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Private school, off-campus academic class and public school district housing premium: Evidence from a natural experiment in Hangzhou, China

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A B S T R A C T

Private schools and off-campus academic classes (so called "shadow education") may affect housing price premiums in high-quality public school districts by offering alternative educational resources. This paper leverages China's recent compulsory education reform—which restricted private school admissions and curtailed shadow education—as a natural experiment to test this hypothesis. Using transaction data from Hangzhou, we find that the reform induced a significant increase in housing prices within high-quality school districts relative to comparable ordinary school district properties. We reveal this effect operates through two distinct channels: restrictions on private schools increased the school district premium, while limitations on off-campus academic classes partially offset this increase. Our findings contribute to a greater understanding of how market-based alternatives to public education influence housing market dynamics.

1. Introduction

The relationship between school quality and housing prices has long been a focal point in urban economics. When public school enrollment is tied to residential location, housing prices in high-quality school districts typically command substantial premiums. This phenomenon, commonly referred to as the "public school district housing premium" in the literature, is extensively documented across various contexts and time periods, and reflects the capitalization of educational quality into property values (Black, 1999; Chan et al., 2020; Dhar & Ross, 2012; Fack & Grenet, 2010; Gibbons et al., 2013).

However, the magnitude of these school district premiums very likely depends on the availability of alternative educational resources. When families have access to high-quality private schools or other supplementary educational services, their willingness to pay for properties in premium public school districts may decrease. Indeed, research in Paris has shown that proximity to private schools tends to dampen public school district premiums (Fack & Grenet, 2010). This suggests that policy changes affecting the availability or quality of educational alternatives could significantly impact local housing market dynamics.

Recent nationwide compulsory education reform in China provides an ideal setting to examine this relationship. This reform introduced two significant changes. First, it mandated that private schools must synchronize their admission processes with public schools and implement a random lottery system for admission when applications exceed available places. Second, it imposed strict

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restrictions on off-campus academic classes. These concurrent policy changes substantially altered the educational landscape. Under the new system, families can no longer secure private school admission through academic merit, and those unsuccessful in the private school lottery face limited options in the public system, given they miss the primary enrollment period for preferred public schools.

We argue that these policy changes have important implications for housing markets. To start, the reform effectively diminished the role of private schools as viable alternatives to high-quality public education, particularly affecting high-achieving students from affluent families who could previously choose between private education and purchasing property in elite school districts. At the same time, restrictions on off-campus academic classes – commonly known as "shadow education" - limited the other channel through which families could supplement their children's education outside the formal school system.

Using comprehensive housing transaction data from Hangzhou, we find that this reform led to higher prices for properties in high-quality school districts relative to comparable properties in other areas. This finding is consistent with recent studies of other Chinese cities, notably Chen and Li (2023) and Zou (2024) in Chengdu. Our findings remain robust across various model specifications, including different time windows, varying sample compositions, alternative matching criteria or school quality measures, and the inclusion of rental prices.

A unique contribution of our study lies in identifying two distinct channels through which the educational reform affected housing markets. While previous studies have primarily focused on the private school restrictions, we argue that changes in the off-campus academic training sector also played a role. Our data reveals that by late 2019, the share of off-campus academic classes in all educational institutions in Hangzhou's urban core had decreased by one-third compared with their pre-reform levels in 2017. Moreover, the spatial distribution of these institutions changed significantly, with a notable reduction in the concentration gap between high quality and ordinary school districts.

Previous research has shown that participating in off-campus academic classes can significantly improve student performance (Luo & Chan, 2022; Wang & Li, 2018; Zhang, 2013; Zhao, 2015). Indeed, enrolling children in off-campus academic tutoring has become a common practice for many families in numerous countries (Wei, 2020; Zhang & Bray, 2020).

This context is important because while public and private schools may be clear substitutes, the interaction between in-school and off-campus educational resources is more nuanced.

On one hand, from an individual perspective, in-school and off-campus educational resources function as potential substitutes in enhancing academic performance. When access to off-campus educational resources becomes restricted, families may respond by increasing their investment in in-school educational resources, potentially driving up school district housing premiums.

On the other hand, rather than being mutually exclusive choices, off-campus tutoring is often used to reinforce and deepen the understanding of subjects taught in school, helping students to excel in high-stakes examinations. Therefore, families with sufficient resources may choose to invest in *both* high-quality in-school education (e.g., via school district housing) and supplementary off-campus tutoring to maximize their children's academic performance.

This argument is supported by empirical evidence. For instance, both Li and Yongmei (2017) and Zhang et al. (2024) find that students with stronger academic foundations, more educated parents, or from families with higher economic status are more likely to enroll in off-campus academic tutoring. Zhang et al. (2024) further show that these same students were also significantly less likely to withdraw from tutoring following the 2021 "Double Reduction" policy. Directly supporting the complementarity argument, Yu and Zhao (2021) observe a crowding-in effect, where increased public education spending incentivizes high-income families to invest even more in supplementary private education, of which off-campus tutoring is a key component.

Furthermore, our data also reveals a higher concentration of off-campus academic training institutions in high-quality school districts. Therefore, restrictions on off-campus educational resources may narrow the educational resource gap between high-quality and ordinary school district neighborhoods, potentially moderating school district housing premiums.

While theoretical predictions about the dominant effect remain ambiguous, restrictions on off-campus academic classes clearly have the potential to affect school district housing premiums. Indeed, the observed changes in these premiums following the reform likely emerged through two distinct channels.

However, previous studies of this reform, including Chen and Li (2023) and Zou (2024), have neglected the role of off-campus academic tutoring. Even in the broader literature, few studies have explored how off-campus academic classes affect school district housing premiums, possibly because the required data is inherently difficult to gather.

To address this, we construct novel measures of off-campus academic services using five years (2017–2021) of Point of Interest (POI) data from Gaode Maps, a leading digital mapping service provider in China. This unique dataset allows us to track the density and distribution of off-campus academic institutions at a granular level, addressing a significant data challenge in the literature.

Our analysis yields two key insights. First, across numerous estimation strategies, the estimated effect of the reform on school district housing premiums remains robust, highlighting the importance of private school restrictions as primary channel driving increased housing premiums in high-quality public school districts.

Second, we find that areas with a higher pre-reform concentration of off-campus academic institutions, and consequently larger post-reform decreases in these services, experienced significantly smaller increases in school district housing premiums after the reform, confirming the impact of off-campus educational resources on school district housing premiums.

These findings have important implications for educational equity. While the reform may have succeeded in narrowing the gap between private and public education, it has inadvertently amplified disparities within the public school system by increasing the cost barrier to accessing high-quality public education through the housing market. This highlights the complex interplay between education policy and housing markets, and the potential unintended consequences of educational reforms on education equity.

The remainder of this paper is organized as follows: Section 2 describes the institutional background, data, and empirical methodology. Section 3 presents our estimates of the reform's overall impact on school district housing premiums. Section 4 analyzes the distinct channels through which the policy affected housing markets. Section 5 concludes with some policy implications and recommendations.

2. Background, methods, and data

2.1. Compulsory education enrollment reform in Hangzhou, 2019–2020

As mentioned earlier, our analysis focuses on Hangzhou, a city located in the Yangtze River Delta. As the capital of Zhejiang province, Hangzhou had a population of 12.2 million and a per capita GDP of RMB 149,857 (approximately 1.85 times the national average) in 2021, positioning it among China's developed second-tier cities.

The analysis concentrates on Hangzhou's six traditional downtown districts: *Shangcheng, Xiacheng, Gongshu, Xihu, Binjiang*, and *Jianggan*. While these districts underwent administrative boundary adjustments in 2021, our study references their pre-adjustment jurisdictions. This area encompasses approximately 40 % of Hangzhou's total population and generates half of its GDP.

As with most Chinese cities, public schools dominate compulsory education in Hangzhou, enrolling over 90 % of primary students as of 2021. The city implements a strict residence-based enrollment policy, where eligibility for specific public schools is tied to each student's family's registered address (hu ji di zhi, 户籍地址). Crucially, only property owners – not renters - can enroll their children in high-quality public schools within their district.

Of course, this policy creates strong incentives for parents to purchase properties in prestigious school districts, generating significant price premiums. Previous studies have documented not only the existence of these premiums in Hangzhou's housing market (Wen et al., 2014) but also their sensitivity to policy changes, including those involving school district boundary adjustments (Peng et al., 2021) and reforms limiting school choice (Wen et al., 2017).

Like many other major Chinese cities such as Shanghai (Chan et al., 2020) and Chengdu (Chen & Li, 2023), Hangzhou's educational landscape comprises three key components: public schools, private schools, and off-campus academic classes. Unlike their public counterparts, private schools traditionally admitted students irrespective of their residential address, accepting applications from across all of Hangzhou's urban districts.

Private schools have historically maintained academic reputations that often surpass that of public institutions. Despite charging higher tuition fees, they frequently emerged as preferred choices for families during enrollment seasons. Their traditional practice of conducting admissions before public schools provided a strategic advantage, in that students unsuccessful in private school applications could still pursue public school enrollment afterwards. This arrangement not only reassured parents but also enhanced the ability of private schools to attract top students while providing an alternative to high quality public school districts, thereby moderating housing price premiums in these areas.

Beyond the formal education system, off-campus academic classes have traditionally played an integral role in China's educational landscape. Research indicates that over half of urban students attend some form of off-campus academic classes to enhance their performance in core subjects (Zhao, 2015). In Hangzhou, these after-school and weekend tutoring programs have become almost essential for families aspiring to secure places in prestigious secondary schools or universities.

This established system underwent significant transformation beginning in 2019. On April 3, the Hangzhou Education Bureau mandated simultaneous admission processes for both public and private elementary schools. This policy change meant that students

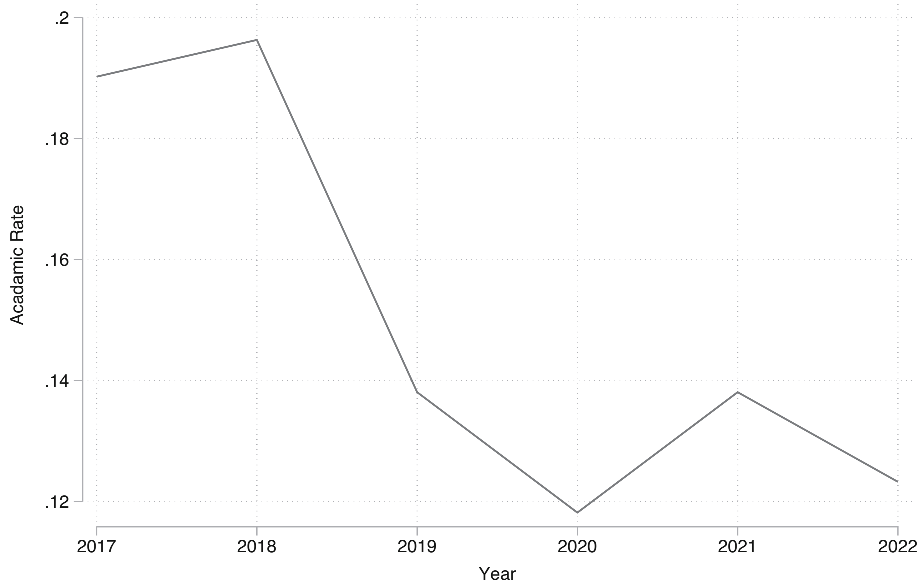

Fig. 1. Proportion of off-campus academic classes among all educational institutions, 2017–2022.

unsuccessful in private school applications could only participate in the second round of public school admissions, when positions at top-tier public schools were typically filled. This substantially diminished the appeal of private schools as an educational alternative.

