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# Quasi-Experimental Evidence of a School Equalization Reform on Housing Prices in Beijing

Wei Ha and Renzhe Yu

*Abstract:* This study examines the effect of a reasonably exogenous school equalization reform in Beijing on housing values. Based on sales records of secondhand housing between 2012 and 2016 in four core urban districts in Beijing straddling the reform, the authors find that reform-induced improvement in school quality is on average associated with a 1.7% increase in housing prices. It takes more than one year for the effect to become noticeable and it intensifies to a 12% increase 24 months after the reform. Furthermore, heterogeneity effects analyses show that the effects tend to concentrate on housing units that are smaller and that are associated with stronger and lasting improvement in school quality. Our results, therefore, cast doubts on whether such school equalization reforms are beneficial to low socioeconomic status, cash-strapped families with improved educational opportunities in the Chinese context.

**Keywords:** event study model, housing prices, school quality, school equalization

Understanding the role that public education plays in shaping economic inequality and social mobility has long been at the center of educational research. Since the Coleman Report in 1966 revealed that academic achievement was more linked to parental characteristics than to school environment (Coleman et al. 1966), generations of researchers and policymakers have been motivated to adopt public policies to attenuate if not eliminate the links between parental background and educational achievement. However, the effects of these policies have been inevitably curtailed by natural forces that put less endowed children at a disadvantage, be it parental educational choices, genetic cognitive and noncognitive abilities, or financial resources (Björklund and Salvanes 2011). Public provision of education financed largely by property taxes in the U.S. context has been modeled by Tiebout (1956) as an efficient market equilibrium whereby residents concurrently make residential choice and choice of the quality of public education. But the results are unlikely to be equal because the quality of one's education depends on parental affordability of more expensive housing. The consequential effect of capitalization of school quality into housing prices has been thoroughly researched in the United States (see the Literature Review), and this effect has further contributed to widespread residential and social segregation (Oates 2006; Chakrabarti and Roy 2015). In



Wei Ha is Associate Dean and a professor in the Graduate School of Education and Institute of Educational Economics, Peking University.

Renzhe Yu is a Master Student in the Graduate School of Education and China Institute for Educational Finance Research, Peking University.

response, starting with California in 1978, all but few states have enacted statutory property tax levy limits to reduce the dependency of school expenditure on local property tax (Nguyen-Hoang 2013).

The capitalization effect of school quality and the undesirable consequences are not uncommon in other national contexts (Gibbons and Machin 2003; Fack and Grenet 2010; Agarwal et al. 2016), but are much less studied especially in developing economies. In China, for example, the 1986 Compulsory Education Law formally established the nine-year compulsory education system spanning primary and lower secondary education, and also stipulated that students be enrolled in schools near their residence. Specific to primary schools, each school in principle enrolls school-aged children exclusively from its predefined attendance zone, and only children whose parents are homeowners are eligible to get enrolled. Although the compulsory education system is primarily financed by local fiscal revenues in general instead of by property taxes (Wang and Liu 2010), this neighborhood schooling policy coinciding with the transition from public provision of housing to private commercial house ownership as the dominant form in urban China from the late 1980s (Li and Huang 2006; Chen and Han 2014) set the stage for the capitalization effect of school quality into housing values.

The regional dispersion of school quality especially within major cities is exceptionally high, which dates back to a policy in the mid-twentieth century that, in an effort to establish a handful of elite schools with limited fiscal resources, heavily preferred a small proportion of schools. These schools, once named "key schools," gradually developed pedagogically superior curriculum and culture, better teacher quality, more scientific management patterns, among others (You 2007). Although these preferential policies were gradually abolished in the 1990s, the long-term uneven distribution of educational resources has underlined the variation in school quality and thereby parental evaluation. As such, the premium parents have to pay for high-quality schools in housing price has become a huge burden and caused major public outcry especially in Beijing, the capital city.[1] It was under this context that the Beijing municipal government launched a six-year school equalization reform in 2014 to improve educational equality. Each year the authority nominates a set of low-performing primary schools, which we refer to as "target schools," and assigns each of them a partner school. Through various formats of cooperation, a target school benefits from its partner school and therefore improves its own attractiveness. The reform can be reasonably regarded as an exogenous shock to school quality, as no prior knowledge of the selection is available to the public and the timing of nominating each target school is confidential until release.

Our study examines the capitalization effect of exogenous variation in school quality caused by this reform. We combine the difference-in-differences model with a boundary-based regression discontinuity strategy. Specifically, we compare the changes in housing prices of adjacent secondhand housing units associated with target schools vis-à-vis nontarget ones. This strategy allows for an estimation of the average policy effect. Furthermore, we also employ an event study model to examine the dynamics of the policy effect over two years after nominating a target school, following Reardon et al. (2012). Using the sales

[1]The unit price of second-hand houses in the attendance zones of the most popular schools can be as high as 300 thousand yuan/m$^2$, five to six times the average value in the Metropolitan Area. By contrast, the average disposable income per capita of urban households in Beijing as of 2015 is 52,859 yuan (Beijing Municipal Bureau of Statistics & NBS Survey Office in Beijing 2015).

records of housing units between 2012 and 2016 in four core urban districts of Beijing, we find that exogenous changes in school quality induced by the first three years of the reform (2014–2016) are on average associated with a 1.7% increase in housing prices, and it takes more than one year for this effect to become noticeable. These results are robust to different control groups. Moreover, the heterogeneity analysis shows that the policy effects are stronger for smaller housing units, for the first wave of the reform, and for target schools that are partnered with junior high schools and key primary schools.

This study provides a direct test of whether housing markets respond to policy-induced changes in school quality and finds positive and significant results. To our knowledge, it is the first of its kind to utilize an exogenous policy shock and house-level data to address this issue in the Chinese context. This study sheds light on the efforts to improve the quality of public schools in both developed and developing settings.

## LITERATURE REVIEW

### Capitalization Effect of School Quality and Empirical Challenges

There has been a long line of research in economics of education that examines the capitalization of school quality into housing prices (see Black and Machin 2011; Nguyen-Hoang and Yinger 2011). A central challenge to this research is the issue of omitted variable bias or simultaneity as housing decision and schooling decision are jointly determined (Black and Machin 2011). In other words, are higher housing prices a reflection of parental valuation of school quality, or higher housing price are just the results of residential segregation that wealthier people tend to live in more expensive neighborhoods? To address these concerns, researchers have exploited various quasi-experimental designs in more recent literature to arrive at causal relations.

Two lines of research are worth mentioning. One is the boundary-based regression discontinuity approach pioneered by Black (1999), which essentially compares houses across boundaries of schools catchment areas to eliminate area-specific unobservable variables. She finds that parents are willing to pay 2.5% more for a 5% increase in test scores, an estimate that is 30 to 40 percent smaller than traditional cross-sectional estimates. A number of researchers (e.g., Fack and Grenet 2010; Dhar and Ross 2012; Gibbons, Machin, and Silva 2013) have taken advantage of this approach to show that one standard deviation increase in public school performance raises housing prices by 1.4% to 3%. A key assumption of this method is that all things but school quality change smoothly across boundaries. However, as parents relocate for schools, there may be significant differences in demographic traits on opposite sides of district boundaries that are associated with schooling decisions (Bayer, Ferreira, and McMillan 2007).

