Unintended Consequences of Demand-side Housing Policies: Evidence from Household Reallocation of Capital*

May 2021

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Abstract: We empirically investigate how households allocate their funds in response to an austere housing-market cooling intervention—the Housing Purchase Restriction (HPR) policy in China—that depresses housing investment demand. Based on a proprietary dataset on individual stock accounts from a large stock brokerage firm, we find a significant increase in new stock accounts and capital inflow to the stock market by the affected households immediately after the HPR implementation, which absorbs 53% of the capital that would have flowed into the housing market. The affected investors more likely steer capital toward investment in the listed real estate developers, a pattern that is prevalent across investor demographics and particularly strong in HPR cities with higher pre-policy house price growth. The affected investors subsequently experienced significant trading losses, especially in real estate stocks. We also find listed real estate developers, compared with the non-listed real estate developers, increase their land purchases after HPR despite the negative demand outlook.

Keywords: housing market, housing demand, housing policies, expectation, extrapolative belief, investment choice, resource allocation

JEL codes: R38; R21; G11; G41

* We are grateful to Sumit Agarwal, Lin Peng, Yu Qin, Johan Sulaeman, Shu Tao, Wei Xiong, Abdullah Yavas, Bohui Zhang, as well as seminar and conference participants at the National University of Singapore, Chinese University of Hong Kong, Tsinghua University, Deakin University, AREUEA, ABFER, and GCREC Annual Conference. We thank Keyang Li, Hao Li, and Rongjie Zhang for their excellent research assistance. Xu and Tu thank the Fundamental Research Funds for the Central Universities (No. 63172096 and 63192241) for financial support. Wu acknowledges the National Natural Science Foundation of China (No. 91546113 and 71874093) for financial support.
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1. Introduction

Housing booms carry significant real implications in the aggregate economy by influencing consumption (e.g., Mian, Rao, and Sufi, 2013; Agarwal and Qian, 2017) or through resource and talent reallocation (e.g., Deng et al., 2015; Charles, Hurst, and Notowidigdo, 2017; Huang, Lin, and Liu, 2018). A rising line of work emphasizes the significant role of housing demand, in addition to credit expansion, in explaining house price dynamics. Albanesi (2019) shows investors' mortgage borrowing in 2007 accounts for approximately 30% of the entire mortgage balance in the US. Gao, Sockin, and Xiong (2019) find housing speculation in the US leads to great price increases in the 2004-2006 period. These findings thus highlight household belief as an important determinant of house price movements (Kaplan, Mitman, and Violante, 2019). Indeed, policymakers are concerned not only about the risk of an excessive credit supply, but also about the housing investment sentiment that could result in an overheated market.

To this end, many housing market regulations aim to curb (speculative) housing demand typically by raising the cost of housing transactions. Extant research generally finds those policies successful in reducing the housing market transaction volume and in tempering the house price growth. Although helpful, the evidence does not yet provide a holistic policy evaluation. One important missing piece of a comprehensive understanding centers on the capital reallocation consequences of such policies. Subsequent to demand cooling interventions, where and how do investors channel the funds that they previously preserved for housing investment? Does such capital reallocation have potential unintended consequences?

Concerns for unintended consequences of capital reallocation stem from the challenges in managing households' expectations, as various macroeconomic policies experienced (e.g., McKay, Nakamura, and Steinsson, 2016; D'Acunto, Hoang, and Weber, 2019). In the housing market context, how demand-side policies serve to reshape household expectations about the housing market's prospect remains unclear. Research has widely established that standard theories do not explain how households or investors form their expectations (Barberis, 2018). Moreover, recent studies document that households use recently experienced house price changes in the local market to project future house price changes (Glaeser and Nathanson; 2017; Kuchler and Zafar, 2019). A plausible

conjecture, hence, is that investors remain anchored on the past positive house price trajectory and place less weight on the negative demand signaled by the policy. This extrapolative expectation in turn affects how they reshuffle their funds after the policy, which may lead to inefficient investment choices and even creates distortions in the real sector.

We investigate how households invest their capital in response to cooling housing policies by exploiting China's Housing Purchase Restriction policy (HPR). Announced in April 2010, HPR prohibited households from further home purchases, especially associated with investment purchases, unless they could meet specific qualifications. Therefore, HPR is a(n extreme) form of cooling policies targeted at curbing speculative housing demand. We focus on this policy due to its several unique advantages. Under HPR, treated households are well defined, because all unqualified investors are excluded from the housing investment market, compared to other demand-oriented policies—via transaction or capital gains taxes, for example—that induce endogenous responses by different types of investors (e.g., Fu, Qian, and Yeung, 2016). Combined with HPR's appealing empirical feature of staggered implementation dates across cities, we study the investment response to the policy by tracing the affected investors' capital flow while controlling for common macroeconomic trends.

In addition, the policy mechanically reduces the housing demand, freeing up an enormous amount of capital. The housing sales (including both new sales and resales) dropped by 32.4%, or 84.7 billion RMB, in the first month following the policy implementation in 45 HPR cities. Such a large decrease in housing demand also paints a negative outlook for the housing market. Sophisticated market participants quickly adjusted their expectation of the real estate sector downward: analyst recommendation levels of the 124 real estate companies listed in Shanghai and Shenzhen stock exchanges experienced a significant downgrade within 60 days after the HPR policy announcement. Taken together, the setting offers a good opportunity to evaluate the capital reallocation outcome by studying the use of the affected investors' funds. If the households' expectation adjusts in response to the arrival of negative information about housing demand, we expect to observe households channel their investment funds away from the housing market—including real estate stocks.

We use a proprietary dataset containing individual stock account information from a

large brokerage firm in China. Specifically, we collect micro-level data on all new individual accounts opened at this brokerage firm between January 2010 and April 2012, and a random sample of individual accounts opened between 2000 and 2009. For each account, we have detailed information on the account opening date and branch, and key demographic characteristics of the investor, which helps us identify individuals' exposure to the HPR policy. For a random subsample of the accounts, we also observe detailed information of their trading records from 2010 onwards. During our sample period, each investor can only hold one stock brokerage account in China, which suggests our data capture the entire stock holdings of individuals in the sample.

We classify the treatment group based on households' exposures of investment demand to HPR. More specifically, we define local investors (i.e., those born in the city according to their ID numbers) of the HPR cities as the treatment group, whose housing investment demand is highly restricted by HPR because housing investment outside their home city is subject to significant information and transaction frictions (Lambson, McQueen, and Slade, 2004). The staggered nature of the policy implementation circumvents the need of a separate control group and allows us to use an event-study approach on the treatment group sample for identification. On the other hand, investment demand is less restricted for households who open/hold stock accounts in an HPR city but have migrated from other cities that had not implemented HPR. These non-local investors in the HPR cities have the option to invest in the housing market in their hometown non-HPR cities. Consequently, to corroborate the treatment group's response as the causal impact of HPR, we classify the non-local investors in the HPR cities as the falsification group and study their stock market participation and trading behavior around HPR in placebo tests to gauge the influence of confounding economic factors.[2]

We start our analysis by showing a large and significant increase in stock market participation following HPR after controlling for time and location fixed effects. Compared with eight weeks before the policy implementation, the weekly number of new stock accounts opened by this treatment group increases by 47% during the four weeks after HPR (with an equal size of capital inflow relative to new accounts opened

[2] We also consider another group of unaffected investors for falsification, i.e., local investors in the cities not affected by HPR, and find similar evidence supporting a causal interpretation of HPR on the treatment group.

before HPR). Moreover, the cash inflows to the existing stock accounts—those opened between 2000 and 2009—held by the treatment group also increases by about 329 RMB per account-week during the four weeks after HPR. The hikes in stock account openings and cash inflows for the treatment group are both stronger in HPR cities with higher pre-HPR house price growth or those experiencing larger post-HPR housing sales decreases. By contrast, the stock account opening behavior and the cash inflows remain unchanged for the treatment group during the two weeks immediately before the HPR announcement, or for non-locals (i.e., migrants from non-HPR cities) in the HPR cities. Taken together, these results support the hypothesis that households move their capital into the stock market when HPR restricts their investments in the housing market.

How large is the capital, which households presumably preserve to invest in the housing market, being channeled into the stock market as a result of HPR? Our back-of-the-envelope calculation shows that within four weeks after HPR, a total of 30.1 billion RMB flows to the stock market in China. To put this number into perspective, we apply the difference-in-differences (DID) analysis to the city-month panel of housing sales, which implies an aggregate shrinkage of new home sales of 60.0 billion RMB in these 45 HPR cities within one month after HPR, or 86.6 billion RMB if we also take the housing resale market into consideration. Given the average down payment of 65.4% in home purchases in China before HPR, we obtain a rough estimate that about 53.2% (30.1/ (86.6*65.4%)) of households' housing investment capital is channeled to the stock market after HPR. Consistently, the Shanghai Composite Index on average increases by 1.2% within one trading day or 2.3% within five trading days after 32 such implementation events in the 45 HPR cities.

A natural question arises regarding the investment choice of the large amount of capital directed to the stock market after the HPR policy. Exploiting the within-investor variation by controlling for account and time fixed effects, we document that the existing accounts held by the treatment group (i.e., local investors who opened stock accounts during the decade before HPR) are 72.9% more likely to purchase real estate stocks in the 12-week window after HPR, relative to the period right before the policy. Again, this effect is stronger in cities with higher pre-HPR house price growth rates or larger post-HPR housing sales decreases. Furthermore, conditional on investing in listed real estate developers, investors devote as much capital to the real estate industry

as before.[3] Both of these effects are robust if we expand the scope of the real estate industry by further including the construction firms or adopting a finer definition by restricting the scope of the real estate industry to developers with ongoing projects in the HPR cities. Additional findings verify such a tilt in the investment preference does not exist in the falsification group (non-local investors in the HPR cities), suggesting confounding factors are unlikely to explain the results.

The documented pattern is prevalent among the treated investors across a wide spectrum of investor characteristics: the post-HPR propensity to invest in real estate stocks is equally strong across different age groups or by their wealth. We also find a strong effect for more experienced investors with a longer account history compared to less experienced investors. In other words, the trading behaviors do not result from a subgroup of investors who lack financial sophistication or literacy. Instead, the findings are consistent with the narrative that the affected households collectively do not adjust their expectations downwards on future house price growth and choose to use the stock market as an indirect way of betting on the housing market.

We explore several alternative explanations to the observed trading preference patterns. First, investors in the treatment group may find it optimal to maintain the same real estate exposure in their overall portfolio, and thus hope to use indirect investments on real estate developers to replace direct investments on housing. However, stock investment has a much shorter holding period than housing investment (typically over five years in China). Thus, buying stocks of listed real state firms are not a substitute for long-term housing investment, or as a hedge to maintain exposure in the housing market (until the policy is relaxed). Notably, we also find a strong preference for investing in real estate developer stocks among affected investors with shorter investment horizons, who presumably are less likely to trade for hedging purposes. Second, the investors in the treatment group may be more informed about the housing market (relative to other sectors) and hope to use this advantage in their stock market investment. However, it is unclear why they become more optimistic about listed real estate developers after HPR, especially when analysts immediately adjust their recommendation levels of listed real estate developers downward after this negative

[3] Similarly, compared with new accounts opened before HPR, the new stock accounts opened by local investors in the HPR cities after the policy are more likely to invest in real estate stocks.

shock. Finally, familiarity bias unlikely explains their investment behavior either because the treated investors are no more likely to invest in local stocks after HPR.

To further evaluate the outcome associated with the investment decision of the treated investors, we study their subsequent trading performance. One year after HPR, the average monthly cumulated abnormal return (CAR) is significantly lower—by 0.55 percentage points—than that in the pre-HPR period for the entire stock portfolio of the existing accounts held by the treatment group investors. The relative loss is even more remarkable in investors' real estate stock holdings: the average monthly CAR for the real estate stocks in the affected investors' portfolio in the post-HPR period is 0.72 percentage points lower than the pre-HPR period, and such a loss is significantly higher than that of their non-real estate stock holdings.

The inefficient allocation in the capital market can create distortions in the real sector as well. Intuitively, the large capital inflow provides additional liquidity and reduces the cost of capital for the listed real estate developers. Moreover, it adds noise to the stock prices. Corporate managers rely on stock prices as an information source to learn about the (industry) fundamentals (e.g., Goldstein, Liu, and Yang, 2021), but they possess limited ability to differentiate noise from information in the stock prices (Dessaint et al., 2018). Such a (noisy) signal may further maintain developers' optimism about the housing market. Indeed, across 32 HPR announcements, real estate firms experienced an average positive stock price change of 0.8% (2.7%) during the three-day (30-day) window. As a result, they may continue investments in real estate projects despite the negative housing demand outlook prescribed by the HPR policy.

We find evidence consistent with this hypothesis. Compared with matched non-listed real estate firms, the listed real estate firms increase their investment in the housing market after HPR. After the HPR policy, the probability that a listed developer purchases at least one residential land parcel in a year almost doubles, compared with comparable non-listed real estate firms. Similar patterns exist if we focus on the number of parcels purchased, the total land area, or the total value of the parcels. In other words, the listed real estate firms exhibited a much higher level of investment than the non-listed real estate firms, despite the pessimistic demand outlook in the housing market as a result of the policy.

