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How Much are Good 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 schools. The spatial and temporal variation in these acquisitions allows us to estimate a hedonic pricing model in the difference-in-difference framework. We find that, when a regular school is acquired by a good school, the average housing price in its designated catchment area increases by seven percent, relative to other regular schools that are not acquired. We also find heterogeneous price effects for different types of acquisitions. In particular, the price premium is larger for fully integrated acquisitions than for partially integrated ones, and larger for vertical acquisitions than for horizontal ones.

Keywords: School acquisition, housing price, hedonic pricing 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 schools. To do so, we utilize government sanctioned school acquisitions in Beijing as a quasi-natural experiment. A school acquisition is a merger of two public schools—one regarded as a "good" school and the other as a "regular" school—with the objective of reducing the disparity in the funding and operational conditions across schools. From the perspective of regular schools, being acquired by a good school defines 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 estimated treatment effect, i.e., the price premium associated with a school acquisition, informs us of individuals' willingness to pay for good schools.[1]

The capitalized value of school quality in the real estate market has received considerable attention in the urban economics literature. We contribute to the field in a number of important ways.

First, in western economies, local public schools are typically financed with local property tax revenue.[2] In this setting, causation can go in two 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. Since both the demand for and the supply of school quality are endogenous, it is necessary to control for supply side variations in order to identify the willingness to pay for good schools (Downes and Zabel 2002, Gibbons and Machin 2003, Cheshire and Shappard 2004, Bayer et al. 2007). In contrast, the funding of public schools in China, and Beijing in particular, comes from general tax revenue that is not directly linked to local property values (Zheng and Kahn

[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.

2008).[3] Therefore, our analysis has the notable advantage that it avoids 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.

Second, following Black (1999), a large part of the literature has relied on the discontinuity imposed by administrative and/or geographic boundaries to control for unobserved heterogeneity. Residential properties on one side of 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). 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 result in biased estimate of the influence of school quality on property values (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., opening of new charter schools (Andreyeva and Patrick 2017), school redistricting (Bogart and Cromwell 2000), school rezoning (Ries and Somerville 2010), 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] Because there are both spatial and temporal variations in school acquisitions, we can embed a hedonic pricing model within the difference-in-differences framework, which offers better control for unobserved heterogeneity compared to cross-sectional analysis alone.

A third advantage of our analysis arises from the fact that in urban areas of China, residential housing consists primarily of apartment units. Typically, each neighborhood is developed by a single real estate company and comprised of similar styled, multi-story buildings, with tens to hundreds of apartments per building. Thus, compared to single family homes, apartments within a given neighborhood

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

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

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

are close substitutes for one another. In addition, there is a one-to-one mapping between neighborhoods and their corresponding schools. This implies that, for our empirical analysis, neighborhood fixed effects are effective at capturing most of the unobserved heterogeneity.

We obtained confidential real estate transactions data from a large brokerage company in China, *Lianjia*, whose market share in Beijing is over 60%. By linking the transactions data to data on school acquisitions, we find a price premium of seven percent for apartments in catchment areas of regular schools after they are acquired by good schools, compared to those that do not experience such acquisitions. This result is robust to alternative model specifications. We find no evidence of free-riding, i.e., individuals purchasing smaller apartments to gain the same school enrollment previleges as those with bigger apartments. In fact, the price premium associated with school acquisitions does not depend on the size of the apartment. On the other hand, we do find heterogeneous price effects for different types of acquisitions. The price premium is significantly larger when the acquiring school and the acquired school operate as one entity (fully integrated acquisition), compared to the case when they continue to operate as separate campuses under the same name (partially integrated acquisition). Similarly, the price premium is significantly larger when a regular elementary school is acquired by a good middle school (vertical acquisition), compared to the case when a regular elementary school is acquired by another, good elementary school (horizontal acquisition).

The rest of the paper is organized as follows. In Section 2 we provide institutional background about the public school system in China in general, and the specific education 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. All tables are in the Appendix.

2 Institutional background

In China, the public education system after the founding of the People's Republic of China in 1949 was modeled after the former Soviet Unions style. For primary and secondary education, limited resources were concentrated to a small number of "key" schools, instead of being spread across all schools equally.[6] Compared to the rest, these key schools had smaller classes, more qualified teachers, better facilities, and more rigorous curricula. The goal of this system was to ensure the steady supply of academically prepared graduates for university studies, especially in fields deemed critical for national interests.[7] At the same time, the rest of the schools were poorly funded. Since key schools tended to be located in urban instead of rural areas, the quality disparity between schools was also more prominent in cities, and Beijing in particular.

