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Housing market investability and stock market participation☆

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ABSTRACT

We identify a causal crowding-out effect of housing market investment on stock market participation. Using a large sample of individual stock trading data and exploiting China's policy on restricting households from purchasing houses, we find a significant increase in the stock market participation among affected households compared to others. Our results are driven by the substitution channel between the housing and stock market: (1) the treatment policy indeed reduces households' investment in housing, (2) the treatment effect is more pronounced with stricter housing purchase restrictions and higher propensity for real property investment, and (3) households' stock turnover rate decreases.

1. Introduction

Various theoretical models predict that investment in the housing market substitutes for households' participation in the stock market (Grossman and Laroque, 1990; Flavin and Yamashta, 2002; Chetty and Szeidl, 2007). However, empirical evidence on this prediction is mixed: While a group of studies finds a negative association between investment in the housing market and the stock market (Fratantoni, 1998; Cocco, 2005), another group finds the opposite (Heaton and Lucas, 2000; Vestman, 2019). The lack of conclusive evidence is possibly due to the endogeneity problem in the empirical setting and lack of individual transaction data. Existing studies are usually based on annual stock holding of households. Transaction-level data can quickly reveal households' stock trading responses to housing market changes, reducing confounding effects with yearly stock participation updates. This paper reveals a causal crowding-out effect of housing market investability on households' stock market participation and trading behavior, using a quasi-natural housing market experiment and individual stock transaction data.

Our analysis uses city-level policy changes in China that restrict residential property investments. Using a panel of 20,331,702 person-month observations from 2009 to 2013, we find that an exogenous decrease in the investability of the local housing market leads to a significant increase in households' stock market participation. Households in the treated cities experienced an increase in the monthly stock investment by 7.6 % from the sample mean relative to households living elsewhere.

Next, we show that the treatment effect is through the substitution channel. First, we show that the city-level housing purchase restriction leads to a significant decrease in housing market investment, with stronger effect in cities with tighter restriction. Second, we show that our treatment effect is mainly driven by the households aged 40–60, who are likely in the life cycle to exhibit greater pre-existing propensity to invest in the housing market. Third, considering that housing market investment is usually characterized by a long-term investment, if households allocate the money that they would have invested in the housing market into the stock market, we expect and indeed find that households' stock turnover rate decreases following the treatment.

Our paper contributes to the literature on households' portfolio choice by identifying a causal substitution effect of housing market investability on stock market participation. Exploiting the housing price appreciation in Shanghai, Lü et al. (2024) show that stock investors who gained housing wealth trade less actively, take less risk, and spend less effort trading. Their findings also suggest a substitution effect between

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Table 1
Summary Statistics.

Panel A: Full sample					
Variable	mean	sd	p5	p50	p95
Netflow/Holding	−0.131	0.503	−1.045	0.000	0.187
Netflow	−22,177.020	103,064.000	−122,756.500	0.000	10,355.000
BSIM	−0.196	0.543	−1.000	−0.151	1.000
Turnover	1.134	2.912	0.000	0.000	6.822
Buy turnover	0.299	0.736	0.000	0.000	1.839
Sell turnover	0.415	0.960	0.000	0.000	2.485
Freq (buy-sell)	−0.423	1.847	−3.000	0.000	1.000
Turnover (share based)	0.635	1.971	0.000	0.000	4.194
GDP per capita	70,154.480	25,772.640	22,865.000	79,524.000	110,420.700
GDP (in 100 million)	766.072	575.237	85.110	605.920	1919.570
Annual income	50,082.170	16,398.030	26,462.000	49,052.000	85,307.000
Housing price (per m^2)	9116.169	4482.316	2891.830	8501.690	16,553.520
Portfolio value	146,362.500	336,072.600	1738.000	36,600.000	655,724.800
Portfolio return	0.046	0.355	−0.287	−0.003	0.483