This paper directly speaks to the effectiveness of housing market interventions (e.g., Fu,

Qian, and Yeung, 2016; Agarwal, Badarinza, and Qian, 2019; Han et al., 2019). Traditional demand-oriented interventions aim at dampening home buying interest by raising the purchase or holding cost (HPR is an extreme form of such policies in which the purchase cost is increased to infinity for the affected investors). Our findings suggest that even when the policy is effective in reducing the house price growth rate in the short run, it will have unintended consequences of capital misallocation through the channel of slow expectation adjustment. By constraining housing investment options without a corresponding change in expectation, the policy leads to suboptimal investment decisions for affected households. The inefficient allocation in the capital market also creates distortions in real estate firms' investment decisions.[4]

Our paper also contributes to the literature on the significant role of expectation in explaining house prices and designing housing policies (Case and Shiller, 2003).[5] The recent work highlights extrapolation as an important mechanism through which households form their housing market expectations (Glaeser and Nathanson, 2017; Gao, Sockin, and Xiong, 2019; Kuchler and Zafar, 2019). Our evidence implies households' tendency to extrapolate past housing returns remains strong even after the arrival of new (negative) information conveyed by the cooling policy, which carries misallocation implications. Specifically, it highlights the importance of an effective policy design to take into consideration of expectation management (e.g., D'Acunto, Hoang, and Weber, 2019).

The rest of the paper proceeds as follows. The next section provides the institutional background about the HPR policy. Section 3 describes the data and empirical strategy adopted in the empirical analysis. The next three sections present the empirical results, with section 4 focusing on the hike in stock market participation after HPR, section 5 on the investment preference, and section 6 on the outcomes. The last section concludes the paper.

[4] Our paper also contributes to the rising literature on the Housing Purchase Restriction policy in China (e.g., Du and Zhang, 2015; Li, Cheng, and Cheong, 2017). The findings in this paper suggest the housing-market-price and transaction-volume response may not provide a comprehensive evaluation of the policy effectiveness.

[5] Optimistic expectation about the housing market underscores the speculative demand for both investors and real estate developers (Fu and Qian, 2014; Mayer and Chinco, 2016; Nathanson and Zwick, 2018). Edelstein and Qian (2014) and DeFusco, Nathanson, and Zwick (2019) investigate how differences in the investment horizon affect price expectations. Bailey et al. (2018) discuss the role of the social network in transmitting house price expectations across housing markets.

2. Policy Background

Most major Chinese cities have witnessed a continuous housing boom since about 2004, of which the most dramatic house price surge occurred between the end of 2008 and early 2010. As part of the stimulus package after the Global Financial Crisis, the Chinese government issued a series of stimulus policies in the housing market since late 2008, including the historically lowest mortgage interest rate, higher loan accessibility for developers, and lower tax rate for housing transactions. As a result, house prices increased by over 40% at the national level between December 2008 and March 2010, and almost doubled during the same period in top cities such as Beijing and Shanghai (Wu, Gyourko, and Deng, 2016; Fang et al., 2015). The price-to-rent ratio also climbed to an incredibly high level in several cities, indicating a potential mispricing in these housing markets (Wu, Gyourko, and Deng, 2012).

To curb the house price surge, the Chinese government swung the pendulum of housing-market intervention policy to the opposite direction in the second quarter of 2010. On April 17, 2010, the State Council released the so-called "Decree Number 10," which included several strict cooling measures to tighten the housing market. As a key policy, for the first time, the central government required that cities with high house price growth rates should set restrictions on housing purchases for non-residents and local multiple-home buyers, or the so-called Housing Purchase Restriction (HPR) policy. As listed in Internet Appendix Table A.1, on April 30, 2010, Beijing, as the first city, announced it would start to implement HPR on May 1, 2010. Then, Lanzhou announced the policy in July. Around October 1, 2010, several other hot markets such as Shenzhen, Shanghai, and Xiamen also issued their HPR policies. During the following 12 months, the HPR policy expanded to 40 other cities, and Zhuhai, as the last city, implemented HPR on November 1, 2011. Thus, in 2010 and 2011, 45 cities in China were implementing HPR.[6] Then, in late 2014, when house prices became relatively stable, most of these cities gradually repealed their HPR policies.

The policy details varied by city, but in general, all the HPR cities imposed restrictions for two groups of home buyers, with the explicit target of curbing investment demand. First, the HPR policy suspended non-local residents from any home purchase in the city.

[6] The city of Shaoxing also imposed restrictions on home purchase for non-local households, but not for local households. Therefore, we do not list Shaoxing as an HPR city in the empirical analysis.

Specifically, one was qualified for home purchase only if (1) she had local household registration, or *hukou*,[7] or (2) she had lived in the city for a long time (e.g., five years for most cities) and could provide certificates of income tax payment or social insurance contribution to demonstrate her length of residence. Second, local residents (i.e., households with local *hukou*) were also prohibited from purchasing multiple homes in the city. In most HPR cities, such as Beijing, Shanghai, and Shenzhen, a local resident could not purchase additional dwelling units if she already owned two units in the city. In a few cities, such as Guangzhou, local residents could only own one dwelling unit. Therefore, HPR policy largely restricts housing investment demand.

The existing literature has empirically investigated the direct impact of HPR on local housing markets. Almost all these empirical studies find a significant decline in housing-market price growth and/or transaction volume after HPR in the short term. Du and Zhang (2015) suggest HPR reduces the annual house price growth rate in Beijing by about 7.7 percentage points. Li, Cheng, and Cheong (2017) adopt data from multiple cities and find that overall the HPR policies are effective in cooling down the housing markets of the treatment cities. They also find a greater price decline in cities with higher pre-HPR price growth rates.

3. Data and Empirical Strategy

3.1 Data

We have access to individual stock investor account and account trading data from a leading stock brokerage company as a major and proprietary data source. The company ranks in the top 10 in the stock brokerage industry in mainland China, with a market share of 2%～4% at the national level, and 150 branches in the 45 HPR cities. Note that before April 2015, by law, each individual investor could only hold one stock account in mainland China, which implies our data can capture the entire stock holdings of sampled individuals during the sample period.

We focus on two types of stock account samples from this company. First, we obtain micro-level information of all individual stock accounts opened between January 2010

[7] *Hukou* is an official certification for permanent residents in a city. See Liu (2005) and Chan and Buckingham (2008), among others, for more institutional background on China's *hukou* system.

(i.e., four months before the central government made the earliest HPR policy announcement in April 2010) and April 2012 (i.e., four months after the last policy announcement in Zhuhai in November 2011). In total, this company opened around 310,000 new individual accounts during the sample period, with about 220,000 accounts (or 71%) in the 45 HPR cities. For each account, we have demographic information on the account holder, such as age range, and key characteristics of the account's trading behaviors as summarized by this brokerage company, including its login frequency, trading frequency, and asset volume. More importantly, we can know the hometown city of the account holder,[8] which plays a key role in our identification strategy as described later.

For a random sample of 73,000 (i.e., 33%) of these new accounts, we can also have access to micro-level information on all trading recorded in these accounts between the account opening date and December 2015. For each trading transaction, we have detailed information on the transaction date, transaction type (sell or buy), stock name, and trading price. Based on the trading records, we can impute the stock holding, as well as its market value and realized trading return, of each account at any time during this period.

Second, we also have access to the information on a random subsample of 17,700 accounts opened between 2000 and 2009 (i.e., the decade before 2010). Similarly, for each account, we can get access to the demographic information and trading-behavior features of the account holder, and all their trading records between January 2010 and December 2015. The summary statistics on major demographic attributes of the existing account sample from the brokerage data are listed in Table 1.

[Insert Table 1 about here]

Besides the stock account data, we also collect two types of firm-level data. The first dataset covers the 1,793 firms listed during or before 2009 on the Shanghai or Shenzhen stock exchanges, including 124 real estate firms and 1,669 non-real estate firms according to the official industry code of the China Securities Regulatory Commission (CSRC). Based on RESSET, a COMPUSTAT-style data vendors in China, we collect

[8] In China, the first 6 digit of an individual's ID number refers to his or her hometown city. It will not change even if she moves to another city and even get local *hukou* in that new city. Although we cannot get access to specific ID number but we could have access to the information of hometown city.

the daily average of analyst recommendation levels on each firm in 2010. We would use this dataset to test how professional analysts respond to the HPR policy in adjusting their recommendation levels for listed firms.

The second firm dataset captures developers' residential land purchase behaviors. More specifically, we firstly collect micro-level data of all the residential land transactions in mainland China between 2008 and 2013 from the official website of the Ministry of Land Resources of China, after which we aggregate the transactions by firm buyers. Between 2008 and 2009, there are 72 listed real estate developers and over 10,000 non-listed developers that purchased at least one residential land parcel in mainland China. For each of these developers, we then construct an annual series between 2008 and 2013 on its residential land purchase behavior, including whether the developer purchases any residential land parcel in the year, the number of parcels purchased, and the total land area and total value of the parcels.

More detailed descriptions of variable definitions are provided in Appendix B.

3.2 Identification and empirical strategy

The main empirical analysis of this paper investigates households' stock market investment response to HPR, including two major parts.

The first part of the empirical analysis focuses on how HPR affects households' stock market participation behaviors. Although a seemingly straightforward strategy is to compare behaviors between households in the HPR cities with those in the non-HPR cities, we choose not to follow this approach because of the endogenous selection of the HPR city. Instead, we exploit the staggered implementation of HPR across cities and examine the behavioral changes of the affected households within the HPR cities, which arguably are more comparable. Because our research target is households' investment behavior in response to HPR, we mainly focus on the exposure of investment demand to HPR, instead of the consumption demand.

More specifically, a household would be defined as a treated household if (1) the investor opens/holds a stock account in one branch of the brokerage company in an HPR city, and (2) the investor was born in this city according to the ID number.[9] On

[9] As described in section 2, the home purchase qualification is determined by *hukou* status and length of

the other hand, the falsification group for a specific HPR city includes households who open/hold stock accounts in this city but, according to their ID numbers, have migrated from other cities that had not implemented HPR.[10] The intuition is that the housing investment demand of households in the treatment group should be highly restricted by the implementation of HPR in the current city, because they lose the most viable options of housing investment due to high information and transaction frictions for housing investments outside their home city (Lambson, McQueen, and Slade, 2004). Instead, the housing investment demand of households in the falsification group is less affected by HPR, because they can choose to continue to pursue housing investment in their hometown cities that are (yet) unaffected by HPR.[11]

Following this identification strategy, we investigate the response using an event study approach. We directly test the responses of the treatment group and identify the effect by exploiting the staggered introduction of HPR announcements across different cities that allows us to explicitly control for time fixed effects that absorb contemporaneous economic trends. As an additional test to capture the counterfactuals, we also study the post-HPR response for the falsification group whose housing investment options are much less affected by the policy but who face similar exposure to other economic factors.

We have two outcome variables for the analysis on stock market participation behaviors, namely, the number of new stock accounts opened and the cash inflows to existing accounts. For new stock account opening behaviors, we introduce the branch-level stock account opening data in the HPR cities by both the treatment and falsification groups, respectively:

$$AO_{treatment,i,t} = \beta_{treatment}{}^{*} I_{POST} + \alpha_i + \delta_t + \varepsilon_{i,t} \quad (1)$$

residence, instead of hometown. But in almost all cases, if an individual has an ID number whose first 6 digits point to the current city, she should have a local *hukou*.

[10] For example, Beijing announced implementation of HPR on April 30, 2010, and Shanghai announced implementation of HPR on October 7, 2010. According to our definition, a migrant from Shanghai to Beijing (according to her ID number) belongs to the falsification group for Beijing because her hometown city had not implemented HPR by April 30, 2010. By contrast, a migrant from Beijing to Shanghai does not belong to the falsification group for Shanghai because her hometown city had implemented HPR by October 7, 2010. Note that the latter case does not belong to the treatment group for Shanghai either and is excluded from the empirical analysis.

[11] We also try using local investors in the non-HPR cities as another falsification group, although it is arguably less comparable with the treatment group. The summary statistics of this alternative falsification group are listed in Table A.2.

$$AO_{falsification,i,t} = \beta_{falsification}{}^{*} 1_{POST} + \alpha_i + \delta_t + \varepsilon_{i,t} \quad (2)$$

where $AO_{treatment,i,t}$ and $AO_{falsificaiton,i,t}$ refer to the number of stock accounts opened in branch i, week t, by the treatment and falsification groups, respectively; 1_{POST} is a dummy that equals one if the accounts were opened in the four weeks (the short-term specification) or 12 weeks (the long-run specification) after the HPR policy announcement date in the corresponding HPR city, and equals zero for the eight weeks before HPR; α_i and δ_t are the branch and time fixed effects, respectively; and $\varepsilon_{i,t}$ is the error term. The coefficient of interest is $\beta_{treatment}$, which represents the effect of HPR on the treatment group, whereas the coefficient of $\beta_{falsification}$ serves as the placebo.