The other line of literature exploits exogenous policy shocks to school quality, perceived or real, to estimate its causal effects on housing values through a difference-in-differences approach using repeat sales records before and after the reform. The first type draws on the school finance equalization reforms that led to significant increases in education spending in poorly financed school districts following legal success in California in 1971. Dee (2000) and Chakrabarti and Roy (2015) find that exogenous increases in educational expenditures induced by school finance reforms have caused housing prices to rise, especially in the lowest spending school districts before the reforms in California and Michigan. The second type of policy shock comes from the rezoning of school districts, usually within a single municipality. Such reforms change the boundaries of individual schools, and relevant studies use house-level data to track the changes of housing values in response to the rezoning (Bogart and Cromwell 2000; Somerville and Ries 2010; Agarwal et al. 2016). The third category of policy shock is concerned with information release about school quality (Figlio and Lucas 2004; Imberman and Lovenheim 2016), and the research mostly finds short-time effects on housing values. A more recent strand of literature examines the penetration of charter schools and, because parents can perceive charter schools as either competitors or complements to traditional public schools, finds conflicting effects on housing values (Horowitz, Keil, and Spector 2009; Andreyeva and Patrick 2016; Brehm, Imberman, and Naretta 2016). However, none of these studies on policy shock answers the question how much parents would value the comprehensive reform of low-performing schools.

## Empirical Research in China

The capitalization of school quality into housing prices has received attention of scholars in China since the 2000s, but most of these studies have only employed hedonic price modeling. A few studies stand out in their attempts to address the omitted variable bias. For example, two studies make use of exogenous policy shocks. Feng and Lu (2013) examine the nomination of Experimental Model Senior High Schools in Shanghai, and they find that an additional Experimental Model Senior High School per kilometer increases housing prices by 17.1%. However, the assumption of a residence-based enrollment policy does not strictly hold for high schools. Similarly, Wen, Xiao, and Zhang (2017) build their research on a natural experiment of "zero school choice" policy and confirm that the policy strengthens the capitalization effect of basic education facilities, particularly junior high schools, into housing prices. While they adopt a difference-in-differences strategy, they fail to test the common time trend assumption, and therefore the between-group differences in the prepolicy time trends of housing prices, if any, may bias the estimated policy effects.

Another two studies take advantage of the institutional setting that only children of homeowners, rather than renters, are entitled to enroll in a local public school. Zheng, Hu, and Wang (2016) control for the difference in unobserved neighborhood traits by comparing the estimates of price premiums from resale prices and from housing rents. They find that a within-zone housing unit is sold 6.8% more than if it were outside the attendance zone of a key primary school. However, it is not uncommon for parents to purchase a small housing unit assigned to an elite school simply for the enrollment rights whilst living in a rented and larger housing unit nearby. This phenomenon induces rent premiums of school quality as well, and therefore they may underestimate the value of a Key Primary School. Zhang and Chen (2017) employ similar strategy and takes into account this phenomenon. They compare the rental-yields of housings assigned to key schools and of those assigned to ordinary schools nearby, and they find an average gap of 0.1–0.35% for different subgroups. The gap is the largest for small apartments, which reflects the aforementioned phenomenon. One important limitation of their study, however, is their estimation of annual rental yields using hedonic price models for the housings whose rents are not available in the dataset. As the missing values in rents may not be random, this estimation may be biased.

Our study, therefore, combines the merits of these prior ones and, by leveraging exogenous, substantive improvement in school quality, provides more precise, and reliable causal estimates of the capitalization effect of school quality.

## THE BEIJING REFORM

The Beijing school equalization reform, announced in 2014, has intended to expand the access to existent high-quality schools without additional resources. For primary schools, a group of low-performing ones are nominated by the government as "target schools," and each of them is then teamed up with a high-quality school or institution. The specific formats of cooperation vary and can be classified into "horizontal integration" or "vertical integration." Horizontal integration enables resource sharing between a target school and its partner by forming alliance, franchising, or merger, where the partner can be a competitive primary school, a university or a research institution. This type of integration directly improves the primary education that target schools provide. Vertical integration, where the partner schools is an elite junior high school, entitles a target school to better secondary education by establishing a nine-year school with the partner or endowing the target school with fixed enrollment slots to the partner (instead of a purely lottery-based enrollment). These measures are expected to increase the quality, or at least the attractiveness, of target schools, thus reducing gaps among primary schools within districts.

The reform sets a six-year phase-in plan and has been implemented in three waves by the time of our study. Within each academic year (from fall to spring), a new wave of target schools is nominated sometime before June by district authorities,[2] but the exact timing of such nomination is somewhat marked by an element of randomness. As the selection of target schools is a governmental decision, and most parents and homeowners (i.e., vendors of secondhand houses) have no prior knowledge, the reform can be regarded as a natural experiment. For a primary school, an attendance zone is usually defined by a list of residential complexes, each including one to several buildings, and each complex is generally assigned to one school only. The attendance zone is almost invariant across academic years, with minor modifications due to the changes in housing stock and adjustments by the local authority (Zheng, Hu, and Wang 2016). In this case, the nomination of target schools sends an almost certain signal that the housing complexes within their attendance zones will increase in educational value. Therefore, the time of nomination is regarded as when the exogenous policy shock comes, and the treatment effect on housing values is of our primary interest.

In light of the literature, these housing values will rise positively in response to improved school quality, and the magnitude of this policy effect equals the value of this improvement. In the meantime, the overwhelming media coverage and the hype of housing agents at the early stage of the reform may get some parents hooked, thus inducing sharp and immediate jumps in housing values. Conversely, there are reasons to cast doubt on this picture, as

[2]District is a county-level administrative division subordinate to municipality.

school quality is not necessarily improved, at least in the short run. The complexity of educational activities indicates that exogenous interventions on a school take much time to effect changes in student performance or other outcomes. This process involves the internalization of external changes (extra fiscal resources, organizational changes, etc.) into classroom practices, and it may be choked off by intrinsic inertia within the education system. Therefore, as parents are rational decision makers, they may wait for the evidence of improvement in schooling outcomes before deciding on housing purchases. All in all, the short-term effect of the reform on housing prices is not clear a priori depending on the rationality of parents, while the long-term effect is expected to be positive on average due to considerable changes in school quality induced by the reform. This study aims to provide empirical evidence of these effects.

## DATA

Beijing has a total of sixteen administrative districts, with 1,040 public primary schools and 460 public lower secondary schools (Beijing Municipal Bureau of Statistics & NBS Survey Office in Beijing 2015). We limit the analysis to primary schools in four of six districts in the metropolitan area (Dongcheng, Xicheng, Haidian, and Chaoyang) and the resale records of housing units (i.e., secondhand ones), within their attendance zones. There are three reasons behind this restriction. First, only primary schools define attendance zones as specific residential complexes, and they enroll students more rigorously from their zones. Second, due to the already high degree of urbanization and the limited number of new housing projects, most housing purchases in the metropolitan area are resale homes, and the housing stock remains inelastic, which is a fundamental assumption behind housing valuations of school quality. Third, the two excluded districts (Fengtai and Shijingshan) are much less developed in terms of educational quality than the other four in the metropolitan area, and even the best schools in those two districts seldom attract school-oriented homebuyers, not to mention the relatively weak target schools. Taken together, the restriction corroborates a full capitalization effect, if any, of the school reform. In this case, the following analyses are built on these schools and housing units, unless otherwise specified.

### Target Schools in the Equalization Reform

Since few government documents are readily available to the public with regard to the selection of target schools, we had to resort to web-based searches. By utilizing Baidu News, the largest search engine of media coverage in China, we compiled the list of target schools along with the timing of nomination, the intended partner school and the format of cooperation for each of them.[3] We also triangulated the information via other relevant websites,

[3]Similar to Google News, Baidu News (http://news.baidu.com/) provides links to a selection of local, national and international news relevant to the user's query, but all the results it displays are from Chinese sources. In our effort to compile the list of target schools, we used a number of keywords in our queries including "Beijing school equalization 2014," "Beijing compulsory education reform," and the like. We carefully read through the retrieved news articles, and added into our list keep only those schools that are reported to be target ones by reliable media outlets and news agencies.

