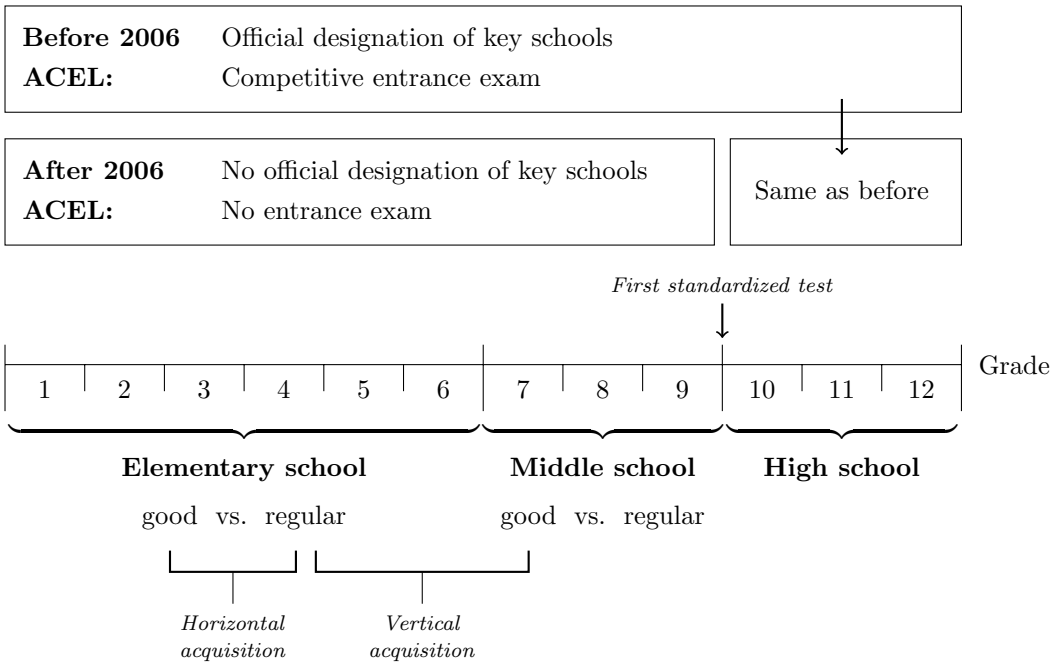
School acquisitions can also be categorized as horizontal or vertical depending on whether the acquiring school is an elementary or a middle school. Traditionally, elementary schools (grades 1–6) are stand-alone primary educational institutions, while middle schools (grades 7–9) and high schools (grades 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 have also experimented with the creation of integrated nine-year schools that cover the entire length of compulsory education. Nine-year schools also 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 (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.

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. Due to this lack of documentary 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.

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 not release school-level data to the public, even when they are internally available.[12] We thus must rely on the second-best measurement option, using 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

[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.

Figure 1: Primary and secondary educational institutions



we cannot distinguish the reputation effect from the schooling quality effect, when regular schools are acquired by good schools.

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.

First, 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. 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.

Note that for 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. Instead, in a supplementary analysis (see the Appendix), we separately consider a mirroring DID analysis using neighborhoods that have always had good schools. That analysis allows us to examine the resource dilution effect of school acquisitions on the acquiring schools, where the treated group consists of good schools that acquired regular schools, and the control group consists of good schools that did not acquire other schools.

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 conduct the following event analysis. 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 $k = \pm 1, \ \pm 2, \ \pm 3, \ ...$, 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 before and after the acquisition event:

$$p_{int} \ = \ \alpha_n \ + \ \beta_t \ + \ \Sigma_k \, \gamma_k A_{n,T+k} \ + \ \theta X_{int} \ + \ \epsilon_{int}. \tag{2}$$

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. 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 be a misleading summary. So understanding the intertemporal pattern of the post-acquisition price effects is important given the staggered treatment timing (acquisition date) in our setting.