Concurrent with these changes, authorities imposed stringent restrictions on off-campus academic classes. To quantify this impact, we analyzed the annual POI data from Gaode Maps, a leading Chinese digital mapping service, tracking the proportion of academic institutions among all educational facilities in urban Hangzhou from 2017 to 2022. As Fig. 1 illustrates, this proportion declined by approximately one-third following the 2019 reform.

While the contraction in off-campus academic services stabilized by 2020, restrictions on private schools intensified. On May 9, 2020, the authorities implemented an even more stringent policy: when applications exceeded available spots, private schools were required to allocate students solely through computerized random selection. This effectively eliminated any certainty in private school admission outcomes. Students not selected in this lottery system then faced very limited options in public sector, typically resulting in enrollment in lower-ranked public schools.

These reforms fundamentally altered the educational landscape in two ways. First, by implementing random admission procedures, they severely curtailed the ability of private schools to select high-achieving students. Second, restrictions on off-campus tutoring limited the families' access to supplementary educational resources outside the formal school system. These changes very likely influenced housing premiums in high-quality public-school districts through multiple channels, a complexity that extant studies have mostly overlooked, likely due to data limitations. However, our access to Gaode Maps' POI data provides an opportunity to estimate the impact of these reforms while explicitly accounting for the role of off-campus academic classes.

2.2. *Process and key milestones of the reform*

A common concern in policy evaluation research is the potential endogeneity of policy implementation. In our context, one might question whether Hangzhou's municipal government formulated these policies in response to observed or anticipated changes in school district housing premiums. However, this concern is less acute in our situation.

Table 1 presents a comprehensive timeline of major policy reforms since 2018, including milestones from the central government, the Hangzhou municipal government, and other comparable cities like Beijing, Shanghai (the nearest municipality to Hangzhou), and Nanjing (capital of neighboring Jiangsu province). For ease of reference, all policy announcements and changes by the Hangzhou municipal government are highlighted in bold.

As shown in Table 1, the reforms in Hangzhou were part of a broader national initiative to restructure China's compulsory education enrollment system. The reform trajectory began in early 2018 when Shanghai pioneered the "public–private simultaneous admissions" policy (*gong* min *tong zhao*, 公民同招), requiring concurrent enrollment processes for public and private schools. Contemporaneously, the Ministry of Education issued guidelines directing local authorities to integrate private school admissions into unified management systems and implement computerized random selection for oversubscribed private schools.

Initially, these directives of the Ministry of Education were treated more as recommendations than mandates, resulting in varied implementation across regions. While Beijing strictly adhered to the Ministry's guidelines, cities like Hangzhou and Nanjing maintained their traditional systems through 2018, allowing private schools to conduct independent admissions.

The regulatory framework gradually tightened. In early 2019, Hangzhou's Party Committee announced the city would adopt the "public–private simultaneous admissions" policy that year. The Ministry of Education reinforced this direction in its 2019 Annual Work Priorities, emphasizing reforms in school admissions and stricter oversight of off-campus academic classes. However, compliance remained incomplete during the 2019 admission cycle, while Hangzhou and Shanghai implemented synchronized admissions, they stopped short of random allocation, and Nanjing continued to permit early private school recruitment.

This partial compliance likely prompted intervention by higher authorities. On June 23, 2019, the Central Committee of the Communist Party of China and the State Council jointly issued a directive explicitly mandating simultaneous admissions and computerized lotteries for oversubscribed private schools. This high-level intervention effectively closed any implementation loopholes. By November 2019, the Hangzhou Education Bureau informed the public that random lotteries would be adopted for private-school admissions the following year, a commitment fulfilled in May 2020, and even Nanjing, which had permitted early private school admissions in 2019, officially pivoted in 2020, introducing simultaneous admissions plus a lottery mechanism.

We have provided references for each policy mentioned in the footnotes of Table 1. It is evident that Hangzhou consistently implemented reforms after both early-adopting cities and ministry mandates, positioning it more as a policy follower than an innovator. Given its status as a second-tier city, Hangzhou's educational market was unlikely to significantly influence national policy direction, thereby minimizing any endogeneity concerns.

Nevertheless, the incremental nature of these reforms presents challenges for precise policy timing identification. While earlier ministry directives and implementations by other cities may have shaped market expectations, Hangzhou's history of delayed compliance suggests that markets likely remained uncertain until formal local implementation. Our empirical analysis confirms this complexity, revealing fluctuations in school district housing premiums both preceding and between the two major policy adjustments.

Given these challenges in comprehensively capturing policy information flows and market reactions, we adopt a pragmatic approach, using Hangzhou Education Bureau's official admission announcement dates in 2019 and 2020 as the primary reform timestamps. To ensure robustness, we also examine alternative timing specifications and conduct parallel-trend analyses to track school district premium evolution throughout the study period.

Moreover, our subsequent placebo tests do not focus on whether changes in housing prices precisely coincided with policy implementation dates. Instead, we concentrate on examining whether factors other than changes in school district housing premiums could have driven these observed shifts.

Table 1
Reform timeline.

Date	Department/City	Simultaneous admissions	Random allocation	Off-campus academic classes
2018.2.10	Shanghai Education Commission[a]	Required	Not Required	–
2018.2.12	Ministry of Education (Annual Enrollment Notice)[b]	Required	Encouraged	–
2018.3.22	**Hangzhou Education Bureau**[c]	**Not Required**	**Not Required**	**–**
2018.4.12	Nanjing Education Bureau[d]	Not Required	Not Required	
2018.4.25	Beijing Education Commission[e]	Required	Encouraged	
2019.1.10	**Hangzhou Party Committee**[f]	**Confirmed to Implement**	**Not Required**	
2019.1.31	Shanghai Education Commission[g]	Required	Not Required	
2019.2.22	Ministry of Education (Annual Work Points)[h]	–	–	Regulation
2019.3.21	Ministry of Education (Annual Enrollment Notice)[i]	Required	Guidance	
2019.4.1	Beijing Education Commission[j]	Required	Required	
2019.4.2	**Hangzhou Education Bureau**[k]	**Required**	**Not Required**	
2019.4.26	Nanjing Education Bureau[l]	Not Required	Not Required	
2019.6.23	CPC Central Committee & State Council[m]	Required	Required	Registration, Regulation
2019.11.15	**Hangzhou Education Bureau (Q&A)**[n]	**Required**	**Required**	
2020.3.11	Shanghai Education Commission[o]	Required	Required	
2020.5.9	**Hangzhou Education Bureau**[p]	**Required**	**Required**	
2020.6.5	Nanjing Education Bureau[q]	Required	Required	

Note. “Simultaneous admissions” means public and private schools to conduct their admissions processes concurrently, “Random allocation” means private schools should implement a public computerized random allocation process when the number of applicants exceeds the places available.
[a] https://edu.sh.gov.cn/jyzt_xwfb_2018_4/20200618/0015-xw_97116.html
[b] http://www.moe.gov.cn/srcsite/A06/s3321/201802/t20180223_327619.html
[c] https://edu.hangzhou.gov.cn/art/2018/3/26/art_1228921851_40432674.html
[d] https://www.njqxq.gov.cn/qxqrmzf/201912/t20191212_1737454.html
[e] https://www.beijing.gov.cn/zhengce/zhengcefagui/201905/t20190522_61132.html
[f] https://hznews.hangzhou.com.cn/kejiao/content/2019-01/11/content_7131285.htm
[g] https://edu.sh.gov.cn/xxgk2_zdgz_rxgkyzs_02/20201015/v2-0015-gw_420022019001.html
[h] http://www.moe.gov.cn/jyb_xwfb/gzdt_gzdt/s5987/201902/t20190222_370722.html
[i] http://www.moe.gov.cn/srcsite/A06/s3321/201903/t20190326_375446.html
[j] https://jw.beijing.gov.cn/xxgk/zxxxgk/201904/t20190402_1446855.html
[k] https://www.hzarchives.org.cn/info/10072
[l] https://edu.nanjing.gov.cn/njsjyj/201905/t20190520_1541640.html
[m] https://www.gov.cn/zhengce/2019-07/08/content_5407361.htm
[n] https://ori.hangzhou.com.cn/ornews/content/2019-11/15/content_7304956.htm
[o] https://edu.sh.gov.cn/xxgk2_zdgz_rxgkyzs_02/20201015/v2-0015-gw_420022020001.html
[p] https://edu.hangzhou.gov.cn/art/2020/5/12/art_1228921942_42919195.html
[q] http://js.people.com.cn/n2/2020/0606/c360307-34067494.html

During this period, China also significantly strengthened regulations on off-campus tutoring institutions. Among the policy documents listed in Table 1, the Ministry of Education's Annual Work Points released on February 22, 2019, called for regulation of off-campus academic training institutions, while the CPC Central Committee and State Council opinion issued on June 23, 2019, further mandated registration management and strict regulation of these institutions. As shown in Fig. 1, these policy changes significantly impacted off-campus academic tutoring centers in Hangzhou.

It should be noted that while multiple Hangzhou government departments, including the Education Bureau, issued corresponding documents to strengthen supervision of tutoring centers in accordance with central directives,[1] the Hangzhou Education Bureau documents we use as reform markers (issued on April 2, 2019, and May 9, 2020, as listed in Table 1) did not mention regulation of off-campus tutoring institutions. From this perspective, the restrictions on private schools and off-campus tutoring centers could be viewed as two distinct reforms.

However, regardless of how we view the relationship between these two aspects of reform, they objectively occurred during approximately the same period. Consequently, the observed changes in school district housing premiums during this time likely result from their combined effects. Ignoring either aspect could lead to misestimation of the other's impact on school district housing premiums. This consideration motivates our attempt to distinguish between these two sources of influence.

A critical distinction should be made between our study period's reforms and the subsequent “Double Reduction” policy implemented in late 2021. The latter represents a more comprehensive overhaul of China's educational landscape, with impacts extending well beyond our period of analysis. Given that our housing transaction data extends only through early 2021, we cannot analyze the impact of the Double Reduction policy.

Moreover, while our study focuses specifically on primary education tutoring services and our definition of academic training institutions encompasses only those serving primary school students, the Double Reduction policy encompasses a broader scope, including high school-level services. This narrower focus may explain why our off-campus academic classes ratio appears more stable

[1] For example, see https://edu.hangzhou.gov.cn/art/2019/9/11/art_1228922845_38719671.html.

after 2021, even as the broader tutoring industry underwent significant transformation.

2.3. Measures of school quality and off-campus academic classes

Previous studies have employed various quantitative measures to assess school quality, including student test scores or GPAs (Black, 1999; Chan et al., 2020; Dhar & Ross, 2012; Machin & Salvanes, 2016), and student socioeconomic backgrounds (Fack & Grenet, 2010). However, such detailed metrics are not publicly available for Hangzhou, necessitating alternative measurement approaches.

Some China-focused research has utilized student performance in academic competitions as a proxy for school quality (Chan et al., 2020). However, this measure presents several limitations in our context. These competitions, while prestigious, are not mandatory for subsequent academic advancement. Their primary value historically lay in improving students' chances of admission to elite private schools. The 2020 reforms, by implementing random admission procedures, effectively nullified this advantage, likely reducing both parental and institutional emphasis on such competitions. Furthermore, the absence of centralized competition data for Hangzhou schools precludes constructing reliable quality metrics from these outcomes.

Given these constraints, we follow the methodological approach of Han et al. (2021), Chen and Li (2023), and Zou (2024), employing a binary reputation-based quality indicator. We compiled comprehensive school rankings from influential local real estate information platforms, particularly Goufangbao (购房宝, www.house178.com) and 19lou (19楼, www.19lou.com) - websites widely consulted by Hangzhou parents for school selection. Schools consistently appearing in top positions across these platforms are classified as high-quality institutions. This methodology identifies 26 high quality schools among our sample of 176 schools.

To validate this classification, we develop an alternative quality measure based on historical housing transaction data. Using 2017 (pre-reform) secondary market transactions, we estimate school quality using hedonic price regression analysis. This model incorporates school-specific dummy variables while controlling for observable housing characteristics. The resulting coefficients capture unobserved school-district factors influencing housing prices, including school quality. These empirically derived estimates serve as a robust check for our reputation-based classifications.