For existing stock accounts opened between 2000 and 2009, we focus on the change in cash inflows around HPR. For this purpose, we examine the account-level weekly cash inflow in the HPR cities:

$$CI_{treatment,k,t} = \beta_{treatment}{}^{*} 1_{POST} + \gamma_j + \delta_t + \varepsilon_{j,t} \quad (3)$$

$$CI_{falsification,k,t} = \beta_{falsification}{}^{*} 1_{POST} + \gamma_j + \delta_t + \varepsilon_{j,t,} \quad (4)$$

where $CI_{treatment,j,t}$ and $CI_{falsification,j,t}$ refer to the weekly amount of cash inflows (in RMB) of account j, week t, held by a household in the treatment or falsification group; 1_{POST} is a dummy that equals one for the four (short-run) or 12 (long-run) weeks after the HPR policy announcement date in the corresponding city, and equals zero for the eight weeks before HPR; γ_j and δ_t are the account and time fixed effects, respectively; and $\varepsilon_{j,t}$ is the error term.

The second part of the empirical analysis investigates change in households' stock market investment behaviors after HPR. Here, we choose to focus on the existing stock accounts held by the treatment group in the HPR cities, because in this sample, we can trace the within-investor change in investment behaviors of the same account after HPR by including individual account fixed effects. More specifically, we have

$$y_{treatment,j,t} = \beta_{treatment}{}^{*} 1_{POST} + \gamma_j + \delta_t + \varepsilon_{j,t} \quad (5)$$

$$y_{falsification,j,t} = \beta_{falsification}{}^{*} 1_{POST} + \gamma_j + \delta_t + \varepsilon_{j,t,} \quad (6)$$

where $y_{treatment,j,t}$ and $y_{falsification,j,t}$ refer to the outcome variable associated with stock account j, week t, held by a household in the treatment or falsification group. For the extensive margin, the outcome variable refers to the account's preference for investing in real estate firms. For the intensive margin, the outcome variable is defined as the dollar amount of real estate stock purchases as a fraction of total purchase volume. 1_{POST} is a dummy that equals one for the 12 weeks after HPR and equals one for the 12 weeks before HPR; γ_j and δ_t are the account and time fixed effects, respectively; and $\varepsilon_{j,t}$ is the error term. The coefficient of interest is $\beta_{treatment}$, which represents the effect of HPR on investors' stock market investment behaviors.

4. HPR and Households' Stock Market Participation

4.1 New stock account openings

We start with households' stock account opening behaviors around HPR. In all 150 branches in the 45 HPR cities, 26,522 new accounts were opened within the four weeks after HPR policy announcements. With a market share of 3%, this number implies a national-level estimate of about 880,000 new accounts. Figure 1 describes the change in the weekly number of new stock-account openings around HPR by the treatment and falsification groups, respectively. Panel A depicts the weekly aggregated number of accounts opened by the treatment group (i.e., local investors in the HPR cities) from four weeks before the HPR policy announcements (i.e., Week -4) to 12 weeks after the events (i.e., Week 12), and Panel B plots the corresponding weekly numbers for the falsification group (i.e., non-local investors in the HPR cities). Although the trends were almost parallel for both groups before HPR, the weekly number of accounts opened by the treatment group increases sharply from around 3,500 per week to over 5,000 per week right after the policy announcement. By contrast, the falsification group does not react to the HPR policy in stock account openings. In Internet Appendix Figure A.1, we extend the scope to 24 weeks after HPR. The weekly number of accounts opened by the treatment group decreases in the longer term but is still higher than the level before HPR. For the falsification group, we do not observe any increase in stock account openings during the whole period.

[Insert Figure 1 about here]

Besides the graphic evidence, we provide the regression results of equation (1) in Panel A of Table 2, with the branch-week number of new stock accounts opened by local investors in the HPR cities as the outcome variable. Column (1) reports the baseline result, which includes the 1_{POST} dummy indicating the four weeks after the policy announcement, and both the branch and year-month fixed effects. The coefficient of 1_{POST} is positive and statistically significant. According to the coefficient, controlling for other factors, households in this treatment group open 11.96 more stock accounts per branch-week as a response to HPR. Given that, on average, households in the treatment group opened 24.93 accounts per branch-week in the HPR cities in the pre-announcement period, this effect can be translated as a 47% hike in new stock account openings by the treatment group after HPR.

[Insert Table 2 about here]

The results above can be interpreted as, when local investors in the HPR cities are restricted in further housing investment by HPR, they choose to reallocate their funds to the stock market. To support the interpretation, we document that this effect is stronger in cities with stricter HPR policies. Although directly measuring the severity of HPR policies in different cities is difficult, in the next two columns, we introduce two proxies. In column (2), we adopt the dummy $1_{HIGHHPG}$, which equals 1 for cities ranked in the top quartile in cumulated house price growth in 2005-2009 (i.e., the 5 years before HPR), with the assumption that the cooling measure should be stricter in cities with higher previous house price surges. In column (3), we measure the severity of HPR from the post-HPR housing transaction outcomes. As described in detail in Appendix C, we first adopt the DID analysis to quantitatively measure the magnitude of housing sales decreases after HPR in each HPR city, and then adopt the dummy $1_{HIGHHSD}$ to label cities in the top quartile of housing sales shrinkage. The interaction terms of both these two proxies with 1_{POST} are positive and statistically significant, which suggests that in cities more severely affected by HPR, treated households open more stock accounts after the policy shock. Finally, in columns (4)-(6), we extend the scope to 12 weeks after HPR policy announcements and find consistent results.[12]

[12] Our additional analysis also suggests these new stock accounts opened by the treatment group are not "zombies"; instead, they are at least as active as accounts opened before HPR. For this purpose, we compare the cash inflows with new accounts opened by the treatment group four weeks after the HPR policy announcement in the HPR cities, and with new accounts opened by the treatment group during

To strengthen the causal relationship between HPR and the increase in stock account openings by the treatment group, we conduct three placebo tests. First, in the first two columns of Panel A, Table 3, we further introduce the dummy 1_{PRE} to equation (1), which equals 1 for the two weeks before the HPR policy announcement. Whereas the dummy 1_{POST} is still significantly positive, 1_{PRE} is not significant in either column (1) (short run) or (2) (long run). The results indicate no pre-trend in stock account openings by the treatment group before the HPR policy announcement.

[Insert Table 3 about here]

Second, in columns (3) and (4) of Panel A, we investigate the number of stock accounts opened by the falsification group (i.e., migrants in the HPR cities from the non-HPR cities). In both two columns, 1_{POST} is not significant.[13] Therefore, households in the falsification group, whose housing investment demands are less exposed to the HPR constraint, do not open more stock accounts after the policy announcement. The results are also robust if we extend the sample to the 24-week post-HPR period.

Finally, we also use households in the non-HPR cities as another falsification group, although it is not our preferred identification strategy, as explained in section 3.2. For this purpose, we match each HPR city with a city that (1) is within the same province, (2) has not implemented HPR, and (3) has the most similar house price growth rate to the HPR city during the previous 12 months. As listed in Panel A of Internet Appendix Table A.3, the dummy 1_{POST} is not significant in this alternative falsification group, which indicates households in the non-HPR cities do not respond to HPR either.

4.2 Cash inflow to existing stock accounts

Besides new stock account openings, we find empirical evidence that for accounts

Week -16 to Week -13. We adopt three indicators on cash inflows, including the dollar amount of the first cash inflow (if it exists) within 12 weeks after account opening, the aggregated dollar amount of cash inflows four weeks after the account opening, and the aggregated cash inflows 12 weeks after the account opening. Controlling for account holders' characteristics and both the branch and year-month fixed effects, for all the three outcomes, we do not find significant changes after HPR, which suggests the cash inflows to these new accounts are at least as high as those to the accounts opened before HPR. The results are available upon request.

[13] We use Chow-test to formally compare the 1_{POST} coefficient between the treatment and falsification groups in Panel A, Table 3 (i.e., column 1 and column 3) and find that the differences are statistically significant (p value= 0.001).

opened by local investors in the HPR cities between 2000 and 2009 (i.e., the decade before HPR), their holders also put more funds in these existing stock accounts after HPR.

The results are listed in Panel B of Table 2. For each randomly sampled account opened by the treatment group (i.e., local investors in the HPR cities) in 2000-2009, we introduce the weekly series of cash inflows from Week -8 to Week 4 or Week 12 around the HPR announcement. Following equation (3), we investigate the change after HPR, controlling for both the account and year-month fixed effects. Column (1) reports the baseline results in the short run, with the 1_{POST} dummy indicating the four weeks after HPR. 1_{POST} is positive and statistically significant. According to the coefficient, investors of the treatment group increase their cash inflows to their existing stock accounts by 329 RMB per week on average after HPR. Again, this finding suggests that when their further investment in the housing market is restricted by HPR, the treated households choose to allocate more funds to the stock market.

We also investigate the city-level heterogeneity in the severity of HPR policies. In column (2), we introduce the interaction term between 1_{POST} and $1_{HIGHHPG}$. The interaction term is positive and statistically significant, which suggests the treated households transfer more funds to their stock accounts after HPR if the city has witnessed higher pre-HPR house price growth (and thus is likely to have a more severe HPR policy). In column (3), we use the dummy $1_{HIGHHSD}$ as the proxy of policy severity. The coefficient of the interaction term is also positive although not significant. In the next three columns, we extend the scope to 12 weeks after HPR and find consistent results.

Similar with the analysis in section 4.1, we also provide placebo tests from three perspectives. For the treatment group, no significant increase in cash inflows to existing stock accounts occurs in the two weeks before HPR (columns (1) and (2), Panel B of Table 3). Additionally, we find no significant change in cash inflows around HPR for accounts held by either the non-local investors in the HPR cities (column (3) and (4), Panel B of Table 3) or the local investors in the non-HPR cities (Panel B of Table A.3).[14]

[14] We use Chow-test to formally compare the 1_{POST} coefficient between the treatment and falsification groups in Panel B, Table 3 (i.e., column 1 and column 3) and find that the differences are statistically significant (p value = 0.005).

These results provide convincing evidence on the causal relationship between HPR and the increase in cash inflows to existing stock accounts held by the treatment group.

4.3 Estimate on the aggregated effect

Based on the above results, in this subsection, we provide back-of-the-envelope calculations on the overall magnitude of funds channeled to the stock market as a response to HPR, as well as its effect on the stock market.

We start with the capital inflows to new accounts opened after HPR. The calculation proceeds as follows (Panel A of Table 4). Column (1) focuses on the short-run estimate (i.e., four weeks after HPR). According to Panel A of Table 2, local investors open, on average, 11.96 more new accounts for each branch-week within the four weeks after HPR. Therefore, around 7,176 (11.96*150*4) additional accounts are opened in all 150 branches in the HPR cities within the four weeks after HPR. On average, the first cash inflow to these new accounts is 44,802 RMB per account. We could then impute that the total volume of cash inflows to these new accounts reaches 0.321 (44,802*7,176) billion RMB at the company level within the four weeks after HPR, which can be translated to a national-level estimate of 10.7 (0.321/0.03) billion RMB based on the company's market share of 3%. In column (2), we extend the scope from four weeks to 12 weeks, and the estimated national-level cash inflow volume increases to 31.9 billion RMB accordingly.

[Insert Table 4 about here]

Panel B turns to the estimate on the aggregated volume of increase in cash inflows to the existing stock accounts. As reported in Panel B of Table 2, we see 329 RMB additional cash inflows per week to each account held by the local investors after HPR, or 1,316 (329*4) RMB for four weeks. According to the official estimate by the China Securities Regulatory Commission, mainland China has 32 million investors in the A-shares stock market. Based on the share of the urban population as reported in the 2010 Population Census, we assume the 45 HPR cities account for 50% of the stock market investors, or 16 million. Our sample further indicates 92% of the stock market investors in the HPR cities are local investors, or 14.7 million. Therefore, we can achieve an estimate of national-level cash inflows of 19.4 (14.7*1.316) billion RMB. In column

(2), we extend the scope from four weeks to 12 weeks, and the estimated national-level cash inflow volume increases to 54.7 billion RMB accordingly. Based on the results of both panels, the overall cash inflows to the stock market due to HPR reaches as high as 30.1 (10.7+19.4) billion RMB within the four weeks after HPR, or 86.6 (31.9+54.7) billion RMB within the 12 weeks.

To provide a more intuitive understanding of the magnitude of such cash inflows to the stock market, we adopt the city-month panel of newly built housing sales and the DID model to estimate the effect of HPR on housing sales. The details are described in Appendix C. The empirical results suggest that, on average, housing sales drop by 32.4% in one month after HPR, which can be translated to an aggregated shrinkage in newly built housing sales of 60.0 billion RMB in these 45 HPR cities. Although we do not have reliable data for the transaction volume of housing resales, we can reasonably assume the policy effect is similar for resale transactions, and thus the total volume of housing sales shrinkage would be 86.6 billion RMB.[15] Considering that the average down-payment ratio in Chinese housing markets was about 65.4% in 2009,[16] this number can be finally translated as a decline of 56.6 billion RMB in households' equity investment in the housing market as a consequence of HPR. In other words, the aggregated cash inflows to the stock market within one month after HPR amounts to 53.2% (30.1/56.6) of the decline in affected households' housing investment during the same period.