Lastly, we explore potentially heterogeneous price effects along several dimensions. First, we investigate whether individuals preferentially purchase smaller (and hence cheaper) apartment units to gain the same school enrollment privileges as those purchasing larger units. If this is the case, we should see a disproportionately high price increase (in percentage terms) of school acquisitions on smaller apartments. We estimate the following model to examine whether the price effect varies by the size of the apartment:

$$p_{int} = \alpha_n + \beta_t + \gamma_1 A_{nt} * I(small) + \gamma_2 A_{nt} * I(large) + \theta X_{int} + \epsilon_{int}, \quad (3)$$

where $I(small)$ and $I(large)$ are indicator variables that equal one if the size of apartment unit i is small or large. We are interested in how γ_1 compares to γ_2.

Second, 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:

$$p_{int} = \alpha_n + \beta_t + \gamma_1 A1_{nt} + \gamma_2 A2_{nt} + \theta X_{int} + \epsilon_{int}, \quad (4)$$

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 (4) 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 transaction data

Since the hedonic pricing model relies on the assumption of a competitive market, where both sellers and buyers take the implicit price of 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 homeowners, they have little market power. This is quite different from the primary market where newly constructed housing units are transacted and

each developer, who 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 provided us with 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 reasons, Lianjia did 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 ownership title categories for apartment units. As urban land is owned by the state, strict regulations exist 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 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

After local governments stopped officially designating key elementary or middle schools, an unofficial list of "good" schools began circulating among interested parties in 2010. To the best of our knowledge, this list was initially compiled by "Wenfeng", a youth education expert in Beijing, to help parents make informed school choices when official data were lacking. The schools were chosen based on

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

Wenfeng's assessment of a wide range of factors, including historical performance, current funding conditions reflected in facilities, teacher quality and class size, close ties to government agencies or universities, the general teaching philosophy and school culture, and its reputation among local residents, etc.[14] This list has been widely shared as a useful guideline and reappears on different websites year after year. For our empirical analysis, we also rely on this list to categorize schools as being either sought-after (on the list) or regular (not on the list).

To track school acquisitions over time, we have compiled a 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 the acquisition (fully or partially integrated, horizontal or vertical).

To link schools to neighborhoods, we have collected admissions guides for all schools in the four core districts. Many of the admissions guides come from the same website `http://www.ysxiao.cn`, which cover all good schools, all regular schools after they are acquired and become sought-after among parents, and some (but not all) regular schools.[15] For the remaining schools, we collect their admissions guides through either online searches or in person visits to schools. A school's admissions guide explicitly lists all neighborhoods in its catchment area, including both their names and addresses. We then manually match each school to its corresponding neighborhoods, linking school acquisitions data to real estate transaction data.[16]

[14] As expected, this list includes all historical key schools, non-key schools that had been acquired by key schools previously, as well as other schools that improved their performance and reputation more recently.

[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 of each neighborhood within its catchment area. Such posted admissions guides are then collected by this website to meet popular demand. Except for a few instances, school catchment areas have remained stable over the years.