For measuring the presence of off-campus academic classes, we construct a ratio of off-campus academic training institutions to total educational POIs using Gaode Maps' comprehensive database. The educational POI category encompasses all training establishments classified under “Science, Education, and Cultural Services – Training Institutions”, explicitly excluding formal schools but including diverse educational service providers. This broad category spans academic tutoring centers, arts education facilities, early childhood programs, sports training centers, and various specialized educational institutions.

From this comprehensive dataset, we manually identify academic training institutions using multiple information sources:

1. Direct naming indicators (e.g., “Primary School Tutoring”);
2. On-site photographs and consumer reviews from Gaode Maps and Dianping.com;
3. Job postings on recruitment websites;
4. Business scope information from Qichacha (qcc.com, an enterprise information platform).

Given our focus on the influence of elementary education on school district premiums, we specifically target institutions serving primary school students. Organizations primarily focused on graduate school entrance preparation, college entrance preparation, or international education are excluded from our classification. This systematic approach identifies 2227 off-campus academic training institutions among 15,429 total educational facilities in Hangzhou's six downtown districts between 2017 and 2021.

To control for regional variations in population density, economic development, and potential data collection disparities across Gaode Maps' coverage, we employ relative rather than absolute measurements, specifically the ratio of academic training institutions to total educational POIs.

We deliberately avoided using a ratio of total POIs as this would include an extensive range of noneducational establishments, from commercial complexes to industrial facilities, roads, ATMs, and even delivery lockers. Such inclusivity would compromise comparability with academic training institutions in our measurement and potentially conflate distinct areas with high concentrations of skilled professionals but lower residential density (e.g., business parks), or high residential density but lower income levels and educational investment, resulting in fewer academic training facilities.

2.4. Methods

Properties in high-quality school districts also often benefit from better infrastructure, prime locations, and wealthier neighborhood demographics. To minimize these confounding factors, researchers typically limit their samples to houses situated near school district boundaries, while controlling for boundary piece-level fixed effects (Black, 1999) or match adjacent property transactions across both sides of the boundary (Fack & Grenet, 2010) for comparability.

In this study, we adopt both methods and further extend them to control for additional differences in housing on either side of school district boundaries.

First, as our main method, we estimate the following hedonic model focusing on properties located along the boundary between high-quality school districts and ordinary school districts:

$$lnPrice_{i,c,s,t} = \alpha + \beta_0 HQSD_{c,s,t} + \beta_1 HQSD_{c,s,t}{*}Reform_{1,t} + \beta_2 HQSD_{c,s,t}{*}Reform_{2,t} + \sum \boldsymbol{X}_{i,c,s,t}\boldsymbol{\gamma} + \varphi_c + \omega_t + \varepsilon_{i,c,s,t} \tag{1}$$

where $lnPrice_{i,c,s,t}$ is the natural logarithm of the average transaction price per square meter of house i in *Xiaoqu* (小区 in Chinese[2]) c of school district s at time t, $HQSD_{c,s,t}$, is the school district quality dummy, that equals 1 if the school district is defined as high quality, and $Reform_{k,t}$ are dummies that equal one if the transaction occurred after the aforementioned policy reforms. Given the two stages of reform in Hangzhou, we construct two reform dummies, which equal 1 after April 3, 2019, and May 9, 2020, respectively.

$\boldsymbol{X}_{i,c,s,t}$ are control variables that include the natural logarithm of per capita disposable income and its interaction with school quality, the natural logarithm of area of the transacted house, the house's age and its square, the floor area ratio[3] and green-space ratio of the *Xiaoqu*, the proportion of off-campus academic classes among all education institutions within a five-kilometer radius of each *Xiaoqu* in 2017, the distance of the *Xiaoqu* to various amenities like its assigned public school, nearest private school, city centers, nearest metro station, closest first-class hospital, and φ_c and ω_t represent *Xiaoqu* and monthly fixed effects, respectively.

Owing to the lack of precise information on school district boundary locations, we define a high-quality school district *Xiaoqu* (or ordinary school district *Xiaoqu*) as a boundary *Xiaoqu* if and only if its distance to the nearest ordinary school district *Xiaoqu* (or high-quality school district *Xiaoqu*) is no more than 0.5 km. This approach yields 37,794 transaction records from 567 boundary *Xiaoqus* across 76 school districts.

In this model, β_1 and β_2 are the estimated changes in premiums following the 2019 and 2020 reforms, respectively, with their sum indicating the cumulative effect of both reforms.

Second, following Fack and Grenet (2010), we employ a boundary matching approach , reporting its results in nearly all regressions as a robustness check. Specifically, for every boundary *Xiaoqu* transaction i within a high-quality school district, we construct a counterfactual transaction i' using ordinary school district housing transactions taking place within a 0.5-km radius in the same month. Subsequently, we difference all variables between the two transactions and estimate the following model:

$$\Delta lnHousePrice_{i,c,s,t-i',c',s',t'} = \beta_0 + \beta_1 Reform_{1,t} + \beta_2 Reform_{2,t} + \Delta\boldsymbol{X}_{i,c,s,t-i',c',s',t'}\boldsymbol{\gamma} + \varphi_{c,c'} + \Delta\varepsilon_{i,c,s,t-i',c',s',t'} \tag{2}$$

where $\Delta lnHousePrice_{i,c,s,t-i',c',s',t'}$ and $\Delta\boldsymbol{X}_{i,c,s,t-i',c',s',t'}$ are the differences in price and other variables between the transaction property i and its counterfactual i', respectively.

Diverging from the approach of Fack and Grenet (2010), we do not construct a control group comprising all ordinary school district properties within a 0.5-km radius in the same month as transaction i. Instead, for each high-quality school district *Xiaoqu c*, we identify an ordinary school district *Xiaoqu c'* within a 0.5-km radius to serve as the counterfactual *Xiaoqu*. This counterfactual relationship is fixed; thus, the *Xiaoqu* fixed effects become *Xiaoqu*-pair fixed effects $\varphi_{c,c'}$ in Eq. (2), and theoretically it can control for all time-invariant differences between the two *Xiaoqus*. Additionally, in constructing the counterfactual for transaction i, we only consider transactions occurring in counterfactual *Xiaoqu* c' in the same month. Consequently, any temporal fluctuations affecting both *Xiaoqus* are eliminated in the differencing process, negating the need to control for time-fixed effects in this regression.

The potential cost of this approach is an increased likelihood of failing to find a suitable counterfactual for transaction i, especially if no secondary market transactions occur in the counterfactual *Xiaoqu* during that month. To mitigate this cost, we choose the counterfactual *Xiaoqu* that provides the highest ratio of suitable counterfactual transactions among those that meet the distance criteria. Furthermore, if multiple transactions in *Xiaoqu* c' satisfy the requirements in that month, we take the average of each variable across transactions to determine the final value of variables for the counterfactual i'.

In this model, as previously, β_1 and β_2 signify changes following the 2019 and 2020 reforms respectively, with their sum indicating the cumulative effect.

2.5. Control variables and fixed effects

Prior research frequently utilizes neighborhood committee-level (Chan et al., 2020) or grid-level (Chen & Li, 2023) data to account for the various disparities between school district and non-school district housing. However, given our aim is to estimate the impact of policies on school district housing premiums, controlling for too many time-varying variables potentially influenced by the policy can lead to incorrect estimation of the policy effect (Cinelli et al., 2022). Therefore, we limit such variables and concentrate on control variables less likely to be affected by the policy. Additionally, we control for fixed effects at the *Xiaoqu* level, which offers two additional advantages.

First, unlike many western cities, most housing units in contemporary Chinese cities are condominiums located in *Xiaoqus*. Properties within a typical *Xiaoqu* share many common characteristics: they are constructed by the same developer, share common amenities, and are managed by a single property management company. Moreover, many *Xiaoqus* restrict access to nonresidents. Therefore, controlling for *Xiaoqu* fixed effects allows us to account for a broad range of observable and unobservable housing characteristics.

Second, this approach addresses concerns about the non-random assignment of school districts. *Xiaoqus* developed by more influential developers or inhabited by more influential residents may be more likely to be allocated to better school districts. Consequently, even adjacent *Xiaoqus* on opposite sides of a school district boundary may not be comparable. However, all housing

[2] A *Xiaoqu* represents a fundamental urban organizational unit in Chinese cities. Most housing units in contemporary Chinese cities are condominiums located in *Xiaoqus,* with each *Xiaoqu* typically comprises hundreds or even thousands of residential units. It has been referred to as residential development project (Chan et al., 2020) or community (Peng et al., 2021) in some previous research papers.

[3] It is the ratio of a *Xiaoqu*'s total floor area to the size of the piece of land upon which it is built (容积率 in Chinese).

units within the same *Xiaoqu* are usually assigned to the same school district. Therefore, any factors affecting changes in school district assignments should vary at the *Xiaoqu* level. Given that school district boundaries seldom change, these factors should be absorbed by the *Xiaoqu* fixed effects.

Consequently, in this paper, we opt to control for *Xiaoqu* fixed effects rather than time-varying neighborhood committee or grid-level characteristics. In the boundary matching regression of Eq. (2), the *Xiaoqu* fixed effects are correspondingly replaced by *Xiaoqu*-pair. As long as the factors leading to *Xiaoqu* c being part of a high-quality school district (while *Xiaoqu* c' is not) are time-invariant, they can be controlled for by these fixed effects.

For comparison purposes, we also report results using only traditional boundary piece-level fixed effects. Additionally, several of our control variables remain constant at the *Xiaoqu* level, including floor area ratio and green-space ratio. While these variables are absorbed by the *Xiaoqu* fixed effects, making their inclusion inconsequential in models with *Xiaoqu* fixed effects, we nevertheless include them to enhance the comparability of results without *Xiaoqu* fixed effects with existing studies, which typically do not control for *Xiaoqu* fixed effects.

The use of more granular fixed effects in estimating school district housing premiums is not without precedent. Dhar and Ross (2012) extended the common practice of controlling for boundary piece-level fixed effects by incorporating fixed effects on each side of the piece of boundary. They argued that this approach effectively mitigates the endogeneity arising from non-random boundaries, and their results indeed showed that the estimated premiums on school district housing were significantly reduced after including these fixed effects. Our use of *Xiaoqu* fixed effects takes this logic one step further, as a *Xiaoqu* must be entirely located on the same side of a boundary piece.

However, given a *Xiaoqu* is a smaller unit than a school district, controlling for *Xiaoqu* fixed effects in cross-sectional data without temporal variation would preclude estimating the impact of changes in school district assignments on housing premiums. This limitation may explain why other studies on school district housing premiums have not employed fixed effects at such a granular level. Nonetheless, our study utilizes panel data to estimate changes in school district housing premiums before and after policy implementation, thus circumventing this issue.

2.6. Data sources

Our empirical analysis draws on multiple data sources. In addition to the previously mentioned POI data from Gaode Maps, our primary housing transaction data, which span from January 2017 to January 2021, were obtained from Tuboshi (兔博士 in Chinese, www.2boss.cn), a real estate data service provider in China. This dataset provides comprehensive transaction details including sale dates, property locations, physical attributes (floor level, square footage), and transaction prices. We supplement this with property-specific information such as green space ratios and floor area ratios. Additionally, we incorporate weekly rental price data at the *Xiaoqu* level from Zhuge Zhaofang (诸葛找房 in Chinese, www.zhuge.com).

Using Gaode Maps' geolocation API, we obtain precise coordinates for key urban amenities including elementary schools, subway stations, first-class hospitals, and city centers. These coordinates enable us to calculate accurate distances between each *Xiaoqu* and its nearest amenities, which serve as important control variables in our analysis.