TABLE 1
Number of target schools, by formats of cooperation

| | *Horizontal integration* | | | | |
|---|---|---|---|---|---|
| | *With primary school* | | | | |
| | *Key* | *Non-key* | *With research institution* | *Vertical integration* | *Total* |
| First wave (13–14) | 20 | 22 | 2 | 30 | 67 |
| Second wave (14–15) | 8 | 18 | 13 | 38 | 73 |
| Third wave (15–16) | 3 | 13 | 2 | 9 | 26 |
| Total | 31 | 53 | 17 | 82 | 166 |

*Notes:* The table reports the numbers of target schools subject to different formats of cooperation in the first three waves (nominated in the three consecutive academic years, respectively) of Beijing's school equalization reform. All the target schools are primary schools in this context. The counts are limited to those located within the four core urban districts (Xicheng, Dongcheng, Haidian, Chaoyang). Horizontal integration refers to partnership, franchising or merger with another high-performing primary school (either formally named key school or non-key school) or a research institution, hence improving the quality of educational activities within the target school via introducing external educational resources. Vertical integration links a target school to an elite junior high school through fixed enrollment slots, and therefore guarantees students high-quality future education. In a few cases, a target school is simultaneously subject to horizontal and vertical integration, so the "total" column does not equal the sum of previous columns.

*Source:* Collated from media coverage.

including online forums, the webpages of some schools, among others. The time of nomination is defined as the posting time of the earliest news article that reports the nomination of that school, as this is when the public first received this information. Because our empirical analysis focuses on changes in housing values across months, we translated the exact time of nomination into the "month of nomination," which is equivalent to a month if the time of nomination falls into the first half of it, and equals to the following month otherwise. Altogether, there are 166 target schools in the three waves, accounting for more than 40% of the population of public primary schools in our area of focus. Tables 1 and 2 show that most of the nominations occurred in the first two waves, and that target schools in the third wave were mostly located at the periphery and of low quality.

The distribution of these schools over the month of nomination is further illustrated in Figure 1. Obviously, there is noticeable variation in the month of nomination, suggesting an element of exogenous variation as described earlier.

## Resale Records of Secondhand Housing Units

We obtained all the resale records of housing units from the Lianjia website, a leading housing agency in China, using a web crawler software in October 2016, but only kept those between July 2012 and May 2016 that straddle the reform.[4] The raw data comprised resale

[4]Lianjia (http://www.lianjia.com/) has a large business in the resale market of houses, with a share over 50% in Beijing. It also has the information of most remaining resales which are not transacted through its agents. More importantly, all of these resale records that Lianjia possesses are posted on its website, which provides a nearly full sample of Beijing's resales.

TABLE 2
Descriptive statistics, with balance tests

| Continuous variables | M (SD) Control | Treatment | All | Group differences (t) |
|---|---|---|---|---|
| Resale's unit price (yuan/$m^2$) | 47,909 | 48,311 | 48,134 | −402.3** |
| | (15,631) | (15,659) | (15,648) | (−3.21) |
| Size ($m^2$) | 79 | 76 | 77.3 | 3.008** |
| | (36) | (38.3) | (37.3) | (10.08) |
| # of bedrooms | 2 | 1.92 | 1.96 | 0.0806** |
| | (.758) | (.776) | (.769) | (13.11) |
| # of public rooms | 1.15 | 1.09 | 1.11 | 0.0553** |
| | (.492) | (.485) | (.489) | (14.17) |
| Total # of floors | 14.3 | 14.2 | 14.2 | 0.0785 |
| | (7.92) | (7.88) | (7.9) | (1.24) |
| Enjoying an elevator | .609 | .558 | .58 | 0.0509** |
| | (.488) | (.497) | (.494) | (12.90) |
| Age (year) | 17.9 | 17.9 | 17.9 | 0.0281 |
| | (9.75) | (8.83) | (9.25) | (0.38) |

| Categorical variables | Percentage Control | Treatment | All | Group differences ($\chi^2$) |
|---|---|---|---|---|
| Type of floor | | | | 1.926 |
| Low (incl. basement) | 29.51 | 29.67 | 29.60 | |
| Medium | 37.16 | 36.64 | 36.87 | |
| High | 33.32 | 33.69 | 33.53 | |
| Decoration | | | | 0.564 |
| Poorly or nondecorated | 25.79 | 26.01 | 25.91 | |
| Well decorated | 34.19 | 33.95 | 34.05 | |
| Others | 40.02 | 40.05 | 40.03 | |
| Orientation | | | | 49.54** |
| Non–south facing | 27.54 | 30.09 | 28.97 | |
| South facing | 35.91 | 34.62 | 35.19 | |
| South and north facing | 36.55 | 35.29 | 35.85 | |
| Observations | 27,925 | 35,522 | 63,447 | |

*Notes:* The table reports the summary statistics of the outcome and explanatory variables for resales of housing units, as well as the balance tests across treatment and control groups. The treatment group consists of resales within residential complexes assigned to target schools. For each of these complexes, three closest nontreated complexes that are no more than 750 m away are identified, and resales within these complexes constitute the control group. One square meter is approximately equal to 10.8 $ft^2$, and one yuan equals $0.145 U.S. dollar (as of April 2017). Total number of floors, elevator availability and age are attributes of the building that contains a resold housing unit. A south and north facing unit has both south facing and north facing room(s), a south-facing unit has room(s) facing south, southwest, or southeast but none facing north, and a non–south-facing unit has no room facing south, southwest, or southeast.
$^{**}p < .01$.

*Source:* Lianjia, a leading housing agency in China.

records of individual housing units. Each record contains transaction information (date, price, etc.), name of the residential complex that the housing unit belongs to, and the physical attributes of the housing unit (size, number of bedrooms, etc.). We also collected the

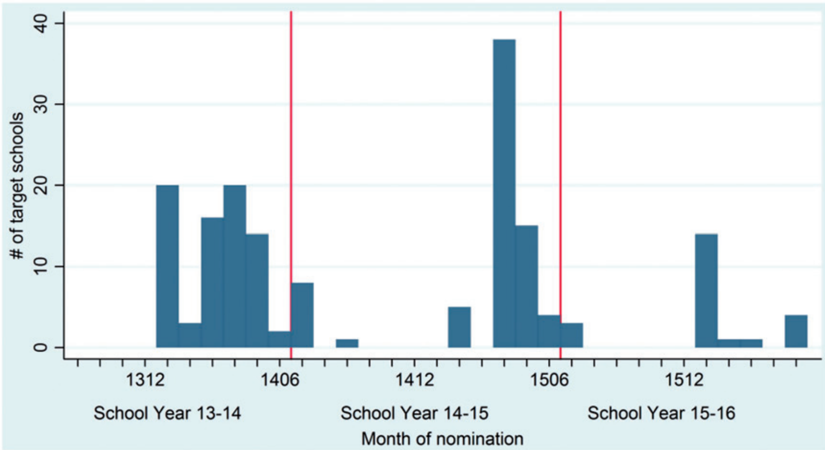


FIGURE 1 Number of target schools, by month of nomination. The figure plots the numbers of target schools in the first three waves (nominated in the three consecutive academic years, respectively) of Beijing's school equalization reform. The counts are limited to those located within the four core urban districts (Xicheng, Dongcheng, Haidian, Chaoyang). For modeling purposes, the month of nomination is defined as including the second half of its precedent and the first half of itself. *Source*: Collated from media coverage.

longitude and the latitude of each residential complex that appears in the resale records from Baidu Map, a representative map service in China.

### Matching Housing Units to Primary Schools

In most cases, all the housing units within the same residential complex are assigned to the same primary school. However, matching individual complexes to the attendance zones of primary schools proved to be another ordeal, as such information is not readily available on the websites of local educational authorities or schools themselves. To resolve this issue, we relied on the printed notices with information on the attendance zone that are put up outside of the gates of each school in preparation for the annual enrollment of students at the end of May each year. Although it was impossible to see the past notices on the spot at the time of our study, we were able to obtain them in two ways. First, we retrieved pictures of some of these notices from several online forums where parents discuss their children's schooling. These pictures were posted every year by parents who came to see the schools' notices. Second, to improve the accuracy of this mapping, we also contracted a research firm to visit each and every public school in May 2015 and May 2016 to systematically take pictures of these notices. With these pictures in hand, we coded the information in them and established a database of school-complex mapping for each academic year.