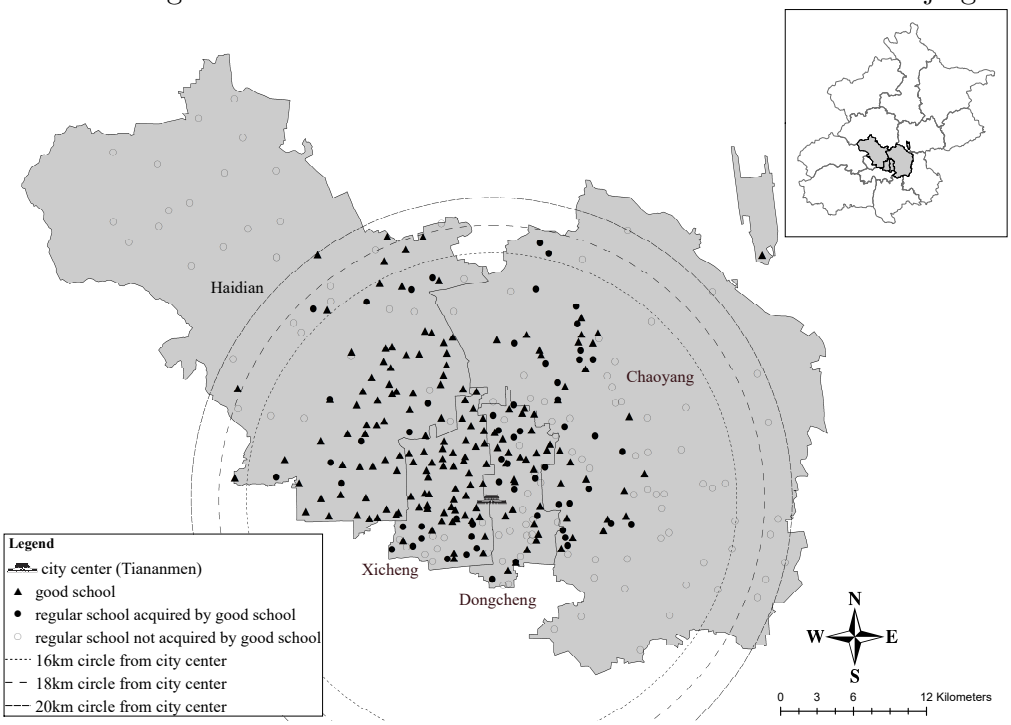
[16] Although neighborhoods are supposed to have their designated schools, there are a few exceptions. For example, if a developer skirted the rules and failed to build a new school for the neighborhood it developed, that neighborhood would not have a designated school. Residents in these neighborhoods can still send their children to school, provided that there is available capacity at one of the nearby regular schools. To account for this uncertainty, we create artificial school IDs for these neighborhoods.

4.3 Sample selection and summary statistics

After having merged the school data with the real estate transaction data, we have taken 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 to those in the core districts. Figure 2 shows the spatial locations of all schools within the four 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.

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



Moreover, 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] Table 1 shows the temporal variations of school acquisitions by year, with the majority of acquisitions taking place in 2014–2015.

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

Year	Acquired	Fully integrated	Partially integrated	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: This table summarizes the number of regular elementary schools acquired each year in the sample period. 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.

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 12.5% of the remaining sample, leaving us with 268,097 observations. Moreover, since we cannot distinguish arms-length transactions from sales among related parties, to minimize potential biases due to related sales, we also exclude transactions with very low unit prices. We set the cutoff for exclusion at 5,000 CNY per square meter.[19] This removes 320 observations. 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, the number of bedrooms, and the number of bathrooms). Removing these observations with missing values, we end up with a final sample of 267,441 observations.[20]

[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 tiny fraction of the transactions involves resale of designated parking spots or units in mixed-use buildings. As parking spots and units in mixed-use buildings 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 CNY 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.

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, 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 58% of the full sample, or 155,512 observations.[21] Table 2 reports the summary statistics.[22]

Table 2: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min.	Max.
Price (,000)	155,512	4,012	2,688	100	181,300
Price per m^2 (,000)	155,512	49.6	19.8	5	156
Size (m^2)	155,512	81.8	39.3	7.4	1,746
Building Age	155,512	20.4	12.7	1	78
No. of Bedrooms	155,512	1.94	0.77	0	9
No. of Bathrooms	155,512	1.19	0.44	0	9
Acquired	155,512	0.148	0.36	0	1
Fully Integrated	155,512	0.058	0.23	0	1
partially Integrated	155,512	0.090	0.29	0	1
Horizontal	155,512	0.099	0.30	0	1
Vertical	155,512	0.049	0.22	0	1

5 Estimation results

As both the acquisition dummy and the sales price 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). Accordingly, all reported standard errors are clustered at the school level.

Column 1 of Table 3 reports the average price effect for regular schools acquired by good schools. Apartment controls include third degree polynomials on the apartment size and the building age, as well as the full set of dummies on the number of bedrooms and the number of bathrooms. Together with the neighborhood fixed effects and year-month fixed effects, this is our main model.[23] Using

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

[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.