School district information is sourced from official announcements by the Hangzhou Education Bureau. For areas with dual school district arrangements – where students can choose between two schools - we treat them as a single district unit and calculate distances

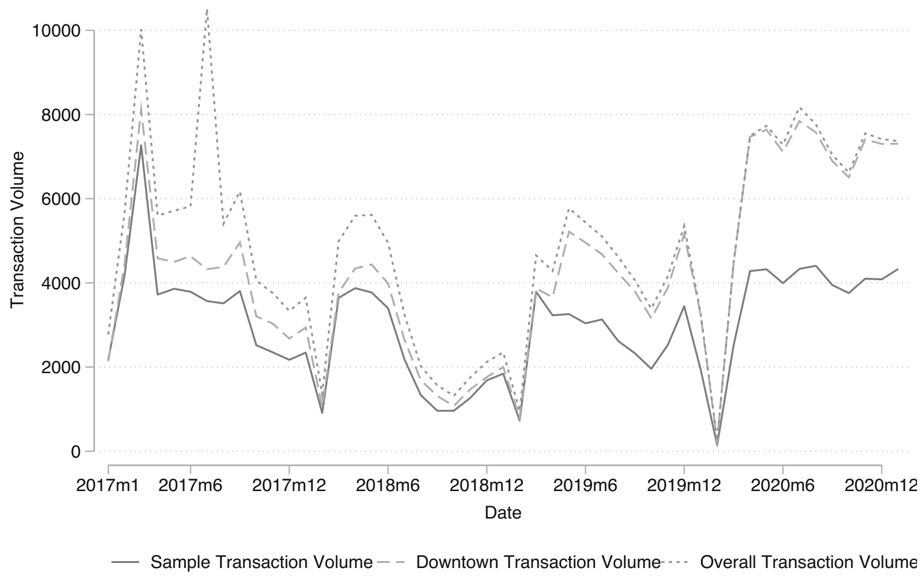

Fig. 2. Overall and sample transaction volume.

based on the nearest school. Similar proximity-based calculations are applied for schools with multiple campuses.

Our final dataset comprises 143,670 housing transactions across 2247 *Xiaoqus* in 176 school districts, while our rental price data covers 1068 *Xiaoqus* from November 2018 to January 2021.

To validate our sample's representativeness, in Fig. 2, we present a comparison between the number of transactions in our sample and the monthly transaction volumes in the downtown area and citywide disclosed by Hangzhou's Housing Management Bureau. As shown, our sample consistently represents a substantial proportion of the total transaction volumes, and the trends between the three datasets are closely synchronized.

Throughout our study period, our sample captures over 70 % of all secondary housing transactions in urban districts and more than 60 % of citywide transactions. In cross-study comparisons, our annual transaction volume exceeds those reported in comparable studies of larger cities: Chan et al. (2020) for Shanghai, Han et al. (2021) for Beijing, and Chen and Li (2023) for Chengdu. This high coverage rate, combined with the large sample size, enhances the reliability of our findings.

We also contrast our sample-based price index with the official index published by the National Bureau of Statistics in Fig. A1 in the Appendix, which shows the trends in both indices are congruent with the sample-based index fluctuating around the official figure.

Both the price trend alignment and substantial transaction coverage indicate that our sample provides a representative view of Hangzhou's housing market dynamics during the study period. The comprehensive nature of our dataset, combined with its high coverage rate and consistency with official statistics, provides a solid foundation for our empirical analysis.

3. Overall policy impact estimation

3.1. Basic results

Table 2 presents our baseline regression results based on Eqs. (1) and (2). Columns (1) through (3) display the boundary regression estimates from Eq. (1). In these regressions, we limit our sample to *Xiaoqus* that have neighboring *Xiaoqus* with different school district quality variables within a 0.5-km radius. We progressively incorporate fixed effects at the administrative district, piece of boundary, and *Xiaoqu* level. All specifications include monthly time fixed effects and comprehensive controls for property characteristics, community attributes, and location-based amenities.

Columns (4) through (6) present the boundary matching regression results based on Eq. (2). In these regressions, the dependent variable is the difference between the transaction unit price of properties in high-quality school district *Xiaoqus* and the average transaction unit price of their matched counterparts' average prices during the same period. Accordingly, all control variables are expressed as differences between the corresponding variables of the paired *Xiaoqus*. The differencing approach eliminates common time trends, obviating the need for time fixed effects. Columns (4) to (6) correspond to columns (1) to (3), respectively, controlling for administrative district, piece of boundary, and *Xiaoqu*-pair fixed effects.

For each specification, we report β_1, and β_2 as estimated by Eqs. (1) or (2), representing the changes in housing premium following each reform. Additionally, for ease of comparison, we report $\beta_1 + \beta_2$ and their corresponding clustered standard errors for each regression, representing the estimated cumulative impact of both reforms on school district housing premiums.

Considering the potential for unobserved *Xiaoqu* characteristics, our subsequent discussion will focus primarily on columns (3) and (6), which include the *Xiaoqu* or *Xiaoqu*-pair fixed effects.

Regardless of the method used, we find that the two reforms significantly increased the school district housing premium overall. Both columns (3) and (6), which control for the most extensive fixed effects, indicate that the two reforms together led to an increase of more than 5 % or 6.5 % in the school district housing premium, respectively. This result is less than the 7.8 % relative price increase for elite school district houses estimated by Chen and Li (2023) and much higher than the 2.8 % increase for their key school district houses.

Although the estimates of the cumulative impact are close, columns (3) and (6) differ slightly in how they attribute the impact of the two reforms. The results in column (3) attribute almost all the impact to the second reform, while the results in column (6) show that

Table 2
Basic results.

Dependent variables: *lnHousePrice* / Δ*lnHousePrice*	(1) Boundary regression	(2) Boundary regression	(3) Boundary regression	(4) Boundary matching	(5) Boundary matching	(6) Boundary matching
Premium change after 2019 reform (β_1)	0.0172	0.0122	−0.0015	0.0131	0.0242**	0.0245**
	(0.0189)	(0.0127)	(0.0082)	(0.0135)	(0.0108)	(0.0099)
Premium change after 2020 reform (β_2)	0.0491***	0.0513***	0.0554***	0.0428***	0.0424***	0.0423***
	(0.0117)	(0.0097)	(0.0085)	(0.0097)	(0.0090)	(0.0085)
Cumulative effect of Reforms ($\beta_1 + \beta_2$)	0.0663***	0.0636***	0.0540***	0.0559***	0.0666***	0.0668***
	(0.0254)	(0.0176)	(0.0136)	(0.0150)	(0.0117)	(0.0115)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	District, month	Piece of boundary, month	*Xiaoqu*, month	District	Piece of boundary	*Xiaoqu*-pair
Observations	27,796	27,796	27,787	7509	7509	7509
R^2	0.5070	0.6202	0.7367	0.3034	0.4787	0.5721

Note. Standard errors in parentheses are clustered at the *Xiaoqu* or *Xiaoqu*-pair level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

the impact of the 2019 reform accounts for more than half of the 2020 impact, or about one-third of the overall effect. However, both consistently indicate that the impact of the 2020 reform is significantly greater than that of the 2019 reform.

3.2. Temporal analysis

While Table 2 demonstrates a significant increase in Hangzhou's school district housing premium following the reforms, the complex reform process described in Section 2 warrants a more detailed temporal analysis of these changes.

To conduct this analysis, we perform a parallel trends test by replacing the reform dummy variables in Eq. (1) with monthly indicators. Fig. 3 presents the estimated coefficients for each month, using April 2019 (the official announcement of the first reform) as the base period, with the vertical dashed lines marking the timing of both reforms.

From 2018 to 2019, cities like Hangzhou and Nanjing displayed a clear willingness and capacity to postpone—or even refuse—the Ministry of Education's reform directives. This might explain why Hangzhou's school district premium did not shift significantly after the State Council explicitly called for simultaneous enrollment and random placement nationwide in 2019.

However, in late 2019, when Hangzhou's Education Bureau announced in a press briefing that random placement would be compulsory in the following admissions cycle, premiums began rising. This upward trend accelerated following the July 2020 completion of the private school admissions lotteries, which effectively eliminated any remaining policy uncertainties.

However, the complex fluctuations in Fig. 3 also suggest the presence of significant anticipatory effects. For instance, in January 2019, a plenary session of the Hangzhou Municipal Party Committee confirmed the implementation of “simultaneous enrollment”. Prior to this announcement, the school district housing premium had been on an upward trend; however, it subsequently reversed course and declined. This reversal may be attributable to the simultaneous confirmation during the meeting that the policy would *not* include random lottery-based admissions for private schools, which likely alleviated prior market anxieties.

Furthermore, the lengthy negotiation periods typical of real estate transactions mean that the price effects of a policy may only manifest after a lag of several days or even months. This means that while the above speculation is logically sound, it remains just one of several possibilities. We cannot account for all potential market influences during this period, nor can we pinpoint precise reasons behind every change in the school-district premium. However, despite the complexity of anticipatory effects and implementation timing, Fig. 3 supports two crucial conclusions: the absence of systematic trends before the reforms and a clear upward trajectory following policy implementation.

Fig. 3 also reveals that the choice of baseline periods or reform timing specification can impact estimation results. To establish the robustness of our findings, we conduct sensitivity analyses with alternative reform timing and sample periods, re-estimating Eqs. (1) and (2). The results are presented in Appendix Table A1.

Specifically, columns (1) and (2) of Table A1 employ the initial official confirmation dates of Hangzhou's reform policies (2019.1.10 and 2019.11.15, respectively), rather than their formal announcement dates, as the reform timing markers. Columns (3) and (4) exclude the entire reform implementation period—from Shanghai's initiation of simultaneous enrollment on February 10, 2018, through to Hangzhou's official announcement of simultaneous enrollment plus random placement on May 12, 2020—thereby directly comparing the pre- and post-reform periods. Columns (5) and (6) omit observations from January through March 2020, corresponding to Hangzhou's most stringent COVID-19 lockdown period.

The results in Table A1 demonstrate the robustness of our primary findings: Hangzhou's school district housing premiums exhibited

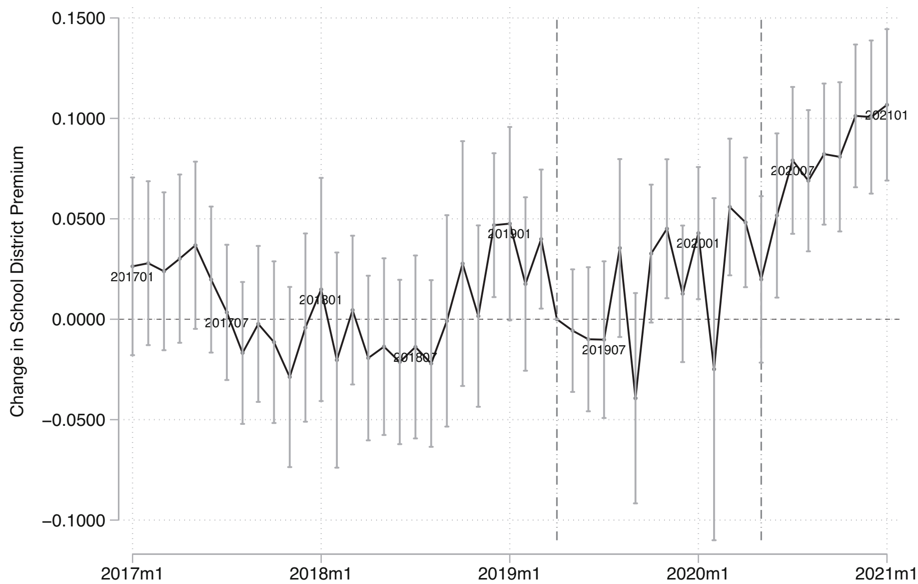

Fig. 3. Monthly change in school district premium.

a significant increase following the reform, regardless of variations in the reform timing specifications or sample period definitions.

