Housing Wealth and Labor Supply of Elderly Workers: Evidence from China

A thesis submitted by

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Abstract

This thesis examines the impact of housing wealth on labor supply decisions among elderly workers in China. It leverages the quasi-natural experiment created by the 2016-2017 House Purchase Restrictions (HPR) and an instrumental variable (IV) strategy using regional average housing wealth as an instrument. The IV regression reveals that increased housing wealth negatively affects labor supply by incentivizing earlier retirement. In contrast, the difference-in-differences (DiD) analysis shows that exogenous house price increases due to HPR spillovers lead to higher labor force participation and delayed retirement among elderly workers. Within the context of the widespread practice in China of parents subsidizing their children's home purchases, I identify a significant intergenerational transfer motive. I argue that this motive can reconcile the observed findings: parents delay retirement to financially support their children in purchasing homes, especially when rising house prices impose greater burdens on the younger generation. These results suggest that while housing wealth reduces labor supply, the housing boom influences elderly workers' labor market behavior through the intergenerational transfer channel. Its role in shaping retirement trends warrants further exploration.

Acknowledgment

I would like to extend my deepest gratitude to my thesis advisor, Prof. Yannis Ioannides, whose patient guidance, insightful feedback, and unwavering support made this work possible. I am also profoundly grateful to Prof. Jeffrey Zabel for his thoughtful comments and constructive suggestions, which greatly enhanced both the rigor and clarity of my analysis. My heartfelt thanks go to my parents for their generous financial support and constant encouragement throughout my studies at Tufts University. I wish to acknowledge the Henken Family, whose Graduate Research Fund provided the financial support necessary to purchase data for the empirical work in this thesis.

Tomori Takamatsu once said:「筋書きのない日々を生きてく僕らは」(We who live our days without a script...)[1]. Two years ago, I would never have imagined myself completing this thesis and experiencing the two most extraordinary years of my life at Tufts. I am grateful to Prof. Alan Finkelstein Shapiro and Prof. Chaewon Baek, whose macroeconomics courses greatly enhanced my understanding of the field. I also thank Prof. Douglas Gollin, who introduced me to the exciting area of Macro Development. Additionally, my sincere appreciation goes to my classmates and friends for their support. Special thanks to Jing Liu, Xinyu Wang, Shuaibo Yin, Ke Jiang, Yiheng You, Wenwei Liang, and Yulong Liu, for their valuable suggestions and emotional support, especially during moments of frustration.

Finally, I wish to thank UberEats. Without their food delivery service during countless coding sessions and late-night writing marathons, this thesis would have been impossible.

[1]Episode 13, BanG Dream! Ave Mujica

Contents

List of Tables

List of Figures

1 Introduction

In contrast to OECD countries, a significantly higher proportion of Chinese workers exit the labor market before reaching the pensionable age (Giles et al., 2023; OECD, 2015). Since the 1990s, the average labor market exit age in China has been declining (Giles et al., 2011). Compared to other Asian developing countries, China's retirement patterns are also distinct. While other Asian countries have seen rising labor market exit ages, China has experienced a decline.

Many studies in advanced economies suggest that factors such as improved worker health, rising educational attainment, and a growing proportion of white-collar jobs delay retirement, beyond the effects of pension reforms (Banerjee and Blau, 2016; Blau and Goodstein, 2007, 2010). These factors are also present in China, yet the average labor market exit age continues to decline, suggesting additional forces are at play. A popular hypothesis is that rapidly rising house prices, by increasing the housing wealth of elderly workers, incentivize earlier retirement. This paper aims to test this hypothesis.

Extensive research exists on China's housing bubble. Between 2004 and 2015, real land prices in 35 large Chinese cities increased nearly fivefold (Wu et al., 2016). Numerous studies argue that this rapid appreciation reflects a bubble. For example, Rogoff and Yang (2020) present evidence such as high vacancy rates and misaligned supply-demand dynamics to support this claim. Other studies suggest that fundamental factors alone cannot explain China's housing prices and highlight significant risks in the real estate market (Han et al., 2018; Mao and Shen, 2019; Wu et al., 2016).