Admissions to these key schools were traditionally merit based. That is, each key school would administer its own entrance examinations to assess the academic ability of all its applicants, and select those with high test scores to fill the capacity. Such entrance exams would mark 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 the economic reform in China brought it back in focus. New policies significantly expanded the student base for public education, especially so at the basic education level. In particular, the Compulsory Education Law that took effect on July 1, 1986 requires all children to attend school for a minimum of nine years, and stipulates that compulsory schooling should be tuition free. While these targets may not yet be fully achieved in rural areas with limited fiscal capacity, they are largely achieved in urban areas such as Beijing.

Besides expanding the student base, new policies also aim 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 amended Compulsory Education Law that took effect on September 1, 2006 put an official end to the earlier system of key schools. It explicitly stipulates 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

[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 would play an outsized role in the early development of the nation.

within a school." In Beijing, the implementation of this law brought a number of important changes to the adminstration of public education.

First, at the compulsory schooling level (elementary and middle schools), 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, each school has its designated catchment area (multiple neighborhoods, not necessarily contiguous), and all children with legal residence ("hukou") in the catchment area are free to enroll. Schools are also prohibited from charging any fees to enroll students who are not otherwise eligible under the proximity principle. For middle schools, several 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 a computer generated lottery. Overall, this implies that the only way parents can influence their children's enrollment eligibility is through their choice of residential locations.

Second, the nine years of compulsory schooling also affect the organizational structures of schools. Traditionally elementary schools are stand-alone entities and cover grades 1–6. Middle schools (grades 7–9), when not stand alone, tend to be organized with high schools (grades 10–12) and form entities for secondary education. This creates a physical transition when students move from elementary to middle schools, i.e., they go to a new campus, be assigned to a new class, and have a new set of teachers and classmates. To minimize such transitional disruptions, experiments have been made to create integrated nine-year (or even twelve-year) schools, where students stay in the same campus and class, and have familiar teachers and classmates. Furthermore, nine-year schools eliminate the uncertainty associated with the lottery for assigning middle schools, 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).

Third, and most importantly for this paper, local governments at both the municipal and the district level in Beijing have promoted school acquisitions as a means to reduce education inequality. In a school acquisition, a good school (what used to be a "key" school) is directed to merge with a regular (non-key) school, with the objective of reducing funding disparities between the two, and equalizing educational resources and opportunities. Acquisitions can be characterized as either fully or partially integrated. In the full integration case, the acquiring school and the acquired school operate as one entity; in the partial integration case, they operate as separate campuses under the same name. Acquisitions can also be characterized as being either horizontal or vertical. In the horizontal case, a good elementary school acquires a regular elementary school, with the resulting entity still covering grades 1–6. In the vertical case, a good middle school acquires a regular elementary school, with the resulting entity forming an integrated nine-year school. These acquisition decisions are made by local governments and not by the schools themselves.

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 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 trends 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 capture 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 sanctioned by local (mostly district-level) governments as a means to reduce disparity across schools, they are arguably a source of exogenous variations. So, 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 $y(S_{nt}=1) - y(S_{n,t-1}=0)$ 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 counterfactual $y(S_{nt}=0) - y(S_{n,t-1}=0)$, 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 $y(S_{mt}=0) - y(S_{m,t-1}=0)$ 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

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

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 "common trend" assumption holds in the data, we compare the price patterns *before* the school acquisitions between the group of neighborhoods that were later treated ($g_c = 1$) and the group of neighborhoods that are never treated ($g_c = 0$). In particular, we estimate the following *pre-treatment* model:

$$y_{ict} \;=\; \phi g_c \,+\, \beta_t \,+\, \delta(t \cdot g_c) \,+\, \theta X_{ict} \,+\, \epsilon_{ict}. \tag{2}$$

A significant estimate of ϕ would suggest a systematic difference in the price levels between the two groups, which is readily accounted for in the DID analysis by the neighborhood fixed effects α_n. On the other hand, a significant estimate of δ would indicate a systematic difference in the price trends (in the form of a linear annual trend) between the two groups, thus raising concerns about the suitability of the DID approach.