Such a huge cash inflow also fuels the stock market in the short run. In Internet Appendix Table A.4, we list the return rates of the stock price index around HPR. More specifically, for the 45 HPR cities, we have 32 event dates (i.e., 13 dates have multiple HPR announcements from different cities). For each event date, we calculate the cumulative return rates of the Shanghai Composite Index during various intervals. For example, the indicator *RETURN* (-1, 1) measures the cumulative growth rate of the Shanghai Composite Index between the last trading day before the specific HPR date and the next trading day. Then, we calculate the average of the return rates of all 32

[15] In 2009, the housing resale market accounted for about 30.7% of total housing sales at the national level. Thus, we can have 60.0 / (1-30.7%) = 86.6 billion RMB.

[16] Taking the year of 2009 as an example, according to the data released by National Bureau of Statistics, at the national level, the developers received 1.59 trillion RMB as down payments from home buyers, and 0.84 trillion RMB as mortgage loans from commercial banks, which implies an average down-payment ratio of 65.4%.

event dates.

According to the results, the averages of *RETURN* (-1, 1) and *RETURN* (-1, 5) reach 1.2% and 2.3%, respectively, both of which are higher than 0 with a significance level of 1%. These results are consistent with the pattern that a huge funding inflow to the stock market within a short time leads to stock prices increase. However, this effect disappears when we extend the interval to 30, 60, or 90 trading days after HPR, and the cumulative growth rate becomes significantly negative if we include 365 trading days after HPR. One may argue the stock price increase might result from the HPR policy itself. For example, investors might believe the cooling measure in the housing market would benefit the macro economy, and thus leads to a positive response in the stock market. To perform a placebo test, in column (2), we calculate the same set of return rates using the date of April 15, 2010, as the event, when the central government first introduced the idea of HPR. The return rates, even in the short term, are either very close to 0 or negative. Therefore, we can rule out the alternative explanation that the short-term stock price increase results from other factors associated HPR (e.g., information signal).

5. Investment Choice in the Stock Market

5.1 Post-HPR change in investment of existing accounts

In the previous section, we provide evidence that affected households reallocate more funds to the stock market as a response to HPR. The next and even more important question is whether the affected households steer away from the housing sector in the stock market, or just use the stock market to indirectly maintain their housing investment. For this purpose, in this subsection, we investigate the treatment group's investment choice in the stock market, especially focusing on their preference for the listed real estate developers.

As discussed in section 3.2, we prefer to adopt the sample of existing accounts held by local investors in the HPR cities, because we can trace the change in investment behaviors of the same accounts around HPR.[17] More specifically, following equation

[17] In untabulated results, we also show the same qualitative results regarding investment preference for the accounts opened around HPR. The findings show similar patterns. The results are available upon request.

(5), we adopt the sampled accounts opened by local investors in the HPR cities between 2000 and 2009, and compare their investment behaviors between 12 weeks before and after the HPR announcement. In our sample of 4,390 accounts held by the treatment group, 115,625 buy transactions occur between Week -12 and Week 12. We aggregate the transaction data at the account-week level for the subsequent empirical analysis.

We start with the analysis at the extensive margin. In column (1), Panel A of Table 5, the outcome variable of $1_{BUY\_REALESTATE}$ equals one if there is at least one buy transaction of any of the 124 listed real estate firms by the account in the week. The dummy 1_{POST} is significantly positive in the model, controlling for both account and year-month fixed effects. The coefficient suggests the propensity of buying real estate stocks is 4.3 percentage points higher after HPR. Given an average propensity of 5.9% before HPR, this effect can be converted to a 72.9% (4.3%/5.9%=72.9%) increase at the extensive margin. In Panel A of Internet Appendix Table A.6, we adopt a broader scope of the real estate industry, which includes not only the 124 listed real estate developers, but also the 39 listed construction firms, and the results remain consistent.

[Insert Table 5 about here]

In the latter two columns of Panel A, we divide the 124 listed real estate companies into exposed and non-exposed groups. Specifically, a listed real estate company is classified in the exposed group if it had any ongoing projects in any of the HPR cities within one year before HPR. 1_{POST} is significantly positive in both the exposed group (column 2) and the non-exposed group (column 3), and the Chow-test suggests no significant difference between the coefficients in these two specifications (p value = 0.73). In other words, investors also prefer real estate firms with higher exposure to HPR, which is consistent with the explanation that the investors in the treatment group use the stock market to indirectly invest in the affected housing markets.

In Panel B, we turn to the results at the intensive margin and use the weekly real estate purchase ratio as the outcome variable, which is defined as the weekly dollar amount of real estate stock purchase divided by the weekly total dollar amount of stock purchase. The regression results reveal that the purchase ratio of real estate stocks is 3.0 percentage points higher after HPR. Given an average ratio of 3.2 percentage points

before HPR, this effect translates into a 93.8% increase in weights of real estate stocks in an individual's weekly post-HPR portfolio.

As a comparison, we replicate the specifications in Table 5 for existing stock accounts held by the falsification groups—non-local households in the HPR cities (Panel A in Internet Appendix Table A.5) and local households in the non-HPR cities (Panel B in Internet Appendix Table A.5). For these two less affected groups, no significant change in investment choice associated with HPR occurs. Therefore, the changes in the treatment group's preference for real estate stocks should result from HPR, instead of other confounding factors.

5.2 Heterogeneity analysis

In Table 6, we further explore the heterogeneity of the investment behavior response. The first two columns of Panel A focus on the city-level heterogeneity in HPR policy severity, with the extensive-margin indicator as the outcome variable. Consistent with the discussions in section 4.1 and 4.2, we adopt indicators $1_{HIGHHPG}$ and $1_{HIGHHSD}$ as the proxies. The interaction terms between both dummies and 1_{POST} are positive and significant, which suggests the investment preference for listed real estate developers is stronger in cities affected more by HPR. The next two columns turn to the intensive margin, with the weekly real estate purchase ratio as the outcome variable. Again, the interaction terms between both policy severity proxies and 1_{POST} are positive and significant. Such a finding is consistent with the literature in which households' expectations highly rely on their recently experienced house price changes in the local market (Gao, Sockin, and Xiong, 2019; Kuchler and Zafar, 2019).

[Insert Table 6 about here]

In Panel B, we explore the heterogeneity at the extensive margin (i.e., propensity to invest) from the investor characteristics perspective, including age, login frequency, trading frequency, account balance volume, and investment experience. We only observe one significant heterogeneity effect: the interaction term with $1_{ACCOUNT\ AGE>MEDIAN}$ is significant and positive, which suggests that the more experienced stock market investors have a stronger preference for real estate stocks after HPR. We do not

find significant heterogeneity effects in the other four aspects. To interpret, the pattern of post-HPR investment preference for real estate firms in the treatment group is not driven by specific cohorts but is prevalent among treated households across a wide spectrum of investor characteristics. Note that even the more active or short-term stock market investors (i.e., with $1_{HIGHTRADINGFREQ}$ equal to one), who presumably follow a different investment strategy relative to a typical housing market investor, exhibit an equally strong preference for trading in real estate stocks after HPR.

Panel C continues to explore the investor-level heterogeneity at the intensive margin with weekly real estate purchase ratio as the outcome variable and find consistent results. The preference for real estate stocks is stronger for more experienced stock market investors. Besides that, we cannot observe any significant heterogeneity effects associated with individual attributes.

5.3 Alternative explanations

The above results reveal that, as a response to HPR, both existing and new stock accounts held by local investors in the HPR cities allocate more funds to the real estate stocks. These behaviors are consistent with the explanation that the affected households do not adjust their expectations on future house price growth downward, and instead choose to use the stock market as an indirect way of betting on house price growth. In this subsection, we explore several alternative explanations.

First and perhaps most importantly, such behavior can arise from investors' hedging motive, assuming their original planned portfolio with a heavy concentration in housing assets is indeed the optimal allocation. However, stock investment has a much shorter holding period than housing investment: the typical holding period for housing is over 20 years, whereas the average holding period is 37 calendar days for a typical stock investor in our sample.[18] Notably, in the heterogeneity analysis above, we also find a

[18] The average holding period for housing investment is calculated based on the statistics released by Ministry of Housing and Urban-Rural Development. The annual transaction volume of housing resales account for 3%-4% in total housing stock in 40 major cities, which implies an average holding period of over 20 years. Meanwhile, according to the data released by Lianjia, a leading housing brokerage company in China, the holding period is beyond five years for over 80% of resold dwelling units in Beijing in 2010-2015. On the other hand, the average stock holding period in our sample is close to the number reported in the official document of the Shenzhen Stock Exchange and quoted by CSRC. Source is from: http://www.csrc.gov.cn/pub/zhejiang/gzdt/201305/t20130530_228814.htm

strong preference for investing in real estate developer stocks among affected investors with shorter investment horizons, who presumably are less likely to trade for hedging purposes. All the evidence jointly suggests pure hedging is unlikely the intended objective for the observed stock-trading behavior in our sample.

Another possible explanation is that these investors are more familiar with the housing market. As a result, they use their information advantage to invest in the stock market. Under this hypothesis, we expect them to possess positive (private) information about the listed real estate developers to rationalize their stock purchase decisions. However, HPR imposes substantial constraints on the demand for housing developers' products (i.e., new homes). Consequently, the policy should negatively affect the operating performance of the developers and hence their stock prices.

We show the impact of HPR on the listed developers' recommendation levels in Table 7. In this model, we exclude the listed financial firms in order to make the recommendation data more comparable. Thus, the dataset includes the 1,521 publicly traded non-financial firms listed on the Shanghai or Shenzhen stock exchanges, including 124 real estate firms and 1,397 non-real estate firms. The data vendor of RESSET collects all analysts' recommendations (5 = *strongly positive*, 1 = *strongly negative*) and calculates the daily average recommendation level for each listed company. We introduce the company-day panel of the average recommendation level between 60 days before April 15, 2010, when the central government announces HPR, and 60 days after.[19] 1_{RE} equals 1 for listed companies in the real estate industry, and 1_{POST} equals 1 for the 60 days after HPR. Analysts adjust their recommendations of the developers downward after the central government announces the policy. As shown in column (1), the listed real estate developers experience a significant downgrade of 0.085 within the 60 days after HPR, which is equivalent to 2.0% of the average recommendation level of listed developers before HPR (4.173). In the next column, we further introduce the interaction term $1_{EXPOSED}$, which equals one if the developer had ongoing projects in any of the HPR cities at the beginning of 2010. These exposed developers presumably are more affected by the policy, where we should expect to observe a negative investment response. Consistent with this conjecture, the analyst

[19] We also try the horizon of 30 days and 90 days, and the results remain qualitatively consistent.

downward adjustment for the exposed firms is large and statistically significant (0.044+0.059=0.103; p value = 0.017).

[Insert Table 7 about here]

Finally, even though the stock investment decisions (into real estate developers) are not information driven, they could reflect investors' familiarity bias; for example, they might be more familiar with the developers with headquarters in their local cities, and thus tend to invest in the related stocks due to familiarity bias. We test the validity of this hypothesis by studying their tendency to invest in local stocks, which is a popular form of familiarity bias (Seasholes and Zhu, 2010). However, we find no evidence that these investors are inclined to invest in local stocks (Panel B of Table A.6).

6. Consequences of Household Capital Reallocation

6.1 Investors' trading performance in the stock market

We start with the *ex post* investment performance of the affected households after HPR. To measure their investment performance in the stock market, we calculate the monthly cumulated abnormal return (CAR) for each sampled account held by the treatment group via the following procedures. First, we compute the daily abnormal return of each stock based on the Fama-French three-factor model. Second, for each stock account, we take the weighted average of daily abnormal return of each stock held in the portfolio, using the dollar amount of holding volume as weights. Finally, we compute the monthly CAR by aggregating the weighted average daily abnormal return in each account-month, using the average dollar amount of the stocks held by the account-month as weights. Besides the monthly CAR for the whole stock holding, for each account, we also calculate the monthly CAR for the real estate stocks and non-real-estate stocks.

We then examine the affected households' performance changes around HPR using the same econometric specification in our main result (e.g., Table 5). The sample period includes one year before and after HPR, and 1_{POST} indicates the post-HPR period. Table 8 shows the results on monthly CAR for the existing accounts held by the treated households. For the overall portfolio performance, the average monthly CAR in the

post-HPR period is significantly lower (by 0.55%) than that in the pre-HPR period. More importantly, the average monthly CAR for the real estate stocks in their portfolio in the post-HPR period is also significantly lower (by 0.72%) than the pre-HPR period, The Chow-test reveals that the 1_{post} coefficient in the regression of real estate stocks is significantly different from the coefficient for non-real-estate stocks' performance (p value = 0.001), which suggests that the treated investors suffer more losses after HPR in their real estate stock investments, compared with their non-real estate stock investments.