While the impact of housing wealth on labor supply has been widely studied in the United States (Begley and Chan, 2018; Farnham and Sevak, 2016; Sánchez-Marcos and Low, 2022; Zhao, 2018; Zhao and Burge, 2017), findings in China are mixed. Some studies argue that housing wealth primarily affects the intensive margin of labor supply (Jiang et al., 2022), while others suggest it primarily impacts the extensive margin (Chen et al., 2023; Li and Xiao, 2020). However, limited research exists on how housing wealth influences the retirement decisions of older workers in China.

This thesis first employs an instrumental variable (IV) approach to examine the causal effect of housing wealth on labor supply decisions among elderly workers. Specifically, the average housing wealth of individuals within the same region serves as an instrument for individual housing wealth. This approach addresses the endogeneity of housing wealth, providing a robust framework for identifying causal relationships between housing wealth and labor market behavior.

Building on this analysis, the thesis leverages house purchase restriction (HPR) policies implemented in 2016 and 2017 in China as a quasi-natural experiment. These policies, introduced to curb speculative demand in the real estate market, restricted families from purchasing additional properties beyond their primary residence. Consequently, house price growth slowed in regulated cities while speculative capital drove abnormal price increases in nearby unregulated cities. This divergence creates a natural experiment, allowing me to study how housing appreciation influences older workers' decisions to reduce or exit labor market participation.

My findings from IV regressions suggest that housing wealth negatively impacts elderly workers' labor supply, leading to lower labor force participation and earlier retirement. However, evidence from HPR indicates that in cities experiencing exogenous house price growth due to HPR spillovers, the treatment effect increases elderly workers' labor force participation and delays retirement. I find a strong (but not causal) evidence of intergenerational transfer/bequest motive, consistent with the widespread practice of Chinese parents subsidizing their children's home purchases. Consequently, when local housing prices rise, although the increased housing wealth of elderly workers might incentivize earlier retirement, the higher housing prices make it more difficult for the younger generation to afford homes. This, in turn, provides elderly workers with a stronger incentive to increase their labor supply or delay retirement to support their children. Therefore, the housing boom is not the primary reason for the declining average labor market exit age among Chinese elderly workers.

Related Literature This paper contributes to the literature on the impact of housing wealth on labor supply (Begley and Chan, 2018; Chen et al., 2023; Farnham and Sevak, 2016; Jiang et al., 2022; Li and Xiao, 2020; Sánchez-Marcos and Low, 2022; Zhao, 2018; Zhao and Burge, 2017). It also relates to studies on HPR policies (Chen et al., 2024; Deng et al., 2022; Liu and Zhang, 2024; Sha et al., 2024; Tian and Wang, 2022).

2 Background

2.1 House Purchase Restriction

To address the rapid surge in urban housing prices and the overheating of real estate markets, the Chinese government implemented House Purchase Restrictions

(HPR) in late 2016 and early 2017[2]. These measures were targeted at major Tier-1 cities, such as Beijing and Shanghai, and some Tier-2 cities, aiming to suppress speculative demand and stabilize housing markets. The policies introduced several restrictions, including raising down payment requirements, increasing mortgage rates for second or third properties, and capping the number of homes that individuals or families could purchase. In many cases, households were prohibited from buying additional properties once they owned two or three units. These regulations reflected the government's effort to curb speculation-driven demand, which had been a significant factor in driving up housing prices.

The HPR was highly effective in achieving their primary goal of cooling down regulated housing markets. The growth rate of house prices in these cities dropped sharply within months of the policy implementation. However, while the restrictions succeeded in curbing local speculation, they also gave rise to unintended consequences that reshaped nearby unregulated housing markets.

Specifically, the restrictions caused a spillover effect, redirecting speculative capital from regulated cities to unregulated ones (Deng et al., 2022). Investors seeking alternative opportunities began purchasing properties in cities located near the regulated urban centers, where housing markets remained unrestricted. These spillovers led to substantial increases in house prices in these unregulated cities, despite the absence of improvements in local economic conditions. The geographical proximity of these unregulated cities to regulated ones played a key role, as it allowed investors to easily gather information and manage their newly acquired properties (Tian and Wang, 2022).