The databases were used to identify the corresponding school for each resale record, where the name of a residential complex served as the key for matching. Due to the incompleteness of the school-complex information in earlier years and the aliases of housing complexes, the matching process generated some missing values. Fortunately, however, we managed to identify most of the resale records assigned to target schools. Other records, even if they were not successfully matched in the previous process, naturally fell into the attendance zones of nontarget schools. Finally, we identified 59,394 resale records of housing units assigned to target schools.

## EMPIRICAL STRATEGY

The biggest challenge to estimation is nonrandom assignment to the treatment, since the reform selects target schools. There are at least three sets of confounding variables: school-level characteristics, especially those related to school quality; features of the residential complexes; and unobserved factors that are spatially correlated. To eliminate these confounding effects, we first introduce residential complex fixed effects to purge the effect of complex-level characteristics, and the multiple observations of resales within a single residential complex make it possible. Moreover, these complex fixed effects almost absorb the confounding effect of school-level features, as most complexes are assigned to the same school across years. Finally, we deal with spatially correlated unobserved factors employing a boundary-based discontinuity strategy following the rationale of Black (1999).

We match each of the residential complexes assigned to target schools with three of the geographically nearest complexes within a 750-m radius that are mapped to a nontarget school. If no match is found in proximity, this complex is removed from the set. Any matched complex can be matched to another complex associated with another target school. After this matching process, we include into the treatment group the resales within the remaining complexes that are assigned to target schools, and include the resales within the matched nontarget complexes into the control group. Finally, the treatment group constitutes 35,522 resale records, while the control group includes 27,925, which provide the counterfactuals in the estimation. Figure 2 depicts the distribution of the treated resale records over the month of nomination of their assigned target schools, which is similar to that of target schools in Figure 1. This means sufficient variation in the timing of policy shock. Figure 3, however, plots the treatment and the control group in a map. It is clear that the resale records are comparatively concentrated at the center of the city with a longer history of urbanization.

We first build a simple two-way fixed-effects model to estimate the average treatment effect on the treated (ATET):

$$\ln UnitPrice_{ijtk} = \delta + \rho Nominate_{it} + \boldsymbol{\beta}'\mathbf{X}_k + Month_t + Complex_j + \varepsilon_{ijtk} \tag{1}$$

where the dependent variable is the logarithm of the resale's unit price of housing unit $k$ within residential complex $j$ assigned to school $i$, resold in month $t$. $Nominate_{it}$ is a dummy variable that indicates the treatment status; that is, $Nominate_{it}$ equals 1 if school $i$ has been nominated as a target school in month $t$. The remaining part of the model consists of an intercept, a vector of house-level characteristics (along with some quadratic terms), month and complex fixed effects, and a random error.

This pooled regression model with fixed effects closely resembles a difference-in-differences estimation (Evans, Murray, and Schwab 1997). Specifically, it compares the changes in resale prices of housing units in close proximity but associated with a target school vis-à-vis a nontarget one. The identifying assumption here is that the prices of housing units assigned

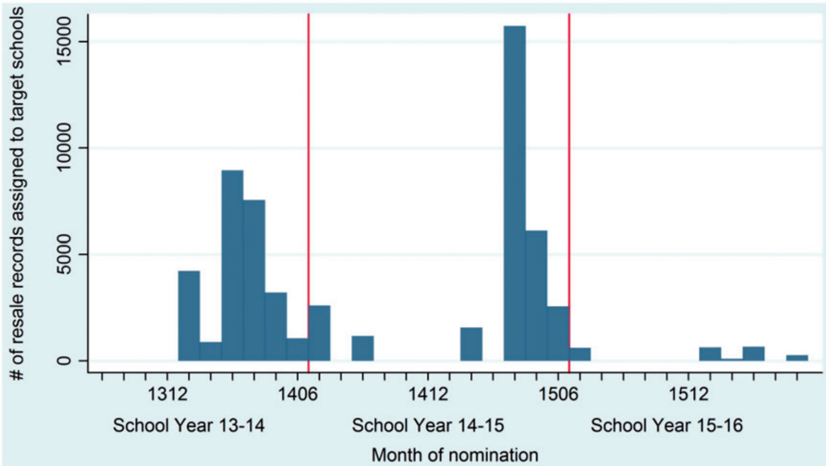


FIGURE 2 Number of resale records assigned to target schools, by month of nomination. The figure plots the numbers of resale records of housing units assigned to target schools in the first three waves (nominated in the three consecutive academic years, respectively) of Beijing's school equalization reform. The counts are limited to those located within the four core urban districts (Xicheng, Dongcheng, Haidian, Chaoyang). The horizontal axis indicates the month of nomination of the target school to which a resale is mapped. For modeling purposes, the month of nomination is defined as including the second half of its precedent and the first half of itself. *Sources*: Profiles of target schools collated from media coverage; information on resale-school mapping collated from school attendance zones.

to target schools would, in the absence of the reform, have had similar trends to those assigned to nontarget schools after controlling for the covariates and fixed effects in Equation 1. When this holds, the coefficient $\rho$ captures the policy effect.

Moreover, it would be interesting to know how the effect changes over time, which sheds light on the dynamics of public responses to the reform. As there is considerable variation in the timing of nomination, we employ an event study model. This model typically introduces a set of dummies which track the policy effects in each of the time periods before and after the event, and it suits the scenario where the policy shock takes place at different time points for different individuals (Sandler and Sandler 2014). In line with Reardon et al. (2012), the estimation equation is given as

$$\ln UnitPrice_{ijtk} = \sum_{m=-44,\, m\neq 0}^{+29} \gamma_m D_{it}^{m} + \boldsymbol{\beta}'\mathbf{X}_k + Month_t + Complex_j + \varepsilon_{ijtk} \qquad (2)$$

where $D_{it}^{m}$ is a dummy equal to one if school $i$ is nominated in month $t-m$. The omitted month (where $m = 0$) is defined as the one preceding the "month of nomination." The superscript $m$ can be negative to accommodate the prenomination months. For housing units in the control group, their dummies will always be zero regardless of $m$. Other components of the model are no different from those in Equation 1. In essence, each of the dummies indicates the estimated policy effect on a treated housing unit relative to a control one when the former is purchased $m$ months away from the nomination of its corresponding target school.

While Equation 2 is seemingly more sophisticated than Equation 1, both follow the same common trend assumption, and this event study model allows for a systematic examination.

