Housing Speculation and Investment in Children's Education: Evidence from House Purchase Restrictions in China

Geng Niu, Xuefeng Xiang, Liuming Yang, and Yang Zhou*

Abstract

Housing and human capital represent two major forms of household wealth. This paper investigates the potential for housing speculation to crowd out household investment in children's education, an endeavor that only pays off in the long run. To address endogeneity concerns, we exploit the unintended spillover effect of staggered house purchase restrictions in China. Using a difference-in-differences approach, we find that house purchase restrictions reduce educational investment of households in nearby unregulated cities. We also provide evidence consistent with a housing speculation channel. These findings shed new light on the socioeconomic consequences of housing market booms.

* Geng Niu, g.niu@swufe.edu.cn, Southwestern University of Finance and Economics Research Institute of Economics and Management; Xuefeng Xiang, XuefengXiang1995@163.com, Southwestern University of Finance and Economics Research Institute of Economics and Management; Liuming Yang, liumingyang@cuhk.edu.hk, The Chinese University of Hong Kong Business School; Yang Zhou (corresponding author), y.zhou@whu.edu.cn, Wuhan University Economics and Management School. We thank Stephan Siegel (the editor), an anonymous referee, Weijie Lu, Xu Gan, Yuan Ren, Zhao Rong, Bo Zhang, Yapei Zhang, and Yunqi Zhang for their helpful comments. Niu acknowledges the financial support received from the National Natural Science Foundation of China (No. 72573125) and the Humanities and Social Science Foundation of the Ministry of Education of China (23XJC790005). Zhou acknowledges the financial support received from the National Natural Science Foundation of China (No. 72273098), the Humanities and Social Science Foundation of the Ministry of Education of China (No. 21YJC790172) and the Fundamental Research Funds for the Central Universities (No. 413000439).

I. Introduction

Housing is the dominant asset on household balance sheets, and fluctuations in its value can significantly shape household behavior with broad economic consequences (Mian, Sufi, and Trebbi,2015). Prior research shows that rising house prices affect not only current households but also children's outcomes, as wealth effects from perceived wealth or borrowing capacity influence schooling decisions (Lovenheim, 2011; Lovenheim and Reynolds, 2013). More recent work highlights a different mechanism: house price booms raise expectations of future growth, fueling speculation (Gao, Sockin, and Xiong, 2020; Mian and Sufi, 2022). Such speculation may crowd out non-housing investments, including children's education. This aspect remains largely unexamined, and our study seeks to fill this gap by exploring a speculation channel through which housing booms may crowd out investment in children's education.

Since the development of human capital theory (Becker, 1967), education spending has been viewed as an investment in future labor-market earnings. Under budget constraints, however, housing and education may act as substitutes. Human capital investment is long term, requiring time before returns materialize (Hanushek, Kinne, Lergetporer, and Woessmann, 2021), whereas housing speculation can yield quicker gains. During housing booms, rising optimism and strong beliefs in further price growth (Glaeser and Nathanson, 2017; Gao et al., 2020) raise the perceived opportunity cost of education. At the same time, higher housing wealth may enhance the capacity to invest in children's education. This tension makes the relationship between housing market dynamics and educational investment an important empirical question.

Identifying the causal effect of housing market dynamics on educational investment is challenging because unobserved local characteristics may drive both. China offers a suitable setting: since 2000, it has experienced a major housing boom that is difficult to reconcile with fundamentals (Glaeser and Nathanson, 2017; Han, Han, and Zhu,2018). To address worsening affordability, governments introduced house purchase restrictions (HPR), which cooled prices in regulated cities but spurred capital flows and sharp price increases in nearby unregulated ones (Deng, Liao, Yu, and Zhang, 2022). Importantly, the timing and jurisdiction of HPR varied, enabling a difference-in-differences (DID) design. Specifically, we compare educational investment of households in unregulated cities that are geographically close to regulated cities (treatment group) to that of households in unregulated cities that are farther away from regulated cities (control group). While the decision to impose HPR is endogenous to local conditions, the induced spillovers to surrounding unregulated cities are arguably exogenous, as restrictions were based on regulated cities' fundamentals rather than those of neighboring areas. Thus, the HPR-induced booms in nearby cities provide plausibly exogenous variation that supports a causal interpretation.

Using data from the China Household Finance Survey (CHFS) for the period 2011–2021, we find that households in nearby non-restricted cities (within 250 km of restricted cities) significantly reduce educational investment by 25.15% following the implementation of the HPR compared to those in farther away cities. Our results remain robust to a range of checks, including a parallel trends test, alternative definitions of treated cities, the inclusion of additional controls, and adjustments for heterogeneous treatment effects. In particular, an intuitive competing transmission channel is that our findings merely reflect rising living costs in nearby unregulated cities following the HPR shock. We mitigate this concern by showing that our estimates remain robust after controlling for local inflation, indicating that the observed effect is not driven by changes in the cost of living.

We continue with various tests to verify the housing speculation channel. First, we show that following the HPR policy shock, both house prices and household attention to the housing market increase significantly in nearby unregulated cities. Moreover, expectations of future house price growth and the likelihood of planning to invest in housing also rise in these areas. These results indicate that the HPR policy heightens households' speculative motives in treated cities. Next, we document that households in treated cities reduce discretionary consumption and stock market investment. This pattern suggests that households may be reallocating financial resources for housing investment. Complementing this, survey responses reveal that treated households are more likely to report needing funds for house purchase and less likely to report needing funds for children's education. Finally, we show that these households lower their aspirations for their children's education. Overall, these results are consistent with households reallocating resources away from education and toward housing investment.

We also perform a series of heterogeneity analyses to further illuminate our findings. First, we show that the negative effect of the HPR policy is more pronounced for households with lower levels of wealth and education. This pattern is consistent with the notion that financially less constrained families are less likely to reallocate resources from education to housing, while parents with higher education are more likely to recognize both the pecuniary and nonpecuniary returns to education (Oreopoulos and Salvanes, 2011). Second, we find that the negative spillover effect of HPR holds across households with children of all ages, and the differences across age groups are not statistically significant. This result casts doubt on the interpretation that housing serves primarily as a savings vehicle for children's college education, since the HPR effect persists even among households with children already in college. Moreover, it suggests that our findings are not simply driven by changes in labor market opportunities, as the crowding-out effect is also significant for households with nonworking-age children. Third, we find that the spillover effect of HPR is stronger among households that acquired additional property following the policy shock.

Finally, we conduct subsample analyses that distinguish between single- and multiple-home owners. We find that the crowding-out effect of HPR on education and discretionary spending is driven primarily by single-home owners rather than multiple-home owners. This finding is consistent with the notion that multiple-home owners experience a stronger wealth effect from rising housing prices.

Our paper contributes to three strands of literature. First, it relates to studies on the economic consequences of housing market dynamics, including effects on labor supply (Johnson, 2014; Zhao and Burge, 2017; Disney and Gathergood, 2018; Glancy, 2021), consumption (Gan, 2010; Musso, Neri, and Stracca, 2011; Mian, Rao, and Sufi,, 2013; Agarwal and Qian, 2017; Kartashova and Tomlin, 2017), and entrepreneurship (Adelino, Schoar, and Severino, 2015; Corradin and Popov, 2015; Schmalz, Sraer, and Thesmar, 2017; Tian and Wang, 2024). Housing values also affect education: while house price gains have been shown to increase college enrollment through wealth and borrowing capacity (Lovenheim, 2011; Lovenheim and Reynolds, 2013; Johnson, 2020), their potential negative effects are less explored. Charles, Hurst, and Notowidigdo (2018) find that booms can reduce enrollment by raising the opportunity cost of college. We add new evidence by identifying a crowding-out effect of housing booms on educational investment through a speculation channel.

Moreover, our study also contributes to the literature on housing speculation. Prior work shows that speculation fuels booms and busts (Gao et al., 2020), is amplified by credit supply expansions (Mian and Sufi, 2022), and can be triggered by policies such as LTV limits on second-home purchases in China (Chen, Wang, Xu, and Zha, 2022). We add to this line of research by documenting a micro-level consequence: a crowding-out effect whereby household capital shifts from educational investment to housing investment. As educational investment is crucial for long-term and sustainable economic growth (Aghion and Howitt, 2008), our findings offer a new perspective to understand the real and unexpected consequences of housing speculation.

Lastly, our paper is linked to research on the effect of housing regulation. While the existing literature has primarily focused on the direct effect of government interventions in housing markets (Fu, Qian, and Yeung, 2016; Han, Lutz, Sand, and Stacey, 2021), our paper examines the spillover effect of the HPR policy on households located in nearby unregulated cities by analyzing its unintended consequence on investment in children's education. The findings suggest that policymakers should pay attention to the potential spillover effect of housing regulations.

II. Institutional background and conceptual framework

China has witnessed a remarkable housing boom since 2003 (Liu and Xiong, 2020). For example, between 2003 and 2011, the average annual growth rate of house prices was around 17% across major cities in China and exceeded 20% in megacities such as Beijing and Guangzhou (Fang, Gu, Xiong, and Zhou, 2016).[1] This dramatic real estate boom prompted substantial concerns within the Chinese government, as the

[1] Land price changes are calculated based on the constant quality land price index for 35 major markets in China created by Wu et al. (2012), namely the Wharton/NUS/Tsinghua Chinese Residential Land Price Indexes (CRLPI). House price changes are calculated based on the constant quality house price index for major cities in China constructed by Fang et al. (2016).

high growth rate of house prices was argued to generate negative externalities for the real economy (Glaeser, Huang, Ma, and Shleifer, 2017). As a result, the Chinese central government introduced measures intended to rein in the housing market by imposing the HPR policy.

The HPR policy included a series of restrictions such as limiting the number of houses a family could purchase, increasing the down payment ratio, and setting a minimum residency requirement before buying a property in regulated cities. The implementation of the HPR policy had considerable variations by jurisdiction and timing (Deng et al. 2022; Tian and Wang, 2024). The HPR policy primarily focused on cooling the property market in tier-one cities, as well as preventing speculative investment in tier-two and tier-three cities.

While the policy was considerably effective in containing rising house prices in the regulated cities, it created a positive spillover effect on house prices in nearby unregulated cities. As shown by Deng et al. (2022), capital spilled from regulated cities to nearby cities due to the increased transaction costs in regulated cities, leading to a surge and boom in house prices in unregulated housing markets. Such a positive spillover effect is stronger for nearby non-HPR cities than for those located farther away.

A sharp rise in local house prices can simultaneously boost expectations of future appreciation and depress rental yields. We argue that in nearby unregulated cities, the former effect dominates, making housing speculation more attractive. Households tend to extrapolate recent price trends (Glaeser and Nathanson, 2017; Kuchler, Piazzesi, and Stroebel, 2023), and evidence shows that such expectations shape real decisions (Armona, Fuster, and Zafar, 2019). Given the surge in treated cities, households had strong incentives to speculate on further gains. This emphasis on capital gains over rental returns is consistent with China's high price-to-rent ratios and low user costs (Glaeser et al., 2017).

The (perceived) opportunity of housing speculation could be particularly appealing in China as Chinese households have limited investment opportunities: the real interest rate on bank deposits is low, the domestic stock market and bond market are underdeveloped, and the tight capital controls restrict investing abroad.

As household resources are limited, increasing the investment in one asset tends to reduce resources for other purposes. The problem of limited budgets might particularly be a concern for households in our study. For example, unlike in developed countries, it is challenging for homeowners in China to extract equity from homes that have appreciated in value owing to underdeveloped mortgage markets and strict policy restrictions (Painter, Yang, and Zhong, 2022). Moreover, during our sample period, China's cautious credit policies resulted in notably higher mortgage rates and down payment requirements compared to the U.S. (Glaeser et al., 2017).