[Insert Table 8 about here]

6.2 Listed developers' behaviors after HPR

In this subsection, we investigate how household investors' capital reallocations affect listed developers' behaviors. The above results suggest that after HPR, investors in the treated group allocate more funds to the listed real estate firms in the stock market, injecting additional liquidity to those firms. In addition, the positive stock reaction offers a (noisy) signal that maintains developers' optimism about the housing market, despite the negative housing demand outlook.[20] Indeed, we find that across 32 HPR announcements, real estate firms experienced an average positive stock price change of 0.8% (2.7%) during the three-day (30-day) window (see Internet Appendix Table A.4). In this case, households' inefficient allocation in the capital market may potentially create distortions in the real sector.

To test this potential externality effect, we adopt 72 real estate developers listed on the Shanghai or Shenzhen stock exchanges, which purchased at least one residential land parcel between 2008 and 2009. Then, from all the non-listed developers with any residential land purchase in 2008-2009, we adopt the propensity score matching and match 72 non-listed real estate firms with the 72 listed developers, using their average

[20] As a recent evidence, Goldstein, Liu, and Yang (2021) survey all the 3,626 Chinese public firms. They find that most firms pay attention to the stock market, mainly for the purposes of learning information from the market to guide real investment decisions. Specifically, in the real estate industry, their results suggest that 89.9% of the listed real estate firms pay attention to the stock market, 74.6% of which hold the purpose of learning information from the stock market.

annual total land transaction price in 2008 and 2009 and registered capital as two predict factors (see the comparison of observables between the listed developers and the matched non-listed developers in Table A.7 in the Internet Appendix). These non-listed firms are thus comparable in size and the past land purchase activity, and therefore serve as a good control group to isolate the investment response due to investor capital reallocation to listed developers. We construct a firm-year panel of these 144 firms between 2008 and 2013, with five outcomes measuring their behaviors in the residential market.

From the extensive margin, the dummy of *BUY* refers to whether the firm purchases any residential land parcel in the specific year, and *NUM* refers to the total number of residential land parcels purchased by the firm-year. From the intensive margin, conditional on the firm purchasing at least one residential land parcel in the year, *TVALUE* refers to the total price of the parcels, *AREA* refers to the total land area of the parcels, and *PRICE* refers to the average price (in land area) of the parcels. 1_{LISTED} equals to one for the listed developers, and zero for the matched non-listed firms. 1_{PRE} equals one for 2009; 1_{DURING} equals one for years 2010 and 2011, when the HPR policy are effective in most of the HPR cities; and 1_{DURING} equals one for years 2012 and 2013.

The results are listed in Table 9. We start with the extensive margin. Compared with matched, comparable non-listed real estate firms, the listed developers significantly *increase* their investment in the housing market after the announcement of HPR by purchasing more residential land parcels despite the negative demand outlook in the housing market accompanying the policy. According to the coefficient, listed developers' annual probability of land purchase almost doubles during HPR (the average *BUY* is 0.443 for listed developers before HPR). The effect mostly remains when most of the cities repeal the HPR policy. Similarly, the annual number of parcels purchased by listed developers increases by 170% during HPR (the average *NUM* is 1.236 for listed developers before HPR), and the effect even enlarges after HPR.

[Insert Table 9 about here]

The next three columns of Table 9 turn to the intensive margin. Conditional on residential land purchase, the listed developers spend at least as much funding as before

during and after HPR, compared with the matched non-listed real estate firms (column 3). The results on the land area (column 4) and average price (column 5) also point to a similar pattern.[21] Taken together, these results are consistent with our hypothesis of distortion in real estate firms' investment behavior as a result of the inefficient capital reallocation after HPR.

7. Conclusion

We empirically investigate how households allocate their funds in response to an austere housing market cooling intervention—the Housing Purchase Restriction policy in China—that depresses housing demand. Based on a proprietary dataset on individual stock accounts from a large brokerage firm, we first show that immediately after HPR, a significant increase occurs in both new stock-account openings and capital inflows by the affected households, absorbing about 54% of the capital that would have flowed into the housing market. Moreover, the new incoming capital is more likely to steer toward the listed real estate developers. Such a tendency to invest in real estate stocks is prevalent across investor demographics and is stronger in HPR cities with more severe policies.

Our paper cautions against the effectiveness of housing market interventions intended to curb housing demand. Our findings suggest that even when the policy is effective in reducing the house price growth rate in the short run, unintended consequences arise once we incorporate the capital reallocation outcomes. By constraining housing investment options without a corresponding change in expectation, the policy leads to suboptimal investment for households. The inefficient allocation in the capital market also serves to reduce the informativeness of stock prices, creating distortions in real estate firms' investment decisions. Our evidence highlights the importance of an effective policy design that takes into consideration expectation management.

[21] As additional analysis, we compare 124 listed developers' investment behaviors (with the cash outflows on investment activities as the indicator) with 124 matched non-real estate listed firms. The results show that, listed developers' investments increase by 58% after HPR, compared with non-real estate listed firms. Such an effect exists for both exposed and non-exposed listed developers. The results are available upon request.

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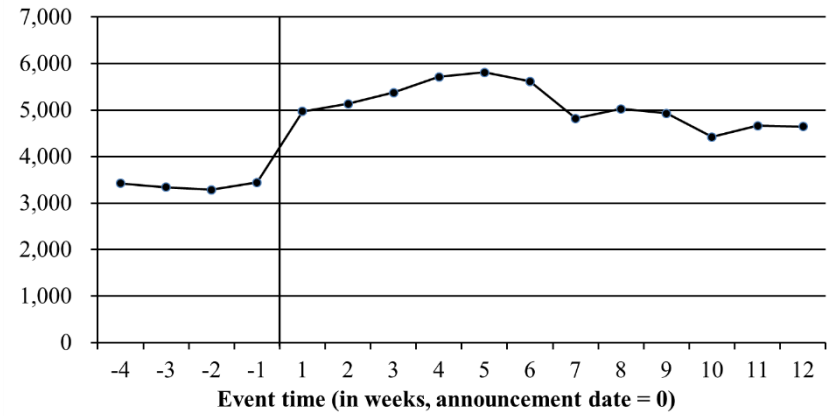
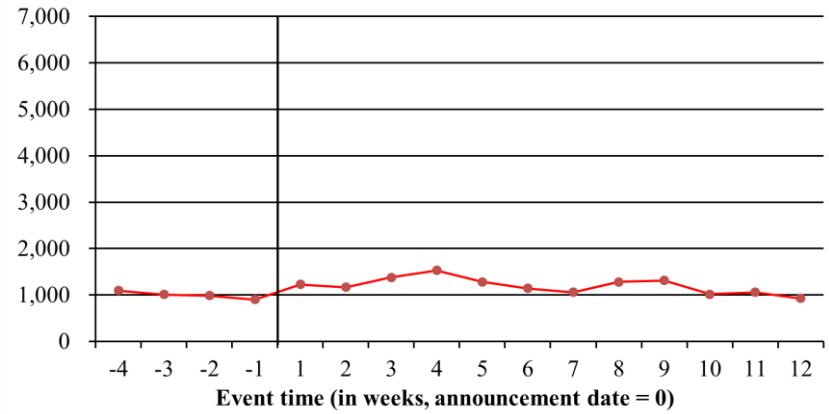
Figure 1: Weekly Number of New Stock Accounts Opened in the HPR Cities



A. Accounts opened by the treatment group



B. Accounts opened by the falsification group

Note: This figure plots the dynamics in the weekly aggregated number of new stock-account openings around HPR policy announcements in 150 branches in the 45 HPR cities by the treatment (local investors in the HPR cities; Panel A) and falsification (non-local investors in the HPR cities; Panel B) groups, respectively, from four weeks before the HPR policy announcements in the corresponding cities to 12 weeks after the events.

Table 1: Summary Statistics: Existing Account Sample

Panel A: Existing accounts held by local investors in HPR cities

	Mean	S.D.	Median
Female	0.46	0.5	
Risk seeking	0.15	0.36	
$1_{HIGHTRADINGFREQ}$	0.44	0.5	
$1_{HIGHACCTACTIVITY}$	0.17	0.38	
$1_{HIGHASSET}$	0.31	0.46	
Age	42.7	12.69	40
# of stocks held monthly	3.5	3.28	2.6
Market value of stock held monthly (thousand RMB)	83.58	288.82	23.79
# of Observations	4,390		

Panel B: Existing accounts held by non-local investors in HPR cities

	Mean	S.D.	Median
Female	0.41	0.49	
Risk seeking	0.12	0.32	
$1_{HIGHTRADINGFREQ}$	0.50	0.50	
$1_{HIGHACCTACTIVITY}$	0.14	0.35	
$1_{HIGHASSET}$	0.22	0.41	
Age	41.0	12.31	40
# of stocks held monthly	3.9	3.40	3
Market value of stock held monthly (thousand RMB)	72.20	208.35	26.32
# of Observations	448		

Note: This table presents the summary statistics of demographic information and trading characteristics (for the trading period of 12 weeks before and after HPR) for existing accounts (opened in 2000-2009) held by local investors in the HPR cities. *# of stocks held monthly* is the average number of stocks held per account per month in the trading period of 12 weeks before and after HPR. *Market value of stock held monthly (thousand RMB)* is the average market value of stock held by the account per month in the trading period of 12 weeks before and after HPR. Please refer to Internet Appendix B for detailed definitions for other variables.

Table 2: Effect of HPR on Stock Market Participation

Panel A: Weekly number of new accounts opened by local investors in the HPR cities

	(1)	(2)	(3)	(4)	(5)	(6)
		Weeks [-8, 4]			Weeks [-8, 12]	
1_{POST}	11.955***	8.789***	7.967***	10.381***	8.828***	8.015***
	(4.48)	(2.72)	(2.88)	(4.01)	(3.06)	(2.85)
$1_{POST} \times 1_{HIGHHPG}$		8.319*			6.478**	
		(1.95)			(2.25)	
$1_{POST} \times 1_{HIGHHSD}$			16.126***			8.195**
			(2.67)			(2.19)
Branch FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,764	1,680	1,728	2,940	2,800	2,880
R-squared	0.596	0.598	0.606	0.582	0.587	0.586

Panel B: Weekly cash inflows (thousands RMB) to existing accounts held by local investors in the HPR cities

	(1)	(2)	(3)	(4)	(5)	(6)
		Weeks [-8, 4]			Weeks [-8, 12]	
1_{POST}	0.329***	0.118**	0.348***	0.310***	0.143***	0.316***
	(6.87)	(2.54)	(6.45)	(6.95)	(3.54)	(6.69)
$1_{POST} \times 1_{HIGHHPG}$		0.400***			0.346***	
		(4.90)			(5.28)	
$1_{POST} \times 1_{HIGHHSD}$			0.009			0.045
			(0.10)			(0.66)
Account FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102,228	102,228	99,936	170,380	170,380	166,560
R-squared	0.181	0.181	0.173	0.165	0.165	0.159

Note: This table shows the effect of HPR on households' stock market participation in the treatment group (local investors in the HPR cities). Panel A reports the results of equation (1), with the weekly number of new accounts opened by the treatment group in the HPR cities as the outcome variable; Panel B reports the results of equation (3), with the weekly cash inflow (thousand RMB) to the existing accounts held by the treatment group in the HPR cities as the outcome variable. In the first three columns of both panels, we introduce the data between eight weeks before the HPR policy announcement date and four weeks after HPR, whereas in the last three columns, we extend the post-HPR period to 12 weeks. 1_{POST} is a dummy indicating the period after the city-specific HPR policy announcement. $1_{HIGHHPG}$ is a dummy that equals 1 if the 5-year (2005-2009) cumulative house price growth rate in the city ranked in the top quartile of all HPR cities. $1_{HIGHHSD}$ is a dummy that equals 1 if the city ranked in the top quartile of the newly built housing trading volume shrinkage during the three months after HPR. We also control for branch fixed effects in Panel A, account fixed effects in Panel B, and year-month fixed effects in both panels. Please refer to Appendix B for detailed variable definitions. Standard errors in both panels are clustered at the branch level. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

Table 3: Effect of HPR on Stock Market Participation: Placebo Tests

Panel A: Weekly number of new accounts opened

	(1)	(2)	(3)	(4)
	Treatment group		Falsification group	
	Weeks [-8, 4]	Weeks [-8, 12]	Weeks [-8, 4]	Weeks [-8, 12]
1_{PRE}	-1.948	-2.329	-1.251	-1.049
	(-1.12)	(-1.36)	(-1.05)	(-0.86)
1_{POST}	10.165***	8.713***	2.185	2.462
	(3.04)	(2.75)	(1.29)	(1.26)
Branch FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Observations	1,764	2,940	1,764	2,940
R-squared	0.596	0.582	0.508	0.468