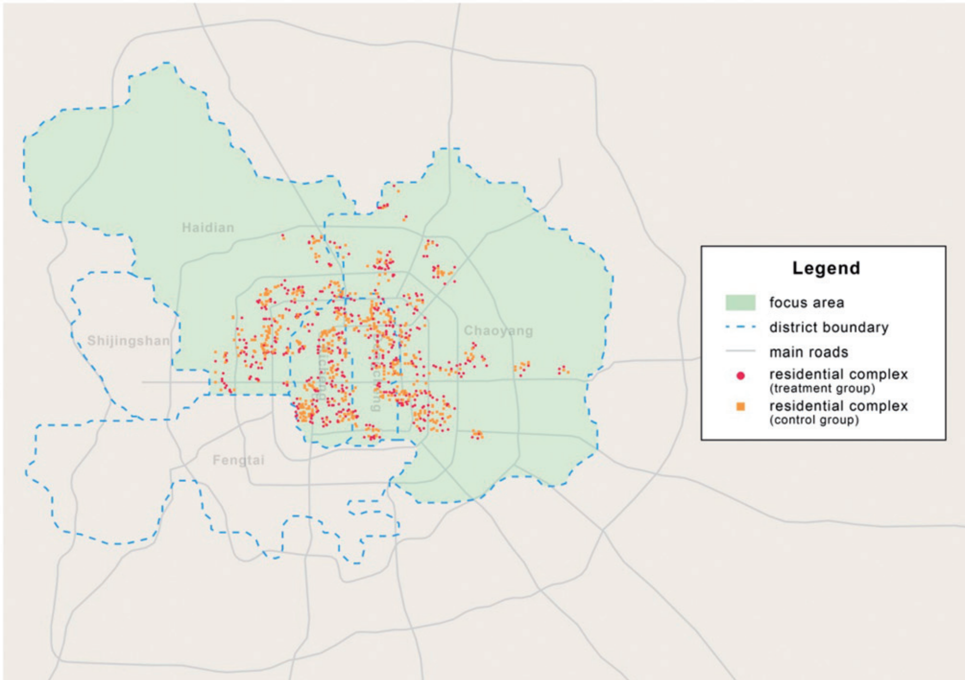


FIGURE 3 Geographical distribution of the residential complexes in the baseline sample. The figure illustrates the geographical distribution of residential complexes that are included in our baseline analysis. The radius of constructing the control group is 750 m. *Sources*: Profiles of target schools collated from media coverage; information on complex-school mapping collated from schools' attendance zones; coordinates of residential complexes retrieved from Baidu Map.

If $\gamma_m$ is not statistically significant for all $m < 0,$ we can deem that the time trend before policy shocks is the same across the groups. Although there is no guarantee that the postreform trends will necessarily be the same, it gives us more confidence in interpreting the $\gamma_m$ estimates for all as the causal effects of the school equalization reform.

## RESULTS

### Baseline Results

The left three columns of Table 2 present the descriptive statistics of the control group, the treatment group and the combined sample (referred to as "baseline sample" hereafter) with a radius of 750 m, respectively. The rightmost column reports the results of balance tests, where the upper part shows the group difference along with the *t*-test result for each continuous variable, and the lower part presents the chi-square test result for each categorical variable.

The average resale's unit price in the baseline sample is approximately 48,134 yuan/m$^2$, a value close to 52,859 yuan (approximately \$7,659 U.S. dollars), the average disposable income per capita of urban households in Beijing as of 2015 (Beijing Municipal Bureau of Statistics & NBS Survey Office in Beijing 2015). On average, a resold housing unit has a size of 77.3 m$^2$ (approximately 832 ft$^2$), with 1.96 bedrooms and 1.11 public rooms, which is a typical setting for a Chinese family with two parents and one child. About 58% of the resold housing units have an elevator available in the building, which is on average 14.2 floors high and 17.9 years old. Slightly more than a third of the units have room(s) facing south and north, which imply favorable lighting and ventilation, and another one third have south-facing room(s) (including southeast and southwest facing), yet no north-facing ones, indicating only advantaged lighting conditions. The rest of them, however, have all room(s) facing directions other than south, southeast, or southwest.

Turning to the fourth column, 6 of 10 variables have statistically significant differences across groups with the large sample size, but the magnitudes of these group differences are in general small. For example, the resale's unit price, the major outcome variable, is 0.8% higher in treatment group. As the statistics include resales within both prereform and postreform times, this gap may to some extent be attributed to the policy effects that raise housing values of the treatment group.

Using the baseline sample, estimates from Equation 1 are shown in Table 3. In column 1, where only the dummy for treatment status and month fixed effects are included, there is a positive but insignificant overall effect of the reform on housing values. Adding the control variables (i.e., attributes of the resold housing units), the estimate increases to a statistically significant value of 0.0496, as shown in Column 2. Finally, we include residential complex fixed effects to account for complex-level and school-level confounding factors in Column 3, which is the preferred specification. The estimated policy effect reduces to 0.0172, significant at the 1% level; that is, the reform on average raises housing values associated with target schools by 1.7%, or nearly 830 yuan/m$^2$ as calculated from Table 2. From the coefficients of size and its quadratic term, it is easy to calculate that, within a reasonable range, a 10-m$^2$ increase is merely linked to a 0.2–0.3% decline in the resale's unit price. By contrast, one extra bedroom raises the unit price by 2.9%, and one additional public room induces a 5.3% increase. The availability of an elevator significantly raises housing values by 2.4%, while total number of floors and age do not exhibit significant effects. Medium-story housing units are resold at higher prices than low-story and high-story ones, and decoration is significantly valued by the market. Orientation also plays an important role as expected, with a 6.3% premium for housing units facing south and north, and 5.2% for those facing south, compared with non–south-facing units.

Figure 4 plots the estimated coefficients in Equation 2 (the $\gamma_m$s) on the dummies indicating the number of months since nomination and their 95% confidence intervals, using the same baseline sample. The figure shows the average trends in housing prices assigned to target schools before and after the nomination, relative to contemporaneous trends in their matched counterparts. In other words, the coefficients $\gamma_m$, when $m > 0$, show how the policy effects change over time in the 24 postnomination months. According to the previous discussions, short-term and long-term effects may be rather different, and the two-year time window is sufficient to capture both types of effects. On the other hand, the prenomination

TABLE 3
Estimated average policy effects on housing values

| *Log of resale's unit price (yuan)* | *(1)* | *(2)* | *(3)* |
|---|---|---|---|
| *Main effect* | | | |
| Nomination of target schools | .0439 | .0496† | .0172* |
| | (.0292) | (.0274) | (.008) |
| *Control variables* | | | |
| Size ($m^2$) | | −.00295** | −.00309** |
| | | (.00104) | (.000378) |
| Size squared | | 9.34e-06** | 4.93e-06** |
| | | (2.70e-06) | (1.01e-06) |
| # of bedrooms | | .0147 | .0286** |
| | | (.0126) | (.0051) |
| # of public rooms | | .0454** | .0531** |
| | | (.0163) | (.00846) |
| Total # of floors | | −.0112** | −.0021 |
| | | (.00171) | (.00146) |
| Enjoying an elevator | | .138** | .0241† |
| | | (.0229) | (.0144) |
| Age (year) | | −.00934* | −.0026 |
| | | (.0044) | (.00246) |
| Age squared | | .000213** | 6.94e-06 |
| | | (.0000748) | (.0000362) |
| Type of floor (low/basement as the reference type) | | | |
| Medium | | .0155** | .0233** |
| | | (.00455) | (.00239) |
| High | | −.00761 | .00871** |
| | | (.00484) | (.00305) |
| Decoration (poorly/nondecorated as the reference type) | | | |
| Well-decorated | | .0535** | .0256** |
| | | (.00722) | (.00177) |
| Others | | .0302** | −.00506 |
| | | (.0105) | (.0033) |
| Orientation (non–south facing as the reference type) | | | |
| South facing | | .000735 | .0519** |
| | | (.0103) | (.00289) |
| South and north facing | | .0181 | .0626** |
| | | (.0117) | (.00354) |
| Month FE | Yes | Yes | Yes |
| Complex FE | No | No | Yes |
| Observations | 63,447 | 63,447 | 63,447 |
| $R^2$ | .166 | .228 | .834 |

*Notes:* The table reports the estimates from Equation (1). Each column represents a separate regression, with different control variables. The common sample used in the regressions consists of resale records of housing units that are assigned to target schools (treatment group) and the records that are nontarget and no more than 750 m away (control group). Standard errors are reported in parentheses and clustered at the complex level.
†$p < .1$, *$p < .05$, **$p < .01$.

coefficients $\gamma_m$, when $m < 0$, provide a formal test on the validity of the assumption in the event study model.

In Figure 4, the assumption of common time trend before the reform seems to hold, as the majority of the prenomination coefficients do not deviate significantly from zero. On average, the reform's effect on housing values is positive but barely significant during the first year after nomination. However, starting from the second postnomination year, housing prices around target schools grow in a rapid and steady manner. Twenty-four months after nomination, the reform induces a 12% increase in housing values. It is hardly surprising that the effects are not immediate because parents may want to wait until they see real changes in school quality such as better classroom practices, school culture and other aspects of education. Also, it may take some time for parents to complete the financial transaction.