An important but intangible asset for a household is the human capital of children, which affects children's labor market earnings in the future (Becker, 1967). Increasing children's ultimate educational outcomes often requires a constant and non-trivial input of monetary and non-monetary resources. However, educational investment normally takes a long time to take effect and even longer before returns materialize (Hanushek et al., 2021). Hence, we conjecture that increased incentives to speculate in housing would induce households to curtail their educational investment in children.

It is important to recognize that parental investment in children's education may be driven by both pecuniary and non-pecuniary motives, including altruism, bequest considerations, and aspirations for intergenerational mobility. In this context, the crowding-out channel we propose can be viewed in two ways: as a portfolio reallocation between housing assets and human capital, or as a shift between (educational) consumption and (housing) investment. This nuance does not alter the core trade-off, that is, housing speculation crowds out education, so we do not distinguish between these interpretations in our study.

III. Data, variables, and empirical strategy

A. Data and variables

Our main dataset is the China Household Finance Survey (CHFS), which is a countrywide longitudinal survey conducted by the Research Center for the China Household Finance Survey at Southwestern University of Finance and Economics (SWUFE). The CHFS has been widely used in studies on household finance in China, including those investigating household savings (Chen, Chen, and He, 2019), investment (Liang and Guo, 2015), and entrepreneurship (Fan, Li, Li, and Zhang, 2022), to name a few. The survey collects detailed demographic information from respondents every two years starting from 2011, including city of residence, homeownership status, age, wealth, income, family size, and children's information. We leverage data from six waves, 2011, 2013, 2015, 2017, 2019, and 2021, to form an unbalanced household-level panel dataset.

Our main dependent variable is the natural logarithm of investment in children's education. Following the literature (Jia, Zhou, and Yang, 2021; Wang et al., 2022), we measure educational investment as the sum of tuition fees, tutoring expenses, costs of extracurricular activities, books and supplies, and other education-related materials and equipment.[2]

To capture the spillover effect of the HPR policy, we manually collected the list of implementing cities and their enactment and suspension dates from official government websites. Following Deng et al. (2022), we construct our core explanatory variable, $HPR\_250\ km_{jt}$, a dummy variable that equals one if an unregulated city j is within 250 km of any city under the HPR in year t, and zero otherwise.

Two points are worth noting. First, following Deng et al. (2022), this paper uses 250 km as the benchmark threshold for identifying spillover effects of the HPR policy. The underlying rationale is that with the development of high-speed rail, a round trip between cities within 250 km can be completed within two hours, greatly facilitating cross-city real estate investment and capital flows. Within this range,

---

[2] Note that our measure of educational investment does not include savings for children's future education.

investors can also easily inspect and manage properties. To ensure our results are not sensitive to this choice, we test alternative thresholds of 100 km and 150 km in robustness checks. Second, $HPR\_250\ km_{jt}$ can be treated as a dummy variable that indicates whether households in non-regulated cities experienced a sharp increase in house prices. Since our goal is to study the impact of house price increases on household educational investment, this variable serves to capture a positive shock to local house prices.

To account for individual heterogeneity that might affect the relationship between HPR spillover and educational investment, we incorporate a comprehensive set of household- and city-level control variables. As we control for household fixed effects in our empirical models, we focus on time-varying household characteristics, such as the age of the household head, family size, family wealth and income, and the number of children in different age groups. At the city level, we control for GDP per capita, population density, fiscal expenditure, fiscal revenue, employment, average wage, financial development, industrial structure, real estate investment, and the number of secondary and primary school teachers. These city-level variables are extracted from the China Stock Market and Accounting Research Database (CSMAR).

B. Sample construction

Based on our research question, the raw CHFS data are processed as follows. First, we keep only families with children still in school. Second, we retain homeowner households for two reasons. For one, there is a fundamental difference in investment decisions between tenants and homeowners. As homeowners already possess property, they tend to have a stronger speculation incentive than tenants do (Gao et al., 2020). The other reason is that homeowners constitute the vast majority of the sample, accounting for around 90% of the total observations.[3] Third, we restrict the sample to households located outside HPR cities. Fourth, we exclude households that changed residences during the sample period.[4] Fifth, to satisfy the standard staggered DID assumption, we exclude households that switch in and out of

---

[3] We adopt time-invariant inclusion rules at the household level. Specifically, to maintain consistency, each household's homeownership status, including whether they are renters or multiple homeowners, is determined according to their status in the first survey wave they participated in. This classification might include households that later sell their homes and become renters. Such cases constitute only a small fraction of the sample, and excluding them does not materially alter our results.

[4] Residential mobility in China is relatively low due to a strong culture of hometown preference and the household registration system that creates barriers to settling permanently outside one's city of registration (Song, 2014). Moreover, our sample consists exclusively of homeowners with children, who should face higher moving costs and thus tend to exhibit particularly low mobility over short periods. In line with this, we find that approximately 99% of households in the CHFS data stay in the same city between consecutive survey waves. This figure is consistent with the findings in other studies using China's household survey data (e.g., Deschenes, Wang, Wang, and Zhang, 2020; Li, Ferreira, Smith, and Zhang, 2021).

treatment, i.e., non-absorbing treatments. Finally, after matching the CHFS data to macro-level data and removing observations with any missing values, we obtain an unbalanced panel with 17,863 observations. The number of households in each wave is as follows: 955 (2011), 2,132 (2013), 4,018 (2015), 4,821 (2017), 3,494 (2019), 2,443 (2021).

Table 1 provide summary statistics for the variables outlined above. The average annual educational investment is 7,066 RMB, which is consistent with values reported in the literature (e.g., Wang, Cheng, and Smyth, 2022; Chen, Yuan, and Zhang, 2023). The median number of children per household is 1, which is consistent with the long-established one-child policy in China. The median household size is 4. Despite the one-child policy, household size in China could be larger than 3 due to the prevalence of intergenerational co-residence, which is common in China owing to strong family ties and cultural traditions. Multigenerational living is widely viewed as an ideal structure in China and plays an essential role in providing support and care within the family (Xu, 2019). It is particularly common for grandparents to live with their adult children to help care for their grandchildren.

[Insert Table 1 approximately here]

C. Empirical design

In China, while the HPR policy is formulated by the central government, its implementation is delegated to local governments. Consequently, the introduction of HPR in regulated cities is endogenous, as it is influenced by numerous local economic and institutional factors. This paper, however, investigates the spillover effect of HPR on nearby unregulated cities. We argue that since the decision to implement home purchase restrictions is mainly determined by the regulated city's own economic conditions instead of those of surrounding cities, the HPR policy can be considered relatively exogenous for surrounding unregulated cities. To some extent, this alleviates potential endogeneity concerns. Following Deng et al. (2022), we use the sudden surge and boom in house prices in nearby unregulated cities that are induced by the unintended geographical spillover generated by the HPR policies as our main identification strategy. We classify households in nearby unregulated cities as the treatment group and those in remote unregulated cities as the control group and perform a standard staggered DID analysis. The regression model is specified as:

$$(1) \quad Y_{ijt} = \beta_0 + \beta_1 HPR\_250\ km_{jt} + \beta_2 X_{ijt} + \beta_3 Z_{jt} + \gamma_i + \tau_t + \varepsilon_{ijt}$$

where Y_{ijt} represents the natural logarithm of educational investment for family *i* in city *j* at time *t*. $HPR\_250\ km_{jt}$ is our key independent variable, which is a dummy variable that equals one if city *j* is within 250 km of a HPR city at time t and zero otherwise. X_{ijt} and Z_{jt} are vectors of household- and city-level time-varying characteristics outlined above. We further control for household fixed effects (γ_i) and year fixed effects (τ_t) to account for shocks that are household- and time-specific. The standard errors are clustered at the city level to account for the correlations in educational investment of households living in the same city.

Note that because the survey is conducted biennially, the reference wave is set to the implementation year itself if it is an odd number. If the implementation year is an even number, the reference wave is assigned to the following (odd) year. For example, a policy enacted in 2014 (even) would use 2015 as the reference wave, while a policy enacted in 2013 (odd) would use 2013 as the reference wave.

IV. Empirical results

A. Benchmark results

Table 2 reports the baseline results examining the spillover effect of the HPR policy on household educational investment in unregulated cities. In column (1) without controls, the regression coefficient of *HPR_250 km* is negative and significant at the 5% level. This suggests that the spillover effect of the HPR policy has led to a significant decrease in educational investment of families in unregulated cities that are geographically close to regulated cities compared with that of families located in distant unregulated cities. After adding household-level controls in column (2), the regression coefficient of *HPR_250 km* remains significantly negative. When we include both household- and city-level characteristics in column (3), the coefficient of interest continues to be negative and highly significant. Taken together, these findings reveal a negative spillover effect of the HPR policy on educational investment by households in nearby unregulated cities.

In terms of economic magnitude, the estimate of *HPR_250 km* in column (3) is -0.2515, which implies that educational investment of households in nearby unregulated cities decreases by 25.15% compared to those in faraway cities after the HPR policy shock. Given the sample mean of 7,066 RMB, this translates to a 1,777 RMB decline in educational investment per year. Using the estimated impact of the HPR shock on housing prices in treated cities (reported later in Table 6), we calculate the elasticity of educational investment with respect to housing price appreciation. The implied elasticity is 2.28 (0.2515/0.1102), indicating that a 1% increase in housing prices induced by the HPR policy in nearby unregulated cities is associated with approximately a 2.28% decline in educational investment. This result suggests that the negative spillover effect of HPR on educational investment is economically significant.

The non-trivial economic magnitude can be understood within China's institutional context. For example, underdeveloped mortgage markets and policy restrictions prevent Chinese households from extracting home equity (Painter et al., 2022). In addition, China adopted prudent credit policies for the housing market during our sample period. Specifically, mortgage interest rates in China tended to exceed those in the United States significantly, and down payment thresholds were also notably high, especially for the second house (Glaeser et al., 2017). Therefore, households may have had to cut a substantial portion of educational expenses for housing investment.

[Insert Table 2 approximately here]

B. Robustness checks

1. Parallel trends test

A basic condition of the DID method is the parallel trend assumption. We are concerned that if educational investment of households in surrounding unregulated cities has already shown a faster-declining trend than that of households in faraway cities before the implementation of the HPR policy, the negative effect estimated in the baseline regression may be overstated. In this section, we alleviate this concern by testing whether the parallel trends assumption is satisfied before the implementation of HPR. Specifically, we decompose the core explanatory variable, *HPR_250 km*, into a set of event-time indicators: *Before 2*, *Before 1*, *After 1*, and *After 2*, to study the dynamic impact on household educational investment. The regression model is specified as follows:

(2) $$Y_{ijt} = \beta_0 + \beta_1 Before\ 2_{jt} + \beta_2 Before\ 1_{jt} + \beta_3 After\ 1_{jt} + \beta_4 After\ 2_{jt} + \beta_5 X_{ijt} + \beta_6 Z_{jt} + \gamma_i + \tau_t + \varepsilon_{ijt}$$

where *Before 2* (*Before 1*) is a dummy variable that equals one for households in the treatment group if the survey wave is two waves (one wave) before the implementation year of the HPR policy. *After 2* (*After 1*) is a dummy variable that equals one for households in the treatment group if the survey wave is two waves (one wave) after the implementation year of the HPR policy.[5] The base group is the survey wave in the year of policy implementation.