Note that when we compare the treated group and the control group, we intentionally exclude neighborhoods within the catchment areas of good schools. In a sense, these neighborhoods can be considered as "always treated", i.e., $A_{nt} = 1$ for all t. The reason we exclude these neighborhoods is two-fold. First, there is no *a priori* reason to expect that the acquisition would have no impact on the acquiring school itself, especially if we expect some resource equalization from the acquiring to the acquired schools, the stated objective of these government sanctioned acquisitions. Thus, if there is any resource dilution effect at all, the subset of good schools that have acquired one or more regular schools should be considered as "treated", compared to the subset of good schools that have never acquired any other schools. Second, and more importantly, while we argue that the selection of regular schools for treatment (i.e., to be acquired) is exogenous and random to our model, the selection of good schools for treatment (i.e., acquiring other schools) is anything but. In fact, we have explored this idea empirically, and unsurprisingly, found that the parallel trends test fails to hold for good schools that later acquired other schools relative to those that did not. Such evidence would suggest potential selection bias and hence challenge the validity of the DID method. For these two reasons, we focus only on the neighborhoods within the catchment areas of regular schools at the beginning of our sample period.

3.3 Heterogeneous treatment effects

In the average effect model, the price effect of school acquisitions is assumed to be constant and does not depend on the apartment characteristics. However, in the public finance literature, it is well understood that for jurisdictions that use property taxes to finance local public goods, individuals can partially free-ride by owning smaller homes and hence paying less in taxes, while enjoying the same public goods as those in larger homes. In our non-tax context, there may be an analogous incentive: Individuals may choose to buy smaller apartments and pay a lower price premium, while still enjoying the same enrollment eligibility as residents of larger apartments. To test whether this is indeed the case, we estimate the following model to examine potentially differential effects of apartment size on the price premium:

$$p_{int} \ = \ \alpha_n \ + \ \beta_t \ + \ \Sigma_{b=1}^{N} \gamma_b A_{nt} * I(unit \ i \in bin \ b) \ + \ \theta X_{int} \ + \ \epsilon_{int}, \tag{3}$$

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 N 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 bigger for smaller apartments than that for larger apartments, this can be interpreted as evidence of free-riding.

Furthermore, in the average effect model, the price effect is assumed to be constant for all regular schools that have been acquired by good schools. As outlined previously, this assumption neglects the difference in the post-acquisition organization structures and management practices. While some schools are perfectly integrated with the "mother" school, namely under the same central administration, sharing of all resources, using the same recruiting process, etc., others are only partially integrated, namely they operate under the same banner but as separate campuses. It is reasonable to expect that the full integration schools experience a bigger improvement in the school quality than the partial integration schools. To better capture these differences, we can further divide the school quality improvement due to acquisition into two categories: those under full integration, and those under partial integration. Thus, we also estimate the following *heterogeneous effects* model:

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

where the variables $A1_{nt}$ and $A2_{nt}$ are mutually exclusive and collectively exhaustive, with $A1_{nt} + A2_{nt} = A_{nt}$. In particular, we are interested in two ways to divide the treatment dummy A_{nt}. First, we compare full integration versus partial integration after the acquisition. A regular school is fully integrated with a good school after the acquisition if they operate as one entity, i.e., resources and opportunities are fully equalized. In contrast, a regular school is partially integrated with a good school after the acquisition if they operate as separate campuses but share the same name, i.e., resources and opportunities are continue to exhibit disparities. Thus, it is not unreasonable to expect that the benefit of being acquired, if any, would be smaller in the partially integrated case than that in the fully integrated case. The parameters γ_1 and γ_2 will inform us whether this is indeed the case, and how big the difference is.

Second, we also compare horizontal versus vertical acquisitions. Horizontal acquisition takes place when a regular elementary school is acquired by a good elementary school, where the treated school may directly benefit by adopting the best practices (in both management and instruction) of the acquiring school and sharing its resources. This implies a direct improvement in its own education quality. In comparison, vertical acquisition takes place when a regular elementary school is acquired by a good middle school, where the primary benefit seems to be a guaranteed (instead of the computer lottery for random assignment) middle school for its students. While parents may value both aspects, there is no *a priori* reason to expect which one would be bigger.

4 Data

For the 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, and most sellers are individual home owners and hence have little market power. This is quite different from the primary market where newly constructed housing units are transacted, and each developer supplies a large number of apartments and hence has significant market power.