Admittedly, these results at best demonstrate a correlation between the policy changes and the rise in the school district housing premium; they are insufficient to establish a causal link. Logically, this increase could also be attributed to other concurrent events. For instance, the onset of the COVID-19 pandemic in early 2020 led to prolonged lockdowns and delayed school openings in Hangzhou, forcing students into extended periods of parent-supervised homeschooling. Arguably, this experience may have led parents to re-evaluate the importance of education, thereby increasing their educational investment and, consequently, the school district housing premium.

Without stricter assumptions and more sophisticated modeling, we cannot entirely rule out this potential channel. Given that the pandemic's impact would likely operate through preferences, one might expect a larger effect in more affluent districts, where households have a greater capacity to translate such preferences into market prices. The evidence, however, suggests the contrary: when we divide the sample into two groups based on the per capita income of their respective administrative districts, the increase in the school district housing premium is, in fact, more pronounced in lower-income areas. The corresponding results are reported in Appendix Table A2.

Furthermore, the complex temporal dynamics of the premium shown in Fig. 3 also suggest that a one-off shock, such as the pandemic, cannot fully account for the observed fluctuations. In summary, while our estimates may be upwardly biased due to confounding effects from the pandemic and other channels, we maintain that the policy reforms remain a key driver of the changes in the school district housing premium during this period.

3.3. Robust tests of sample and matching methods

Beyond implementation complexity, methodological choices and data selection criteria could potentially influence our findings. Table 3 presents several robust checks addressing these concerns.

Our baseline analysis excluded *Xiaoqus* that experienced school district reassignment during the study period. However, even when a *Xiaoqu*'s assigned district remains unchanged, modifications to district composition could affect overall quality and associated premiums. Moreover, the addition or removal of other *Xiaoqus* from a district might introduce concerns with endogeneity.

To account for this, we further restricted our sample by excluding all school districts that underwent any compositional changes during the study period. Columns (1) and (2) of Table 3 replicate the specifications for columns (3) and (6) of Table 2 using this restricted sample, respectively. The results remain fundamentally unchanged.

Next, in columns (3), (4), and (5) of Table 3, we refine our matching methods and re-estimated the results from column (6) of Table 2. With column (3), we increase the matching distance limit between *Xiaoqus* from 0.5 km to 1 km. In column (4), while maintaining the 1-km distance, we alter the criterion for selecting control *Xiaoqus* to random selection rather than maximizing the overlap on transaction periods. In column (5), we adjusted the matching criterion to a nearest-neighbor approach, choosing the closest *Xiaoqu* to each high-quality school district *Xiaoqu* as its control.

Despite these adjustments, our main conclusions remain consistent, the two reforms significantly increased the premium on school district housing, with the impact of the second reform being substantially greater than that of the first.

The consistency in results across these alternative specifications strengthens our confidence in the baseline findings, suggesting our results are not artifacts of specific methodological choices.

3.4. Robust tests of school quality measures

The discrete, subjective nature of our school quality measure warrants additional robustness verification. Given the absence of reliable historical data on graduate placements or tournament results for Hangzhou's primary schools, we develop an alternative approach to constructing the school quality variable.

Table 3
Robust tests of sample and matching methods.

Dependent variables: *lnHousePrice* / Δ*lnHousePrice*	Exclude changed school districts		(3) 1-km radius Boundary matching	(4) 1-km radius Boundary random matching	(5) Nearest boundary matching
	(1) Boundary regression	(2) Boundary matching			
Premium change after 2019 reform (β_1)	0.0034	0.0214*	0.0030	0.0017	0.0023
	(0.0106)	(0.0122)	(0.0092)	(0.0147)	(0.0106)
Premium change after 2020 reform (β_2)	0.0637***	0.0436***	0.0354***	0.0491***	0.0364***
	(0.0091)	(0.0106)	(0.0061)	(0.0084)	(0.0087)
Cumulative effect of reforms ($\beta_1 + \beta_2$)	0.0672***	0.0650***	0.0383***	0.0508***	0.0388***
	(0.0144)	(0.0141)	(0.0105)	(0.0177)	(0.087)
Control variables	Yes	Yes	Yes	Yes	Yes
Fixed effects	*Xiaoqu*, month	*Xiaoqu*-pair	*Xiaoqu*-pair	*Xiaoqu*-pair	*Xiaoqu*-pair
Observations	25,234	5900	15,580	8954	10,456
R^2	0.7377	0.5795	0.6076	0.5992	0.6355

Note. Standard errors in parentheses are clustered at the *Xiaoqu* or *Xiaoqu*-pair level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

Specifically, as shown in Eq. (3), we estimate a hedonic model similar to Eq. (1):

$$lnPrice_{i,c,s,t} = \alpha + \sum \beta_k PS_k + \sum \boldsymbol{X_{i,c,s,t}}\boldsymbol{\gamma} + \varphi_s + \omega_t + \varepsilon_{i,c,s,t} \tag{3}$$

which includes all control variables $\boldsymbol{X_{i,c,s,t}}$, month fixed effects ω_t, and district fixed effects φ_s. Meanwhile, we set a dummy variable PS_k for each school district k in our sample and include it in the regression, which equals 1 if the property transaction falls within that school district, and 0 otherwise.

Using pre-reform (2017) transaction data, we estimate the school-specific coefficients β_k as our revealed-preference measures of school quality. These coefficients theoretically capture school quality premiums when controlling for other price determinants.

While perfect controls remain elusive, meaning β_k necessarily incorporates some unobserved district-level variation not related to school quality, our use of *Xiaoqu* or *Xiaoqu*-pair fixed effects helps mitigate concerns about time-invariant unobservables in subsequent analyses.

Using the school quality metric derived above, we revise Eq. (1) as the following specifications:

$$lnPrice_{i,c,s,t} = \alpha + \beta_1 SQ_{c,s,t} {}^*Reform_{1,t} + \beta_2 SQ_{c,s,t} {}^*Reform_{2,t} + \sum \boldsymbol{X_{i,c,s,t}}\boldsymbol{\gamma} + \varphi_c + \omega_t + \varepsilon_{i,c,s,t} \tag{4}$$

where $SQ_{c,s,t}$ represents our estimated school quality measure.

Columns (1)–(2) of Table 4 presents the estimation results using this alternative specification. For comparability, column (1) employs an identical sample as Table 2, and is restricted to *Xiaoqus* along original high-quality district boundaries. Then in columns (2), we reconstruct the sample based on our new quality measures, which encompasses all *Xiaoqus* adjacent to any school district boundary.

For each specification, we report both the direct coefficient estimates and the computed cumulative reform effects with bootstrapped standard errors.

Overall, the results in both columns broadly corroborate our baseline findings, while the interactions between school quality and the 2019 reform become significant and negative, we still have a significant positive coefficient for the 2020 reform interactions and the cumulative effect of the reforms. This reinforces our earlier conclusion that the reforms amplified school district premiums, with the effects primarily manifesting after the 2020 policy change.

In sum, our findings remain robust even after substituting the school quality variable. Nevertheless, a notable pattern emerges in the first two columns of Table 4: the estimated reform effects weaken when we expand the samples from high-quality to all district boundaries, regardless of methodology. This attenuation likely reflects households switching from private to public schools concentrating on top-tier public schools. Consequently, the reforms mostly impacted higher-quality districts, with only modest effects on mid- and lower-tier districts.

The last two columns of Table 4 provide multiple tests of this heterogeneity hypothesis. For brevity, we report only the computed cumulative reform effects results. Column (3) restricts the boundary regression sample to boundary *Xiaoqus* in the top 50 % of school quality rankings. The estimated effects increase significantly compared with column (2) of Table 4, supporting our heterogeneity conjecture.

In column (4) of Table 4, we construct a squared term of school quality measures. We include both school quality and its square, along with their interactions with the reform variables, in the boundary-regression framework. For brevity, we report only the computed cumulative reform effects results. The interaction for the squared term is significantly positive, and this further supports our heterogeneity argument.

Together, these findings support our inference that the reform effects concentrated in high-quality districts. Moreover, they increase confidence in our regression-based school quality measures, as the results remain robust to these alternative specifications.

3.5. Heterogeneity in housing size

To further validate our empirical strategy and findings, we explore additional dimensions of heterogeneous effects that align with our theoretical predictions. Theoretically, an increase in property area enhances the living experience, but does not offer additional or better school admission qualifications. Consequently, when the school district housing premium rises, budget-constrained families may be compelled to sacrifice living space to acquire smaller properties that grant admission to the same high-quality primary schools.

Therefore, we segregate all property transactions based on whether the area exceeds 90 square meters and conduct separate regressions for each group. Table 5 presents these stratified estimates. We anticipate the policy would have a greater impact on the premiums of smaller properties.

Our findings are consistent with the theoretical expectations outlined earlier. In Table 5, regardless of the estimation method used, the policy's effect on the school district housing premium is larger and more significant for smaller properties. This difference is particularly evident following the first reform.

3.6. Robust tests for rental prices

Theory suggests rents and housing prices are closely linked, with most price determinants affecting both markets similarly. However, in China, public school enrollment rights typically attach to property ownership rather than tenancy. Consequently, school district premiums should manifest in purchase prices but not in rents. This distinction provides an additional identification strategy: if

Table 4
Robust tests on school quality.

Dependent variables: *lnHousePrice*	(1) Original boundary *Xiaoqus*	(2) All boundary *Xiaoqus*	(3) *Xiaoqus* with top half school quality	(4) All boundary *Xiaoqus*
SQ * 2019 reform (β_1)	−0.0470**	−0.0203*	−0.0465**	
	(0.0218)	(0.0112)	(0.0202)	
SQ * 2020 reform (β_2)	0.1099***	0.0712***	0.1368***	
	(0.0210)	(0.0094)	(0.0181)	
($\beta_1 + \beta_2$)	0.0629***	0.0509***	0.0903***	
	(0.0212)	(0.0139)	(0.0243)	
SQ * Overall Reform Effect				0.0228
				(0.0188)
SQ^2 * Overall Reform Effect				0.1109***
				(0.0420)
Control variables	Yes	Yes	Yes	Yes
Fixed effects	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*, month
Observations	27,786	107,621	58,400	107,621
R^2	0.7385	0.7832	0.7747	0.7834

Note. Standard errors in parentheses are bootstrapped and clustered at the *Xiaoqu* level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

Table 5
Heterogeneity in housing size.