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

Table 3: Average price effect for the acquired schools

ln(Price)	(1)	(2)	(3)	(4)	(5)	(6)
Acquired	0.068***	0.070***	0.071***	0.076***	0.068***	0.066***
	(0.017)	(0.017)	(0.017)	(0.018)	(0.019)	(0.015)
Apartment Controls	✓	✓	✓	✓		✓
Neighborhood FE	✓	✓	✓	✓	✓	
Community FE						✓
Adj. R-sq	0.948	0.942	0.942	0.940	0.749	0.908
No. clusters	305	294	226	192	305	305
Observations	155,512	143,561	128,995	115,641	155,512	155,512

Notes: This table reports the average price effects when regular schools are acquired by good schools. Year-month fixed effects are included in all regressions. Apartment controls include third degree polynomials of the apartment size and the building age, as well as the full set of dummies on the number of bedrooms and the number of bathrooms. 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 or who do not have a designated school; column 4 further excludes neighborhoods that are more than 16 kilometers away from the city center; columns 5 an d 6 use all observations. Standard errors are clustered at the school level and reported in parentheses. Significance levels: *0.10, **0.05, and ***0.01.

the full sample, we find an average price increase of 6.8% for apartments in the catchment areas of regular schools when they are acquired by good schools, and the effect is highly significant at the 1% level.

For sensitivity tests, we estimate the same model specification using increasingly restrictive samples in columns 2–4, and consider alternative model specifications in columns 5–6. Column 2 excludes observations with the apartment size over 140 square meters (column 2);[24] column 3 further excludes neighborhoods whose school quality change is due to redistricting or who do not have a designated school; and column 4 further excludes neighborhoods that are more than 16 kilometers away from the city center. As alternative model specifications, column 5 excludes the apartment controls from the right-hand side variables, and column 6 uses *community* instead of neighborhood fixed effects.[25] The estimate remains qualitatively and quantitatively robust. Recall that an average apartment sells for 4 million CNY, so the 7% price increase translates into a price premium of

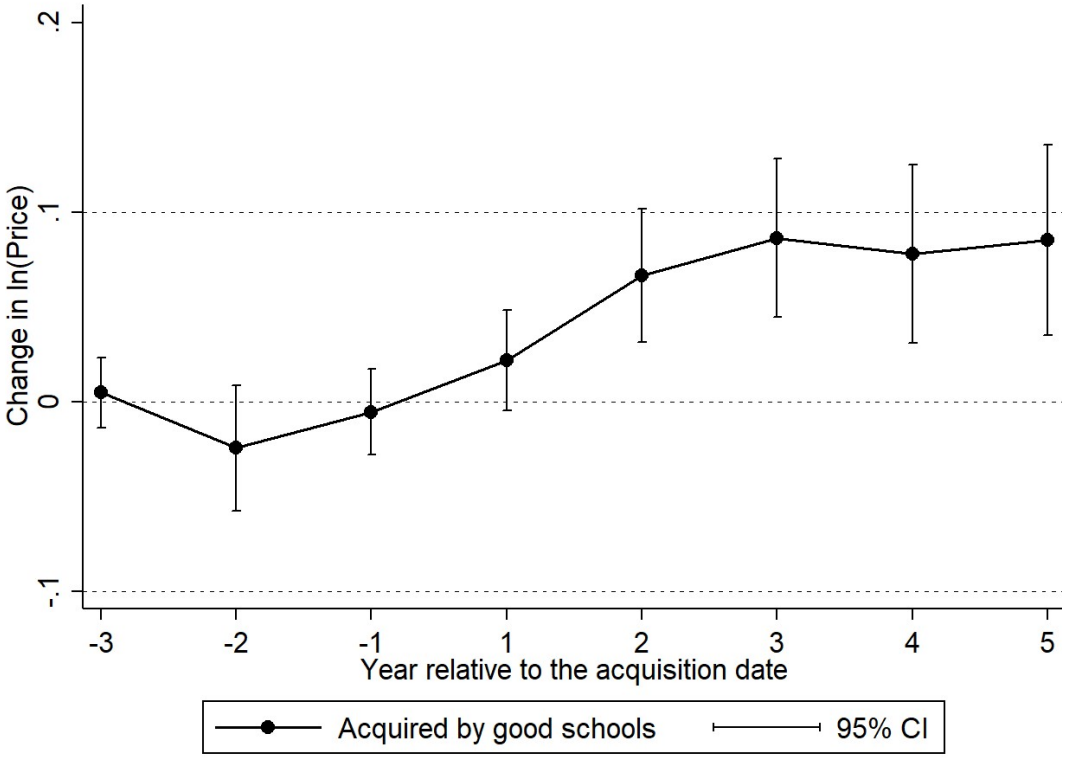
[24]In Beijing, different rules and regulations apply to housing units at or below 140 square meters (regarded as "regular" housing units) versus those above 140 square meters (regarded as "luxury" housing units), e.g., there are different requirements for the holding period to qualify for capital gains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