The real estate resales 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 been recording relevant information in a centralized database, first as a pilot program in early 2009 and then as a full rollout in late 2011. Through the end of 2018, Lianjia has provided us approximately $590,000$ sales record in total.[8]

For each completed transaction, Lianjia provides us the following information. First, we have the transaction information, namely the sales date, the sales 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, so we cannot match transactions as repeated sales of the same unit. Nevertheless, the name and address of the neighborhood allows us to match the real estate transactions data with the corresponding school information, as will be described in detail later. Furthermore, we also know certain physical characteristics of the apartment unit, including its size and the number of bedrooms and bathrooms, as well as 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 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, with stringent limitations imposed on those carrying shorter durations (e.g., 40 or 50 years). Furthermore, there are different ownership title categories for the apartment units themselves, where those facing minimal resale restrictions are designated "commodity" housing units ("shang pin fang"), while those facing various additional 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 the official designation of key 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. As a starting point, we have downloaded this unofficial list, with the good schools listed by each of the sixteen

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

districts in Beijing.[9] 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. Specifically, acquisitions can be regarded as fully integrated if the two schools operate as one, and partially integrated if they operate as separate campuses under the same name. Furthermore, acquisitions can be viewed as horizontal if the acquiring school is a good elementary school, and vertical if the acquiring school is a good middle school. Note that by our definition, the acquired school is always a regular elementary school.

From the same website http://www.ysxiao.cn, we also collect all admissions guides available, most of them are for sought-after schools, including good schools and regular schools after they are acquired by good schools.[10] 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 the real estate transactions 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.

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

[10] 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 some photographed by its own staff. Note that we do not have complete coverage of all schools. In particular, since there is little parental interest outside the catchment area of a regular school, the website has very few admissions guide for regular schools that are not acquired by good schools.

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 necessarily comparable to those in the core districts. Moreover, in our real estate data, observations in the core districts are overwhelmingly resale transactions of 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 cour districts, which account for 52% $(306,999)$ of all $(590,137)$ observations.[11] For these districts, good real estate transactions data coverage began in September 2011. Thus, our sample period is from September 2011 through December 2018, leaving us $306,520$ core disstrict observations.[12]

Second, for comparability, we restrict our sample to observations with 70-year usufruct land rights and ownership title designation as "commodity" housing units.[13] 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 $23,600$).[14] This step removes 321 observations, leaving a remaining sample of $268,358$.

Finally, as we discussed in Section 3.2, we focus only on neighborhoods that, at the beginning of the sample period, fall within the catchment areas of regular

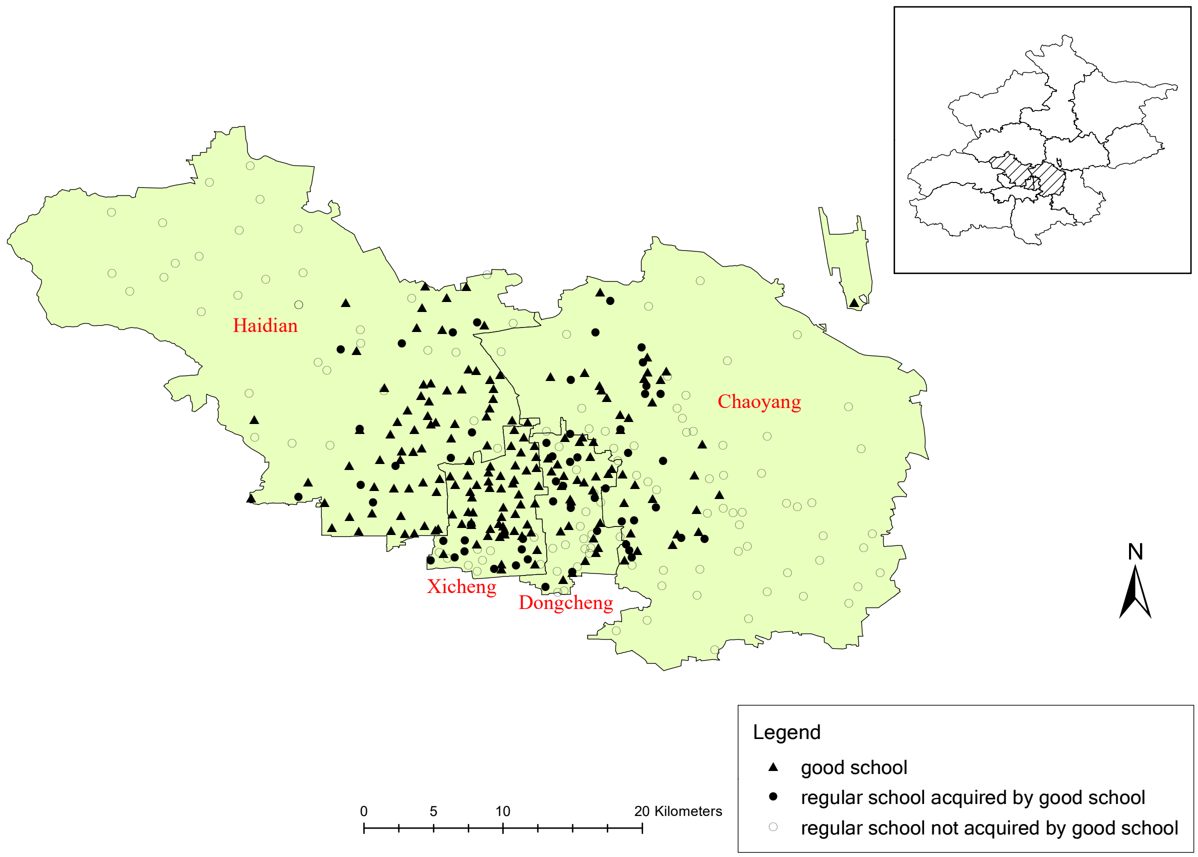
[11]In fact, we find that the pretreatment parallel trends assumption fails to hold in the outer twelve districts. This suggests that the DID method may not be suitable for that sub-sample.