[5] Note that the relatively short time span of the sample prevents us from estimating leads and lags

Figure 1 plots the dynamic regression results. We notice that the coefficients of both *Before 2* and *Before 1* are not significant, indicating that there is no significant difference in household educational investment between the treatment and control groups before the event date. In contrast, the coefficients of *After 1* and *After 2* are significantly negative, suggesting that educational investment of households in nearby unregulated cities significantly decreases after the implementation of the restriction policy. This evidence validates the parallel trends assumption and ensures that our results are not driven by pre-existing trends.

[Insert Figure 1 approximately here]

2. Alternative definitions of treated cities

In the benchmark analysis, we define the treatment group as households in unregulated cities located within 250 km of regulated cities. To assess the robustness of our results, we consider five alternative definitions of treated cities. First, we reduce the spillover distance threshold from 250 km to 150 km. Second, we further narrow this threshold to 100 km. Third, we use geographic adjacency and define an unregulated city as treated only if it is geographically bordering a regulated city. Fourth, we define an unregulated city as treated if it is both within 250 km of a regulated city and in the same province as that regulated city. Fifth, the treatment effect may be influenced by whether an unregulated city is located near one or

---

of three periods or more for all treated cities.

multiple regulated cities, or by a change in the number of nearby regulated cities, such as a reduction from three to two. To ensure a cleaner identification of the effect, we conduct a subsample analysis focusing exclusively on treated cities that have only one nearby regulated city.

Table 3 reports the regression results from robustness tests using alternative definitions of HPR spillover. The coefficients of the core explanatory variables are negative and significant. Their magnitudes are consistent, falling within a narrow range of 23% to 30%. This suggests that the results obtained from the baseline regressions are not sensitive to the specific choice of spillover distance. Moreover, the coefficient of interest is 28.22% for the 150 km threshold and 30.43% for the 100 km threshold, indicating that the spillover effect strengthens as the distance to regulated cities decreases.

[Insert Table 3 approximately here]

3. Additional control variables

In this section, we test the robustness of the benchmark results by adding more control variables that may confound the spillover effect of HPR on the investment in children's education. First, economic shocks to regulated cities might spill over to nearby unregulated cities, and the strength of this spillover effect could decrease with distance. Given this concern, we check whether our results are sensitive to the inclusion of the exposure of an unrelated city to the economic activity of the nearest regulated city. Specifically, for each unregulated city in our sample, we calculate the beta of its GDP growth to the nearest regulated city's by using data from year 2003 to the beginning of the sample period. We then compute the economic exposure of an unregulated city to the nearest regulated city as the corresponding beta times the regulated city's GDP. Column (1) of Table 4 shows that the estimated effect of the HPR policy on educational investment hardly changes after controlling for intercity economic connections.

Second, higher house prices induced by the HPR policies may raise the cost of living for households in unregulated cities through inflation, thereby crowding out educational investment. To rule out this possibility, we incorporate city-level CPI data from the WIND database into the regression. Column (2) of Table 4 presents the results. We find that controlling for inflation does not alter the impact of HPR on educational investment.

Third, Charles et al. (2018), who also study the nexus between the housing market and education, document that housing booms improve labor market opportunities, thereby lowering college enrollment. As such, it is possible that increased labor market opportunities make children less likely to go to college, thus leading to a decrease in educational investment. To account for this possibility, we obtain data on the city-level total employment in the FIRE (finance, investment, and real estate) sector and that in the construction sector from CSMAR, and additionally control for the natural logarithms of FIRE employment and construction employment. As shown in column (3) of Table 4, the inclusion of these two variables does not change our main findings, as the coefficient of *HPR_250 km* remains negative and statistically significant.

[Insert Table 4 approximately here]

4. Heterogeneous treatment effects

Recent literature has shown that staggered DID regressions with two-way fixed effects may produce biased estimates if treatment effects are heterogeneous or if some comparisons receive negative weights (de Chaisemartin and D'Haultfoeuille, 2023). To address this concern, we perform a set of diagnostic tests. First, we employ the stacked DID approach developed by Cengiz, Dube, Lindner, and Zipperer(2019), which estimates treatment effects by re-aligning cohorts at their treatment dates and stacking the resulting datasets. Specifically, for each HPR event, we include a clean control group and a treated group. The clean control group consists of household-year observations in cities that have not yet experienced any spillover effect from nearby HPR cities as of a given year, as well as households in cities that never experience any spillover effect during the sample period. We construct a cohort identifier and stack the cohort-specific datasets. This procedure eliminates biases from differential timing and heterogeneous effects, as suggested by Baker, Larcker, and Wang(2022). Second, we re-estimate the baseline regression using the DIDM estimator developed by de Chaisemartin and D'Haultfoeuille (2020), which is robust to treatment effect heterogeneity and avoids the negative weighting problem. Third, we apply the two-stage DID (DID2S) procedure proposed by Gardner (2022), which partials out fixed effects in the first stage to correct for bias from differential treatment timing. Fourth, we drop the last period of the data of the panel data following recommendations from de Chaisemartin and D'Haultfoeuille (2023) and Jakiela (2021) and rerun the baseline DID regression to mitigate the potential influence of negative weights.

Table 5 reports the results of these diagnostic tests. We find that the impact of HPR on educational investment remains robustly negative and significant. The point estimates under stacked DID, DIDM, and DID2S are similar to our baseline estimate, and with the final period removed, the estimated treatment effect is virtually unchanged. Overall, these findings confirm the robustness of our findings.

[Insert Table 5 approximately here]

V. Further analysis

A. Mechanism analysis: Evidence of housing speculation

We contend that the negative effect of the HPR policy on educational investment of families in surrounding unregulated cities is likely driven by a substitution effect between housing and educational investment. Deng et al. (2022) document that the HPR policy in regulated cities creates a positive spillover effect, triggering a sharp rise in housing prices in adjacent unregulated cities. As suggested by Gao et al. (2020) and Mian and Sufi (2022), a sharp increase in house prices alters price growth expectations among potential buyers and fuels speculative activity. In this context, we hypothesize that rising house prices in nearby unregulated cities lead households to anticipate further appreciation. When expected returns on housing investment rise, households may reallocate funds toward real estate, thereby crowding out educational expenditure due to extrapolative expectations in the housing market. Overall, the HPR policy stimulates housing speculation in surrounding unregulated cities and reduces investment in children's education. This crowding-out effect could be especially pronounced in China, both because Chinese households lack formal investment channels and real estate investment serves as a major investment vehicle, and because educational expenses are non-trivial in China. Under a fixed budget constraint, housing and educational investments are to some extent substitutes. To verify this housing speculation channel, we conduct several tests as follows.

1. House prices and attention to housing market

The key assumption regarding the negative impact of the HPR policy on educational investment by families in nearby unregulated cities lies in its positive shock to the real estate market in those cities. Therefore, we first need to verify whether the HPR policy produces a positive spillover effect on housing prices in surrounding unregulated cities.

High-quality house price data are not easily accessible in China, especially for medium and small cities. For example, the National Bureau of Statistics (NBS) in China provides a house price index based on the average selling price of newly-built housing units, which ignores transactions of existing properties and fails to control for housing quality (Wu, Gyourko, and Deng, 2012; Fang et al., 2016). To tackle this issue, we obtain a house price index from CityRE, a leading real estate data provider in China. CityRE constructs a constant-quality hedonic house price index using listing and transaction data from national and local real estate brokers, covering more than 300 cities in China. The quality of the CityRE index has been well recognized in recent studies (e.g., Deng et al., 2022; Ma and Zhang, 2024).

We now use the natural logarithm of the annual house price index as the dependent variable and report the results in column (1) of Table 6. *HPR_250 km* enters positively and significantly in the regression, confirming a positive spillover effect of the HPR policy on the real estate market in surrounding unregulated cities. In economic terms, the estimated coefficient of 0.1102 implies that house prices in nearby unregulated cities increase by 11.02% relative to the control group after the HPR policy.

Next, we check whether the positive impact of the HPR policy on the real estate market in surrounding unregulated cities heightens household attention to these markets. We use data from Baidu, the largest search engine in China. Baidu provides an index, similar to Google Trends, called the Baidu Index, which measures the search intensity of specific keywords (Vaughan and Chen, 2015). Using this tool, we construct a city-level search index based on keywords related to the housing market (e.g., "house price" and "house purchase"). This index serves as the dependent variable to capture the impact of the HPR policy on housing market attention in surrounding unregulated cities.

Columns (2) and (3) in Table 6 present the results. The positive coefficients of *HPR_250 km* in the two regressions indicate that the HPR policy significantly increases search volumes for terms such as "house price" and "house purchase" among households in surrounding unregulated cities relative to those in farther unregulated cities. In terms of economic magnitude, the estimated coefficients of *HPR_250 km* are 0.1254 and 0.1181, which imply that the HPR policy raises search volumes for "house price" and "house purchase" by 12.54% and 11.81%, respectively, in nearby unregulated cities. Therefore, the HPR policy and the ensuing house price surge significantly elevate housing-related attention among households in these cities, providing strong support for the housing speculation channel.

[Insert Table 6 approximately here]

2. House price expectation and house purchase intention

We have found that the HPR policy indeed had a spillover effect on the real estate market in surrounding unregulated cities, not only driving up house prices but also significantly increasing attention to the housing market in these cities. To further validate the housing speculation channel, we examine households' subjective assessment of the housing market and their intentions to invest in housing. Specifically, we construct two dummy variables based on the CHFS questionnaire. *House price expectation* is a dummy variable that equals one if the respondent expects house prices to rise in the next three months and zero otherwise. *House purchase intention* is a dummy variable that equals one if the respondent has a plan to invest in a house and zero otherwise. We rerun regressions with these two variables as the dependent variables, respectively.

Table 7 reports the impact of the HPR policy on house price expectations and house purchase intentions of families in surrounding unregulated cities versus those in faraway unregulated cities. Note that the question about house price expectations was only asked in 2011 and 2019, and the question about house purchase intention has some missing values. Therefore, the number of observations in Table 7 is smaller than those in the benchmark regressions.[6] As shown in column (1) of Table 7, the coefficient of *HPR_250 km* is significantly positive, indicating that households living in nearby unregulated cities and experiencing the house price boom are more likely to have positive expectations of future house price growth than those located in faraway cities. The positive coefficient of interest in column (2) of Table 7 further shows that the HPR policy significantly increases the intentions of families in surrounding unregulated cities to purchase a house. These results demonstrate that the introduction of the HPR policy strengthens households' expectations of future house price growth and their intentions to invest in the housing market in those cities, which corroborates the housing speculation hypothesis.

[Insert Table 7 approximately here]

3. Reallocation of household resources

[6] We have to replace household fixed effects with city fixed effects in the regression of house price expectations due to the lack of household-level panel data for this specific question.

The speculative demand for housing investment may crowd out not only educational expenditures, but also other categories of household consumption and investments in alternative assets. In other words, the HPR policy may induce a broader reallocation of household resources. To test this conjecture, we first consider two other types of expenditures, namely basic expenditure and discretionary expenditure. The former covers necessities such as food, clothing, and commuting, while the latter includes non-essential items like entertainment and travel. Panel A of Table 8 shows the regression results using the logarithms of basic and discretionary expenditures as dependent variables. While basic consumption shows no significant change, households in treated cities significantly reduce discretionary spending by 13.69% following the HPR policy. This distinction may be attributed to the downward rigidity in adjusting basic consumption.