Panel B: Differences between treated and control group in the pre-event period

	Treatment		Control		Test of differences	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon test
	(1)	(2)	(3)	(4)	(3) - (1)	(4) - (2)
Age	41.122	39.000	42.472	41.000	83.182	90.450
Gender	0.484	0.000	0.491	0.000	10.056	10.056
Netflow/Holding	−0.222	0.000	−0.187	0.000	66.136	69.524
Netflow	−38,109.953	0.000	−30,208.647	0.000	72.683	72.989
BSIM	−0.168	−0.134	−0.169	−0.131	−1.074	−0.762
Turnover	2.069	0.378	1.736	0.089	−109.642	−158.883
Buy turnover	0.549	0.043	0.466	0.000	−107.255	−148.968
Sell turnover	0.745	0.138	0.632	0.000	−113.863	−153.101
Freq (buy-sell)	−1.065	0.000	−0.993	0.000	19.765	7.276
Turnover (share based)	1.655	0.136	1.533	0.165	−32.551	−20.531
GDP per capita	55,529.310	55,616.000	51,202.460	54,654.000	−261.234	−119.630
GDP	491.406	396.400	594.749	381.460	287.455	−113.897
Wage	37,331.640	35,835.000	38,372.140	34,888.000	106.752	−31.317
Housing price	6913.570	6843.000	6388.080	4499.200	−190.305	−313.978
Portfolio value	152,291.300	41,636.000	150,821.200	42,382.000	−5.085	8.665
Portfolio return	0.103	0.027	0.091	0.025	−31.395	−13.486

Panel A presents the summary statistics of the full sample, which consists of 20,331,702 investor-month observations from April 2009 to March 2010 and from January 2012 to December 2013. Panel B compares investor characteristics between the treated and control groups in the pre-treatment period. All continuous variables are winsorized at the 1st and 99th percentiles.

the real estate and stock market. We complement this study by focusing on the overall housing-market investability, rather than solely on housing price changes.

2. Institutional background and sample construction

China's real estate market has experienced a significant boom since early 2000 due to urbanization and privatization of housing market. To curb excessive speculation in the real estate industry, China's state council issued the "New National 10 Stipulations" on April 17, 2010, which required that commercial banks stop providing mortgages for families purchasing a third or more home in certain cities. Many major cities issued their own regulatory requirements following this, requiring that only local residents are eligible for home purchases and multiple house purchases are strongly discouraged.[1]

Our data is from one of the largest brokerage house in China, with demographic information on investors' gender and age, and detailed information on each trade. Stock market data are from the CSMAR database. Macroeconomic variables and housing transaction information are from CEIC Data.

Our sample consists of 844,688 investors from April 2009 to December 2013. We exclude the period from April 2010 to December 2011, when the implementation of the purchase restriction policy experienced revisions. We also drop months with Spring Festivals, when the stock market was closed. We define the treatment group as investors who reside in a city that implemented the purchase restriction policy, and the rest as control group.

To capture changes in stock market participation, we compute *Netflow* as the difference between buy value and sell value each month. The variables *Netflow/Holding* and *BSIM* (buy and sell imbalance) are computed as *Netflow* normalized by portfolio holding in the previous month and by the sum of buy and sell values, respectively. The variables *Buy turnover* and *Sell turnover* are the buy value and sell value each month divided by portfolio holding value in the previous month, respectively. *Turnover* is the sum of *Buy turnover* and *Sell turnover*.

Table 1 Panel A provides summary statistics. Table 1 Panel B shows that treated investors have more liquidation than purchase of stocks, higher stock trading turnover and stock holding.