[12]Core district observations prior to September 11 period 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.

[13]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.

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

Figure 1: School locations in the four core districts of Beijing



schools instead of good schools. These neighborhoods account for 60% of the remaining sample, or 161,145 observations, which constitute the final sample used in our empirical analysis. This sample spans 87 months and four core districts, consisting of 127 communities and 1,880 neighborhoods. Figure 1 shows the spatial locations of all schools within the sample. Table 1 reports the summary statistics.

5 Estimation results

As both the school quality indicator (the acquisition variable) and the sales price (the outcome variable) are serially correlated, the difference-in-differences 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.

5.1 Average treatment effect

Table 2 reports the estimated average treatment effect model (1), where columns 1-3 use the neighborhood fixed effects specification, and columns 4-6 use the community fixed effects specification. Within each specification, we estimate the model three times by increasingly restricting our sample: the first column uses all observations, the second column excludes observations with apartment size over $140m^2$,[15] and the third column further excludes observations in neighborhoods whose school quality change is due to redistricting instead of school acquisition. At the expense of losing observations, the control group and the treated group arguably become more homogeneous when moving from the first to the third columns. All models include year-month fixed effects.

It is easy to see that when a regular school is acquired by a good school, this significantly increases the average sales price for apartments in its catchment area. For example, in column 1, we see that the price premium is 6.6% after the acquisition, holding everything else constant. Other control variables are of the expected signs and reasonable magnitudes as well. We see that a 1% increase in the apartment size increases its total sales price by 0.85%, indicating a positive but diminishing marginal returns on the size of the apartment. Having one more bedroom increases the total sales price by 2.5%, while having one more bathroom increases the price by 1.4%. It is interesting to note that the coefficient on the age of the building is significantly positive when the model uses neighborhood fixed effects, indicating that within a given neighborhood, older buildings (earlier developments) seem more desirable than newer buildings.

Moving to column 2, the price impact of being acquired by a good school is 6.8%, quantitatively similar to that in column (1). This suggests that the price premium is not driven by outliers on the right tail of the distribution. The coefficients on other control variables remain qualitatively unchanged. Quantitatively, having one more bathroom increases the price by 2.9% after we exclude the very large apartments from the sample, doubling the effect in column (1). Further excluding the small number of neighborhoods subject to redistricting (column 3) hardly changes the results.

[15]In Beijing, different rules and regulations apply to housing units at or below $140m^2$ (regarded as "regular" housing units) versus those above $140m^2$(regarded as "luxury" housing units), e.g., different requirements on the holding period to qualify for capital gains tax exemption, different requirements on the mortgage down payment, etc.

Next, to address the concern that using neighborhood fixed effects may over-fit the model, we repeat the estimations using community fixed effects instead. On average each community consists of about fifteen neighborhoods, thus representing a much coarser grid and involving much fewer parameters. Nor surprisingly, there is a small decrease in the fit of the model measured as the adjusted R-square, but our main results are robust. For example, in column 4, the estimated price effect is 7.0% after the acquisition, a small increase compared to the 6.6% reported in column 1. Other control variables remain qualitatively similar, with one interesting flip of the sign regarding the age of the building. Thus, within a given community (consisting of multiple neighborhoods), it is newer buildings (later developments) that command higher prices than older buildings, opposite to what we have found using neighborhood fixed effects.[16] When the very large apartments and redistricting neighborhoods are excluded, the main results remain stable (columns 5 and 6). Overall, the price impact of school acquisition remains both qualitatively and quantitatively stable at about 7%.