We next probe whether treated households rebalance their portfolios in response to the HPR shock, focusing on household stock market participation. Following the literature (e.g., Malmendier and Nagel 2011; Giannetti and Wang 2016), we employ two measures: *Stock ownership*, a dummy variable indicating whether the household owns stocks directly or indirectly through mutual funds and *Stock share*, the fraction of household wealth invested in stocks. These two measures capture the extensive and intensive margins of stock market participation, respectively.

Panel B of Table 8 presents the results for household portfolio choice. The coefficient of *HPR_250 km* is negative and significant in both regressions, indicating that households in treated cities significantly reduce stock investment after the HPR shock. In terms of economic magnitude, the HPR policy reduces the probability of stock ownership by 2.52 percentage points and the proportion of household wealth allocated to stocks by 0.85 percentage points relative to distant unregulated cities. These findings support the argument that housing booms resulting from the HPR spillovers dampen the attractiveness of other investment opportunities relative to real estate.

[Insert Table 8 approximately here]

4. Reasons for needing funds

To provide more direct evidence of the crowding-out effects of housing speculation, we examine changes in households' reported reasons for needing funds around the policy shock. In principle, if the story of housing speculation holds, households should substitute housing investment for educational investment. Consequently, we would expect them to be more likely to report needing funds for housing investment and less likely to report needing funds for education.

We utilize a two-step set of questions regarding household credit demand to test our conjecture. In the first step, respondents are asked: "Does your household currently need funds?" If the response is "yes", a follow-up question is presented: "What is the primary reason your household needs funds?" Response options include "for house purchase", "for health expenses", "for children's education", "for financial investment", "for automobile purchase", "for gifts and ceremony", and "for basic living expenses". We restrict our analysis to households responding "yes" in the first step. Specifically, we construct two dummy variables: *For house purchase*, which indicates whether the primary reason for needing funds is house purchase, and *For children's education*, which indicates whether the primary reason is children's education. Using the same DID approach as in our benchmark analysis, we examine the impact of HPR on households' stated reasons for needing funds.[7] Table 9 shows the regression results. We observe that households in nearby unregulated cities become more likely to need funds for house purchase and less likely to need them for children's education after the implementation of the HPR policy. This finding bolsters our conjecture regarding the housing speculation channel.

[Insert Table 9 approximately here]

5. Parental aspirations for children's education

When parents perceive the opportunity to earn "easy money" through speculation during a housing boom, their subjective belief in the importance of education may weaken. Accordingly, they may adjust their aspirations for children's education. This shift has important long-term implications, as parental aspirations play a critical role in determining children's educational outcomes (Hao and Yeung, 2015).

To shed light on the spillover effect of the HPR policy on parental aspirations for children's education, we turn to another dataset, the China Family Panel Studies

---

[7] Note that because this two-step question covers a subsample of households with very limited overlap across panel waves, we have to replace household fixed effects with city fixed effects.

(CFPS), as the CHFS does not collect information on educational aspirations. The CFPS is a large-scale survey conducted by the Institute of Social Science Survey (ISSS) at Peking University in collaboration with the Survey Research Center at the University of Michigan. The CFPS data have been increasingly used in the literature to study household behaviors in China (e.g., Huang and Zhang, 2021). We use waves from 2010 to 2020 to construct an unbalanced panel.

We measure aspirations for children's education using the survey question "What is the highest education level you would like your child to achieve?" Based on this question, we construct a variable, *Aspirations for children's education*, which represents the number of years of schooling that parents desire for their children. Then we run a regression of this variable using the same specification as in the benchmark analysis. Table 10 reports the regression results. We find that the HPR policy significantly reduces the educational aspirations of parents living in nearby unregulated cities. Specifically, we estimate that parents' desired years of schooling for their children in treated cities fall by about 0.29 years, or approximately 3–4 months, on average. Relative to the sample mean of 14.62 years, which corresponds to a college degree, this decline is small (about 2% of the mean). When standardized by the sample standard deviation (2.76 years), the effect amounts to approximately 10% of the standard deviation. While modest, this effect is non-negligible, especially considering that parental educational aspirations are typically highly stable and exhibit limited variation over time (Schörner and Bittmann, 2024). Thus, even a small estimated effect may reflect a meaningful shift in parental expectations. Most importantly, the significantly negative effect is informative: it suggests that parents are unlikely to view housing primarily as a savings vehicle to finance future education. If that channel were dominant, one would expect desired years of schooling to increase, not decrease. This evidence appears consistent with the conjecture that the perceived opportunities to profit from housing speculation may weaken parents' belief in the value of education.

[Insert Table 10 approximately here]

B. Heterogeneity analyses

We conduct three heterogeneity analyses to further reveal the substitution effect between housing investment and educational investment. First, we consider whether the effect differs across families with different levels of education and wealth. More educated individuals may place more weight on educational investment and be less affected by the speculative demand for housing investment. Hence, we hypothesize that the HPR effect is weaker for households with higher educational attainment. In addition, because financially constrained families are more likely to substitute housing investment with educational investment, we expect the HPR effect to be attenuated by the wealth level. Accordingly, we construct two dummy variables. *High education* is a dummy variable that equals one if the respondent's years of schooling are above the sample median and zero otherwise. *High household wealth* is a dummy variable that equals one if the household's total wealth is above the sample median and zero otherwise. To account for potential correlation between these two variables, we include both wealth and education in the same regression. Specifically, we add two interaction terms: one between *HPR_250 km* and *High education*, and another between *HPR_250 km* and *High household wealth*, while controlling for both main effects. Column (1) of Table 11 reports the results. We find that the coefficients of both interaction terms are significantly positive. Hence, consistent with our conjecture, the crowding-out effects of housing investment on educational investment are weaker for families with higher levels of wealth and those with higher levels of education. The heterogeneity across wealth levels further suggests that easing borrowing constraints may help mitigate the crowding-out effect of housing speculation, as wealthier households are generally less financially constrained.

Second, an alternative interpretation of our results is that households may be speculating in housing as a means to save for their children's college expenses, intending to sell properties later to fund education. In other words, they may view housing investment as an optimal savings vehicle during a housing boom. If this is the case, we expect the HPR effects to be weaker for households with children in university.

To test this savings vehicle hypothesis, we examine whether the spillover effect of HPR on educational investment varies across children's age groups. Specifically, we create two dummy variables: one indicating whether the household has a child in primary or secondary school (*Primary and secondary school*), and another indicating whether the household has a child in university (*College*). Households with children in kindergarten serve as the base group. Then we interact *HPR_250 km* with these two dummy variables and exclude households with children spanning multiple age groups to ensure clear comparability. As shown in column (2) of Table 11, both interaction terms are statistically insignificant, suggesting that the HPR effects do not differ significantly across children's age groups. This finding to some extent contradicts the savings-for-college interpretation, as the HPR effects remain significant even among households with children in university. In addition, this result indicates that increased labor market opportunities cannot fully explain our results, as the crowding-out effects persist even in households with children who are not of working age.

Third, if the HPR policy indeed leads households to prioritize housing investment over education spending, its impact should be stronger among those who actually purchased a home after the policy implementation compared to those who did not. To test this conjecture, we define a dummy variable, *Homebuyer*, which equals one if the respondent purchased a home after the HPR policy shock and zero otherwise. We then incorporate an interaction term between *HPR_250 km* and *Homebuyer* into the regression model. The results, presented in column (3) of Table 11, show a significantly negative coefficient of the interaction term. This supports our hypothesis, indicating that the effects of the HPR policy are stronger among households that acquired additional property following the policy shock.

[Insert Table 11 approximately here]

C. Comparing single-home and multiple-home owners

The impact of rising housing prices on consumption may vary across households with different homeownership status. For example, multiple-home owners are more likely to benefit from the wealth effect of rising house prices. To capture this heterogeneity, we conduct subsample analyses that distinguish between single-home households and multiple-home owners. Table 12 reports OLS estimates examining differential spillover effects of HPR on three expenditure categories across single-home and multiple-home owners. The results show that HPR significantly reduces educational investment and discretionary consumption among single-home households. By contrast, the effects for multiple-home owners turn positive, though statistically insignificant. These findings suggest that the speculative effect of HPR on education spending is driven primarily by single-home owners. This heterogeneity suggests that the relative strength of the speculative and wealth effects of housing booms varies across households with different homeownership status.

Using automobile sales data, Deng et al. (2022) find that the HPR policy increases automobile consumption. The difference between their results and ours may be partly attributable to heterogeneous responses across household types, as documented in our study, as well as to the characteristics of automobile consumption in China. Vehicle ownership per capita in China was 0.19 in 2020 (Duan et al., 2024), indicating that a considerable proportion of households do not own a car. For these households, automobile spending is effectively censored at zero and therefore has no scope for further decline. In addition, automobile purchasers in China tend to be substantially wealthier than the average population (Grier et al., 2016). As a result, automobile sales data in China are likely to capture a selective sample of relatively wealthier households. Moreover, wealthier households tend to have higher automobile consumption, so their consumption behavior could exert a greater influence on city-level automobile sales examined in Deng et al. (2022). Meanwhile, wealthier households are more likely to own more housing assets and thus experience stronger wealth effects from rising housing prices. Overall, the positive effects documented in Deng et al. (2022) may disproportionately reflect the consumption responses of households in the relatively right-hand tail of the wealth distribution, who are more likely to benefit from increases in housing prices. Consequently, rising house prices might simultaneously induce cutbacks in education spending among many homeowners in our household-level data, while increasing aggregate automobile consumption driven by wealthier homeowners in city-level sales data.

[Insert Table 12 approximately here]

VI. Welfare implications

An important question is how our findings affect individual and social welfare. While a precise answer is beyond this paper's scope, we outline some plausible implications.

First, we calculate the Sharpe ratio (defined as the mean divided by the standard deviation) of the housing price growth rate and our results indicate that the Sharpe ratio for treated cities during this period is 1.0989, suggesting that housing investments in these cities generate a positive risk-adjusted return ex post.

However, individual investors may not necessarily realize positive capital gains if they did not sell at the appropriate time; without realized sales, appreciation does not translate into realized returns. China's housing market slowed sharply after our sample period, with BIS (Bank of International Settlements) data showing a 20% decline in residential housing prices between 2021Q3 and 2024Q4. In light of the subsequent downturn, households in nearby unregulated cities were unlikely to realize capital gains unless they sold their properties in a timely manner. In fact, data in our sample suggest that few households sold their properties after the HPR shock, reflecting China's prevalent buy-and-hold strategy (Glaeser et al., 2017). This tendency is reinforced by the lack of recurrent property tax, higher levies on short-term property sales, and prolonged boom-driven beliefs in ever-rising prices. Thus, households in treated cities appear unlikely to have possessed superior information and may ultimately have been unable to realize capital gains.

Second, rational investment involves diversification. Excessive allocation to real estate amplifies portfolio risk, a concern especially relevant in China where housing constitutes about 60% of household assets (Li, Li, Lu, and Xie, 2020) and most sampled households already own homes.

Third, even if housing investment ultimately pays off for some households, its impact on future educational spending remains uncertain. The perceived prospect of making fortunes through speculation may weaken parents' beliefs in the importance of education, and the success in speculation could further strengthen such beliefs.

Fourth, educational investment is cumulative, with early inputs enhancing the returns to later ones (Cunha, Heckman, Lochner, and Masterov, 2006). Insufficient investment at critical stages can cause lasting harm to human capital (Attanasio, Meghir, and Nix, 2020). Therefore, a reduction in current educational investment induced by housing speculation might cause an irreversible welfare loss for the household in the long run.