## Robustness check

In our specification, the proximity-based control group serves to control for the spatially correlated unobservables and therefore construct reliable counterfactuals. To further validate the results in the previous subsection, we use 300-m and 1,500-m radii separately, and re-estimate the policy effects using these different control groups. The average policy effects and their dynamics in these three cases are reported or illustrated in Table 4 and Figure 5, respectively.

In Table 4, all three columns report significant and positive policy effects on housing values, and this effect becomes stronger as the radius becomes smaller. In the 300-m case, the effect is as large as 3%. As narrower bands may signify greater similarity in unobservables across treatment and control groups, these results reasonably corroborate the reform's average effects on housing values.

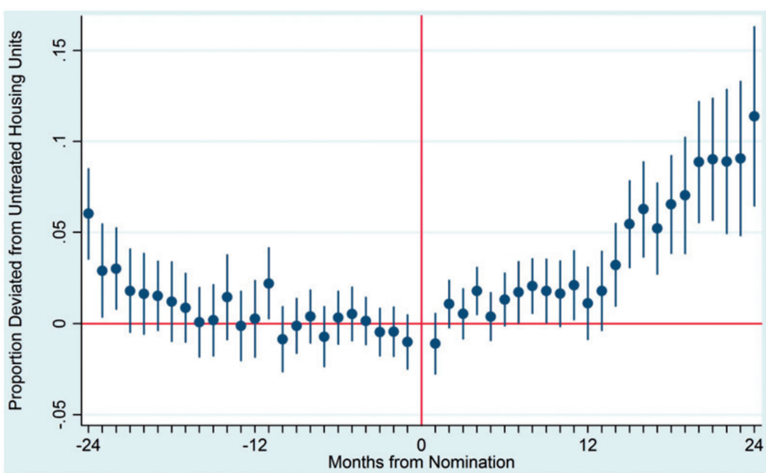


FIGURE 4 Estimated dynamics of the policy effects on housing values. The figure plots the estimated coefficients of the dummies in Equation 2. The dots and spikes illustrate the point estimates and 95% confidence intervals, respectively. The sample consists of resale records of housing units that are assigned to target schools (treatment group) and the records that are nontarget and no more than 750 m away (control group).

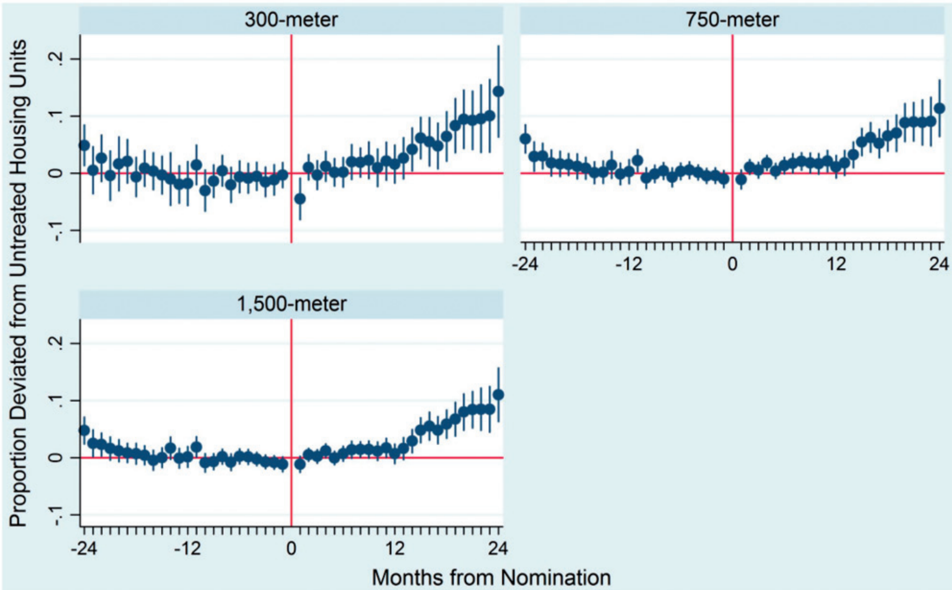


FIGURE 5 Estimated dynamics of the policy effects on housing values, with different control groups. The figure plots the estimated coefficients of the dummies in Equation 2. The dots and spikes illustrate the point estimates and 95% confidence intervals, respectively. Each subfigure represents a separate regression with a different radius of constructing the control group.

TABLE 4
Estimated average policy effects on housing values, with different control groups

| *Log of resale's unit price (yuan)* | *(1)* *300 m* | *(2)* *750 m* | *(3)* *1,500 m* |
|---|---|---|---|
| Nomination of target schools | .0302* | .0172* | .013† |
| | (.0149) | (.008) | (.00678) |
| Control variables | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes |
| Complex FE | Yes | Yes | Yes |
| Observations | 23,078 | 63,447 | 76,888 |
| $R^2$ | .807 | .834 | .835 |

*Notes:* The table reports the estimates from Equation (1). Each column represents a separate regression with a different radius of constructing the control group. Control variables are the same as in the Column 3 of Table 3. Standard errors are reported in parentheses and clustered at the complex level.
†$p < .1$, *$p < .05$.

In Figure 5, the time trends are quite similar across three subfigures. First, prenomination estimates are not significantly different from zero, verifying the validity of the model. Second, the positive effects start to become increasingly noticeable since the twelfth month after nomination. These common patterns suggest that it takes approximately a year before the housing market starts to respond to the school equalization reform.

## EFFECT HETEROGENEITY

In this section, we examine the heterogeneous effects of the school equalization reform on housing prices arising from the variation in the way target schools are integrated with partner schools, the timing of the reform, and the inherent characteristics of housing units.

### Different House Sizes

Unlike in the United States, where fiscal zoning was practiced to prevent free-riding on the local tax bases (Oates 2006), the current enrollment policy in Beijing only requires that parents be homeowners within a given attendance zone and does not set a lower bound on housing consumption. For economic purposes, therefore, it is quite common for parents to buy a small housing unit assigned to an elite school *only* to enroll whilst living in a rented unit nearby. For this reason, we expect that prices of smaller housing units will be more sensitive to the reform. To test this heterogeneity, we review the distribution of the baseline sample over house sizes, and use the 20%, 50%, and 80% percentiles to divide the resales into four groups: 50 $m^2$ or below, 50–65 $m^2$, 65 to 100 $m^2$, and 100 $m^2$ or above.

Using these four subsamples, we first rerun the models in Equation 1 to see the variation in average policy effects, and report the results in Table 5. It is obvious that smaller housing units respond more actively. The group of the smallest units ($\leq 50\,m^2$) is the only one that has a significantly positive policy effect and its estimate is 3.7%, more than twice as large as the baseline estimate. The estimates for the other three groups are not statistically significant, and the magnitude decreases from 1.6% to 0.8% as the range of house size jumps. This indicates that the positive effect is primarily driven by price hikes in smaller housing units.

Figure 6 depicts the estimated coefficients from Equation 2 for each group and more clearly illustrates how the policy effect diminishes as the house size increases. The tail that curls upward in the first subfigure gradually becomes flat through the next three. For the smallest housing units in the first subfigure, the reform raises housing values by around 20% in the 24th postnomination month, although this effect emerges as late as the thirteenth month. In the fourth subfigure, by contrast, we can hardly identify any significant policy effect on very large housing units throughout the two years after the nomination.

These results together imply that the value of school quality is much more tightly bound with small housing units than with large ones. This further reflects parents' cost-effective way of thinking when purchasing schooling opportunities through the housing market.