5.2 Pretreatment price trends

The validity of our DID method depends critically on the "common trend" assumption, so we test whether there is any significant difference in the price trends for the treated group and the control group, as given in (2). Recall that the year-month fixed effects already capture both the linear trend and seasonality fluctuations that are common across the treated group and the control group. Our interest is whether the linear trends are different across the two groups. Since different "treated" neighborhoods have different dates when their associated regular schools were acquired by good schools, our pretreatment truncation of the observations is neighborhood specific (using the date of the public announcement). In other words, for neighborhoods in the control group, all observations are included; for neighborhoods in the treated group, only those observations before their acquisition announcement dates are included in the estimation. Again, in the pretreatment test we consider both neighborhood fixed effects and community fixed effects, each

[16] This is an interesting example of Simpson's Paradox, namely certain relationship found at the aggregate (community) level may be overturned when the data is examined at a more dis-aggregate (neighborhood) level.

estimated three times using increasingly restrictive samples similar to those in Table 2. The results are reported in Table 3.

Note that in the pretreatment analysis, neighborhood fixed effects already perfectly account for any level difference, so the only test is whether there is any significant trend difference between the treated and the control groups. We find that the linear trend difference is consistently insignificant (columns 1–3). Furthermore, the coefficients on other control variables are both qualitatively and quantitatively similar to those reported in Table 2, i.e., using both pre- and post-treatment data, lending additional support that the underlying structure for price determination has barely changed as a result of school acquisitions.

When community fixed effects are used, the pre-treatment analysis tests both potential level and trend differences between the two groups. It is reassuring that not only the trend difference is consistently insignificant, but also the level difference (columns 4–6). Taken together, these results show that there are no discernible differences between the two price patterns, one for the treated group and the other for the control group, in either levels or trends.

5.3 Heterogeneous effects

To examine potentially differential effects of the apartment size on price premiums (3), we create three subgroups: small (up to 50m^2), medium (50–90m^2), and large being the default subgroup (over 90m^2), which are interacted with the school acquisition dummy. Table 4 reports the estimated effects. While there are diminishing marginal returns to the size of the apartment, we cannot detect any significant difference in its impact on the price premium after school acquisitions.[17] This indicates that the free-riding incentive does not play a significant role in our setting. We attribute this to two reasons. 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.[18] On the demand side, it is reasonable to assume that most households cannot afford to buy more than one apartment (e.g., one for school enrollment eligibility and the other

[17]We have also directly interacted the variable of apartment size with school acquisition dummy, instead of dividing them into subgroups, and the estimate is similarly insignificant.

[18]In our sample, only 16% of observations are units at or below 50m^2 in size, 4% are at or below 40m^2, and a mere 0.6% are at or below 30m^2.

as a primary residence). For these households, the desire to have a comfortable living space must counterbalance potential free-riding considerations. Overall, we find no evidence that school acquisitions lead to heterogeneous price increases for small or medium sized apartments.

Next, we examine the potentially heterogeneous price effects by the nature of the acquisitions (4). Table 5 reports the estimated pirce effects for fully integrated versus partially integrated acquisitions, as well as a test for the statistical significance of their difference. While both types of acquisitions have significantly positive price effects, the price premium is larger for fully integrated acquisitions (9.3–10.9%) than for partially integrated acquisitions (4.5–5.3%), and this difference is significant at least at the 10% level. This result is consistent with our expectation: A regular school benefits more from being acquired by a good school when both schools' resources and opportunities are fully equalized, as is the case under full integration.

Finally, Table 6 reports the estimated price effects for horizontal versus vertical acquisitions, together with a test for the statistical significance of their difference. Both horizontal and vertical acquisitions result in significant price premiums, but the effect is smaller for horizontal acquisitions (5.4–5.7%) than for vertical acquisitions (9.5–10.3%). This difference is significant at least at the 10% level. To interpret this result, note that a horizontal acquisition improves the schooling quality of the regular school, which benefits its students for six years (grades 1–6). Thus, our treatment effect translates roughly into a price premium of 1% per year for a better-quality elementary school (or about $40,000$ yuan a year on average). On the other hand, a vertical acquisition may or may not directly improve the schooling quality of the regular school; however, it does give students a guaranteed placement in a good middle school. If the vertical acquisition improves the regular school quality, students will benefit from the vertical acquisition for a full nine years (grades 1–9). This again implies a price premium of approximately 1% per year, comparable to the per-year premium associated with horizontal acquisitions. If the vertical acquisition does not improve the regular school quality, students will benefit from teh vertical acquisition for three years (grades 7–9). This implies a price premium of about 3% per year for better-quality middle schools, three time as large as that for better-quality elementary schools.