Fifth, shifting investment from human capital to housing may be socially suboptimal. Consistent with evidence on resource misallocation in housing booms (Chen and Wen, 2017; Charles et al., 2018), our findings suggest that by crowding out education, speculation might lower aggregate human capital and, in turn, long-run productivity and growth.

VII. Conclusion

This paper explores the impact of the spillover effect of the HPR policy on educational investment in nearby unregulated cities. After controlling for household and city characteristics, we find that compared with faraway unregulated cities, the HPR policy significantly reduces the educational investment of families in nearby unregulated cities. Our additional analyses reinforce the housing speculation channel. Following the HPR policy shock, house prices, market attention, and expectations of future price growth rise in nearby unregulated cities, while discretionary consumption, stock investment, and educational aspirations fall. These phenomena are consistent with households reallocating resources toward housing. Heterogeneity tests show that the HPR effect is stronger for less wealthy and less educated households, persists across children's age groups, and is more pronounced among households that acquired additional property. Finally, the crowding-out effect is concentrated among single-home households, rather than those owning multiple homes.

Our paper provides a new perspective on the real impact of housing market dynamics: housing speculation, resulting from a rapid and dramatic increase in house prices, may increase the relative attractiveness of housing investment and crowd out other investments, particularly those that only pay off in the very long run. The speculation channel differs from but complements the wealth effect and collateral effect of housing booms documented in existing studies. As education is the main driver of long-term growth and development, while real estate booms might stimulate local economies in the short run, the induced housing speculation may, to some extent, lead to distortions in capital allocation and welfare losses in the long run.

Government interventions in housing markets are not unique to China. Our results suggest that transaction restrictions and related policies can have spillover effects on household decisions beyond housing itself, which warrant attention from policymakers and researchers. The extent and direction of these spillovers are likely to depend on institutional characteristics. We acknowledge that our estimates are identified in, and perhaps amplified by, China's institutional context, where access to home-equity extraction is limited, capital markets are relatively underdeveloped, and a prolonged housing fever has occurred. As such, the magnitudes we report should not be directly extrapolated to other economies. A productive direction for future work is to test the substitution effect we propose in settings with different housing market characteristics, financial infrastructures, and policy instruments.

Finally, our study focuses on a relatively short-term span after the HPR policy shock. When more long-term data become available, it would be interesting to explore the long-term effects on households' wealth accumulation, children's educational outcomes and their labor-market performance.

References

Adelino, M., Schoar, A., & Severino, F. (2015). House prices, collateral and self-employment. *Journal of Financial Economics*, *117*(2), 288-306.

Agarwal, S., & Qian, W. (2017). Access to home equity and consumption: evidence from a policy experiment. *Review of Economics and Statistics*, *99*(1), 40-52.

Aghion, P., & Howitt, P. (2008). The economics of growth. *MIT Press*, *Cambridge*, *MA.*

Armona, L., Fuster, A., & Zafar, B. (2019). Home price expectations and behaviour: Evidence from a randomized information experiment. *The Review of Economic Studies, 86*(4), 1371-1410.

Attanasio, O., Meghir, C., & Nix, E. (2020). Human capital development and parental investment in India. *Review of Economic Studies*, 87(6), 2511-2541.

Baker, A. C., Larcker, D. F., & Wang, C. C. (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics*, *144*(2), 370-395

Becker, Gary S. (1967), Human capital and the personal distribution of income. *University of Michigan Press*, *Ann Arbor*, *MI.*

Cengiz, D., Dube, A., Lindner, A., & Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics*, *134*(3), 1405-1454.

Charles, K. K., Hurst, E., & Notowidigdo, M. J. (2018). Housing booms and busts, labor market opportunities, and college attendance. *American Economic Review*, *108*(10), 2947-2994.

Chen, K., Wang, Q., Xu, T., & Zha, T. (2022). Impacts of LTV policy on the mortgage market via housing speculation. Working Paper.

Chen, K., & Wen, Y. (2017). The great housing boom of China. *American Economic Journal: Macroeconomics, 9*(2), 73-114.

Chen, Y.J., Chen, Z., & He, S. (2019). Social norms and household savings rates in China. *Review of Finance*, *23*(5), 961-991.

Chen, Y.Y., Yuan, M., & Zhang, M. (2023). Income inequality and educational expenditures on children: Evidence from the China Family Panel Studies. *China Economic Review*, *78*, 101932.

Corradin, S., & Popov, A. (2015). House prices, home equity borrowing, and entrepreneurship. *Review of Financial Studies*, *28*, 2399-2428.

Cunha, F., Heckman, J. J., Lochner, L., & Masterov, D. V. (2006). Interpreting the evidence on life cycle skill formation. *Handbook of the Economics of Education*, 1, 697-812.

de Chaisemartin, C., & D'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, *110*(9), 2964-2996.

de Chaisemartin, C., & D'Haultfoeuille, X. (2023). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *The Econometrics Journal*, *26*(3), C1-C30.

Deng, Y., Liao, L., Yu, J., & Zhang, Y. (2022). Capital spillover, house prices, and consumer spending: quasi-experimental evidence from house purchase restrictions. *Review of Financial Studies*, *35*(6), 3060-3099.

Deschenes, O., Wang, H., Wang, S., & Zhang, P. (2020). The effect of air pollution on body weight and obesity: Evidence from China. *Journal of Development Economics*, *145*, 102461.

Disney, R., & Gathergood, J. (2018). House prices, wealth effects and labour supply. *Economica*, *85*, 449-478.

Duan, L., Song, L., Wang, W., Jian, X., Heijungs, R., & Chen, W. Q. (2024). Urbanization inequality: evidence from vehicle ownership in Chinese cities. *Humanities and Social Sciences Communications*, *11*(1), 1-12.

Fan, G., Li, H., Li, J., & Zhang, J. (2022). Housing property rights, collateral, and entrepreneurship: Evidence from China. *Journal of Banking & Finance*, *143*, 106588.

Fang, H., Gu, Q., Xiong, W., & Zhou, L. A. (2016). Demystifying the Chinese housing boom. *NBER Macroeconomics Annual*, *30*(1), 105-166.

Fu, Y., Qian, W., & Yeung, B. (2016). Speculative investors and transactions tax: Evidence from the housing market. *Management Science*, 62(11), 3254-3270.

Gan, J. (2010). Housing wealth and consumption growth: evidence from a large panel of households. *Review of Financial Studies*, *23*(6), 2229–2267.

Gao, Z., Sockin, M., & Xiong, W. (2020). Economic consequences of housing speculation. *Review of Financial Studies*, *33*(11), 5248–5287.

Gardner, J. (2022). Two-stage differences in differences. Working paper.

Giannetti, M., & Wang, T. Y. (2016). Corporate scandals and household stock market participation. *Journal of Finance*, *71*(6), 2591-2636.

Glaeser, E.L., & Nathanson, C.G. (2017). An extrapolative model of house price dynamics. *Journal of Financial Economics*, *126*(1),147-170.

Glaeser, E.L., Huang, W., Ma, Y., & Shleifer, A.V. (2017). A real estate boom with Chinese characteristics. *Journal of Economic Perspectives*, *31*(1), 93-116.

Glancy, D. (2021). Housing bust, bank lending & employment: Evidence from multimarket banks. *Journal of Banking & Finance*, *127*, 106111.

Grier, K. B., Hicks, D. L., & Yuan, W. (2016). Marriage market matching and conspicuous consumption in China. *Economic Inquiry*, *54*(2), 1251-1262.

Han, B., Han, L., & Zhu, G. (2018). Housing price and fundamentals in a transition economy: The case of the Beijing market. *International Economic Review*, 59(3), 1653-1677.

Han, L., Lutz, C., Sand, B.M., & Stacey, D.G. (2021). The effects of a targeted financial constraint on the housing market. *Review of Financial Studies*, *34*(8), 3742-3788.

Hanushek, E.A., Kinne, L., Lergetporer, P., & Woessmann, L. (2021). Patience, risk-taking, and human capital investment across countries. *Economic Journal*, *132*(646), 2290–2307.

Hao, L., & Yeung, W. J. J. (2015). Parental spending on school-age children: Structural stratification and parental expectation. *Demography*, *52*(3), 835-860.

Huang, W., & Zhang, C. (2021). The power of social pensions: evidence from China's new rural pension scheme. *American Economic Journal: Applied Economics*, 13(2), 179-205.

Jakiela, P. (2021). Simple diagnostics for two-way fixed effects. Working paper.

Jia, N., Zhou, Y., & Yang, T. (2021). "Selective two-child" policy and household resource allocation. *China Economic Review*, *68*, 101639.

Johnson, W.R. (2014). House prices and female labor force participation. *Journal of Urban Economics*, *82*, 1-11.

Johnson, Rucker C. (2020). The impact of parental wealth on college degree attainment: Evidence from the housing boom and bust. *AEA Papers and Proceedings*, *110*, 405-410.

Kartashova, K., & Tomlin, B. (2017). House prices, consumption and the role of non-Mortgage debt. *Journal of Banking & Finance*, *83*, 121-134.

Kuchler, T., Piazzesi, M., & Stroebel, J. (2023). Housing market expectations. *In Handbook of Economic Expectations*,163-191.

Li, H., Li, J., Lu, Y., & Xie, H. (2020). Housing wealth and labor supply: Evidence from a regression discontinuity design. *Journal of Public Economics, 183*, 104139.

Li, M., Ferreira, S., Smith, T. A., & Zhang, X. (2021). Air pollution and noncognitive traits among Chinese adolescents. *Health Economics*, *30*(2), 478-488.

Liang, P., & Guo, S. (2015). Social interaction, internet access and stock market participation—an empirical study in China. *Journal of Comparative Economics*, 43(4), 883-901.

Liu, C., & Xiong, W. (2020). China's real estate market. The Handbook of China's Financial System. *Princeton University Press*, *Princeton, NJ.*

Lovenheim, M.F. (2011). The effect of liquid housing wealth on college enrollment. *Journal of Labor Economics*, *29*, 741-771.

Lovenheim, M.F., & Reynolds, C.L. (2013). The effect of housing wealth on college choice: evidence from the housing boom. *Journal of Human Resources*, *48*, 1-35.

Ma, C., & Zhang, S. (2024). Can housing booms elevate financing costs of financial institutions?. *Journal of Development Economics, 167*, 103230.

Malmendier, U., & Nagel, S. (2011). Depression babies: Do macroeconomic experiences affect risk taking?. *Quarterly Journal of Economics*, *126*(1), 373-416.

Mian, A.R., & Sufi, A. (2022). Credit supply and housing speculation. *Review of Financial Studies*, *35*(2), 680-719.

Mian, A.R., Rao, K., & Sufi, A. (2013). Household balance sheets, consumption, and the economic slump. *Quarterly Journal of Economics*, *128*, 1687-1726.

Mian, A.R., Sufi, A., & Trebbi, F. (2015). Foreclosures, house prices, and the real economy. *Journal of Finance*, *70*, 2587-2634.

Musso, A., Neri, S., & Stracca, L. (2011). Housing, consumption and monetary policy: How different are the US and the euro area?. *Journal of Banking & Finance*, *35*(11), 3019-3041.

Painter, G., Yang, X., & Zhong, N. (2022). Housing wealth as precautionary saving: Evidence from urban China. *Journal of Financial and Quantitative Analysis, 57*(2), 761-789.

Oreopoulos, P., & Salvanes, K.G. (2011). Priceless: the nonpecuniary benefits of schooling. *Journal of Economic Perspectives*, *25*, 159-184.