### Different Waves of Target Schools

Parents may respond more positively to the first wave of the reform due to the overwhelming media coverage, as well as the hype from housing agents at the beginning of the reform. Therefore, we expect a stronger policy effect on housing prices associated with the first wave of target schools. For comparison, we report the results from Equation 1 using the resale records assigned to the first and the second wave of target schools (with matched control group records no more than 750-m apart) in Table 6. Consistent with the hypothesis, the

TABLE 5
Estimated average policy effects on housing values across housing units with different sizes

| *Log of resale's unit price (yuan)* | *(1)* *≤50 $m^2$* | *(2)* *50–65 $m^2$* | *(3)* *65–100 $m^2$* | *(4)* *>100 $m^2$* |
|---|---|---|---|---|
| Nomination of target schools | .0374* | .0157 | .0112 | .00806 |
| | (.0158) | (.0107) | (.00882) | (.00775) |
| Control variables | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |
| Complex FE | Yes | Yes | Yes | Yes |
| Observations | 12,070 | 19,644 | 19,744 | 11,989 |
| $R^2$ | .839 | .86 | .885 | .892 |

*Notes:* The table reports the estimates from Equation 1. Each column represents a separate regression and uses resale records with different house sizes. The radius of constructing the control group is 750 m. Control variables are the same as in the Column 3 of Table 3. Standard errors are reported in parentheses and clustered at the complex level.
$^*p < .05$.

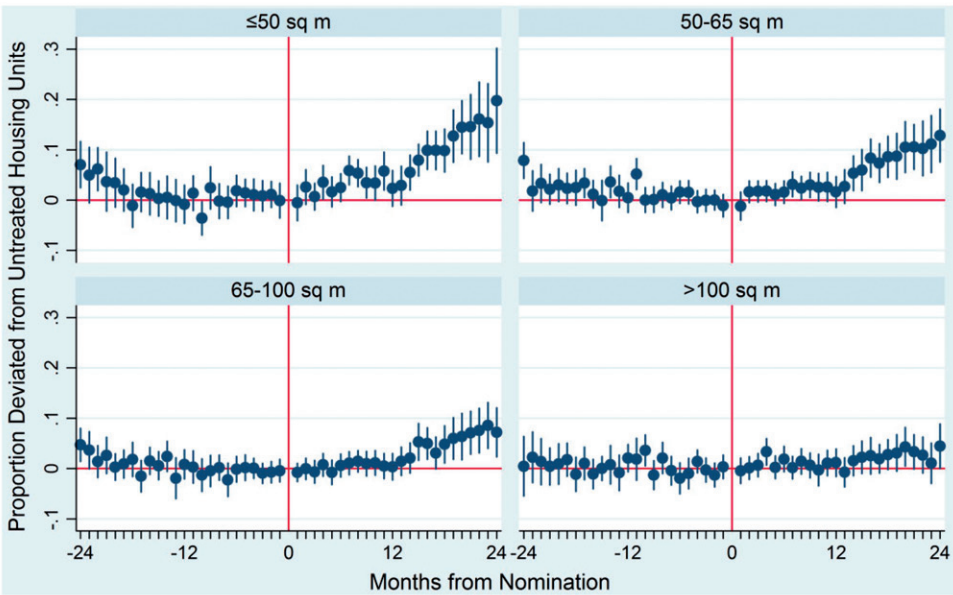


FIGURE 6 Estimated dynamics of the policy effects on housing values across housing units with different sizes. The figure plots the estimated coefficients of the dummies in Equation 2. The dots and spikes illustrate the point estimates and 95% confidence intervals, respectively. Each subfigure represents a separate regression and uses resale records with different house sizes. The radius of constructing the control group is 750 m.

estimated effect jumps to 6.1% for the first wave, a much larger figure than the baseline estimate. By contrast, the second wave has neither economically or statistically significant effect.

Moreover, we compare the dynamics of the policy effects for the first two waves of reform, as illustrated by Figure 7. In line with the previous results, the first wave of reform

TABLE 6
Estimated average policy effects on housing values, the first two waves of target schools

| *Log of resale's unit price (yuan)* | *(1)* *First wave (13–14)* | *(2)* *Second wave (14–15)* |
|---|---|---|
| Nomination of target schools | .0613** | −.00207 |
| | (.0163) | (.00941) |
| Control variables | Yes | Yes |
| Month FE | Yes | Yes |
| Complex FE | Yes | Yes |
| Observations | 26,776 | 35,126 |
| $R^2$ | .802 | .858 |

*Notes:* The table reports the estimates from Equation 1. Each column represents a separate regression and uses resale records of housing units assigned to the first and second wave of target schools (nominated in Academic years 13–14 and 14–15, respectively). The radius of constructing the control group is 750 m. Control variables are the same as in the Column 3 of Table 3. Standard errors are reported in parentheses and clustered at the complex level.
**$p < .01$.

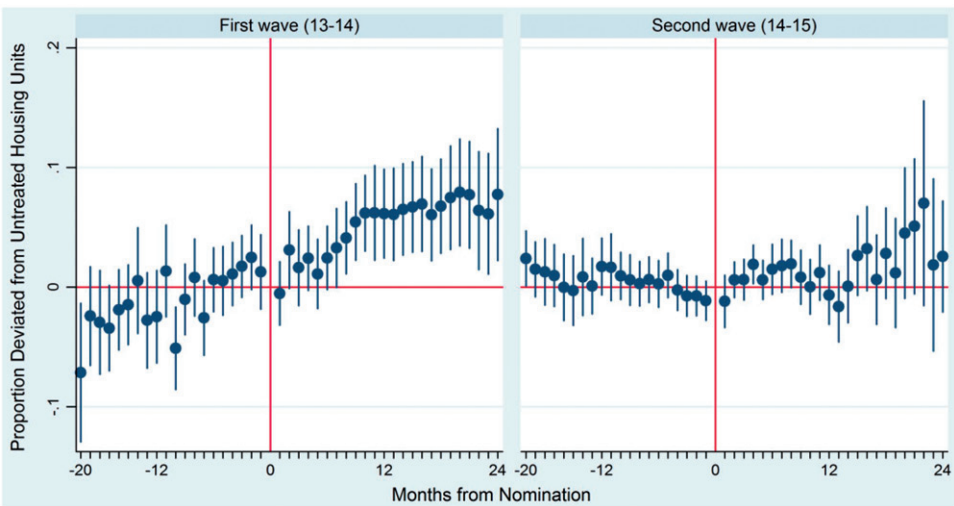


FIGURE 7 Estimated dynamics of the policy effects on housing values, for the first two waves of target schools. The figure plots the estimated coefficients of the dummies in Equation 2. The dots and spikes illustrate the point estimates and 95% confidence intervals, respectively. Each subfigure represents a separate regression and uses resale records of housing units assigned to the first and second wave of target schools (nominated in academic years 13–14 and 14–15, respectively). The radius of constructing the control group is 750 m.

starts to exert significant influence on housing prices in the sixth month after nomination, and this effect gradually increases with slowing speed and stops at around 8% in the 24th month. For the second wave, however, the policy effect is not significant at any time.

In addition to the argument that this sharp rise in housing prices is a response to the public awareness at the inception of the reform, there is at least one other contending explanation. For example, the target schools in the first wave may be integrated with schools of higher quality than those in the second wave and therefore are more attractive to the parents. Indeed, there are more target schools integrated with key schools in the first wave as shown in Table 1. When reviewing the sample distribution over formats of cooperation in each of the regressions, however, we do not find between-subsample differences extremely large. In this case, the observed patterns in our estimation results are likely a mixture of the two reasons.

## Target Schools Under Different Formats of Cooperation

Different formats of cooperation may benefit a target school (and its pupils) in different ways. For example, horizontal integration improves the quality of six-year primary education that pupils will enjoy, whereas vertical integration may bring benefits, perceived or real, of improvement in the quality of three-year secondary education to follow. Given that high schools enroll students primarily based on high-stakes testing scores and junior high schools enroll students partly through academic performance and partly through a lottery system, parents may put more emphasis on getting their children into an elite junior high school and therefore highly value vertical integration. In contrast, horizontal integration will translate into improved quality of primary education, but parents need to face more uncertainty on whether the improved resources would indeed improve learning outcomes six years down the line.