[2]For example, in Hangzhou:https://www.hangzhou.gov.cn/art/2016/9/18/art_1229063383_1721345.html

2.2 Intergenerational Transfer

Intergenerational financial transfers in China have been widely observed and discussed in both media and academic research. A 2017 survey highlighted in the *South China Morning Post* found that seven out of ten parents in Hong Kong would provide financial assistance to help their children purchase a home, reflecting a widespread willingness among parents to support their children's housing needs.[3] Similarly, *Business Insider* notes that in China, most millennials rely heavily on financial support from their families to afford housing, a pattern that contrasts with generational dynamics in Western countries.[4] Additionally, a 2024 report by *The Diplomat* underscores how rising housing prices have made it nearly impossible for young adults in China to buy homes without parental assistance, further emphasizing the critical role of intergenerational financial transfers in facilitating homeownership.[5]

Extensive research has explored intergenerational financial transfers in China. Li (2010) uses household survey data from Guangzhou and Shanghai, finding that 58.1% and 31.6% of the younger generation in these cities, respectively, received financial support from their families when purchasing houses. Similarly, Zhang and He (2024) show that in China, the socioeconomic status of parents plays a more significant role than that of young homeowners in determining multifaceted

[3]South China Morning Post. 2017. "Would you buy your children a flat in Hong Kong? Seven out of 10 parents would." Available at: https://www.scmp.com/business/money/wealth/article/2119076/would-you-buy-your-children-flat-hong-kong-seven-out-10

[4]Business Insider. 2021. "America's Housing Crisis Is Generational. China's Would Be Familial." Available at: https://www.businessinsider.com/america-china-housing-market-crisis-comparison-generation-families-2021-11

[5]The Diplomat. 2024. "China's Real Estate Crisis: Why the Younger Generation Is Not Buying Houses Anymore." Available at: https://thediplomat.com/2024/12/chinas-real-estate-crisis-why-the-younger-generation-is-not-buying-houses-anymore

housing qualities for the younger generation.

Some studies suggest that this financial support reflects an exchange relationship, where parents provide material assistance in return for care and/or emotional support (Li and Shin, 2013; Tang and Wang, 2022; Xu, 2024). Other research highlights cultural and economic factors, arguing that homeownership enhances the younger generation's competitiveness in the marriage market, thus incentivizing parents to provide financial assistance (Du and Wei, 2013; Wei and Zhang, 2011; Wei et al., 2017). These findings underscore both the cultural and economic motivations driving parental financial support in China, aligning with the assumptions underlying this analysis.

3 Empirical Strategy

3.1 Instrumental Variable Estimation

This paper employs an instrumental variable (IV) approach to address the endogeneity of housing wealth in evaluating its impact on labor supply decisions among elderly workers. While housing wealth can influence labor supply decisions, it may also be shaped by an individual's labor supply, resulting in a bidirectional relationship. To address this issue, I use the average housing wealth of individuals living in the same district as an instrument for individual housing wealth, following methodologies similar to Fu et al. (2016) and Chung (2022). This approach is intuitively appealing, as a respondent’s housing wealth is likely correlated with their neighbors’ housing wealth, while the neighbors’ housing values are unlikely to be directly influenced by the respondent’s labor decisions, such as labor force participation (LFP). Note that renters were excluded from the sample. This strategy captures exogenous variation in housing wealth that is plausibly independent of an individual's labor supply behavior.

The analysis utilizes survey data from the China Family Panel Studies (CFPS), a nationally representative longitudinal survey [6]. CFPS covers diverse topics such as family structure, migration, event history, and labor market outcomes, with integrated modules for both rural and urban respondents. The baseline survey was conducted in 2010, followed by biennial follow-up surveys. For this study, I use all six waves of data from 2010 to 2020, which include information on individual characteristics, housing wealth, and labor supply outcomes such as labor force participation, retirement status, and weekly working hours.

The sample is restricted to individuals aged 45 and above who reside in urban areas. Table 1 presents the definition of variables. Table 2 presents the descriptive statistics for the key variables used in the analysis.