Schmalz, M.C., Sraer, D.A., & Thesmar, D. (2017). Housing collateral and entrepreneurship. *The Journal of Finance*, *72*, 99-132.

Schörner, K., & Bittmann, F. (2024). Children's aspirations, their perceptions of parental aspirations, and parents' factual aspirations—gaining insights into a complex world of interdependencies. *European Sociological Review*, *40*(6), 981-995.

Song, Y. (2014). What should economists know about the current Chinese hukou system?. *China Economic Review, 29*, 200-212.

Tian, X., & Wang, Y. (2025). Housing speculation and entrepreneurship, *Review of Finance*, rfaf056

Vaughan, L., & Chen, Y. (2015). Data mining from web search queries: A comparison of Google Trends and Baidu Index. *Journal of the Association for Information Science and Technology*, *66* (1), 13-22.

Wang, H., Cheng, Z., & Smyth, R. (2022). Parental misbeliefs and household investment in children's education. *Economics of Education Review*, *89*, 1-15.

Wu, J., Gyourko, J., & Deng, Y. (2012). Evaluating conditions in major Chinese housing markets. *Regional Science and Urban Economics*, 42(3), 531-543.

Xu, H. (2019). Physical and mental health of Chinese grandparents caring for grandchildren and great-grandparents. *Social Science & Medicine*, *229*, 106-116.

Zhao, L., & Burge, G. (2017). Housing wealth, property taxes, and labor supply among the elderly. *Journal of Labor Economics*, *35*, 227-263.

TABLE 1
Summary statistics

This table reports the summary statistics of the main variables used in the empirical analysis.

	Mean	Median	Min	Max	S.D.	N
Educational investment	7,066.323	3,300	30	400,000	11,218.623	17,863
Log(Educational investment)	7.673	8.102	3.401	12.899	1.923	17,863
HPR_250 km	0.489	0	0	1	0.384	17,863
Age	47.803	46	18	95	11.144	17,863
Household size	4.632	4	2	16	1.474	17,863
Log(Household income)	10.046	10.609	0	15.416	2.283	17,863
Log(Household wealth)	12.538	12.672	3.912	16.811	1.308	17,863
No. children	1.511	1	1	8	0.721	17,863
No. preschool	0.319	0	0	5	0.553	17,863
No. primary school	0.552	0	0	6	0.643	17,863
No. junior high school	0.275	0	0	4	0.483	17,863
No. senior high school	0.221	0	0	4	0.452	17,863
No. college	0.144	0	0	3	0.380	17,863
Log(GDP per capita)	10.593	10.559	9.037	12.293	0.511	17,863
Log(Population density)	5.621	5.768	1.792	7.877	0.882	17,863
Log(Fiscal expenditure)	15.093	15.023	12.499	18.250	0.815	17,863
Log(Fiscal revenue)	13.970	13.873	11.780	18.169	1.047	17,863
Log(Employment)	12.898	12.821	9.007	16.329	1.069	17,863
Log(Average wage)	10.909	10.904	9.924	12.214	0.337	17,863
Log(Loans)	16.729	16.667	14.535	20.598	0.975	17,863
Tertiary Industry	0.404	0.403	0.188	0.817	0.076	17,863
Log(Real estate investment)	14.153	14.038	10.168	17.715	1.214	17,863
Log(No. secondary teacher)	9.762	9.838	7.018	11.760	0.814	17,863
Log(No. primary teacher)	9.783	9.843	6.781	11.800	0.852	17,863

TABLE 2

Benchmark results

This table reports the estimates from OLS regressions that examine the spillover effect of HPR on educational investment. The sample is restricted to homeowner households residing in unregulated cities who have children in school. All regressions include year fixed effects and household fixed effects, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(Educational investment)		
	(1)	(2)	(3)
HPR_250 km	-0.2422**	-0.2411***	-0.2515***
	(0.0931)	(0.0922)	(0.0958)
Age		-0.0025	-0.0025
		(0.0025)	(0.0025)
Household size		0.0203	0.0217
		(0.0325)	(0.0326)
Log(Household income)		0.0199**	0.0208**
		(0.0087)	(0.0088)
Log(Household wealth)		0.0585**	0.0622**
		(0.0246)	(0.0238)
No. preschool		0.4064***	0.4038***
		(0.0710)	(0.0714)
No. primary school		0.2465***	0.2508***
		(0.0566)	(0.0558)
No. junior high school		0.3608***	0.3671***
		(0.0682)	(0.0669)
No. senior high school		0.7965***	0.8012***
		(0.0784)	(0.0790)
No. college		0.8061***	0.8154***
		(0.0898)	(0.0902)
Log(GDP per capita)			0.4647***
			(0.1655)
Log(Population density)			0.1574*
			(0.0857)
Log(Fiscal expenditure)			0.1467
			(0.1581)
Log(Fiscal revenue)			-0.4131**

			(0.1692)
Log(Employment)			0.0576
			(0.0583)
Log(Average wage)			0.0568
			(0.3983)
Log(Loans)			-0.0582
			(0.1646)
Tertiary industry			0.4523
			(0.9900)
Log(Real estate investment)			-0.1011
			(0.0947)
Log(No. secondary teacher)			0.4266*
			(0.2521)
Log(No. primary teacher)			-0.1537
			(0.2779)
Year FE	YES	YES	YES
Household FE	YES	YES	YES
Observations	17,863	17,863	17,863
R-squared	0.5301	0.5458	0.5475

TABLE 3
Alternative definitions of treated cities

This table reports the estimates from OLS regressions that examine the spillover effect of HPR on educational investment using alternative definitions of treated cities. The sample is restricted to homeowner households residing in unregulated cities who have children in school. HPR_150 km is a dummy variable that equals one if a city is within 150 km of a regulated city that is under the house purchase restrictions and zero otherwise. HPR_100 km is a dummy variable that equals one if a city is within 100 km of a regulated city that is under the house purchase restrictions and zero otherwise. HPR_neighbor is a dummy variable that equals one if a city has a neighboring city that is under the house purchase restrictions and zero otherwise. HPR_same province is a dummy variable that equals one if a city is both within 250 km of a regulated city that is under the house purchase restrictions and in the same province as that regulated city and zero otherwise. In column (5), we further exclude from the sample the unregulated cities that are within 250 km of more than one regulated city under the house purchase restrictions. In the remaining sample, HPR_only one city is defined as a dummy variable that equals one if a city is within 250 km of a regulated city that is under the house purchase restrictions and zero otherwise. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(Educational investment)				
	(1)	(2)	(3)	(4)	(5)
HPR_150 km	-0.2822**				
	(0.1117)				
HPR_100 km		-0.3043**			
		(0.1431)			
HPR_neighbor			-0.2866***		
			(0.1002)		
HPR_same province				-0.2511**	

				(0.1196)	
HPR_only one city					-0.2315*
					(0.1317)
Controls	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Household FE	YES	YES	YES	YES	YES
Observations	17,863	17,863	17,863	17,863	9,317
R-squared	0.5476	0.5475	0.5479	0.5474	0.5582

TABLE 4
Additional control variables

This table reports the estimates from OLS regressions that examine the spillover effect of HPR on educational investment with additional control variables. The sample is restricted to homeowner households residing in unregulated cities who have children in school. Intercity economic connection is the economic exposure of an unregulated city to the nearest regulated city. CPI is the city-level CPI. FIRE employment is the natural logarithm of total employment in the FIRE (finance, insurance, and real estate) sector. Construction employment is the natural logarithm of total employment in the construction sector. Other controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(Educational investment)		
	(1)	(2)	(3)
HPR_250 km	-0.2307**	-0.3045***	-0.2520***
	(0.1006)	(0.1074)	(0.0909)
Intercity economic connection	0.2428		
	(0.1888)		
CPI		-0.0448	
		(0.0512)	
FIRE employment			0.2041
			(0.1345)
Construction employment			-0.0265
			(0.0934)
Controls	YES	YES	YES
Year FE	YES	YES	YES
Household FE	YES	YES	YES
Observations	17,787	14,615	17,863
R-squared	0.5464	0.5399	0.5478

TABLE 5

Diagnostic tests for heterogenous treatment effects

This table reports the results of diagnostic tests for heterogenous treatment effects. The sample is restricted to homeowner households residing in unregulated cities who have children in school. We use a stacked DID model in column (1), a DIDM model in column (2), and a DID2S model in column (3). In column (4), we exclude the last wave of the data from the analysis. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(Educational investment)			
	Stacked DID	DIDM	DID2S	Excluding the Last wave
	(1)	(2)	(3)	(3)
HPR_250 km	-0.2873***	-0.2077**	-0.2108***	-0.2435***
	(0.0942)	(0.0935)	(0.0695)	(0.0827)
Controls	YES	YES	YES	YES
Year x Cohort FE	YES	NO	NO	NO
Household x Cohort FE	YES	NO	NO	NO
Year FE	NO	YES	YES	YES
Household FE	NO	YES	YES	YES
Observations	48,589	9,604	15,420	14,140
R-squared	0.5794	-	-	0.5597

TABLE 6

House prices and attention to housing markets

This table reports the estimates from OLS regressions that examine the spillover effect of HPR on house prices and attention to housing markets. Log(House price index), Log(Search for house price), and Log(Search for house purchase) are the natural logarithms of the house price index from CityRE, the annual intensity of Baidu web searches for the keyword "house price", and the annual intensity of Baidu web searches for the keyword "house purchase", respectively. The controls are the same as the city-level characteristics included in column (3) of Table 2, but their coefficients are suppressed for brevity. All regressions include year fixed effects and city fixed effects, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(House price index)	Log(Search for house price)	Log(Search for house purchase)
	(1)	(2)	(3)
HPR_250 km	0.1102***	0.1254***	0.1181***
	(0.0243)	(0.0369)	(0.0361)
Controls	YES	YES	YES
Year FE	YES	YES	YES
City FE	YES	YES	YES
Observations	2,133	2,219	2,219
R-squared	0.9373	0.9005	0.9088

TABLE 7

House price expectation and house purchase intention

This table reports the estimates from OLS regressions that examine the spillover effect of HPR on house price expectation and house purchase intention. The sample is restricted to homeowner households residing in unregulated cities who have children in school. House price expectation is a dummy variable that equals one if the respondent expects house prices to rise in the next three months and zero otherwise. House purchase intention is a dummy variable that equals one if the respondent has a plan to invest in a house and zero otherwise. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	House price expectation	House purchase intention
	(1)	(2)
HPR_250 km	0.1642***	0.0584**
	(0.0357)	(0.0230)
Controls	YES	YES
Year FE	YES	YES
City FE	YES	NO
Household FE	NO	YES
Observations	2,814	12,789
R-squared	0.0660	0.5096

TABLE 8
Reallocation of household resources

This table reports the estimates from OLS regressions that examine the spillover effect of HPR on the reallocation of household resources. The sample is restricted to homeowner households residing in unregulated cities who have children in school. Panel A presents the estimated spillover effect of HPR on other expenditure categories. Log(Basic expenditure) is the natural logarithm of basic expenditure, which includes expenses on necessities such as food, clothing, and commuting. Log(Discretionary expenditure) is the natural logarithm of discretionary expenditure, covering non-essential items such as entertainment and travel. Panel B presents the estimated spillover effect of HPR on portfolio choice. Stock ownership is a dummy variable that equals one if the respondent owns stocks directly or indirectly through mutual funds and zero otherwise. Stock share is the fraction of household wealth invested in stocks. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