Dependent variables: *lnHousePrice* / Δ*lnHousePrice*	Boundary regression		Boundary matching	
	(1) Less than 90 m^2	(2) More than 90 m^2	(3) Less than 90 m^2	(4) More than 90 m^2
Premium change after 2019 reform (β_1)	0.0175**	−0.0231	0.0337***	−0.0051
	(0.0072)	(0.0171)	(0.0102)	(0.0143)
Premium change after 2020 reform (β_2)	0.0641***	0.0568***	0.0453***	0.0360**
	(0.0084)	(0.0136)	(0.0089)	(0.0147)
Cumulative effect of reforms ($\beta_1 + \beta_2$)	0.0816***	0.0337*	0.0790***	0.0309*
	(0.0096)	(0.0173)	(0.0119)	(0.0178)
Control variables	Yes	Yes	Yes	Yes
Fixed effects	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*-pair	*Xiaoqu*-pair
Observations	21,272	7224	4943	2551
R^2	0.8034	0.7641	0.6493	0.5878

Note: Standard errors in parentheses are clustered at the *Xiaoqu* or *Xiaoqu*-pair level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

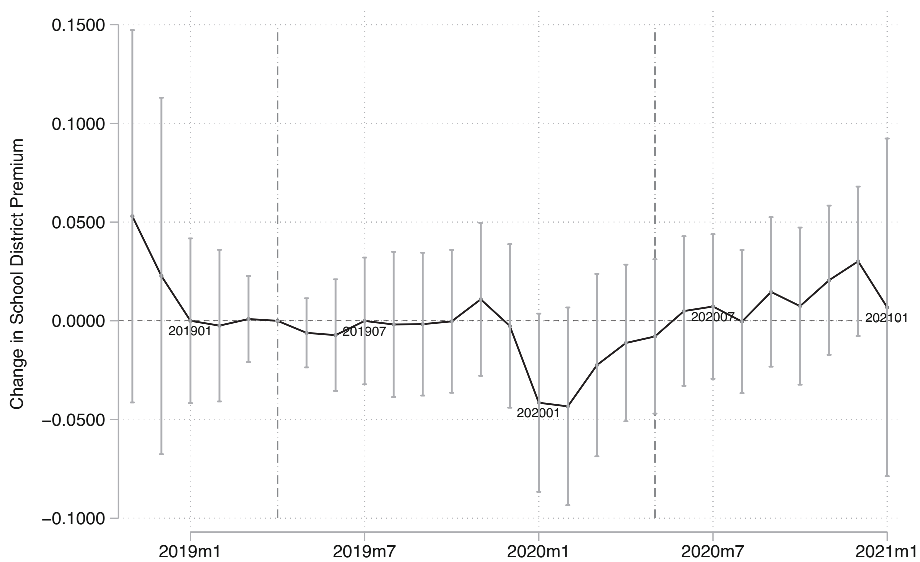

Fig. 4. Monthly change in school district rent premium.

the observed price differential changes truly reflect school district premiums, rental differentials should remain stable around the reforms.

To implement this test, we collected weekly listed rental prices for *Xiaoqus* in Hangzhou from November 2018 to January 2021 using data from Zhuge Zhaofang (Zhuge.com). Employing the boundary regression method outlined in Eq. (1), we estimated the changes in listed rental price differentials between high-quality and ordinary school district housing on either side of the boundaries during this period. Using April 2019 (the official announcement of the first reform) as the base period, we plot the monthly changes with vertically dashed lines marking the timing of both reforms in Fig. 4.

Comparing Figs. 3 and 4 reveals starkly different patterns in price and rental differentials. Rental differentials show no significant reform response, though they exhibit a notable decline in late 2019, likely reflecting COVID-19 lockdown effects.

Some families, constrained by high school district housing prices and aiming to balance educational opportunities with living standards, adopt a dual-property strategy: namely purchasing smaller apartments within desirable school districts to secure enrollment eligibility while renting larger residences nearby for short-term living. The delayed reopening of Hangzhou's primary and secondary schools until April 2020 due to the COVID-19 pandemic likely reduced rental demand in premium school districts, potentially contributing to decreased rental premiums.

While current data limitations preclude comprehensive exploration of this hypothesis, notably, this pandemic-related disruption is absent in the housing price differentials. For our core research question, these findings sufficiently demonstrate significant divergence between rental and housing price premium trends during this period, supporting our earlier observation that changes in housing price differentials reflect increased school district premiums.

We further examine this divergence through direct price-to-rent ratio analysis. Theoretically, we anticipate a marked change in the price-to-rent ratio between high-quality and ordinary school district *Xiaoqus* around the time of the reforms.

Concretely, we compute each *Xiaoqu*'s average weekly housing price, align it with rental data, and derive a price-to-rent ratio, then treat this ratio as the outcome variable in Eqs. (1) and (2) to assess whether any notable shifts occur before and after the reforms. Given that Fig. 4 points to an anomalously low rent premium during the COVID-19 outbreak, we also try excluding the first three months of 2020—when the pandemic had its strongest effect—and re-run the estimations. The corresponding results are presented in Table 6.

It is important to note that, because our rent and housing price data are from different sources, we can only match them for a subset of *Xiaoqus*. Consequently, the sample size in Table 6 is much smaller than for the previous regressions. Moreover, our rental data begins in late 2018, leaves few pre-2019 reform observations successfully matched, which could be the reason for the insignificant coefficient of the 2019 reform in Table 6. Finally, our rental data reflects listing prices, rather than transaction values, which differs from the housing price data. Nevertheless, despite these drawbacks, we believe that the findings presented in Table 6 provide valuable corroborating evidence for our main findings.

All four specifications in Table 6 demonstrate significant increases in the price-to-rent ratios of high-quality school district properties relative to other properties following the reform. These effects become more pronounced when excluding the COVID-19 period. This amplification aligns with our expectations: given that the pandemic-induced compression of rental differentials (shown in Fig. 4) preceded the 2020 reform, including this period would likely attenuate our estimated reform effects on the price-to-rent ratios.

These rental market analyses provide compelling support for our main findings. The absence of reform-induced changes in rental premiums, coupled with the significant increase in the price-to-rent ratios of post-reform high-quality school district properties, strongly suggests that our documented effects are indeed driven by changes in school district premiums.

3.7. Additional placebo tests

While rental market analysis provides strong supporting evidence, systematic differences between rental and purchase markets beyond school access rights might still raise concerns. Although we cannot exhaustively rule out all alternative explanations, we design several placebo tests comparing price movements across *Xiaoqu* pairs with identical school attendance rights. These tests help exclude the possibility that our main results reflect broader neighborhood-level changes unrelated to school district premiums.

Our first placebo test addresses a fundamental question: given our focus on cross-boundary properties, could observed effects simply reflect price increases specific to properties along boundaries within high-quality districts? Column (1) of Table 7 examines this

Table 6
The effect of reform on the ratio of housing prices and rent.

Dependent variables: price-to-rent ratio / difference of price-to-rent ratio	Boundary regression		Boundary matching	
	(1) Original boundary sample	(2) Exclude COVID period	(3) Original *Xiaoqu*-pair	(4) Exclude COVID period
Change of price-rent ratio after the 2019 reform (β_1)	0.0002	−0.0008	−0.0007	−0.0010
	(0.0010)	(0.0010)	(0.0011)	(0.0011)
Change of price-rent ratio after the 2020 reform (β_2)	0.0035***	0.0051***	0.0021**	0.0030**
	(0.0008)	(0.0010)	(0.0011)	(0.0012)
Control variables	Yes	Yes	Yes	Yes
Fixed effects	*Xiaoqu*, week	*Xiaoqu*, week	*Xiaoqu*-pair	*Xiaoqu*-pair
Observations	6562	5437	1130	934
R^2	0.7176	0.7141	0.4742	0.4425

Note. Standard errors in parentheses are clustered at the *Xiaoqu* or *Xiaoqu*-pair level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

possibility by comparing boundary properties within high-quality districts to their nearest non-boundary counterparts within the same district. If price increases were indeed boundary specific, we would expect rising relative prices for boundary properties.

However, the results in column (1) of Table 7 show no significant changes in the within-district price differentials around the reforms. This suggests price changes occurred between rather than within districts, supporting our school district premium interpretation.

Even if premium changes manifest only along district boundaries, concerns might persist that factors beyond admission rights distinguish cross-boundary properties, driving any observed differential changes. For instance, if certain *Xiaoqu* characteristics correlate with high-quality district assignment, significant differences might exist between adjacent cross-boundary properties. While *Xiaoqu* fixed effects substantially address this concern, we can further examine whether assignment-related endogeneity explains our results.

Typically, *Xiaoqus* are arranged to their nearest school. Following this logic, *Xiaoqus* assigned to more distant but better schools might possess particular characteristics influencing their school district assignment. If our earlier results were driven by unobserved features tied to non-random school district assignments, we would expect to see *Xiaoqus* with such characteristics exhibit larger increases in housing prices following the reforms.

We test this hypothesis using several specifications with the estimates reported in columns (2)–(5) of Table 7. We introduce a new variable, *NotNearest*, indicating *Xiaoqus* assigned to high-quality schools despite having closer ordinary schools. Table A3 in Appendix shows these *Xiaoqus* command higher prices within their districts after controlling for fixed effects and observables—particularly assigned school distance—suggesting potentially relevant unobserved characteristics.

Column (2) modifies our matching approach, comparing adjacent properties within high-quality districts with different *NotNearest* values. Column (3) employs boundary regression restricted to high-quality districts near boundaries, including *NotNearest*-reform interactions. This setup estimates differential price changes between *Xiaoqus* with different *NotNearest* values within high-quality districts. The results are insignificant, implying no discernible difference in price changes between these two groups.

Subsequently, in columns (4) and (5), we extend the regressions from columns (3) and (6) of Table 2 by including interaction terms between *NotNearest* and the two reforms. The results show that the coefficients of these new interaction terms are consistently insignificant, while the estimated coefficients for the original reform variables remain both significant and stable. This reinforces the robustness of our original results and suggests that unobserved factors related to school district assignments are unlikely to explain our main findings.

Collectively, these placebo tests strongly support our interpretation that observed price differential changes reflect shifts in school district premiums rather than alternative factors. The consistency of results across multiple identification strategies—different control groups, alternative quality measures, and various placebo tests—substantially strengthens our conclusions about reform-induced changes in school district premiums.

However, some questions warrant further investigation. Most notably, our findings consistently show that the increase in school district premiums following the first reform was much smaller than after the second reform, with many specifications yielding insignificant coefficients for the first reform. Given that the timing of the first reform coincided with a significant decline in off-campus academic classes, could these smaller or insignificant coefficients mask the underlying effects of tutoring restrictions?

This question merits careful examination. To disentangle these effects, we need to control for the influence of tutoring restrictions while estimating policy impacts—a challenge we address in the following section.

4. Disentangling the policy effects

4.1. Reform effects on off-campus academic classes

As illustrated in Fig. 1, the ratio of off-campus academic institutions to all educational establishments declined significantly

Table 7
Placebo tests.

Dependent variables: *lnHousePrice* / Δ*lnHousePrice*	(1) Same high-quality district matching	(2) *NotNearest* matching in high-quality districts	(3) Boundary regression in high-quality districts	(4) Boundary regression	(5) Boundary matching
Premium change after 2019 reform (β_1)	0.0006	0.0021		−0.0047	0.0368*
	(0.0038)	(0.0057)		(0.0098)	(0.0214)
Premium change after 2020 reform (β_2)	−0.0020	0.0007		0.0537***	0.0313**
	(0.0031)	(0.0044)		(0.0090)	(0.0149)
NotNearest * 2019 reform			0.0142	0.0137	−0.0171
			(0.0120)	(0.0089)	(0.0234)
NotNearest *2020 reform			−0.0142	−0.0013	0.0152
			(0.0127)	(0.0090)	(0.0181)
Control variables	Yes	Yes	Yes	Yes	Yes
Fixed effects	*Xiaoqu*-pair	*Xiaoqu*-pair	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*-pair
Observations	11,492	7629	13,279	27,787	7509
R^2	0.4225	0.1614	0.7213	0.7369	0.5722

Note: Standard errors in parentheses are clustered at the *Xiaoqu* or *Xiaoqu*-pair level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

following the reforms. Table 8 provides a more detailed analysis, presenting the absolute numbers and relative proportions of academic training institutions among education-related institutions and all POIs within a five-kilometer radius of each *Xiaoqus* with varying school quality, both before and after the reforms.

From 2017 (pre-reform) to 2021 (post-reform), Table 8 shows a major drop in nearly every category of institutions, likely reflecting the economic slump triggered by COVID-19 and subsequent intermittent lockdowns. To mitigate these effects, we primarily use the ratio of off-campus academic classes to all education institutions to capture how off-campus tutoring might influence the school-district housing premium.

The data presented in Table 8 corroborates our analytical framework proposed in Section 1. Both before and after the reform, properties in premium school districts exhibited significantly higher concentrations of academic training institutions, both in absolute and proportional terms, compared with those in ordinary school districts. Furthermore, the reform led to a reduction in the disparity of academic training resources between premium and ordinary school districts.