3. Empirical results

We implement a standard DiD test through the following regression:

$$Netflow/Holding = \alpha + \beta_1 Treat \times After + Control + Account\ FE + Yearmonth\ FE + \varepsilon. \quad (1)$$

[1] The regulatory landscape continuously evolved throughout the sample period, with various cities implementing additional local measures to mitigate housing market impacts. These local interventions, however, differ in both timing and intensity, introducing potential confounding factors that could blur the observed effects of the primary policy. To maintain a clear focus on the primary policy's impact, we carefully selected our sample period to exclude windows in which such local policies could introduce contamination. Our approach allows us to capture the direct effects of the primary policy.

Table 2
Baseline Regression.

Dep. var.:	(1) Netflow/Holding	(2)	(3)	(4)	(5) Netflow	(6) BSIM	(7) Freq(buy-sell)
After × Treat	0.013**	0.011*	0.012***	0.010***	3575.171***	0.005***	0.173***
	(0.006)	(0.006)	(0.003)	(0.003)	(919.904)	(0.002)	(0.033)
After	0.094***						
	(0.003)						
Treat	−0.035***						
	(0.006)						
GDP per capita				0.302**	−5.61e+04	−0.084	0.325
				(0.138)	(4.61e+04)	(0.098)	(1.507)
GDP				3.786	1.54e+07***	8.161	192.513*
				(10.207)	(3.02e+06)	(7.842)	(115.917)
Wage				−0.561**	−2.77e+05***	0.564**	−3.500
				(0.261)	(7.62e+04)	(0.272)	(3.273)
Housing price				−0.179	2.24e+05	0.102	−1.325
				(1.079)	(2.39e+04)	(0.489)	(8.701)
Portfolio value				0.075***	−1.10e+05***	0.027***	−1.59e+04***
				(0.006)	(5.30e+03)	(0.004)	(0.037)
Portfolio return				−0.038***	−1.29e+04***	−0.024***	−0.231***
				(0.002)	(248.477)	(0.001)	(0.010)
City FE	No	Yes	No	No	No	No	No
Year-month FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Account FE	No	No	Yes	Yes	Yes	Yes	Yes
R-squared	0.008	0.014	0.219	0.220	0.363	0.069	0.245
N	20,331,702	20,331,702	20,331,702	20,331,702	20,331,702	7268,866	20,331,720

This table reports the DiD tests that examine the impacts of housing restriction policies on investors' stock market participation. *Netflow* is the difference between buy value and sell value each month; *Netflow/Holding* is defined as *Netflow* divided by portfolio holding value in the previous month; *BSIM* is defined as *Netflow* divided by the sum of buy and sell values; *Freq(buy-sell)* is calculated as the frequency of buy trades minus that of sell trades. The indicator variable *Treat* takes the value of one if the investor resides in a city that implemented housing restriction policy, and zero otherwise. The indicator variable *After* takes the value of one for the period from January 2011 to December 2012, and zero for the period from April 2009 to March 2010. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by residential cities are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1 %, 5 %, and 10 % levels, respectively.

As shown in Table 2, the coefficients on *After* × *Treat* are positive and significant at the 1 % level across all columns, suggesting a positive effect of housing restriction policies on stock market participation. The economic magnitude is also sizeable. The coefficient on *Treat* × *Post* is 0.01 in column (4) indicates that the housing restriction policy leads to an increase in the monthly stock investment by 7.6 % (=0.01/0.131) from the sample mean. In column (5), we find that the restriction policy causes an increase of 3575 RMB in net inflow into the stock accounts of investors in the treated group.

To address the concern that the main dependent variable may reflect changes in the price of stocks traded around the event period, we scale *Netflow* by the sum of buy and sell values (*BSIM*) in column (6). This variable can help relieve the above concern as the effect of price change is likely to be cancelled out between the numerator and denominator. We continue to find a positive and significant coefficient on *Treat* × *Post*. Furthermore, we examine the frequency of buy orders versus sell orders in column (7). We find that the restriction policy leads to a significant increase in the frequency of buy orders relative to sell orders.[2] These results indicates that our findings are indeed driven by households' greater participation in the stock market.[3]

We further implement the trend analysis by estimating the following regression:

$$Netflow \Big/ Holding = \alpha + \sum_{j=-3}^{-1} \beta_j \; Treat \times Before\_j + \sum_{j=1}^{8} \gamma_j Treat \times After\_j + Control + Account\ FE + Yearmonth\ FE + \varepsilon_{i,t}. \quad (2)$$

Specifically, we replace the indicator *After* in Eq. (1) with eleven new lead and lag indicator variables: *Before_3-Before_1*, and *After_1 –After_7*, with March 2010 as the base.