To probe this issue, we first compare the policy effects of horizontal and vertical integration. Based on Equation 1, the first and the fourth column of Table 7 report the estimated average effects of these two formats of cooperation, respectively. For horizontal integration, the estimate is 1.8% and marginally statistically significant, in contrast with a 2.4% effect significant at the 5% level for vertical integration. We also estimate how the effect changes over time with Equation 2, and present the results in the first subfigures of Figure 8. In line with the average effects, horizontal integration starts to take effect at the 13th postnomination month, while vertical integration lifts the time trend from zero soon after nomination and later causes a leap in the second year. These results show that parents value vertical integration more than horizontal integration in this equalization reform.

Moreover, we examine whether the parents are sensitive to quality of partner schools that integrate the target schools. We limit the sample to target schools that are horizontally integrated and compare those that are integrated with a key school and those integrated with a non-key school. Employing the same strategy, we report the estimated average effects in the second and the third columns of Table 7, respectively. As expected, integration with a key school raises housing value by 4.7%, significant at the 1% level, but the alternative case results in a marginally significant 2.5% effect. The last two subfigures of Figure 8 show the dynamic effects in these two cases. In the former case, the effect has an upward trend and becomes significantly positive one and a half years after nomination; in the latter case, by contrast, there is a much more modest time trend and the effects are much less precisely estimated. These results show that the quality of partner schools also matters in parental valuation, which provides another piece of evidence on the capitalization effect of school quality into housing values.

TABLE 7
Estimated average policy effects on housing values across target schools subject to different types of change

| | *Horizontal integration* | | | |
|---|---|---|---|---|
| *Log of resale's unit price (yuan)* | *(1)* *Overall* | *(2)* *With key school* | *(3)* *With non-key school* | *(4)* *Vertical integration* |
| Nomination of target schools | .0181 | .0472** | .0249 | .024* |
| | (.0111) | (.014) | (.0156) | (.0116) |
| Control variables | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |
| Complex FE | Yes | Yes | Yes | Yes |
| Observations | 37,274 | 9,373 | 23,149 | 35,053 |
| $R^2$ | .835 | .805 | .795 | .818 |

*Notes:* The table reports the estimates from Equation (1). Each column represents a separate regression and uses resale records of housing units assigned to target schools under different types of change. The radius of constructing the control group is 750 m. Control variables are the same as in the Column 3 of Table 3. Standard errors are reported in parentheses and clustered at the complex level.
$^*p < .05$, $^{**}p < .01$.

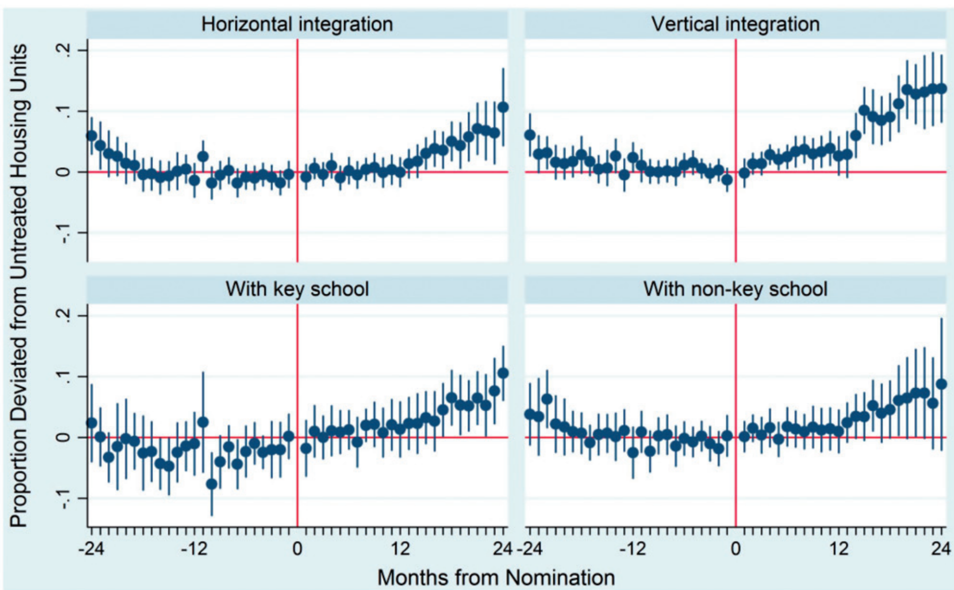


FIGURE 8 Estimated dynamics of the policy effects on housing values across target schools subject to different types of change. The figure plots the estimated coefficients of the dummies in Equation 2. The dots and spikes illustrate the point estimates and 95% confidence intervals, respectively. Each subfigure represents a separate regression and uses resale records of housing units assigned to target schools under different types of change. The radius of constructing the control group is 750 m.

## CONCLUSION AND DISCUSSIONS

Capitalization of school quality into housing prices has been a thoroughly studied research topic in the United States and increasingly so in China (see Black and Machin 2011; Feng and Lu 2013; Zheng, Hu, and Wang 2016; Wen, Xiao, and Zhang 2017; Zhang and Chen 2017). This study takes advantage of a reasonably exogenous school equalization reform starting in 2014 in Beijing to causally examine the response of housing prices to school quality.

First, we estimate the average effects of the reform using linear fixed-effects model with interpretation of a difference-in-differences specification and a boundary discontinuity design. Second, we also apply an event study model combined with the same boundary discontinuity to demonstrate how these effects change over time. In general, this piece of research shows that housing values do positively respond to the reform in a consistent and incremental fashion. Pooling the past three waves of this reform (2014–2016) together, we find that, on average, the reform raises housing values associated with target schools by 1.7%. It takes one year for this effect to rise above zero, and until the end of the second year, it can be as large as 12%. The effects from the first wave of the reform are much sharper than the second wave due to a combination of the sudden nature of the first reform and better quality schools associated with the first wave of reform. These effects tend to be driven primarily by smaller housing units that are much more responsive at 20% two years after the reform. This underscores the fact that parents tend to minimize their total expenses in the absence of local zoning restrictions. Vertical integration that allows pupils to enjoy better quality secondary education is found to be more favorable to parents than horizontal integration as a whole. Within the horizontal integration, housing units associated with key primary schools experienced much stronger increase in prices.

These results have several policy implications in the Chinese context. First, the increased housing values in the neighborhoods of improved schools imply that the expanded high-quality educational opportunities intended to target low socioeconomic status families may inversely be secured to high-incomers under the neighborhood schooling policy. Policymakers are therefore advised to revise the schooling policy while equalizing school resources for the sake of higher accessibility of high-quality education. Second, small housing units, which are most responsive to school quality, should be the target of education or housing policies that aim to tackle educational inequality associated with gaps in housing prices. Third, as parents attach more importance to lower secondary education than to primary education, local authorities may as well prioritize the development of junior high schools in the face of fiscal constraints.

There are several limitations in this study that could be tackled in future research. First, although the exogenous reform enables an estimate of the average value of an improved school, it is not feasible to calculate this value as a function of school-level test scores or expenditures owing to the lack of data. With an increased availability of administrative data in China, more precise estimates will be possible in the future. Second, the results do not reveal how it is that the effects transmit from the school reform to the housing market. Considering our hypotheses about reactions from parents and housing agents in the postreform era, a useful step forward would be to analyze discussion and comments pertaining to school reform left by parents in online forums. By leveraging text-mining techniques, future research can extract important features from the discussions, which can potentially explain the fluctuations of housing values. Third, future research may incorporate household data, which are not available at the moment, to see how the reform changes the demographic composition in the attendance zones of target schools and whether it benefits disadvantaged families as intended.

## ACKNOWLEDGMENTS

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