Panel B: Weekly cash inflows (thousands RMB) to existing accounts

	(1)	(2)	(3)	(4)
	Treatment group		Falsification group	
	Weeks [-8, 4]	Weeks [-8, 12]	Weeks [-8, 4]	Weeks [-8, 12]
1_{PRE}	0.028	-0.004	0.047	-0.045
	(0.87)	(-0.14)	(0.31)	(-0.36)
1_{POST}	0.347***	0.307***	0.092	0.063
	(6.44)	(6.33)	(0.46)	(0.39)
Account FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Observations	102,228	170,380	9,480	15,800
R-squared	0.181	0.165	0.174	0.123

Note: This table shows the effect of HPR on households' stock market participation. Panel A reports the results of equation (1) (columns 1 and 2) and equation (2) (columns 3 and 4), with the weekly number of new accounts opened by the specific group as the outcome variable; Panel B reports the results of equation (3) (columns 1 and 2) and equation (4) (columns 3 and 4), with the weekly cash inflow (thousand RMB) to the existing accounts held by the specific group as the outcome variable. In columns (1) and (2) of both panels, we focus on the accounts opened/held by the treatment group (i.e., local investors in the HPR cities); in columns (3) and (4), we focus on the falsification group (i.e., non-local investors in the HPR cities). In columns (1) and (3) of both panels, we introduce the data between eight weeks before the HPR policy announcement date and four weeks after HPR; in columns (2) and (4), we extend the post-HPR period to 12 weeks. 1_{POST} is a dummy indicating the period after the city-specific HPR policy announcement. 1_{PRE} is a dummy indicating two weeks before the city-specific HPR policy announcement. We also control for branch fixed effects in Panel A, account fixed effects in Panel B, and year-month fixed effects in both panels. Please refer to Appendix B for detailed variable definitions. Standard errors are clustered at the branch level. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

Table 4: Aggregated Cash Inflows to the Stock Market after HPR

Panel A: Capital inflows to new accounts opened by the treatment group after HPR

	(1)	(2)
	4-Week Window	12-Week Window
Numbers of increased new accounts after HPR	7,176	18,683
Average first deposit to new accounts (RMB)	44,802	51,229
Company-level total inflows (million RMB)	321	957
National-level total inflows (billion RMB)	10.7	31.9

Panel B: Capital inflows to existing accounts held by the treatment group after HPR

	(1)	(2)
	4-Week Window	12-Week Window
Per account increase in cash inflows after HPR (RMB)	1,316	3,720
Imputed numbers of local investors in the HPR cities, 2010 (million)	14.72	14.72
National-level total inflows (billion RMB)	19.4	54.7

Note: This table provides the back-of-the-envelope calculation on the national-level volume of cash inflows to the stock market after HPR. Panel A focuses on the cash inflows associated with the new stock accounts opened by the treatment group (local investors in the HPR cities) after HPR, and Panel B focuses on the increased cash inflows to existing accounts held by the treatment group after HPR. Column (1) of both panels adopts the window of four weeks after HPR, whereas column (2) extends the scope to 12 weeks.

Table 5: Preference for Real Estate Stocks

Panel A: Extensive margin

	(1)	(2)	(3)
	$1_{BUY\_REALESTATE}$	$1_{EXPOSED}$	$1_{NONEXPOSED}$
1_{POST}	0.043***	0.011***	0.024**
	(3.72)	(3.58)	(2.58)
Account FE	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
Observations	26,375	26,375	26,375
R-squared	0.321	0.378	0.281

Panel B: Intensive margin

	(1)	(2)	(3)
	Weekly RES buy amount/Weekly total buy amount	*Weekly expo RES buy amount/Weekly total buy amount*	*Weekly non-expo RES buy amount/Weekly total buy amount*
1_{POST}	0.030***	0.006***	0.015***
	(4.82)	(2.64)	(2.71)
Account FE	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
Observations	26,375	26,375	26,375
R-squared	0.357	0.427	0.309

Note: This table reports the results of equation (5) and shows the effect of HPR on the stock investment pattern of existing accounts held by the treatment group (local investors in the HPR cities). Panel A reports the results at the extensive margin, in which we introduce all the buy transactions of the stock accounts held by the treatment group between 12 weeks before HPR and 12 weeks after HPR. In column (1), the outcome variable, $1_{BUY\_REALESTATE}$, equals 1 if there is at least one buy transaction of any of the 124 listed real estate developers in the week; the outcome variable in column (2), $1_{EXPOSED}$, equals 1 if there is at least one buy transaction involves any listed developer with ongoing housing projects in the HPR cities in the week, whereas in column (3), the outcome variable, $1_{NONEXPOSED}$, refers to other non-exposed listed developers. Panel B reports the results at the intensive margin. The outcome variable is the weekly real estate purchase ratio, which is constructed by using the weekly dollar amount of real estate stock purchases as a fraction of total purchase volume. In column (1), we include all the buy transactions on listed developers, whereas in columns (2) and (3), we focus on the exposed and non-exposed developers, respectively. 1_{POST} is a dummy indicating the period after the city-specific HPR policy announcement. We also control for account fixed effects and year-month fixed effects in all the three panels. Please refer to Appendix B for detailed variable definitions. Standard errors are clustered at the branch level. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

Table 6: Preference for Real Estate Stocks: Heterogeneity Analysis

Panel A: City-level heterogeneity; Extensive and intensive margin

	(1)	(2)	（3）	（4）
	$1_{BUY\_REALESTATE}$		*Weekly RES buy $/Weekly total buy $*	
1_{POST}	0.024*	0.029**	0.020***	0.024***
	(1.96)	(2.21)	(2.87)	(3.18)
$1_{POST} \times 1_{HIGHHPG}$	0.061***		0.031***	
	(4.32)		(3.80)	
$1_{POST} \times 1_{HIGHHSD}$		0.035**		0.015*
		(2.52)		(1.70)
Account FE	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
Observations	26,375	25,825	26,375	25,825
R-squared	0.322	0.323	0.357	0.358

Panel B: Investor-level heterogeneity; Extensive margin

INTERACTIVE TERMS	(1) Age > median age	(2) High trading freq	(3) High acc activity	(4) High asset	(5) Account age>Median
	$1_{BUY\_REALESTATE}$				
1_{POST}	0.044***	0.042***	0.042***	0.044***	0.037***
	(3.99)	(3.59)	(3.63)	(3.73)	(3.04)
$1_{POST} \times 1_{interactive\ term}$	-0.004	0.001	0.004	-0.004	0.021**
	(-0.37)	(0.15)	(0.37)	(-0.43)	(2.19)
Account FE	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes
Observations	26,375	26,375	26,375	26,375	26,375
R-squared	0.321	0.321	0.321	0.321	0.321

Panel C: Investor-level heterogeneity; Intensive margin

INTERACTIVE TERMS	(1) Age > median age	(2) High trading freq	(3) High acc activity	(4) High asset	(5) Account age>Median
	Weekly res buy amount/Weekly total buy amount				
1_{POST}	0.029***	0.038***	0.031***	0.033***	0.025***
	(4.64)	(4.94)	(4.78)	(5.00)	(3.92)
$1_{POST} \times 1_{interactive\ term}$	0.000	-0.011	-0.004	-0.009	0.017***
	(0.01)	(-1.59)	(-0.61)	(-1.52)	(2.62)
Account FE	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes
Observations	26,375	26,375	26,375	26,375	26,375
R-squared	0.357	0.357	0.357	0.357	0.357

Note: This table shows the heterogeneity in the effect of HPR on the stock investment pattern of existing accounts held by the treatment group (local investors in the HPR cities). Panel A reports the results from the perspective of city-level variance, and Panels B and C focus on the investor-level variance. In all the panels, we introduce all the weekly buy transactions of the stock accounts held by the treatment group between 12 weeks before HPR and 12 weeks after HPR. The outcome variable for the first two columns of Panel A and Panel B, $1_{BUY\_REALESTATE}$, equals 1 if there is at least one buy transaction of any of the 124 listed real estate developers in each week. The last two columns of Panel A and Panel C report the results by using the weekly real estate purchase ratio as an outcome variable, which is constructed by using weekly dollar amount of real estate stock purchases as a fraction of total purchase volume. 1_{POST} is a dummy indicating the period after the city-specific HPR policy announcement. In Panel A, $1_{HIGHHPG}$ is a dummy that equals 1 if the 5-year (2005-2009) cumulative house price growth rate in the city ranked in the top quartile of all HPR cities; $1_{HIGHHSD}$ is a dummy that equals 1 if the city ranked in the top quartile of the newly built housing trading volume shrinkage during 3 months after HPR. In Panels B and C, we explore the account-specific heterogeneity from age (the dummy of $1_{ABOVE\_MEDIAN\_AGE}$ equals 1 if the account holder is beyond median age of the sample), login frequency (the dummy of $1_{HIGHACCTACTIVITY}$ equals 1 if the account holder frequently logs in to her account as recorded by the brokerage company), trading frequency (the dummy of $1_{HIGHTRADINGFREQ}$ equals 1 if the brokerage company classifies the account as having a high trading frequency), account-balance volume (the dummy of $1_{HIGHASSET}$ equals 1 if the outstanding balance of the account ranks in the top 10% of all accounts in the brokerage company), and investment experience (the dummy $1_{ABOVE\_MEDIAN\_ACCOUNTAGE}$ equals 1 if the age of the stock account is beyond the median value of the sample), respectively. We also control for account fixed effects and year-month fixed effects in all panels. Please refer to Appendix B for detailed variable definitions. Standard errors are clustered at the branch level. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

Table 7: Effect of HPR on Listed Developers' Recommendation Levels

	(1)	(2)
	Average recommendation level	
1_{POST}	-0.017**	-0.017**
	(-2.03)	(-2.05)
$1_{RE} \times 1_{POST}$	-0.085**	-0.044
	(-2.35)	(-0.72)
$1_{RE} \times 1_{POST} \times 1_{EXPOSED}$		-0.059
		(-0.81)
Number of Analysts	-0.000	-0.000
	(-0.03)	(-0.02)
Firm FE	Yes	Yes
Year-month FE	Yes	Yes
Observations	130,115	130,115
R-squared	0.739	0.740

Note: This table shows the effect of HPR on analysts' recommendations on listed real estate developers. We introduce the company-day panel between February 13, 2010 (i.e., 60 days before the central government's announcement of HPR on April 15, 2010) and June 14, 2010 (i.e., 60 days after April 15, 2010). The outcome is *Average recommendation level*, the company-day level average of all analysts' recommendations on the firms (5 = *strongly positive*, 1 = *strongly negative*). The sample covers 1,521 publicly traded non-financial firms listed on the Shanghai or Shenzhen stock exchanges, including 124 real estate firms and 1,397 non-real estate firms according to the official industry code of the China Securities Regulatory Commission. 1_{POST} equals 1 for the period after April 15, 2010, 1_{RE} equals 1 for real estate firms, and $1_{EXPOSED}$ equals 1 if the developer had projects in any HPR cities at the beginning of 2010. We also control for the number of analysts, firm fixed effects, and year-month fixed effects. Please refer to Appendix B for detailed variable definitions. Standard errors are clustered at the firm level. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

Table 8 Trading Performance for Existing Accounts Held by the Treated Investors in HPR cities

	(1)	(2)	(3)
	Monthly CAR (%)	*Monthly CAR for real estate sector (%)*	*Monthly CAR for Non-real estate sector (%)*
1_{POST}	-0.549***	-0.719**	-0.544***
	(-4.81)	(-2.29)	(-4.87)
Account FE	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes
Observations	93,609	16,200	90,686
R-squared	0.148	0.333	0.149

Note: This table shows the change in treated accounts' trading performance around HPR. For each account held by the treatment group, we include the monthly CAR between 1 year before and after HPR. Column (1) investigates the monthly CAR for the investor's whole stock holding, column (2) focuses on the monthly CAR of real estate stocks, and column (3) focuses on the non-real estate stocks. *Monthly CAR (%)* is the aggregated daily weighted average CAR for the stocks in the monthly holding of the investor, where the daily weighted average CAR is calculated as the daily average CAR weighted by the average market volume of the stock holding. 1_{POST} indicates the period after the HPR policy announcement date. We also control for account fixed effects and year-month fixed effects. Please refer to Appendix B for detailed variable definitions. Standard errors are clustered at the branch level. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