[25]On average each community consists of about fifteen neighborhoods, so this specification uses a much coarser grid and involves far fewer parameters.

280,000 CNY (or over 40,000 USD), which is both statistically and economically significant.

For the event analysis, we estimate (2) to obtain the annual price differences between the treated schools and the control schools, with the default period being more than 3 years prior to the acquisition date. For visual illustration, Figure 3 graphs the point estimates and the corresponding 95% confidence intervals for the main model, estimated using all observations.

Figure 3: Intertemporal price effects for the acquired schools



Notes: This figure graphs the point estimates and 95% confidence intervals for the event analysis when regular schools are acquired by good schools. The regression model includes apartment characteristics, neighborhood fixed effects, and year-month fixed effects as controls, and is estimated using all observations.

As a falsification test, it is reassuring to see that all coefficients for the periods prior to acquisition are insignificant, supporting the common trend assumption between the two groups. 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 the 1% level. Moreover, except for the first year, the treatment effects do not appear to vary

strongly over time. Other estimation results using different samples or alternative model specifications are reported in Table A1 of the Online Appendix.

Finally, we estimate the heterogeneous price effects in models (3) and (4), and the results are reported in Table 4.

Table 4: Heterogeneous price effects

ln(Price)	(1)		(2)		(3)	
Acquired	Small Apt.	Large Apt.	Partial Int.	Full Int.	Horizontal	Vertical
	0.067***	0.069***	0.052**	0.096***	0.056**	0.096***
	(0.018))	(0.017)	(0.023)	(0.025)	(0.023)	(0.022)
$H_0: \gamma_1 = \gamma_2$						
P-value	0.87		0.19		0.21	

Notes: This table reports the heterogeneous price effects for small versus large apartments, for partially versus fully integrated acquisitions, and for horizontal versus vertical acquisitions. All regressions include apartment controls, neighborhood fixed effects, and year-month fixed effects, and are estimated using all observations. Standard errors are clustered at the school level and reported in parentheses. Significance levels: *0.10, **0.05, and ***0.01.

In column (1), we report the price effects for small ($\leq 80m^2$) versus large ($> 80m^2$) apartments as described in (3). The coefficients are virtually identical, indicating that the price effect (in percentage terms) does not vary by apartment size.[26] Thus, individuals can pay less (in absolute terms) for the same enrollment eligibility if they buy smaller apartments. The presence of such benefits in the data suggests there must be countervailing cost concerns associated with buying smaller apartments, e.g., crowded living conditions for families with school-aged children.

In column (2), we report the price effects for partially versus fully integrated acquisitions, as described in (4). We find that the price increase is 4.4% larger for fully instead of partially integrated acquisitions. While this difference is economically meaningful, it is not statistically significant. Column (3) reports the estimated price effects for horizontal versus vertical acquisitions. Again, while the price effect is 4% larger for vertical instead of horizontal acquisitions, the difference is statistically insignificant. We also extend the event analysis approach to account for the heterogeneous price effects of different types of acquisitions, and these figures can be found in the Online Appendix (Figures A1 and A2).

6 Conclusion

As an investment in the human capital of their children, parents are willing to pay for better quality public schools in the form of higher residential housing

[26] We have also directly interacted the apartment size with the school acquisitions dummy. The coefficient on this interaction term is also insignificant.

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 enrollment eligibility in sought-after schools. Using a hedonic pricing model embedded in the DID framework, we find that regular schools acquired by good schools see an average price increase of 7% for apartments in their catchment areas, relative to regular schools that are not acquired. The price increase (in percentage terms) does not vary by the apartment size. We also find heterogeneous price effects for different types of acquisitions, where the price increase is 4.4% larger for fully instead of partially integrated acquisitions, and 4% larger for vertical instead of horizontal acquisitions. However, while these differences are economically meaningful, they are not statistically significant.