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 difference-in-difference 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 of schools that are not acquired. We find no evidence of a differential impact of apartment size on the price premium associated with school acquisitions. Furthermore, we find that the price effect for fully-integrated acquisitions is about twice as large as that for partially-integrated acquisitions, and the price effect for vertical acquisitions is also about twice as large as that for horizontal acquisitions.

These findings deserve some discussion in the broader policy context. First, there is an obvious flip side of our question: is there a price penalty for apartments in the catchment areas of good schools that acquire regular schools? To evaluate the welfare effects of government sanctioned schools acquisitions, the price effect on acquiring schools must be taken into account as well. For example, the absence of a price penalty would suggest that school acquisitions do not diminish the quality of the acquiring school, and hence increase total welfare, whereas a large penalty would suggest that the welfare effect is largely redistributive. For technical reasons, it is beyond the scope of the present paper to answer this question. In particular, although the school acquisitions we studied can be viewed as quasi-experimental shocks to the acquired schools, they are not exogenous to the acquiring schools. Our analysis rejected the parallel trends assumption for good schools, some of which were directed by local governments to acquire regular schools, while others were not. Thus, the current difference-in-difference framework would no longer be adequate, and other methods would be needed, to properly account for the potential selection biases.

Second, given the stated policy objective of reducing education inequality and equalizing education resources and opportunities, our results on heterogeneous price effects suggest that fully integrated acquisitions are more effective than partially integrated ones, and vertical acquisitions are more effective than horizontal ones. However, in the data, we observe more partially integrated acquisitions than fully integrated ones (60% vs. 40%), and more horizontal acquisitions than vertical ones (66% vs. 34%). This discrepancy may be explained by larger costs associated with fully integrated and vertical acquisitions. A comprehensive policy analysis would have to link the costs of school acquisitions (both explicit and implicit) to their benefits, in order to determine the most effective type of acquisition.

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 policy equalizes education resources and opportunities *ex ante*, even though there will still be education inequality *ex post*. In contrast, government sanctioned school acquisitions aim to reduce education inequality both *ex ante* and *ex post*. With additional data, real estate price effects under both regimes could be compared. In particular, the difference in the price effects across policy regimes might shed light on parents' risk attitude regarding education inequality.

Appendix

Table 1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min.	Max.
sales date	161,145	20,404	698	18,871	21,545
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 2: Average treatment effect

	Neighborhood FE			Community FE		
	(1)	(2)	(3)	(4)	(5)	(6)
Acquired	0.066***	0.068***	0.068***	0.070***	0.069***	0.069***
	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)
log(space)	0.848***	0.860***	0.860***	0.838***	0.857***	0.858***
	(0.012)	(0.013)	(0.013)	(0.015)	(0.016)	(0.016)
No. bedrooms	0.025***	0.026***	0.026***	0.035***	0.031***	0.031***
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
No. bathrooms	0.014**	0.029***	0.028***	0.061***	0.069***	0.069***
	(0.006)	(0.007)	(0.007)	(0.008)	(0.010)	(0.010)
log(age)	0.076***	0.085***	0.085***	–0.076***	–0.069***	–0.069***
	(0.012)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)
Year-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.949	0.941	0.941	0.910	0.900	0.900
No. neighborhoods	1,880	1,787	1,784	1,880	1,787	1,784
Observations	161,145	148,889	148,852	161,145	148,889	148,852

Notes: Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table 3: Pretreatment trends

	Neighborhood FE			Community FE		
	(1)	(2)	(3)	(4)	(5)	(6)
treated group				–0.004	–0.008	–0.008
				(0.014)	(0.015)	(0.015)
trend for treated group	0.003	0.003	0.003	0.003	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
log(space)	0.851***	0.862***	0.862***	0.854***	0.876***	0.876***
	(0.010)	(0.009)	(0.009)	(0.014)	(0.014)	(0.014)
No. bedrooms	0.024***	0.025***	0.025***	0.030***	0.026***	0.026***
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)
No. bathrooms	0.009**	0.022***	0.022***	0.055***	0.060***	0.060***
	(0.004)	(0.005)	(0.005)	(0.007)	(0.009)	(0.009)
log(age)	0.081***	0.092***	0.092***	–0.068***	–0.061***	–0.061***
	(0.012)	(0.012)	(0.012)	(0.010)	(0.010)	(0.010)
Year-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.951	0.943	0.943	0.910	0.900	0.900
No. neighborhoods	1,880	1,781	1,778	1,880	1,781	1,778
Observations	139,573	128,638	128,622	139,573	128,638	128,622

Notes: Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table 4: Heterogeneous effects by apartment size