As plotted in Figure IA1 of the Internet Appendix, the coefficients on all the lead variables are small and statistically insignificant, suggesting parallel trends leading up to the treatment. Also, we find persistent and significant increases in stock market investment for investors in the treated group starting from Month 1 after the implementation of the housing restriction policy, amounting to 1 % increase per month. The absence of any significant lead effects has three noticeable implications. First, the policy of constraining house market investments does not seem to be fully anticipated by households in the treated cities. Second, even if some households anticipated such policy changes, the actual stock trading decisions did not change until the policies took effect. Third, the treatment effect on stock market participation is not due to policymakers merely reacting to past stock trading activities, mitigating any concern about reverse causality.

4. Channel tests

4.1. Evidence from the housing market

Our finding relies on the assumption that the city-level housing purchase restriction is impactful enough to crowd out investors from the housing market to the stock market. In this subsection, we provide direct evidence on this impact.

In Panel A of Table 3, we consider newly built residential properties sold in each city-year from 2006 to 2013. We find that floor space sold

[2] Another variable of interest is the number of newly opened accounts. However, our dataset does not include such information at the city level.

[3] Households in the treatment cities may invest heavily in local stocks (i.e., local bias) that may be directly affected by the restriction policies. To address the possibility that our results are mainly driven by the price change in the local stock, we re-estimate our baseline regression by excluding firms located in the treated cities from the analysis. Our inference is largely unchanged. The results are reported in column (1) of Table IA1 of the Internet Appendix.

Table 3
Channel test: Evidence from the housing market.

	(1)	(2)	(3)	(4)
Panel A: Housing Market				
Dep. var.:	Log floor space sold		Log transaction value	
After × Treat	−0.267***	−0.191***	−0.208***	−0.144***
	(0.056)	(0.052)	(0.059)	(0.053)
Urbanization rate		−2.573***		−3.554***
		(0.903)		(1.062)
Marriage rate		16.912		10.135
		(12.269)		(14.346)
Birth rate		0.103***		0.058
		(0.025)		(0.036)
Second-hand property		0.202***		0.306***
transactions		(0.062)		(0.073)
Other control	Same as column (4) of Table 2			
Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
R-squared	0.922	0.942	0.955	0.967
N	2039	921	1676	801
Panel B: Cross sectional analysis				
Dep. var.: Netflow/ Holding	Property trading volume decline		Total residents / residents with hukou	
After × Treat	−0.026***	−0.038***	−0.001	−0.003
	(0.004)	(0.005)	(0.008)	(0.007)
After × High	0.013**	0.010**	−0.011*	−0.008
	(0.006)	(0.005)	(0.006)	(0.006)
After × High × Treat	0.037***	0.047***	0.017*	0.018**
	(0.009)	(0.006)	(0.009)	(0.008)
Urbanization rate		0.140		0.166
		(0.134)		(0.132)
Marriage rate		3.721		2.875
		(3.119)		(3.969)
Birth rate		0.002*		0.002*
		(0.001)		(0.001)
Second-hand property		−0.009**		−0.011***
transactions		(0.004)		(0.004)
Other control	Same as column (4) of Table 2			
Year-month FE	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes
R-squared	0.22	0.224	0.222	0.224
N	20,331,720	18,288,931	17,782,835	18,285,699