Our data reveals a broad-based decline in institutional numbers across all categories between 2017 (pre-reform) and 2021 (post-reform), which might partially reflect economic disruptions associated with COVID-19 and its associated lockdowns. However, the ratio of off-campus academic training institutions to all educational establishments, or their ratio to all POIs, both metrics show significant post-reform declines, which should be the results of the reforms alone.

Beyond these aggregate trends, Table 8 reveals systematic differences between high-quality and ordinary school districts in both the absolute numbers and relative concentrations of off-campus academic institutions. Moreover, these cross-district differentials exhibited significant changes following the reforms, suggesting heterogeneous policy effects across neighborhoods of varying school quality. Such heterogeneity could potentially contribute to the observed changes in school district housing premiums.

To further substantiate this point, we specify the number of off-campus academic classes—or their proportion among all education institutions (*AcademicRate* hereafter)—within 5-km radius of each *Xiaoqu* as the dependent variable. Controlling for a range of *Xiaoqu* attributes and fixed effects, we then estimate a model analogous to Eq. (1) to examine the changes in these measures before and after the reforms, and how such changes differ across school districts with different quality. The results are presented in Table 9.

Table 9 reveals that, after controlling for neighborhood characteristics and fixed effects, off-campus academic training institutions experienced a more pronounced decline in high-quality school districts, both in absolute numbers and as a proportion of all educational establishments. However, this differential effect becomes insignificant in the boundary sample, likely due to the similarity in neighborhoods across district boundaries.

In columns (3) and (5) of Table 9, we further introduce, in both the full-sample regression and the boundary-sample regression, an interaction between each *Xiaoqu*'s *AcademicRate* in 2017[4] and the two reforms. Once this new interaction term is included, the original interactions of school quality and reforms become insignificant.

This finding suggests that the observed differences in *AcademicRate* changes across properties within different school districts before and after the reforms were primarily driven by their initial *AcademicRate* levels in 2017, rather than by differences in school district quality.

Regardless of the underlying reasons, the reforms have objectively reshaped the distribution of off-campus educational resources across different school districts. In principle, this redistribution could influence property-choice decisions and, by extension, the school district housing premium.

4.2. Isolating the effect of off-campus academic classes

Having established that the reforms affected off-campus academic classes, we present two strategies to isolate the policy's impact on the school-district housing premium via that channel.

For simplicity, the following discussion and reported results are based on Eq. (1) or the boundary regression method, while the results using Eq. (2) or the boundary matching approach are reported in the Appendix.

First, we augment Eq. (1) by incorporating *AcademicRate* variables into the regression, yielding Eq. (6):

$$\begin{aligned} lnPrice_{i,c,s,t} = {} & \alpha + \beta_0 HQSD_{c,s,t} + \beta_1 HQSD_{c,s,t}{}^*Reform_{1,t} + \beta_2 HQSD_{c,s,t}{}^*Reform_{2,t} + \beta_3 AcademicRate_{i,c,s,t} \\ & + \beta_4 HQSD_{c,s,t}{}^*AcademicRate_{i,c,s,t} + \sum \boldsymbol{X_{i,c,s,t}\gamma} + \varphi_c + \omega_t + \varepsilon_{i,c,s,t} \end{aligned} \tag{5}$$

For the boundary-regression-based Eq. (1), we incorporate *AcademicRate* and its interaction with HQSD into the new Eq. (6). This setup allows us to distinguish between the overall decline in off-campus tutoring and the relative changes in this decline across different school districts—both of which can potentially affect the school district housing premium.

Column 1 of Table 10 presents the estimation results using Eq. (6). When compared with column (3) of Table 2, the coefficient relating to the 2019 reform becomes negative after the inclusion of the *AcademicRate* variables. However, the sum of β_1 and β_2 remains significantly positive.

Nevertheless, these results warrant some careful consideration. For instance, we use the *AcademicRate* data from the end of each year to approximate the relevant variable at the time of each transaction, which could introduce significant measurement errors. Moreover, the reforms may not be the only factors influencing the *AcademicRate*; other factors like the COVID-19 pandemic or

[4] For *Xiaoqus* completed after 2017, we also construct their *AcademicRate* measure using the 2017 ratio of academic training institutions in their vicinity based on GPS coordinates.

Table 8
The quantity and proportions of different types of POIs within 5-km radius of each *Xiaoqus*.

	Quantity			Off-campus academic classes proportions		
	High-quality school district	Ordinary school district	Difference (H-O)	High-quality school district	Ordinary school district	Difference (H-O)
2017						
Off-campus academic classes	516.7711	410.3074	106.4637**			
	(8.2884)	(6.4746)	(10.4283)			
All educational institutions	2429.769	1994.952	434.817***	0.2064	0.1971	0.0093**
	(38.086)	(29.807)	(47.971)	(0.0010)	(0.0007)	(0.0012)
All POIs	127,320.7	104,446.0	22,874.8***	0.0038	0.0039	−0.0001
	(1974.45)	(1611.24)	(2549.24)	(0.0001)	(0.0001)	(0.0000)
2021						
Off-campus academic classes	254.0422	209.9496	44.0926***			
	(3.8064)	(3.0214)	(4.8342)			
All educational institutions	1662.755	1412.725	250.031***	0.1489	0.1443	0.0045***
	(24.301)	(19.278)	(30.852)	(0.0007)	(0.0005)	(0.0008)
All POIs	105,217.3	89,304.2	15,913.0***	0.0023	0.0023	−0.00003
	(1409.01)	(1184.06)	(1851.99)	(0.0000)	(0.0000)	(0.00002)

Note: Standard errors are in parentheses; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

Table 9
The effect of the reform on off-campus academic classes.

Dependent variables:	Quantity of off-campus academic classes	Proportion of off-campus academic classes among all educational institutions (*AcademicRate*)			
		Full sample		Boundary sample	
	(1)	(2)	(3)	(4)	(5)
HQSD *2019 reform	−31.3679***	−0.0067***	−0.0004	−0.0011	−0.0000
	(3.4044)	(0.0007)	(0.0004)	(0.0007)	(0.0005)
HQSD * 2020 reform	3.4315	0.0001	−0.0000	−0.0003	−0.0003
	(2.1681)	(0.0004)	(0.0003)	(0.0004)	(0.0003)
$AcademicRate_{2017}$ *2019 reform			−0.4920***		−0.4722***
			(0.0121)		(0.0317)
$AcademicRate_{2017}$ *2020 reform			−0.0564***		−0.0490***
			(0.0046)		(0.0152)
Control variables	Yes	Yes	Yes	Yes	Yes
Fixed effects	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*, month
Observations	6768	6768	6667	2190	2190
R-squared	0.9319	0.9662	0.9872	0.9895	0.9925

Note: Standard errors in parentheses are clustered at the *Xiaoqu* or *Xiaoqu*-pair level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

economic downturns may also have a role to play. These factors could, in turn, influence the school district housing premium through other channels. For example, during an economic downturn, households might reduce their investment in education, leading to a decrease in both the *AcademicRate* and the school district housing premium.

If such factors are present, they may not only bias the coefficient of the *AcademicRate*, but controlling for it could introduce collider bias between these factors and policy changes (Cinelli et al., 2022), creating spurious correlations and new endogeneity in the model, thereby biasing the estimated effects of the two reforms.

If these concerns are valid, the credibility of the estimation results in Table 10 may be in doubt. However, measurement errors in the *AcademicRate* variable would primarily reduce the significance of this variable and weaken its ability to capture the channel, so even considering these errors, our earlier conclusions remain valid.

To address potential endogeneity arising from the inclusion of *AcademicRate* variables, we adopt a second strategy. In Eq. (7), we modify Eq. (1) by including interactions between each *Xiaoqu*'s $AcademicRate_{i,c,s,2017}$ (which is the *AcademicRate* of *Xiaoqu* i in 2017) and the two reforms, along with a triple interaction involving whether the *Xiaoqu* belongs to a high quality school district.

$$\begin{aligned} lnPrice_{i,c,s,t} = {} & \alpha + \beta_1 HQSD_{c,s,t}{}^*Reform_{1,t} + \beta_2 HQSD_{c,s,t}{}^*Reform_{2,t} + \beta_3 AcademicRate_{i,c,s,2017}{}^*Reform_{1,t} \\ & + \beta_4 AcademicRate_{i,c,s,2017}{}^*Reform_{2,t} + \beta_5 HQSD_{c,s,t}{}^*AcademicRate_{i,c,s,2017}{}^*Reform_{1,t} \\ & + \beta_6 HQSD_{c,s,t}{}^*AcademicRate_{i,c,s,2017}{}^*Reform_{2,t} + \sum \boldsymbol{X_{i,c,s,t}}\gamma + \varphi_c + \omega_t + \varepsilon_{i,c,s,t} \end{aligned} \tag{6}$$

This approach effectively treats the interactions between $AcademicRate_{i,c,s,2017}$ and the two reforms as proxies for the actual *AcademicRate*. Because $AcademicRate_{i,c,s,2017}$ is measured prior to the reforms, it is unlikely to correlate with unobserved factors that simultaneously affect both the post-reform housing premium and *AcademicRate*. Furthermore, given our boundary-based estimation and *Xiaoqu* fixed effects, it is also unlikely that $AcademicRate_{i,c,s,2017}$ would influence the premium through any alternative channels.

Table 10
Isolating the effect of academic rate on the public school housing price premium.

Dependent variables: *lnHousePrice*	All boundary sample			Different school districts	
	(1)	(2)	(3)	(4) High-quality	(5) Ordinary
Impact of 2019 reform via private school channel (β_1)	−0.0201**	0.2119**	0.1952**		
	(0.0098)	(0.1060)	(0.0894)		
Impact of 2020 reform via private school channel (β_2)	0.0524***	0.2024*	0.2118*		
	(0.0086)	(0.1250)	(0.1245)		
AcademicRate	1.0027*		1.0483***		
	(0.5284)		(0.3969)		
*AcademicRate * HQSD*	−0.4277**		−0.5519***		
	(0.1833)		(0.1518)		
$AcademicRate_{2017}$ *2019 reform		−0.3245	0.0442	−1.5189***	−0.1613**
		(0.3368)	(0.2736)	(0.2129)	(0.0823)
$AcademicRate_{2017}$ *2020 reform		−0.2126**	−0.1650***	−0.2195**	−0.0187
		(0.0871)	(0.0866)	(0.1015)	(0.0425)
$AcademicRate_{2017}$*HQSD *2019 reform		−0.9481*	−1.0407**		
		(0.5064)	(0.4213)		
$AcademicRate_{2017}$*HQSD *2020 reform		−0.7139	−0.7427		
		(0.5912)	(0.5883)		
Fixed effects	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*, month
Observations	27,787	27,787	27,787	26,283	90,366
R-squared	0.7369	0.7638	0.7641	0.8096	0.7440

Note: Standard errors in parentheses are clustered at the *Xiaoqu* level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

Hence, using it in this way circumvents many of the endogeneity issues that arise from controlling directly for *AcademicRate*.

Admittedly, $AcademicRate_{i,c,s,2017}$ cannot fully account for all potential post-reform changes in *AcademicRate*; the reform could act through additional channels that remain unobserved. In that case, controlling for interactions with $AcademicRate_{i,c,s,2017}$ might not entirely isolate the reform's impact on the school district premium via this channel.

However, columns (3) and (5) of Table 9 demonstrate that, once we account for $AcademicRate_{i,c,s,2017}$, the interaction between the reforms and school district quality no longer significantly affects the actual *AcademicRate*, suggesting that our controls sufficiently capture the most of the reform's effects on the housing premium via *AcademicRate*. This means controlling for $AcademicRate_{i,c,s,2017}$ and its interactions could effectively close the *AcademicRate* channel.

The results of Eq. (7) are reported in column (2) of Table 10. Even with the new interaction terms, β_1 and β_2 remain significantly positive, which confirms that the reforms increased premiums through the restriction on private school enrollment advantages.