These findings deserve some discussion in a broader policy context. The flip side of our story centers on the good schools involved in these acquisitions. On the one hand, although the estimates are imprecise, there appears to be no significant price penalty for good elementary schools that acquire regular schools, relative to those that do not (see the Appendix). So for horizontal acquisitions, the significant price increases experienced by the acquired schools are not driven primarily by the redistributive effects, i.e., no significant price decreases for the acquiring schools. On the other hand, while it is important to understand the price impact on good middle schools that acquire regular elementary schools in vertical acquisitions, this is beyond the scope of the present paper given the current available data. In particular, due to the random lottery assignment within each bloc of middle schools, a good middle school acquiring a regular elementary school would reduce its capacity for admitting students from other elementary schools, thus affecting the admission probability at each of the middle schools in the bloc it belongs to. To analyze this, we would need to collect information on which middle schools are designated as a bloc, which neighborhoods their collective catchment area consists of, and more importantly, what the capacity of each of the schools in the bloc is, data we currently do not have. We leave this topic for future research.

To curb the considerable price premiums for apartments in the catchment areas of sought-after schools, local governments are considering changes to the rules governing enrollment eligibility. Instead of the current rule, where each elementary school has its own catchment area, an alternative rule could be adopted that is similar to that for middle schools, i.e., designating multiple elementary schools as a bloc with a collective catchment area, and assigning students within the area to different schools by random lottery. This 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*. When such reforms are implemented, the price comparison between the two regimes can inform us of parents' risk attitudes regarding educational inequality.

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Appendix.

In this Appendix, we consider the mirroring DID analysis for good schools. Recall that (in Section 4.3) we divided the final sample into two sub-samples: one corresponding to neighborhoods with regular elementary schools at the beginning of the sample period, and the other corresponding to neighborhoods with good elementary schools. 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 5. 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.

Table 5: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min.	Max.
Price (,000)	111,929	4,886	3,066	110	63,650
Price per m^2 (,000)	111,929	63.3	25.9	5	150
Size (m^2)	111,929	78.8	37.0	7.8	640
Building Age	111,929	25.4	14.9	1	113
No. of Bedrooms	111,929	2.01	0.76	0	9
No. of Bathrooms	111,929	1.16	0.41	0	6
Acquiring	111,929	0.10	0.31	0	1

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

The average price effect is reported in Table 6. Parallel to the structure of Table 3 in the main text, we estimate the model using increasingly restrictive samples as well as different model specifications. Column 1 uses apartment controls, neighborhood fixed effects and year-month fixed effects on the right-hand side, and is estimated using all observations. We find that acquiring regular schools does not have a significant impact on the average apartment price in the catchment areas of the acquiring schools. This result seems robust when we exclude apartments larger than 140 square meters (column 2), further exclude the schools who became good through previous partially integrated acquisitions (column 3), neighborhoods that are more than 16 km away from the city center (column 4); or when we exclude apartment controls from the right-hand side (column 5), or when we use community instead of neighborhood fixed effects (column 6). Overall, we find no

significant price change for goods schools who acquire regular schools versus those who do not.