In Panel A, we provide direct evidence that housing restriction policies reduce housing market investability. The dependent variables are the logarithm of floor space sold in columns (1) and (2) and the logarithm of total transaction value in each city-year in columns (3) and (4) for new properties. In Panel B, we examine the heterogeneous treatment effects based on housing market transaction and residence population. The dependent variable is *Netflow/Holding*, defined as the difference between buy value and sell value each month divided by portfolio holding value in the previous month. In columns (1) and (2), *High* is a dummy variable that equals one if the decline in new residential properties sold in the year after the policy compared to the level in the pre-event year is greater than the sample median, and zero otherwise. In columns (3) and (4), *High* is a dummy variable that equals one if the ratio of total residents to residents with hukou in the pre-event year is greater than the sample median, and zero otherwise. In addition to the control variables included in Table 2, we also control for urbanization rate, marriage rate, birth rate, and second-hand property transaction volume in columns (2) and (4). We control for city and year fixed effects in Panel A, and account and year-month fixed effects in Panel B.All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by residential cities are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1 %, 5 %, and 10 % levels, respectively.

and housing transactions experienced a respective decline of 19.1 % and 14.4 % in treated cities after the restriction policy. This finding is consistent with Somerville et al. (2020) and provides direct evidence that the purchase restriction policy indeed reduces housing market investability.

In Panel B of Table 3, we explore the heterogeneous effects of housing purchase restrictions on investors' *Netflow/Holding*. Investors in cities that are more likely to be hit by the housing purchase restriction should be more impacted by the purchase restriction policy. In columns (1)-(2), the *High* dummy equals one if the decline in new residential properties sold in the year after the implementation of the policy compared to the level in the pre-event year is greater than the sample median. In columns (3)-(4), the *High* dummy equals one if the ratio of total residents to residents with hukou in the pre-event year is greater than the sample median. In all columns, we find that coefficients on *After* × *Treat* × *High* are positive and significant, suggesting that investors in cities where the housing purchase restriction is more likely to bind indeed invest more in the stock market.

Overall, we show evidence that the city-level housing restriction leads to a significant decrease in the housing market liquidity and the effect is stronger in cities where the housing purchase restriction is tighter.

4.2. Evidence from household life cycle and investors' stock turnover

Existing literature shows that relatively younger households tend to purchase a property for their own dwelling, while relatively senior households are more likely to purchase a house for investment purposes (Chiuri and Jappelli, 2003; Guisoand Sodini, 2013). Thus, we expect the treatment effect to be stronger for senior households.

In Table 4 Panel A, we implement the DiD test for five age groups, [20, 30), [30, 40), [40, 50), [50, 60), and greater than 60. We find that our treatment effect is mainly driven by investors aged [40, 50) and [50, 60). After the adoption of the housing purchase restriction, investors aged 40 to 50 increased their investment in the stock market by 1 % compared to investors of a similar age in the control group. Stock investment of younger investors, is not affected by the regulation.[4,5]

Housing market investment is usually characterized as a long-term investment. If households allocate into the stock market the money that they would have planned to put in the housing market as a long-term investment, we expect these households' stock turnover to decrease. We examine this in Table 4 Panel B. The dependent variable in column (1) is *Turnover*, and the coefficient on *After* × *Treat* indicates that turnover declines by 7.5 % for investors in the treated group after the regulation. However, if the policy leads to a decline in market prices, it could contribute to the observed negative turnover. To examine this possibility, in column (2) we use share-based turnover rate as the non-price-related turnover measure (i.e., number of shares traded normalized by total shares in the portfolio). The coefficient on *After* × *Treat* is still negative and significant at the 1 % level, indicating that our results are unlikely driven by the changes in stock price.[6] Columns (3) and (4)

[4] The significance of the age groups 40-50 and 50-60 likely reflects the investment behavior of more financially experienced individuals. Households in these age brackets are at the peak of their wealth accumulation and may have a better understanding of how policy changes could impact various industries, particularly those related to retail and real estate. Therefore, one alternative explanation is that they might anticipate a decline in real-estate- and retail-related stocks affected by the housing policy and thus purchase these stocks to capitalize on their expected recovery or undervaluation. This behavior could reflect opportunistic buying rather than simply reallocating resources from real estate to the stock market. As a robustness check, we re-estimate Table 4 Panel A by removing the stocks in the real estate and retail industries, and we show that our inference is largely unchanged (see Table IA2 of the Internet Appendix).