Panel A. Other expenditure categories		
	Log(Basic expenditure)	Log(Discretionary expenditure)
	(1)	(2)
HPR_250 km	0.0374	-0.1369**
	(0.0241)	(0.0588)
Controls	YES	YES
Year FE	YES	YES
Household FE	YES	YES
Observations	17,863	17,863
R-squared	0.7266	0.6550
Panel B. Portfolio choice		
	Stock ownership	Stock share
	(1)	(2)
HPR_250 km	-0.0252***	-0.0085*
	(0.0089)	(0.0046)
Controls	YES	YES
Year FE	YES	YES

Household FE	YES	YES
Observations	12,666	12,666
R-squared	0.7449	0.6162

TABLE 9

Reasons for needing funds

This table reports the estimates from OLS regressions that examine the spillover effect of HPR on reasons for needing funds. The sample is restricted to homeowner households residing in unregulated cities who have children in school and report needing funds. For house purchase is a dummy variable that equals one if the respondent's primary reason for needing funds is house purchase and zero otherwise. For children's education is a dummy variable that equals one if the respondent's primary reason for needing funds is children's education and zero otherwise. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	For house purchase	For children's education
	(1)	(2)
HPR_250 km	0.0370**	-0.0388**
	(0.0183)	(0.0175)
Controls	YES	YES
Year FE	YES	YES
City FE	YES	YES
Observations	8,257	8,257
R-squared	0.1072	0.1842

TABLE 10

Parental aspirations for children's education

This table reports the estimates from an OLS regression that examines the spillover effect of HPR on parental aspirations for their children's education. The sample is restricted to homeowner households residing in unregulated cities who have children in school. Aspirations for children's education is the number of years of schooling that parents desire for their children to complete. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Aspirations for children's education
	(1)
HPR_250 km	-0.2864**
	(0.1213)
Controls	YES
Year FE	YES
Household FE	YES
Observations	18,869
R-squared	0.4641

TABLE 11
Heterogeneity analyses

This table reports the estimates from OLS regressions that examine the heterogeneity in the spillover effect of HPR on educational investment. The sample is restricted to homeowner households residing in unregulated cities who have children in school. High education is a dummy variable that equals one if the respondent's years of schooling are above the sample median and zero otherwise. High household wealth is a dummy variable that equals one if the respondent's household wealth is above the sample median and zero otherwise. Primary and secondary school is a dummy variable that equals one if the respondent has a child in primary or secondary school and zero otherwise. College is a dummy variable that equals one if the respondent has a child in university and zero otherwise. Homebuyer is a dummy variable that equals one if the respondent purchased a home after the HPR policy shock and zero otherwise. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(Educational investment)		
	(1)	(2)	(3)
HPR_250 km	-0.5379***	-0.2889**	-0.2239*
	(0.1124)	(0.1204)	(0.1294)
HPR_250 km×High education	0.2710*		
	(0.1373)		
HPR_250 km×High household wealth	0.4009***		
	(0.0826)		
HPR_250km×Primary or secondary school		0.0697	
		(0.1118)	
HPR_250km×College		0.2478	
		(0.2050)	
HPR_250 km×Homebuyer			-0.1684**
			(0.0742)
Controls	YES	YES	YES
Year FE	YES	YES	YES
Household FE	YES	YES	YES

Observations	17,863	13,298	17,863
R-squared	0.5508	0.5523	0.5482

TABLE 12
Comparing single-home and multiple-home owners

This table reports the estimates from OLS regressions that examine the differential spillover effect of HPR between single-home and multiple-home owners. The sample is restricted to homeowner households residing in unregulated cities who have children in school. Log(Basic expenditure) is the natural logarithm of basic expenditure, which includes expenses on necessities such as food, clothing and commuting. Log(Discretionary expenditure) is the natural logarithm of discretionary expenditure, covering non-essential items such as entertainment and travel. Single-home and Multiple-home represent the subsamples of single-home and multiple-home owners, respectively. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(Educational investment)		Log(Basic expenditure)		Log(Discretionary expenditure)	
	Single-home	Multiple-home	Single-home	Multiple-home	Single-home	Multiple-home
	(1)	(2)	(3)	(4)	(5)	(6)
HPR_250 km	-0.2604**	0.0565	0.0385	0.0607	-0.1228**	0.0376
	(0.1276)	(0.1679)	(0.0253)	(0.0443)	(0.0624)	(0.0849)
Controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Household FE	YES	YES	YES	YES	YES	YES
Observations	14,150	3,713	14,150	3,713	14,150	3,713
R-squared	0.5416	0.5144	0.7169	0.7280	0.6451	0.6443

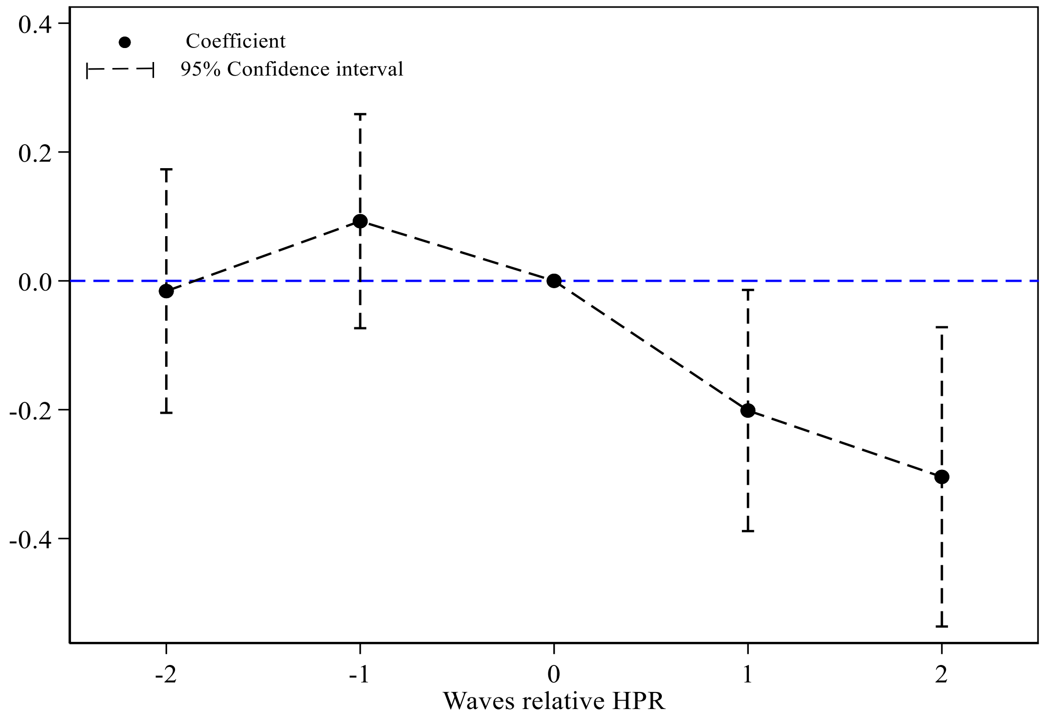

FIGURE 1
Parallel trends test

This figure shows the estimated coefficients and the 95% confidence intervals of a series of wave dummies for the survey waves before and after the HPR policy, estimated from specification (2). The base group is the survey wave in the implementation year of the HPR policy.

Online Appendix

A1. Excluding households in never-treated cities

In the benchmark analysis, households in never-treated cities form part of the control group. Some may be concerned that never-treated units might behave differently from other eventually treated units. To address this concern, we drop never-treated controls and use only later-treated units as controls. Table A3 reports the results. The coefficient of *HPR_250* remains significantly negative, suggesting that using only later treated units as controls does not materially alter our findings.

A2. Addressing attrition bias

The inclusion of household fixed effects should absorb all initial sorting into different cities, thereby mitigating selection problems (Deschenes et al., 2020). However, there may still be selection issues due to attrition from the panel sample (Aragón et al., 2017), if households exposed to the HPR shock are more or less likely than others to drop from the panel sample. In the revised manuscript, we follow the literature (Deschenes et al., 2020) and empirically test and rule out this form of selection bias. Specifically, we add to the benchmark regression a dummy variable called *Drop in the next wave*, which equals one if the household drops from the panel sample in the next wave of the survey. This additional explanatory variable should not be significant under the null hypothesis that attrition is not systematic. Table A4 shows the corresponding empirical results. The coefficient of *Drop in the next wave* is insignificant and that of *HPR_250km* remains negative and significant. Hence, we fail to reject the null hypothesis, suggesting that attrition bias is less of a concern in our analysis.

A3. Educational investment of households in regulated cities

While our primary focus is on households in non-regulated cities, we also explore whether educational investment increased in regulated cities after the HPR policy. For this purpose, we use regulated cities as the new treatment group while maintaining the same control group as in our main specification. Table A5 presents the corresponding results. We find a positive but statistically insignificant impact of the HPR policy on educational investment of families in regulated cities, relative to those in distant unregulated cities.

Caution is warranted in interpreting these results, as the implementation of the HPR policy was non-random, making the estimates susceptible to endogeneity. Moreover, since households in regulated cities had already experienced an extended period of housing market growth prior to the policy, many may have developed a persistent preference for housing investment. As a result, the HPR policy can hardly suppress their speculative motives. In fact, the literature suggests that out-of-town housing demand from households in nearby regulated cities contributes greatly to rising house prices in unregulated cities following the HPR policy (Deng et al., 2022). Hence, we caution readers against interpreting the results from the regulated cities sample.

A4. Falsification test

To further examine the potential influence of omitted variables, we follow the literature (e.g., Li et al., 2016; Wang et al., 2021) and conduct a falsification test by randomly assigning the HPR treatment to cities. Specifically, for each year, we randomly select the same number of pseudo-treated cities as the actual number of treated cities in that year. We implement this random selection procedure throughout our sample period without replacement. The treatment status of a household in a given year is determined based on whether it resides in one of these randomly chosen pseudo-treated cities. We then re-estimate our benchmark DID specification using this artificially generated treatment assignment. As such, the treatment status of a household is not reassigned across years and the randomized city-level HPR indicator mimics the unconditional distribution of the actual treatment.

Figure A1 presents the probability distribution of the estimated coefficients from 1,000 simulations, along with the benchmark estimate from column (3) of Table 2 (represented by a vertical dashed line). This distribution approximates a normal distribution with a mean near zero, indicating that there is hardly any effect with the randomly assigned HPR policy shocks. We also observe that all the simulated effect sizes are smaller in absolute value than the benchmark estimate. Overall, these results lend further support to the notion that the negative spillover effect of HPR on educational investment among families in nearby unregulated cities is unlikely to be driven by unobserved factors.

References

Aragón, F. M., Miranda, J. J., & Oliva, P. (2017). Particulate matter and labor supply: The role of caregiving and non-linearities. *Journal of Environmental Economics and Management*, *86*, 295-309.

Deng, Y., Liao, L., Yu, J., & Zhang, Y. (2022). Capital spillover, house prices, and consumer spending: quasi-experimental evidence from house purchase restrictions. *Review of Financial Studies*, *35*(6), 3060-3099.

Deschenes, O., Wang, H., Wang, S., & Zhang, P. (2020). The effect of air pollution on body weight and obesity: Evidence from China. *Journal of Development Economics*, *145*, 102461.

Li, P., Lu, Y., & Wang, J. (2016). Does flattening government improve economic performance? Evidence from China. *Journal of Development Economics*, *123*, 18-37.

Wang, F., Milner, C., & Scheffel, J. (2021). Labour market reform and firm-level employment adjustment: Evidence from the hukou reform in China. *Journal of Development Economics*, *149*, 102584.