Table 9: Post-HPR Developer Investment Response

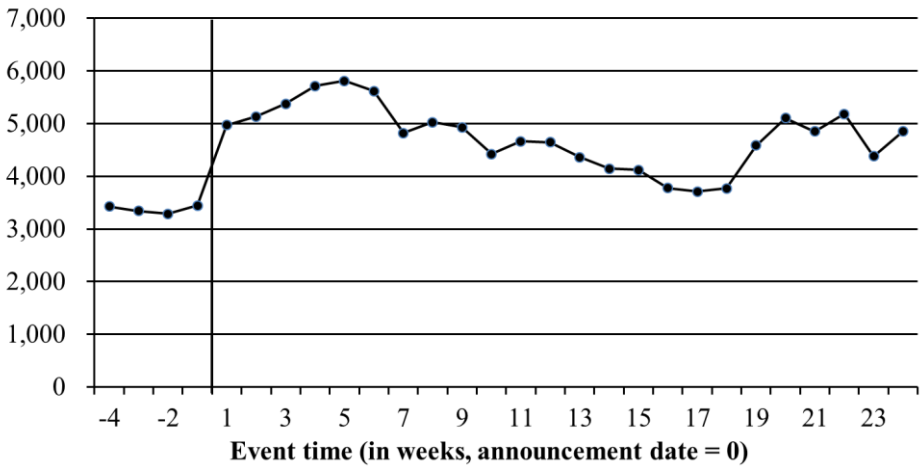
	(1)	(2)	(3)	(4)	(5)
	Extensive Margin		Intensive Margin		
	BUY	*NUM*	ln(*TVALUE*)	ln(*AREA*)	ln(*PRICE*)
1_{PRE} * 1_{LISTED}	-0.111	-1.264	-0.369	-0.0664	0.0921
	(-0.968)	(-1.504)	(-0.584)	(-0.136)	(0.210)
1_{DURING} * 1_{LISTED}	0.431***	2.111***	0.663	0.0735	0.176
	(4.488)	(3.423)	(1.166)	(0.158)	(0.435)
1_{POST} * 1_{LISTED}	0.375***	3.451***	0.990*	0.0175	0.147
	(4.033)	(5.160)	(1.708)	(0.0372)	(0.328)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	864	864	372	372	372
R-squared	0.397	0.600	0.710	0.598	0.648

Note: This table investigate the effect of HPR on listed real estate developers' residential land purchasing behaviors. In the first two columns, we adopt the firm-year panel between 2008 and 2013, including 72 listed real estate firms in mainland China and 72 non-listed firms matched by PSM. In columns (3) to (5), we only include the firm-years with residential land purchase. *BUY* is the dummy variable that equals 1 if the firm purchased at least one residential land parcel in the year; *NUM* refers to the total number of land parcels purchased by the firm in the year; *TVALUE* refers to the total value of land parcels purchased by the firm in the year; *AREA* refers to the total land area of land parcels purchased by the firm in the year; *PRICE* refers to the average price (yuan per sqm of land area) of land parcels purchased by the firm in the year. 1_{LISTED} equals 1 for the listed developers, and 0 for the matched non-listed developers. 1_{PRE} equals 1 for 2009; 1_{DURING} equals 1 for 2010 and 2011; 1_{POST} equals 1 for 2012 and 2013. Robust standard errors are used in all the regressions. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

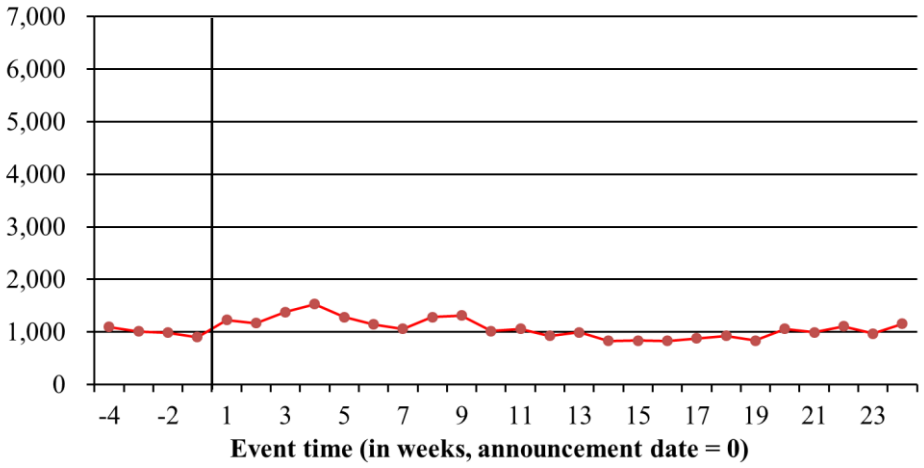
INTERNET APPENDIX

NOT FOR PUBLICATION

Figure A.1: Weekly Number of New Stock Accounts Opened in the HPR Cities (24-Week Period)



A. Accounts opened by the treatment group



B. Accounts opened by the falsification group

Note: This figure plots the dynamics in the weekly aggregated number of new stock account openings around HPR policy announcements in 150 branches in the 45 HPR cities by the treatment (local investors in the HPR cities; Panel A) and falsification (non-local investors in the HPR cities; Panel B) groups, respectively, from four weeks before the HPR policy announcements in the corresponding cities to 24 weeks after the events.

Table A.1: Milestones of HPR Policies in 45 HPR Cities

City	Announcement	Implementation	Cancelation	City	Announcement	Implementation	Cancelation
Beijing	2010/4/30	2010/5/1	-	Chengdu	2011/2/15	2011/2/15	2014/7/16
Lanzhou	2010/7/7	2010/7/7	2014/9/3	Guiyang	2011/2/16	2011/2/16	2014/9/1
Shenzhen	2010/9/30	2010/9/30	-	Nanjing	2011/2/19	2011/2/19	2014/9/21
Xiamen	2010/9/30	2010/10/1	2015/1/16	Taiyuan	2011/2/19	2011/2/19	2014/8/4
Shanghai	2010/10/7	2010/10/7	-	Shijiazhuang	2011/2/19	2011/2/20	2014/9/26
Ningbo	2010/10/9	2010/10/9	2014/7/31	Wuxi	2011/2/20	2011/2/21	2014/8/30
Fuzhou	2010/10/10	2010/10/10	2014/9/22	Yinchuan	2011/2/20	2011/2/22	2014/8/26
Hangzhou	2010/10/11	2010/10/11	2014/7/29	Shenyang	2011/2/25	2011/2/25	2014/9/12
Zhoushan	2010/10/12	2010/10/12	2014/8/2	Xi’an	2011/2/25	2011/2/25	2014/9/1
Sanya	2010/10/12	2010/10/12	2014/10/7	Xining	2011/2/25	2011/2/25	2014/9/6
Tianjin	2010/10/13	2010/10/13	2014/10/17	Harbin	2011/2/27	2011/2/28	2014/8/16
Wenzhou	2010/10/14	2010/10/14	2013/8/6	Haikou	2011/2/28	2011/2/28	2014/7/22
Guangzhou	2010/10/15	2010/10/15	-	Kunming	2011/3/1	2011/3/1	2014/8/11
Dalian	2010/10/18	2010/10/19	2014/9/3	Changsha	2011/3/4	2011/3/4	2014/8/6
Suzhou	2010/11/3	2010/11/3	2014/9/3	Urumqi	2011/3/10	2011/3/10	2014/10/23
Zhengzhou	2011/1/5	2011/1/6	2014/8/9	Foshan	2011/3/19	2011/3/19	2015/5/1
Wuhan	2011/1/14	2011/1/15	2014/9/24	Jinhua	2011/3/23	2011/3/23	2014/8/1
Nanchang	2011/1/21	2011/2/1	2014/8/12	Hohhot	2011/3/31	2011/3/31	2014/6/26
Jinan	2011/1/21	2011/3/1	2014/7/10	Xuzhou	2011/4/14	2011/5/1	2014/8/1
Hefei	2011/1/25	2011/1/25	2014/8/2	Taizhou	2011/8/25	2011/9/1	2014/8/19
Qingdao	2011/1/28	2011/1/31	2014/9/1	Quzhou	2011/9/11	2011/9/11	2014/7/23
Changchun	2011/1/28	2011/1/28	2014/7/19	Zhuhai	2011/11/1	2011/11/1	2014/9/26
Nanning	2011/2/13	2011/3/1	2014/10/1				

Note: The data are collected by the authors from the official documents in the corresponding HPR cities.

Table A.2: Summary Statistics: Existing Accounts Held by Local Investors in the non-HPR Cities

	Mean	S.D.	Median
Female	0.46	0.50	
Risk seeking	0.13	0.34	
$1_{HIGHTRADINGFREQ}$	0.45	0.50	
$1_{HIGHACCTACTIVITY}$	0.17	0.38	
$1_{HIGHASSET}$	0.26	0.44	
Age	40.6	11.21	40
# of stocks held monthly	7.9	12.6	4.4
Market value of stock held monthly (thousand RMB)	999.25	6713.84	111.20
# of Observations	1,705		

Note: This table presents the summary statistics of demographic characteristics and trading characteristics (for the trading period of 12 weeks before and after HPR) for existing accounts (opened in 2000-2009) held by local investors in the non-HPR cities. *# of stocks held monthly* is the average number of stocks held per account per month in the trading period of 12 weeks before and after HPR. *Market value of stock held monthly (thousand* RMB*)* is the average market value of stock held by the account per month in the trading period of 12 weeks before and after HPR. Please refer to Appendix B for detailed definitions for other variables.

Table A.3: Effect of HPR on Stock Market Participation: Additional Placebo Tests

Panel A: Weekly number of new accounts opened by local investors in the non-HPR cities		
	(1)	(2)
	Weeks [-8, 4]	Weeks [-8, 12]
1_{PRE}	0.626	-1.222
	(0.39)	(-0.84)
1_{POST}	1.140	-2.827
	(0.26)	(-0.90)
Branch FE	Yes	Yes
Year-Month FE	Yes	Yes
Observations	1,968	3,280
R-squared	0.536	0.535

Panel B: Weekly cash inflows (thousands RMB) to existing accounts held by local investors in the matched non-HPR cities		
	(1)	(2)
	Weeks [-8, 4]	Weeks [-8, 12]
1_{PRE}	0.044	0.040
	(0.41)	(0.42)
1_{POST}	-0.064	-0.067
	(-0.57)	(-0.63)
Account FE	Yes	Yes
Year-Month FE	Yes	Yes
Observations	32,424	54,040
R-squared	0.354	0.333

Note: This table shows the effect of HPR on the stock market participation of local investors in the matched non-HPR cities. Panel A reports the results of equation (2), with the weekly number of new accounts opened by this group as the outcome variable; Panel B reports the results of equation (4), with the weekly cash inflow (*thousand* RMB) to the existing accounts held by this group as the outcome variable. In column (1) of both panels, we introduce the data between eight weeks before the HPR policy announcement date and four weeks after HPR, whereas in column (2), we extend the post-HPR period to 12 weeks. 1_{POST} is a dummy indicating the period after the city-specific HPR policy announcement. 1_{PRE} is a dummy indicating two weeks before the city-specific HPR policy announcement. We also control for branch fixed effects in Panel A, account fixed effects in Panel B, and year-month fixed effects in both panels. Please refer to Appendix B for detailed variable definitions. Standard errors are clustered at the branch level. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

Table A.4: Stock Market Reactions to HPR

	(1) Average of 32 HPR events (whole market)	(2) Average of 32 HPR events (real estate stocks)	(3) April 15, 2010
Return (-1, 1)	0.012***	0.008***	0.003
	(3.46)	(7.29)	-
Return (-1, 5)	0.023***	0.012***	-0.053
	(3.42)	(9.49)	-
Return (-1, 30)	0.018*	0.027***	-0.171
	(1.84)	(11.92)	-
Return (-1, 60)	-0.009	0.016***	-0.223
	(-0.74)	(4.96)	-
Return (-1, 90)	-0.020	0.008***	-0.162
	(-1.60)	(2.20)	-
Return (-1, 365)	-0.208***	-0.152***	-0.247
	(-17.24)	(-23.50)	-

Note: This table shows the stock market reaction for HPR. In column (1), we list the average stock market return rates, as well as the associated t statistics, around the HPR announcement events. For the 45 HPR cities, we have 32 event dates (i.e., 13 dates had multiple HPR announcements from different cities). For each event date, we calculate the cumulative return rates of the Shanghai Composite Index during various intervals. For example, the indicator of *Return (-1, 1)* measures the cumulative growth rate of the Shanghai Composite Index between the last trading day before the specific HPR date and the next trading day. We then calculate the average of the return rates of all these 32 event dates, as well as the t statistics. In column (2), we focus on real estate stocks (the aggregated prices of 124 stocks) and compute the same return indicators as column (1). ***, **, and * represent the significance levels at 1%, 5%, and 10%, respectively, in these two columns. In column (3), we calculate the corresponding stock market return indicators around April 15, 2010, when the central government first released the HPR policy; the t statistics are not applicable here.