[5] To provide more evidence on the nuanced effects of the policy on different economic groups or regions, we conduct additional cross-sectional analysis. We split the sample based on 2009 urbanization growth rate above or below sample median across different cities in columns (1) and (2) and based on housing price in 2009 above or below sample median across different cities in columns (3) and (4). We show that our treatment effect is stronger for cities with faster urbanization and cities with high housing prices (see Table IA4 of the Internet Appendix).

[6] We re-estimated column (2) by excluding real estate stock with similar results (column (2) Table IA1 of the Internet Appendix).

Table 4
Channel Test: Evidence from the stock market.

Panel A: Stock market participation by age cohorts

	(1)	(2)	(3)	(4)	(5)
Dep. var.: Netflow/Holding	[20, 30)	[30,40)	[40,50)	[50,60)	>60
After × Treat	−0.002	−0.000	0.037***	0.017*	0.003
	(0.006)	(0.003)	(0.005)	(0.009)	(0.004)
R-squared	0.195	0.229	0.252	0.255	0.242
N	1190,334	2928,975	2817,234	1684,463	1011,382

Panel B: Stock turnover

	(1)	(2)	(3)	(4)
Dep. var.:	Turnover	Turnover (share based)	Buy turnover	Sell turnover
After × Treat	−0.075***	−0.220***	−0.033***	−0.042***
	(0.021)	(0.046)	(0.009)	(0.012)
R-squared	0.541	0.407	0.516	0.53
N	20,331,702	20,331,702	20,331,702	20,331,702
Control	Same as column (4) of Table 2			
Year-month FE	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes

In Panel A, we re-estimate our baseline regression (column (4) of Table 2) by different age groups, namely [20, 30), [30, 40), [40, 50), [50, 60), and greater than 60. The dependent variable *Netflow/Holding* is defined as the difference between buy value and sell value each month divided by portfolio holding in the previous month. In Panel B, we examine the effect of the housing restriction polices on households' stock turnover. The dependent variable *Turnover* is defined as the sum of buy value and sell value each month divided by portfolio holding in the previous month. *Turnover (share based)* is defined as the sum of buy shares and sell shares each month divided by portfolio shares held in the previous month. *Buy turnover* and *Sell turnover* are defined as buy value and sell value each month divided by portfolio holding in the previous month, respectively. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by residential cities are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1 %, 5 %, and 10 % levels, respectively.

show that the declines in sell and buy turnover are comparable. Our empirical findings support the view that in response to the housing restriction, households invest more in the stock market with the long-term money that they would have planned to invest in the housing market.[7]

5. Conclusion

Houses are the most important assets for households in most countries, and the investability of the housing market has important implications for households' portfolio choices. Possibly due to the endogeneity problem associated with households' investment decisions and the absence of micro-level stock trading data, there is a lack of conclusive empirical evidence on the causal relation between housing market investment and stock market participation. Based on a large data set of household stock trading and a quasi-natural experiment, this paper identifies a causal negative effect of housing market investability on stock market participation. Our findings highlight a substitution effect between housing and stock markets, providing empirical evidence of a crowding-out effect where housing investments decrease stock market participation.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econlet.2025.112219.

Data availability

The data that has been used is confidential.

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[7] A related question is: Do investors shift their trading targets between local and non-local stocks following the shock? Table IA3 of the Internet Appendix shows no significant evidence that investors shift stock trading between local and non-local stocks.