The regression equation is as follows:

$$Y_{i,j,t} = \beta \times house\_net\_asset_{i,t} + \Gamma X_{i,t} + \eta_j + \alpha_i + \delta_t + \epsilon_{i,t}. \tag{1}$$

where $Y_{i,t}$ represents labor outcomes, including retirement, labor force participation (LFP), and weekly working hours. $house\_net\_asset$ is the main variable of interest, measured in ten thousand RMB. $X_{i,t}$ denotes a series of control variables, including age, age squared, gender, number of children, non-housing net assets, health status, individual income. η_j captures local macroeconomic fundamentals

[6]The data are from China Family Panel Studies (CFPS), funded by Peking University and the Nationl Natural Science Foundation of China. The CFPS is maintained by the Institute of Social Science Survey of Peking University

Variable	Definition
retire	=1 if the respondent is retired.
lfp	=1 if the respondent is in the labor force.
hours	Average weekly hours worked over the past 12 months.
employ	=1 if the respondent is currently employed.
House Net Asset /10,000	Net value of housing assets (10 k CNY).
age	Respondent’s age (years).
sex	=1 if male, 0 if female.
child_number	Total number of children.
non-House Net Asset /10,000	Net value of non-housing assets (10 k CNY).
health	Self-reported health on a 1 (exellent) – 5 (poor) scale.
Income /10,000	Annual personal income (10 k CNY).
marriage	=1 if unmarried, =2 married or cohabiting.
ue	Natural log of city-level unemployment rate.
GDP (ln)	Natural log of city-level real GDP per capita.
edu	Categorical education level (1 = no schooling ⋯8 = PhD).

Table 1: Definition of Variables

Variable	Mean	Std. Dev.	Min	Max
retire	0.192	0.394	0	1
lfp	0.941	0.235	0	1
hours	44.295	22.436	0.1	100
employ	0.934	0.249	0	1
House Net Asset	33.808	56.440	-218.579	2479.407
child_number	1.887	0.973	0	8
Age	54.935	7.781	45	90
marriage	0.918	0.274	0	1
edu	2.580	1.300	1	7
Health	2.999	1.224	1	5
Income	3.874	5.334	0	128.096
Non-House Net Asset	11.858	54.776	-106.260	4108.814
ue	1.125	0.233	0.262	1.656
GDP (ln)	10.657	0.449	9.077	11.895

Table 2: Descriptive Statistics

such as the unemployment rate and GDP per capita, α_i represents individual fixed effects, and δ_t accounts for time fixed effects.

Table 3 presents the regression results. From Table 3 , I observe that increased net housing wealth has a statistically significant negative impact on labor force participation. Specifically, a 10% increase in net housing value reduces LFP by 0.15%. This implies that increase the net housing value would by one standard deviation, on average, decrease LFP by 2.56%.

3.2 Home Purchase Restrictions

I use city-level data on house prices and GDP to conduct a pre-trend analysis. Monthly house price data is sourced from Wind[7]—a financial data provider comparable to

[7]Wind: https://www.wind.com.cn/portal/en/EDB/index.html

Table 3: Regression Results: Instrumental Variable

VARIABLES	(1) retire	(2) lfp	(3) hours	(4) retire	(5) lfp	(6) hours
house_net_asset	-5.55e-05*	-0.000131***	-4.66e-05	-2.66e-05	-0.000458***	-0.00688
	(3.30e-05)	(4.00e-05)	(0.00351)	(0.000111)	(0.000134)	(0.0158)
Controls	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Instrumental Variable				✓	✓	✓
Observations	14,828	14,519	5,467	14,828	14,519	5,467
R-squared	0.805	0.727	0.684	0.002	-0.002	0.005

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Bloomberg in the U.S. and Anjuke—a Chinese platform comparable to Zillow.[8] Meanwhile, city-level GDP data is obtained from the National Bureau of Statistics of China and Chinese Statistical Yearbook. Additionally, I incorporate survey data from the China Family Panel Studies (CFPS). The sample is restricted to individuals aged 45 and above who reside in urban areas.

I begin by identifying regulated cities and categorizing cities into treatment and control groups. Figure 1 illustrates the locations of these cities, 4 is the list and implementation time of the HPR cities.

Table 4: HPR City List

City	**Implementation**
Langfang	2016.4.2
Xiamen	2016.8.31

to be continued

[8]House prices data from Wind includes 100 cities in China, which is the best monthly dataset available to us. Although other datasets encompass more cities, most are not panel data, meaning that many cities' house prices are only incorporated into the dataset as time progresses. Deng et al. (2022) uses data from CityRE, a superior dataset for house prices, but I do not have access to it.