	Neighborhood FE			Community FE		
	(1)	(2)	(3)	(4)	(5)	(6)
Acquired	0.068***	0.069***	0.070***	0.079***	0.078***	0.079***
	(0.011)	(0.011)	(0.011)	(0.016)	(0.016)	(0.016)
Acquired*($size \leq 50$)	0.008	0.013	0.013	0.000	0.005	0.005
	(0.019)	(0.018)	(0.018)	(0.024)	(0.024)	(0.024)
Acquired*($50 < size \leq 90$)	–0.007	–0.008	–0.008	–0.017	–0.018	–0.018
	(0.010)	(0.009)	(0.009)	(0.014)	(0.014)	(0.014)
log(space)	0.849***	0.861***	0.861***	0.838***	0.858***	0.858***
	(0.012)	(0.012)	(0.012)	(0.014)	(0.015)	(0.015)
No. bedrooms	0.025***	0.026***	0.026***	0.035***	0.031***	0.031***
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
No. bathrooms	0.013**	0.028***	0.027***	0.060***	0.068***	0.068***
	(0.005)	(0.007)	(0.007)	(0.008)	(0.010)	(0.010)
log(age)	0.075***	0.086***	0.086***	–0.076***	–0.069***	–0.069***
	(0.012)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)
Year-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.949	0.941	0.941	0.910	0.900	0.900
No. neighborhoods	1,880	1,787	1,784	1,880	1,787	1,784
Observations	161,145	148,889	148,852	161,145	148,889	148,852

Notes: Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table 5: Heterogeneous effects: full vs. partial integration

	Neighborhood FE			Community FE		
	(1)	(2)	(3)	(4)	(5)	(6)
Full integration	0.093***	0.093***	0.094***	0.109***	0.105***	0.106***
	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)
Partial integration	0.051***	0.053***	0.053***	0.045***	0.046***	0.046***
	(0.016)	(0.016)	(0.016)	(0.014)	(0.014)	(0.014)
Difference	–0.042**	–0.040*	–0.041*	–0.064***	–0.060***	–0.060***
	(0.020)	(0.021)	(0.021)	(0.019)	(0.019)	(0.019)
log(space)	0.848***	0.859***	0.860***	0.837***	0.856***	0.856***
	(0.012)	(0.013)	(0.013)	(0.015)	(0.016)	(0.016)
No. bedrooms	0.025***	0.026***	0.026***	0.035***	0.031***	0.031***
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
No. bathrooms	0.014**	0.029***	0.028***	0.061***	0.070***	0.069***
	(0.006)	(0.007)	(0.007)	(0.008)	(0.010)	(0.010)
log(age)	0.076***	0.086***	0.087***	–0.076***	–0.070***	–0.070***
	(0.011)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)
Year-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.949	0.941	0.941	0.910	0.900	0.900
No. neighborhoods	1,880	1,787	1,784	1,880	1,787	1,784
Observations	161,145	148,889	148,852	161,145	148,889	148,852

Notes: Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

Table 6: Heterogeneous effects: horizontal vs. vertical acquisition

	Neighborhood FE			Community FE		
	(1)	(2)	(3)	(4)	(5)	(6)
Horizontal acquisition	0.054***	0.057***	0.057***	0.055***	0.054***	0.054***
	(0.015)	(0.015)	(0.015)	(0.013)	(0.013)	(0.013)
Vertical acquisition	0.095***	0.095***	0.095***	0.103***	0.102***	0.102***
	(0.012)	(0.012)	(0.012)	(0.014)	(0.013)	(0.013)
Difference	0.040**	0.038*	0.038*	0.049***	0.048***	0.048***
	(0.019)	(0.020)	(0.020)	(0.018)	(0.017)	(0.017)
log(space)	0.848***	0.859***	0.860***	0.837***	0.856***	0.856***
	(0.012)	(0.013)	(0.013)	(0.015)	(0.016)	(0.016)
No. bedrooms	0.025***	0.026***	0.026***	0.035***	0.031***	0.031***
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
No. bathrooms	0.014**	0.029***	0.028***	0.061***	0.070***	0.069***
	(0.006)	(0.007)	(0.007)	(0.008)	(0.010)	(0.010)
log(age)	0.076***	0.086***	0.086***	–0.076***	–0.070***	–0.070***
	(0.012)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)
Year-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.949	0.941	0.941	0.910	0.900	0.900
No. neighborhoods	1,880	1,787	1,784	1,880	1,787	1,784
Observations	161,145	148,889	148,852	161,145	148,889	148,852

Notes: Significance levels: *0.10, **0.05, and ***0.01. All estimations use cluster-robust standard errors that are clustered on neighborhoods.

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