Moreover, the significantly positive β_1, comparable in magnitude to β_2 validates our earlier conjecture: the smaller or insignificant effects observed for the first reform were likely due to concurrent restrictions on off-campus academic classes that offset the reform's positive impact on school district premiums.

Besides that, β_1 and β_2 in column (2) of Table 10 are noticeably larger in magnitude than the earlier estimates given the introduction of the triple interaction terms. However, given the median value of $AcademicRate_{i,c,s,2017}$ in our sample is about 0.2, when evaluated at that level, the estimated housing premium changes closely align with earlier results from Table 2.

In column (3) of Table 10, we extend the results in column (1) by including contemporary *AcademicRate* and its interaction with school quality. A comparison with column (2) reveals no substantial change in either the magnitude or significance of β_1 or β_2. Hence, these new variables do not introduce or remove any additional channels that might alter the estimates for β_1 or β_2. This also shows that the various interaction terms involving $AcademicRate_{i,c,s,2017}$ effectively shut down the pathway from the reform to *AcademicRate*.

These results mirror our earlier findings: even with the new method of controlling for off-campus academic classes, the reform-related coefficients remain robustly significant, highlighting that the restriction on private schools is a critical channel through which the reforms influence the school-district premium.

We replicated the analysis using the boundary matching method for the first three columns of Table 10, with results reported in Appendix Table A4. These findings are consistent with those presented in Table 10.

Focusing specifically on off-campus tutoring, the results in Tables 9 and 10 imply that neighborhoods with a higher pre-reform *AcademicRate*—and thus experiencing a steeper decline in that ratio once the reforms took effect—witness a smaller rise in the school-district housing premium. In other words, by curtailing off-campus academic classes, the reforms modestly reduced the housing premium.

To determine how this happened, we estimate the impact of *AcademicRate* separately on properties in high-quality and ordinary school districts. The results are reported in column (4) and (5) of Table 10, which indicate that the reform-induced reduction in *AcademicRate* negatively affected both property types significantly, with a more pronounced impact on high-quality school district properties, leading to decreased school district premiums.

Nevertheless, this downward force is noticeably weaker than the upward pressure that takes place through restricting private-school admissions. Overall, the reforms still culminate in a net increase in Hangzhou's school-district housing premium.

One potential concern regarding the results in Table 10 is that policy changes restricting off-campus academic tutoring might not have been synchronized with policies weakening private school enrollment advantages. To address this, in columns 1, 3, and 4 of

Appendix Table A5, we re-estimated columns 2, 4, and 5 of Table 10 using June 23, 2019—when the central government explicitly called for stringent regulation of off-campus training institutions—as the reform timing. Beyond timing concerns, we might worry that the closure of tutoring centers was gradual rather than immediate. Therefore, in column 2 of Appendix Table A5, we replaced the interaction between $AcademicRate_{i,c,s,2017}$ and reform timing with interactions between $AcademicRate_{i,c,s,2017}$ and all monthly dummies as our control variables.

All these results remain consistent with Table 10, further demonstrating the robustness of our findings.

5. Conclusion

This study concludes that the reforms from 2019 to 2020—which weakened private schools' ability to selectively admit top students and imposed restrictions on off-campus academic tutoring institutions—have significantly increased the school district housing premium in Hangzhou.

From the perspective of educational equity, this implies that although the reform has eliminated certain unfairness, it has introduced new inequities in other respects. Given the limited and highly unequal distribution of public educational resources, from an educational equity perspective, our findings suggest the need for more in-depth reforms in the future.

Declaration of competing interest

None.

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Appendix A. Appendix

Table A1
Robust tests for periods.

Dependent variables: *lnHousePrice* / Δ*lnHousePrice*	Using first announced time		Exclude reform period		Exclude COVID period	
	(1) Boundary regression	(2) Boundary matching	(3) Boundary regression	(4) Boundary matching	(5) Boundary regression	(6) Boundary matching
Premium change after 2019 reform (β_1)	0.0095	0.0314***			−0.0041	0.0180*
	(0.0139)	(0.0112)			(0.0098)	(0.0100)
Premium change after 2020 reform (β_2)	0.0524***	0.0343***			0.0603***	0.0494***
	(0.0098)	(0.0098)			(0.0093)	(0.087)
Cumulative effect of Reforms ($\beta_1 + \beta_2$)	0.0620***	0.0727***	0.0840***	0.0760***	0.0562***	0.674***
	(0.0171)	(0.0112)	(0.0354)	(0.0111)	(0.0138)	(0.012)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	*Xiaoqu*, month	*Xiaoqu*-pair	*Xiaoqu*, month	*Xiaoqu*-pair	*Xiaoqu*, month	*Xiaoqu*-pair
Observations	27,787	7509	15,810	4412	26,837	7234
R^2	0.7365	0.5742	0.7535	0.5571	0.7338	0.5686

Note. Standard errors in parentheses are clustered at the *Xiaoqu* or *Xiaoqu*-pair level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

Table A2
Heterogeneity in districts with different income levels.

Dependent variables: *lnHousePrice* / Δ*lnHousePrice*	Boundary regression		Boundary matching	
	(1) High-income districts	(2) Low-income districts	(3) High-income districts	(4) Low-income districts
Premium change after 2019 reform (β_1)	0.0095	−0.0095	0.0068	0.0283**
	(0.0179)	(0.0114)	(0.0102)	(0.0112)
Premium change after 2020 reform (β_2)	0.0389**	0.0573***	0.0280	0.0401***
	(0.0161)	(0.0101)	(0.0191	(0.0093)
Control variables	Yes	Yes	Yes	Yes
Fixed effects	*Xiaoqu*, month	*Xiaoqu*, month	*Xiaoqu*-pair	*Xiaoqu*-pair
Observations	7554	20,233	1679	5698
R^2	0.6521	0.7630	0.6494	0.5005

Note: High-income Districts measn *Shangcheng, Xiacheng*, and *Binjiang*, while low-income districts means *Xihu, Gongshu*, and *Jianggan*. Standard errors in parentheses are clustered at the *Xiaoqu* or *Xiaoqu*-pair level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

Table A3
Impact of not arranging to nearest school on house prices.

	High-quality school districts	High-quality school districts	Ordinary school districts	Ordinary school districts
NotNearest	0.0002	0.0373***	−0.0046	−0.0007
	(0.0026)	(0.0037)	(0.0108)	(0.0019)
Control variables	No	Yes	No	Yes
Fixed effects	school district, month	school district, month	school district, month	school district, month
R-Squares	0.6558	0.6960	0.6615	0.6479
Observations	41,189	30,546	156,745	114,057

Note: Standard errors in parentheses are clustered at the school district level, *, **, and *** denote p < 0.1, p < 0.05 and p < 0.01, separately.

Table A4
Isolating the effect of academic rate on public school housing price premium using boundary matching.

Dependent variable:Δ*lnHousePrice*	(1)	(2)	(3)
Impact of 2019 reform via private school channel (β_1)	−0.0088	0.2657***	0.3109***
	(0.0139)	(0.1011)	(0.0932)
Impact of 2020 reform via private school channel (β_2)	0.0367***	0.2955**	0.2664**
	(0.0092)	(0.1286)	(0.1281)
AcademicRate	−0.5027**		−0.6790***
	(0.2255)		(0.2181)
Difference of *AcademicRate*	0.7035		−1.8512
	(2.9100)		(3.1422)
$AcademicRate_{2017}$		−1.1492**	−1.5757***
*2019 reform		(0.5030)	(0.4455)
Academic $Rate_{2017}$		−1.1942*	−1.0908*
*2020 reform		(0.6206)	(0.6205)
Diff of $AcademicRate_{2017}$		−4.6979	−5.5789
*2019 reform (β_5')		(3.2103)	(3.4648)
Diff of $AcademicRate_{2017}$		1.6745	1.3704
*2020 reform (β_6')		(3.3559)	(3.4394)
Fixed effects	*Xiaoqu*-pair	*Xiaoqu,* month	*Xiaoqu,* month
Observations	7509	27,787	27,787
R-squared	0.5728	0.7638	0.7641

Note: Standard errors in parentheses are clustered at the *Xiaoqu*-pair level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

Table A5
Isolating the effect of academic rate on public school housing price premium using reform timing on national level.

Dependent variables: *lnHousePrice*	All boundary sample		Different school districts	
	(1)	(2)	(3) High-quality	(4) Ordinary
Impact of 2019 reform via private school channel (β_1)	−0.0185	0.0137*		
	(0.0122)	(0.0080)		
Impact of 2020 reform via private school channel (β_2)	0.0502***	0.0611***		
	(0.0075)	(0.0078)		
*HQSD *NationalReform*	0.4342***			
	(0.1364)			
$AcademicRate_{2017}$	−0.2085*		−0.7292***	−0.0981**
**NationalReform*	(0.1164)		(0.1530)	(0.0559)
$AcademicRate_{2017}$**HQSD *NationalReform*	−1.8660***			
	(0.6427)			
$AcademicRate_{2017}$**All Month Dummy*		Yes		
Fixed effects	*Xiaoqu,* month	*Xiaoqu,* month	*Xiaoqu,* month	*Xiaoqu,* month
Observations	27,787	27,787	26,283	90,366
R-squared	0.7589	0.7646	0.8096	0.7404

Note: Standard errors in parentheses are clustered at the *Xiaoqu* level; *, **, and *** denote $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

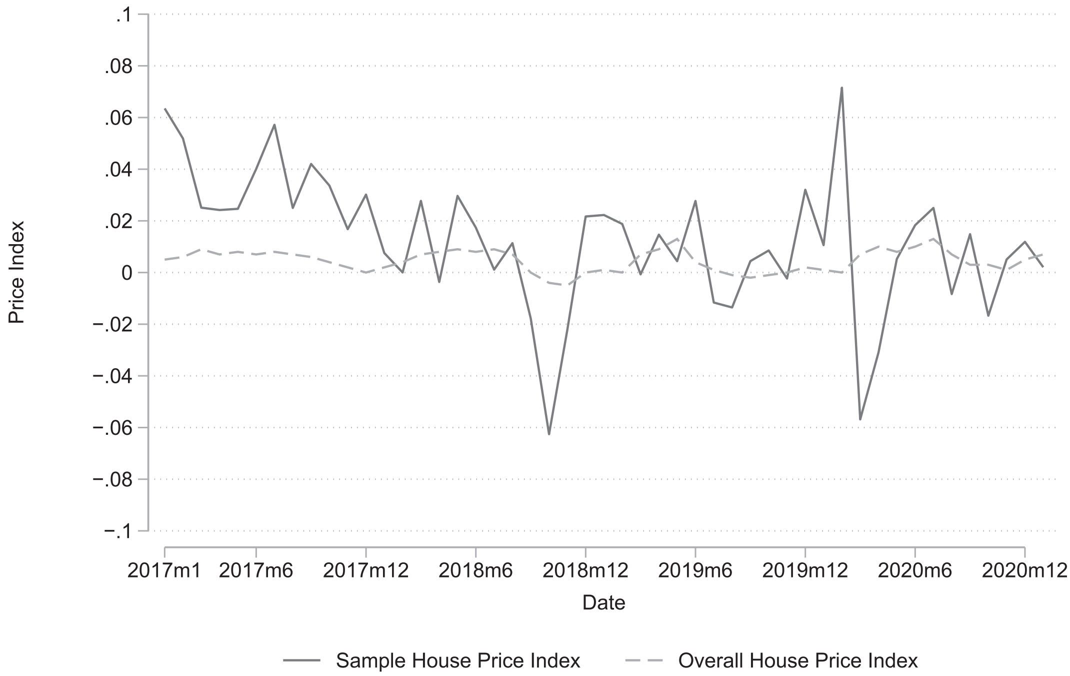

Fig. A1. Overall and sample-calculated house price indexes.

Data availability

Data will be made available on request.

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