TABLE A1

Definitions and sources of variables

Variable	Definition	Source
Household-level variables		
Log(Educational investment)	The natural logarithm of investment in children's education. Educational investment is the sum of tuition, tutoring fees, the cost of extracurricular activities, the cost of books and supplies, and the cost of other education-related materials and equipment.	CHFS
Age	The respondent's age.	CHFS
Household size	The number of household members.	
Log(Household income)	The natural logarithm of household income (RMB).	CHFS
Log(Household wealth)	The natural logarithm of household wealth (RMB).	CHFS
No. children	The number of children in the household.	CHFS
No. preschool	The number of preschool children.	CHFS
No. primary school	The number of children who are primary school students.	CHFS
No. junior high school	The number of children who are junior high school students.	CHFS
No. senior high school	The number of children who are senior high school students.	CHFS
No. college	The number of children who are college students.	CHFS

City-level variables		
HPR_250 km	A dummy variable that equals one if a city is within 250 km of a regulated city that is presently under the house purchase restrictions and zero otherwise.	Manual data collection
Log(GDP per capita)	The natural logarithm of GDP per capita (RMB).	CSMAR
Log(Population density)	The natural logarithm of population density (the number of people per square kilometer).	CSMAR
Log(Fiscal expenditure)	The natural logarithm of fiscal expenditure (Ten thousand RMB).	CSMAR
Log(Fiscal revenue)	The natural logarithm of fiscal revenue (Ten thousand RMB).	CSMAR
Log(Employment)	The natural logarithm of total employment.	CSMAR
Log(Average wage)	The natural logarithm of average wage (RMB).	CSMAR
Log(Loans)	The natural logarithm of the financial sector's loans outstanding (Ten thousand RMB).	CSMAR
Tertiary industry	The ratio of value-added tertiary industry to local GDP.	CSMAR
Log(Real estate investment)	The natural logarithm of real estate investment (Ten thousand RMB).	CSMAR
Log(No. secondary teacher)	The natural logarithm of the number of secondary school teachers.	CSMAR
Log(No. primary teacher)	The natural logarithm of the number of primary school teachers.	CSMAR

TABLE A2

Lists of treated cities and never-treated cities

This table presents the lists of treated cities and their respective treatment years in Panel A and the list of never-treated cities in Panel B.

Panel A. Treated cities and treatment years

Treatment year	Treated city	Treatment year	Treated city	Treatment year	Treated city
2010	Hengshui	2016	Jilin	2016	Ziyang
2010	Huizhou	2016	Jingmen	2017	Baise
2011	Anqing	2016	Jingzhou	2017	Baoji
2011	Baiyin	2016	Jining	2017	Bijie
2011	Baotou	2016	Kaifeng	2017	Changde
2011	Changzhou	2016	Liaocheng	2017	Chaoyang
2011	Chizhou	2016	Linfen	2017	Chenzhou
2011	Danzhou	2016	Longyan	2017	Chifeng
2011	Datong	2016	Lu’an	2017	Dingxi
2011	Haidong	2016	Luoyang	2017	Guigang
2011	Heyuan	2016	Luzhou	2017	Guyuan
2011	Hezhou	2016	Ma’anshan	2017	Hechi
2011	Huangshan	2016	Meishan	2017	Hengyang
2011	Lishui	2016	Meizhou	2017	Huludao
2011	Nantong	2016	Mianyang	2017	Jinzhou
2011	Ordos	2016	Nanchong	2017	Laibin
2011	Shangrao	2016	Nanping	2017	Lianyungang
2011	Shanwei	2016	Nanyang	2017	Liupanshui
2011	Shaoguan	2016	Neijiang	2017	Liuzhou
2011	Shizuishan	2016	Pingdingshan	2017	Loudi
2011	Shuozhou	2016	Pingxiang	2017	Maoming
2011	Taizhou (Jiangsu)	2016	Putian	2017	Panjin
2011	Tongling	2016	Puyang	2017	Qingyang
2011	Ulanqab	2016	Sanming	2017	Qinzhou
2011	Wuhai	2016	Shangqiu	2017	Rizhao
2011	Wuzhong	2016	Shantou	2017	Shangluo
2011	Wuzhou	2016	Siping	2017	Shaoyang
2011	Xuancheng	2016	Songyuan	2017	Shiyan
2011	Yunfu	2016	Suining	2017	Tianshui
2011	Zhongwei	2016	Suizhou	2017	Tongchuan
2016	Anyang	2016	Suqian	2017	Weinan
2016	Bengbu	2016	Suzhou	2017	Xiangtan
2016	Binzhou	2016	Tai'an	2017	Xiangyang
2016	Bozhou	2016	Tieling	2017	Xianyang

2016	Changzhi	2016	Tongliao	2017	Xinzhou
2016	Chaozhou	2016	Weifang	2017	Yancheng
2016	Chuzhou	2016	Wuhu	2017	Yantai
2016	Deyang	2016	Xingtai	2017	Yingkou
2016	Dezhou	2016	Xinxiang	2017	Yiyang
2016	Dongying	2016	Xinyang	2017	Yongzhou
2016	Fuyang	2016	Xinyu	2017	Zhanjiang
2016	Fuzhou (Jiangxi)	2016	Xuchang	2017	Zhuzhou
2016	Guang'an	2016	Ya'an	2017	Zunyi
2016	Handan	2016	Yibin	2018	Anshan
2016	Heze	2016	Yichun	2018	Benxi
2016	Huaian	2016	Yueyang	2018	Daqing
2016	Huaibei	2016	Yuncheng	2018	Fushun
2016	Huanggang	2016	Zhangzhou	2018	Fuxin
2016	Huangshi	2016	Zhoukou	2018	Luliang
2016	Ji'an	2016	Zhumadian	2018	Qujing
2016	Jiaozuo	2016	Zibo	2018	Suihua
2016	Jieyang	2016	Zigong	2018	Yulin

Panel B. Never-treated cities

Baicheng, Bayannur, Bazhong, Chenzhou, Chifeng, Chongqing, Chuxiong Yi Autonomous Prefecture, Dali Bai Autonomous Prefecture, Dazhou, Enshi Tujia and Miao Autonomous Prefecture, Guangyuan, Haixi Mongol and Tibetan Autonomous Prefecture, Hegang, Heihe, Hengshui, Heyuan, Hezhou, Honghe Hani and Yi Autonomous Prefecture, Huaihua, Hulunbuir, Jiamusi, Jiayuguan, Jiuquan, Jixi, Liangshan Yi Autonomous Prefecture, Lijiang, Linxia Hui Autonomous Prefecture, Longnan, Pingliang, Qiannan Buyi and Miao Autonomous Prefecture, Qiqihar, Shaoguan, Shuangyashan, Taizhou (Jiangsu), Tongren, Xiangxi Tujia and Miao Autonomous Prefecture, Xiangyang, Xing'an League, Yan'an, Yanbian Korean Autonomous Prefecture, Yichang, Yichun, Yushu Tibetan Autonomous Prefecture, Zhaotong

TABLE A3

Excluding households in never-treated cities

This table reports the estimates from an OLS regression that examines the spillover effect of HPR on educational investment using a sample of homeowner households with children in school in unregulated cities that were ever treated at some point during the sample period. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(Educational investment)
	(1)
HPR_250 km	-0.3848***
	(0.1034)
Controls	YES
Year FE	YES
Household FE	YES
Observations	12,579
R-squared	0.5509

TABLE A4

Addressing attrition bias

This table reports the estimates from an OLS regression that addresses attrition bias. The sample is restricted to homeowner households residing in unregulated cities who have children in school. *Drop in the next wave* is a dummy variable that equals one if the household drops from the panel sample in the next wave of the survey. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(Educational investment)
	(1)
HPR_250 km	-0.2528***
	(0.0964)
Drop in the next wave	0.0330
	(0.0646)
Controls	YES
Year FE	YES
Household FE	YES
Observations	17,863
R-squared	0.5476

TABLE A5

Educational investment of households in regulated cities

This table reports the estimates from an OLS regression that examines the effect of HPR on educational investment of households in regulated cities. The sample is restricted to homeowner households residing in regulated cities who have children in school. Cities within 250 km of any regulated city are excluded from the sample. *HPR_regulated city* is a dummy variable that equals one if a city is presently under the house purchase restrictions and zero otherwise. The controls are the same as those in column (3) of Table 2, but their coefficients are suppressed for brevity. Standard errors are clustered at the city level and are reported in parentheses. The significance levels of 1%, 5%, and 10% are denoted by ***, **, and *, respectively.

	Log(Educational expenditure)
	(1)
HPR_regulated city	0.0011
	(0.1668)
Controls	YES
Year FE	YES
Household FE	YES
Observations	13,795
R-squared	0.5607

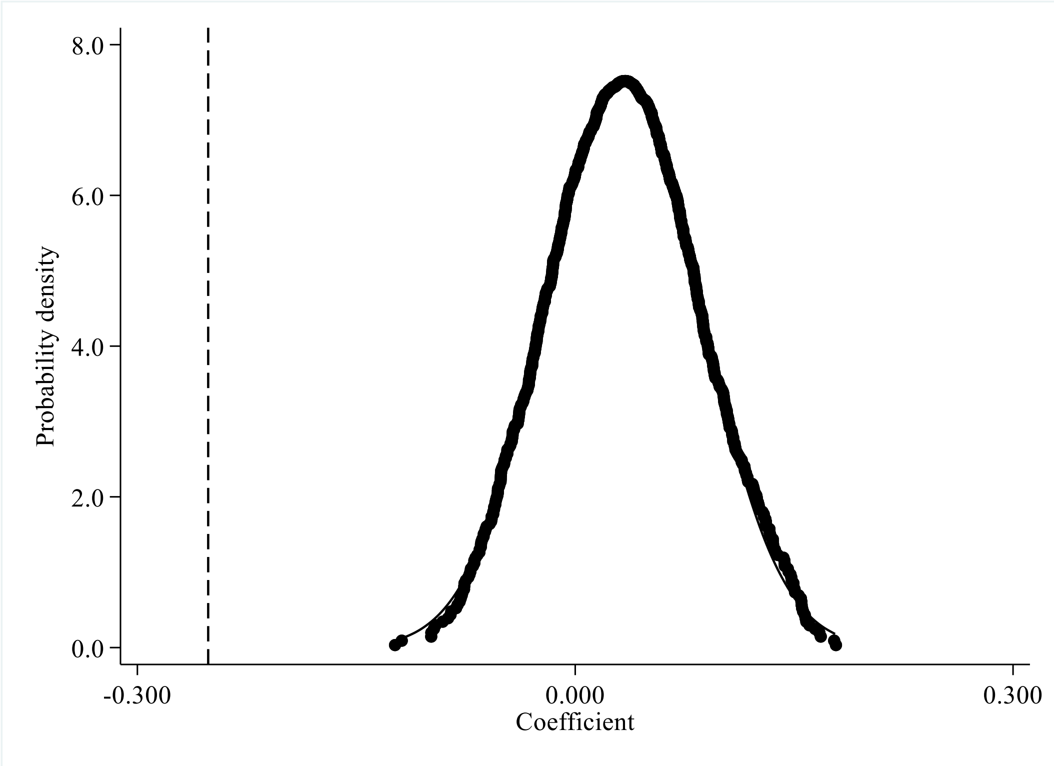

FIGURE A1

Falsification test

This figure plots the probability density function of the estimated coefficients from 1,000 simulations that randomly assign treatment status to cities. The vertical dashed line represents the estimated coefficient in column (3) of Table 2 (-0.2515).