Table A.5: Preference for Real Estate Stocks: Placebo Tests

Panel A: Existing accounts of the non-local investors in HPR cities

	(1)	(2)
	$1_{Buy\_REALESTATE}$	*Weekly RES buy amount/Weekly total buy amount*
1_{POST}	0.006	0.004
	(0.49)	(0.12)
Account FE	Yes	Yes
Year-month FE	Yes	Yes
Observations	3,063	3,063
R-squared	0.286	0.325

Panel B: Existing accounts of local investors in the matched non-HPR cities

	(1)	(2)
	$1_{Buy\_REALESTATE}$.	*Weekly RES buy amount/Weekly total buy amount*
1_{POST}	-0.007	-0.007
	(-0.93)	(-1.21)
Account FE	Yes	Yes
Year-month FE	Yes	Yes
Observations	14,365	14,365
R-squared	0.329	0.335

Note: This table reports the results of equation (6) with two placebo tests. Panel A presents the effect of HPR on the stock investment pattern of existing accounts held by the falsification group in the HPR cities and Panel B shows the effect of HPR on the stock investment pattern of existing accounts held by local investors in the matched non-HPR cities. Column (1) reports the results at the extensive margin, in which we introduce all the buy transactions of the stock accounts between 12 weeks before HPR and 12 weeks after HPR. The outcome variable of column (1), $1_{BUY\_REALESTATE}$, equals 1 if there is at least one buy transaction of any of the 124 listed real estate developers in each week. Column (2) reports the results at the intensive margin. The outcome variable is the weekly real estate purchase ratio, which is constructed by using weekly dollar amount of real estate stock purchases as a fraction of total purchase volume. 1_{POST} is a dummy indicating the period after the city-specific HPR policy announcement. We also control for account fixed effects and year-month fixed effects. Please refer to Appendix B for detailed variable definitions. Standard errors are clustered at the branch level. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

Table A.6: Trading Behavior in Real Estate Stocks: Robustness

Panel A: Extensive margin; Alternative definition of real estate stocks		
	$1_{BUY\_REALESTATE2}$	*Weekly RES buy amount/Weekly total buy amount*
1_{POST}	0.042***	0.027***
	(3.12)	(3.30)
Account FE	Yes	Yes
Year-month FE	Yes	Yes
Observations	26,375	26,375
R-squared	0.338	0.375

Panel B: Extensive margin; Local stocks		
	$1_{BUY\_LOCAL\_FIRM}$	*Weekly local firm buy amount/Weekly total buy amount*
1_{POST}	0.003	0.002
	(1.25)	(1.26)
Account FE	Yes	Yes
Year-month FE	Yes	Yes
Observations	26,375	26,375
R-squared	0.305	0.307

Note: This table provides further robustness checks on the results of Table 5. In both panels, we introduce all the buy transactions of the stock accounts held by the treatment group (local investors in the HPR cities) between 12 weeks before HPR and 12 weeks after HPR. Panel A replicates the regressions in Panels A and B of Table 5. In column (1), $1_{BUY\_REALESTATE2}$, equals 1 if there is at least one buy transaction involves any of the 124 listed real estate developers or 39 listed construction firms in each week; in column (2), the outcome variable is the weekly real estate purchase ratio, which is constructed by using the weekly dollar amount of real estate stock purchases as a fraction of total purchase volume. Panel B replicates the same regression for local stocks. The outcome variable in column (1), $1_{BUY\_LOCAL\_FIRM}$, equals 1 if there is at least one buy transaction involves any listed firms headquartered in the local city in the week. In column (2), the outcome variable is the weekly local firm purchase ratio, which is constructed by using the weekly dollar amount of local stock purchases as a fraction of total purchase volume. 1_{POST} is a dummy indicating the period after the city-specific HPR policy announcement. We also control for account fixed effects and year-month fixed effects in both panels. Please refer to Appendix B for detailed variable definitions. Standard errors are clustered at the branch level. ***, **, and * represent significance levels at 1%, 5%, and 10%, respectively.

Table A.7: Summary Statistics of Listed and Matched Non-Listed Real Estate Firms

	Listed firms		Non-listed firms		difference	t statistics
	Mean	Std. Dev.	Mean	Std. Dev.		
NUM (in 2008 and 2009)	1.24	2.26	1.49	2.95	-0.26	-0.59
TVALUE (in 2008 and 2009)	249.73	375.22	254.32	405.93	-4.60	-0.07
AREA (in 2008 and 2009)	97.78	158.71	131.32	342.71	-33.54	-0.75
SOE	0.43	0.50	0.33	0.47	0.10	1.20
Registered capital; in million yuan.	2041.16	1760.05	2366.63	4955.85	-325.47	-0.53

Note: This table compares the 72 listed real estate firms in mainland China and 72 matched non-listed firms in the residential land buyer dataset. We firstly collect micro-level data of all the residential land transactions in China between 2008 and 2013 from the official website of the Ministry of Land Resources of China and aggregate the transactions by firm buyers. Between 2008 and 2009, there are 72 listed real estate developers that purchased at least one residential land parcel in mainland China. From all the non-listed developers with any residential land purchase in 2008-2009, we adopt the propensity score matching and match 72 non-listed real estate firms with the 72 listed developers, using their average annual total land transaction price in 2008 and 2009 and the registered capital as two predict factors. The results in this table suggest there are no significant differences between the listed developer sample and the matched non-listed developer sample in residential land purchase behaviors in 2008-2009 (including number of land parcels purchased, total value, and total area), ownership structure (whether the firm is an SOE or not), and registered capital.

Appendix B: Data Appendix

(1) Branch-level variables

Weekly number of new accounts is the number of new stock accounts opened in the branch-week.

1_{POST} is a dummy indicating the period after the city-specific HPR policy announcement.

1_{PRE} is a dummy indicating the two weeks before the city-specific HPR policy announcement.

$1_{HIGHHPG}$ is a dummy variable that equals 1 if the 5-year cumulative house price growth rate in 2005-2009 of the city ranks in the top quartile of all HPR cities, calculated based on the constant-quality price index for newly built housing sales as described in detail in Wu, Deng, and Liu (2014).

$1_{HIGHHSD}$ is a dummy that equals 1 if the city ranks in the top quartile of the newly built housing trading volume shrinkage within 3 months after HPR, calculated based on the difference-in-differences model as described in detail in Appendix C.

(2) Account-level variables

Weekly cash inflow is the dollar amount (RMB) of cash inflows to the account in the week.

Female indicates a female investor.

Age is the estimated age of the investor when opening their account. Each investor will choose their age range at the time of the account opening. If the investor is below 20, we use 20 instead. If the investor is above 70, we use 70 instead. For the rest, we use the lower bound of each age range.

Risk seeking is a dummy indicating high risk preference of the investor, which is from the ex-post survey conducted by the company.

$1_{BUY\_REALESTATE}$ is a dummy that equals 1 if there is at least one buy transaction involves any of the 124 listed real estate developers in the week.

$1_{EXPOSED}$ is a dummy that equals 1 if the buy transaction involves any listed real estate developer with ongoing housing projects in any of the HPR cities by the end of 2009.

$1_{NONEXPOSED}$ is a dummy that equals 1 if the buy transaction involves any listed real estate developer without any ongoing housing projects in the HPR cities by the end of 2009.

Weekly res(expo/nonexpo) buy amount/Weekly total buy amount is constructed by using the weekly dollar amount purchase of real estate stocks (exposed stocks/non-exposed stocks) as a fraction of total purchase volume.

$1_{BUY\_REALESTATE2}$ is a dummy that equals 1 if there is at least one buy transaction involves any of the 124 listed real estate developers or 39 listed construction firms in the week.

$1_{BUY\ LOCAL\ FIRM}$ is a dummy that equals 1 if there is at least one buy transaction involves any listed firms headquartered in the local city in the week.

$1_{AGE>\ MEDIAN\ AGE}$ is a dummy that equals 1 if the account holder is beyond the median age of the sample.

$1_{HIGHTRADINGFREQ}$ is a dummy that equals 1 if the account holder is classified as a high-frequency investor according to the company's algorithm.

$1_{HIGHACCTACTIVITY}$ is a dummy that equals 1 if the account holder frequently logs in to her account as recorded by the brokerage company.

$1_{HIGHASSET}$ is a dummy that equals 1 if the outstanding balance of the account ranks in the top 10% of all sampled accounts.

$1_{ACCOUNT\_AGE>MEDIAN}$ is a dummy that equals 1 if the number of years since the account opening is larger than the median value of the sample.

# of stocks held monthly is the average number of stocks held per account per month in the trading period of 12 weeks before and after HPR.

Market value of stock held monthly (thousand RMB*)* is the average market value of stock held by the account per month in the trading period of 12 weeks before and after HPR.

Monthly CAR (%) is the monthly CAR for the stock holding of the account. Specifically, tt is calculated based on the following procedures. First, we compute the daily abnormal return of each stock based on the Fama-French three-factor model. Second, for each stock account, we take average of the daily abnormal return for stocks held in the portfolio, weighted by the dollar amounts of each stock held in the account. Finally, we aggregate the computed daily weighted average of CAR in each account-month as monthly CAR.

(3) Firm-level attributes

Average recommendation level is the company-day level average of all analysts' recommendation levels on the firm. RESSET, a COMPUSTAT-style data vendor in China, collects all analysts' recommendations (5 = *strongly positive*, 1 = *strongly negative*), and calculate the daily average recommendation levels (from 1 to 5) for each listed company.

BUY is a dummy that equals 1 if the firm purchases at least one residential land parcel in the year, and 0 otherwise.

NUM is the number of residential land parcels purchased by the firm in the year.

TVALUE is the total price of residential land parcels purchased by the firm in the year.

AREA is the total land area of residential land parcels purchased by the firm in the year, conditional on any residential land purchase.

PRICE is the average price (in land area) of residential land parcels purchased by the firm in the year, conditional on any residential land purchase.

1_{LISTED} is a dummy that equals 1 for the listed real estate companies, and 0 for the matched non-listed real estate companies.

Appendix C: Effect of HPR on Housing Sales and Prices

In this appendix, we focus on the effect of HPR on the housing markets. We start with the monthly series of total transaction volume (in million RMB) in newly built housing markets in 90 major cities between January 2010 and December 2012, which are reported by local housing authorities in the corresponding cities. These 90 major cities include 41 HPR cities (another 4 HPR cities, Taizhou, Zhuhai, Zhoushan, and Quzhou, are not included) and 49 non-HPR cities.

We adopt the standard difference-in-differences method to investigate how HPR affects housing transactions; that is,

$$\ln(VOLUME_{it}) = \beta \times TREAT_i \times POST_{it} + \theta_t + \gamma_i + \varepsilon_{it}, \quad \text{(C-1)}$$

where $VOLUME_{it}$ represents the total value of newly built transactions in city i in year-month t; $TREAT_i$ equals 1 for the HPR cities, and 0 for the non-HPR cities; $POST_{it}$ equals 1 in city i for the 1-month or 3-month period after the HPR announcement event, 0 in city i for the 12 months before HPR, and null in city i in the month when the policy was released. θ_t refers to the year-month fixed effects; γ_i refers to the city fixed effects; and ε_{it} is the i.i.d. error term. The standard errors are clustered at the city level.

We apply two strategies in selecting the control group. First, we set the 49 non-cities as the control group. One potential problem here is the HPR and non-HPR cities might not be totally comparable. For this reason, we also try a second setting. For each HPR city, we choose other HPR cities as the control group, if the matched cities did not implement HPR between 12 months before the HPR event (i.e., the month when the treated city announced the HPR policy) and 3 months after that. Following this strategy, we could only merge control cities for 21 of the 41 HPR cities.

The results are listed in Panel A, Table C.1. In columns (1) and (2), we only adopt 1 month after HPR, and in columns (3) and (4), we adopt 3 months. Columns (1) and (3) use the first strategy in selecting the control group, and columns (2) and (4) use the second strategy. All results suggest housing transactions significantly drop after HPR. Taking column (1) as the example, according to the coefficient, the housing transaction volumes drop by 32.4% (1-exp (-0.391) =0.324) within 1 month after HPR. On average, the monthly transaction volume of the 41 HPR cities reached 181334 million RMB in 2009, or can be expected to reach about 185,507 million RMB for all the 45 HPR cities (we can have the annual housing sales volume for the other 4 HPR cities). Thus, the coefficient can be converted to a decrease in transaction volume of 185,507*(1-exp (-0.391)) =60,033.75 million RMB for all the 45 HPR cities. Similarly, according to column (3), the total decrease in the 3 months after HPR reaches 185507*(1-exp (-0.335)) *3 =158420.30 million RMB.

In addition, we also try regressions for each city. Most HPR cities witnessed a statistically and economically significant drop in housing transactions after the HPR policy. We use the results to construct the dummy of $I_{HIGHHSD}$ in the heterogeneous analysis.

Table C.1: Effect of HPR on Total Value of Transaction (41 HPR cities)

	(1)	(2)	(3)	(4)
	1 month after HPR		3 months after HPR	
	Control group: HPR cities	Control group: non-HPR cities	Control group: HPR cities	Control group: non-HPR cities
	ln (total value of newly built housing units sold)			
1_{POST}	0.001	0.001	0.007	-0.033
	(0.46)	(0.49)	(0.18)	(-1.00)
$1_{TREAT} \times 1_{POST}$	-0.391***	-0.335***	-0.482***	-0.356**
	(-2.94)	(-3.61)	(-2.99)	(-2.23)
1_{TREAT}	-	-	-0.011	-0.026
	-	-	(-0.67)	(-1.01)
City FE	YES	YES	YES	YES
Year-month FE	YES	YES	YES	YES
Observations	23,901	27,996	5,180	3,696
R-squared	0.785	0.787	0.787	0.780