Table 6: Average price effect for acquiring schools

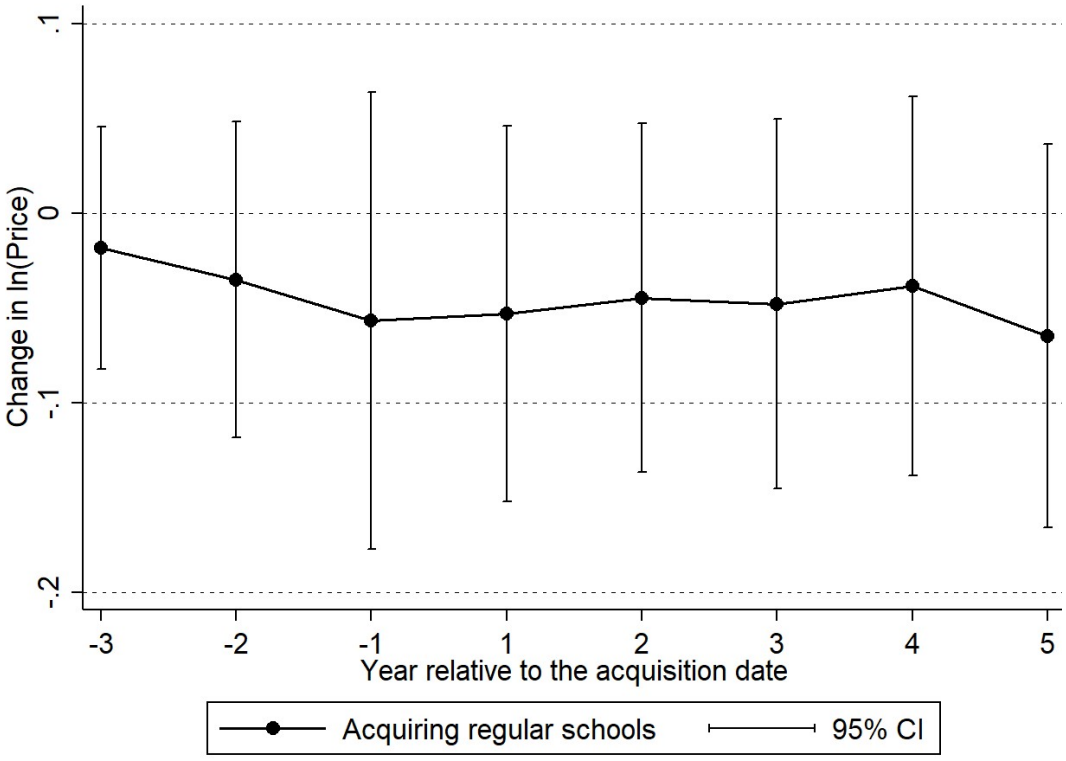
ln(Price)	(1)	(2)	(3)	(4)	(5)	(6)
Acquiring	–0.011	–0.010	–0.016	–0.015	–0.006	0.025
	(0.013)	(0.013)	(0.012)	(0.013)	(0.016)	(0.022)
Apartment controls	✓	✓	✓	✓		✓
Neighborhood FE	✓	✓	✓	✓	✓	
Community FE						✓
Adj. R-sq	0.939	0.931	0.926	0.926	0.752	0.891
No. clusters	177	176	143	139	177	177
Observations	111,929	103,946	84,196	78,479	111,929	111,929

Notes: Year-month fixed effects are included in all regressions. Apartment controls include third degree polynomials of the apartment size and the building age, as well as the full set of dummies on the number of bedrooms and the number of bathrooms. For estimation, column 1 uses all observations; column 2 excludes observations with the apartment size over 140 square meters; column 3 further excludes neighborhoods with schools that were originally regular but had been acquired by good schools through partially integrated acquisitions before our sample period; column 4 further excludes neighborhoods that are more than 16 kilometers away from the city center; columns 5 an d 6 use all observations. Standard errors are clustered at the school level and reported in parentheses. Significance levels: *0.10, **0.05, and ***0.01.

The validity of this DID result again depends on the common trend assumption, so we perform the event analysis. Figure 4 below graphs the intertemporal price effects for the acquiring schools, with the default period being more than 3 years prior to the acquisition date. The model includes apartment characteristics, neighborhood fixed effects, and year-month fixed effects as controls, and is estimated using all observations.

We find that all coefficients for the periods prior to acquisition are insignificant, consistent with the common trend assumption. All coefficients for the post-acquisition periods are also insignificant. However, compared to Figure 3 in the main text, this model cannot be estimated as precisely (much larger standard errors), and the results appear less robust across model specifications (see Table A2 in the Online Appendix).

Figure 4: Intertemporal price effects for the acquiring schools



Notes: This figure graphs the point estimates and 95% confidence intervals for the event analysis when good schools acquire regular schools. The regression model includes apartment characteristics, neighborhood fixed effects, and year-month fixed effects as controls, and is estimated using all observations.

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