City	Implementation
Hangzhou	2016.9.19
Nanjing	2016.9.26
Beijing	2016.9.30
Chengdu	2016.10.1
Guangzhou	2016.10.1
Sanya	2016.10.1
Tianjin	2016.10.1
Zhengzhou	2016.10.1
Hefei	2016.10.2
Jinan	2016.10.2
Wuxi	2016.10.2
Wuhan	2016.10.3
Shenzhen	2016.10.4
Suzhou	2016.10.4
Fuzhou	2016.10.6
Zhuhai	2016.10.6
Dongguan	2016.10.7
Foshan	2016.10.8
Nanchang	2016.10.8
Shanghai	2016.10.19
Jiaxin	2016.12.3

to be continued

City	Implementation
Xi'an	2017.1.1
Ganzhou	2017.3.14
Qingdao	2017.3.16
Shijiazhuang	2017.3.17
Changsha	2017.3.18
Baoding	2017.3.19
Cangzhou	2017.3.23
Zhongshan	2017.3.26
Lanzhou	2017.4.7
Haikou	2017.4.14
Tangshan	2017.4.14
Qinghuangdao	2017.4.15
Quanzhou	2017.4.17
Jiangmen	2017.4.22
Ningbo	2017.4.24
Nanning	2017.5.26
Zhangjiakou	2017.5.26
Huai'an	2017.5.30

I first employ the following regression to trace out the spatial extent of the HPR shock’ s spillover on city housing prices:

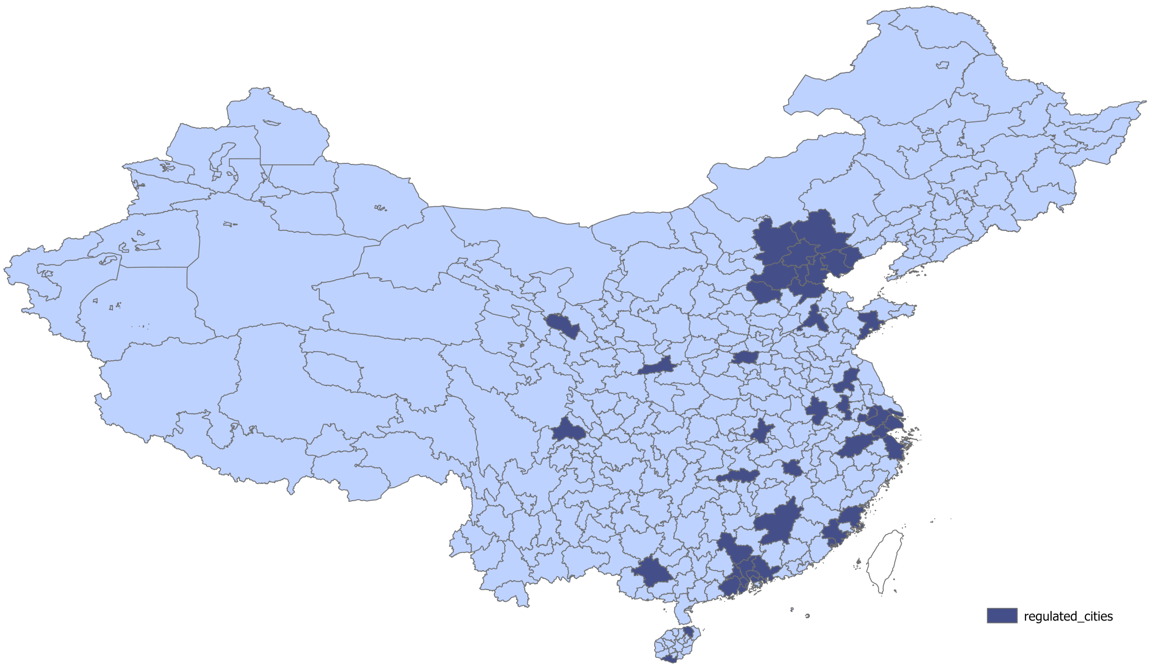

Figure 1: Regulated Cities

Hong Kong, Macau and Taiwan are not included in the analysis

$$
\begin{aligned}
\ln(\text{price}_{jt}) = \alpha &+ \beta_{0\_50}\left(\text{Post}_t \times \mathbf{1}\{0 < d_j \leq 50\}\right) + \beta_{50\_100}\left(\text{Post}_t \times \mathbf{1}\{50 < d_j \leq 100\}\right) \\
&+ \beta_{100\_150}\left(\text{Post}_t \times \mathbf{1}\{100 < d_j \leq 150\}\right) + \beta_{150\_200}\left(\text{Post}_t \times \mathbf{1}\{150 < d_j \leq 200\}\right) \\
&+ \beta_{200\_300}\left(\text{Post}_t \times \mathbf{1}\{200 < d_j \leq 300\}\right) + \beta_{300\_500}\left(\text{Post}_t \times \mathbf{1}\{300 < d_j \leq 500\}\right) \\
&+ \mu_j + \delta_t + \varepsilon_{jt},
\end{aligned} \tag{2}
$$

where the dependent variable $ln(\text{price\_}it)$ is the natural logarithm of the average housing price in city j at time t. I control for unobserved, time-invariant heterogeneity across cities via city fixed effects μ_j and for common shocks over time via year fixed effects δ_t. I partition cities into distance bins $\{0, 50, 100, 150, 200, 300, 500\}$ km from the nearest regulated city and take those beyond 500 km as the omitted baseline. The coefficients $\beta_{k\text{–}1\_k}$ measure the postpre change in log-prices for each bin. Table 5 reports that all bins up to 300 km exhibit statistically significant price increases following the policy shock, with magnitudes that decay monotonically in distance, while the 300500 km bin is indistinguishable from zero. This spatial decay of spillover effects strongly corroborates the quasi-experimental identification. So I designate cities that are over 300km as the control group and cities within 300km as treatment groups.

Compared to the 250km boundary adopted by Deng et al. (2022) and the 200km threshold used by Tian and Wang (2022), my specification extends the distance bins so that cities beyond 300km serve as a truly unaffected control group. For additional robustness, I reclassify the spillover and control groups using alternative distance cutoffs in robustness checks section.

Table 5: Housing Price Spillovers Estimation

VARIABLES	$\ln(\text{price})$
Post × Bin (0–50 km)	0.3869***
	(0.0483)
Post × Bin (50–100 km)	0.1746***
	(0.0539)
Post × Bin (100–150 km)	0.1534***
	(0.0524)
Post × Bin (150–200 km)	0.1311**
	(0.0540)
Post × Bin (200–300 km)	0.1102**
	(0.0540)
Post × Bin (300–500 km)	0.0255
	(0.0581)
Constant	3.7494***
	(0.0123)
Observations	2,605
R-squared	0.8293
City FE	✓
Time FE	✓

Robust standard errors in parentheses, clustered by city_id.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figures 2 and 3 presents the log difference in average house prices between treatment and control group cities. The dataset is from Anjuke, a real estate sales and research platform, which provides a comprehensive monthly series of city-level house prices. Although this dataset is not perfect—it is unbalanced with many cities joining as time proceeds—Figure 2 shows the log difference in house prices without any adjustments, indicating a roughly 20% jump before and after the implementation of HPR. To ensure that the jump is not driven by newly entering cities, in Figure 3 I restrict the sample to cities that entered the dataset before April 2014. Figure 3 then shows that the estimated jump is approximately 10%. In conclusion, given this classification of treatment and control groups and the best available house prices dataset, the magnitude of the house price shock is estimated to be around 10% to 20%[9]. Despite the figures' imperfections, when combined with the results of equation (2), we can be confident that a positive house-price shock occurred in areas proximate to regulated cities.

The regression equation is specified as follows:

$$Y_{i,t} = \beta \times \text{Treat}_i \times \text{Post}_t + \theta \times \text{Regulated}_i \times \text{Post}_t + \Gamma X_{i,t} + \eta_{j,t} + \alpha_i + \delta_t + \epsilon_{i,t} \quad (3)$$

where $X_{i,t}$ represents individual time-varying controls, including marital status, education, number of offspring, health status, income, non-housing assets, and age. α_i denotes individual-level fixed effects, and δ_t represents time fixed effects. The

[9]Depending on the dataset and the classification of treatment and control groups, the magnitude of this jump varies; in Deng et al. (2022) and Tian and Wang (2022), it is approximately 30%. Because my specification classifies cities up to 300km from a regulated city as "treated" —thereby including more distant locations that experience attenuated spillovers—the estimated price jump is naturally smaller, in line with the spatial decay of the effect.

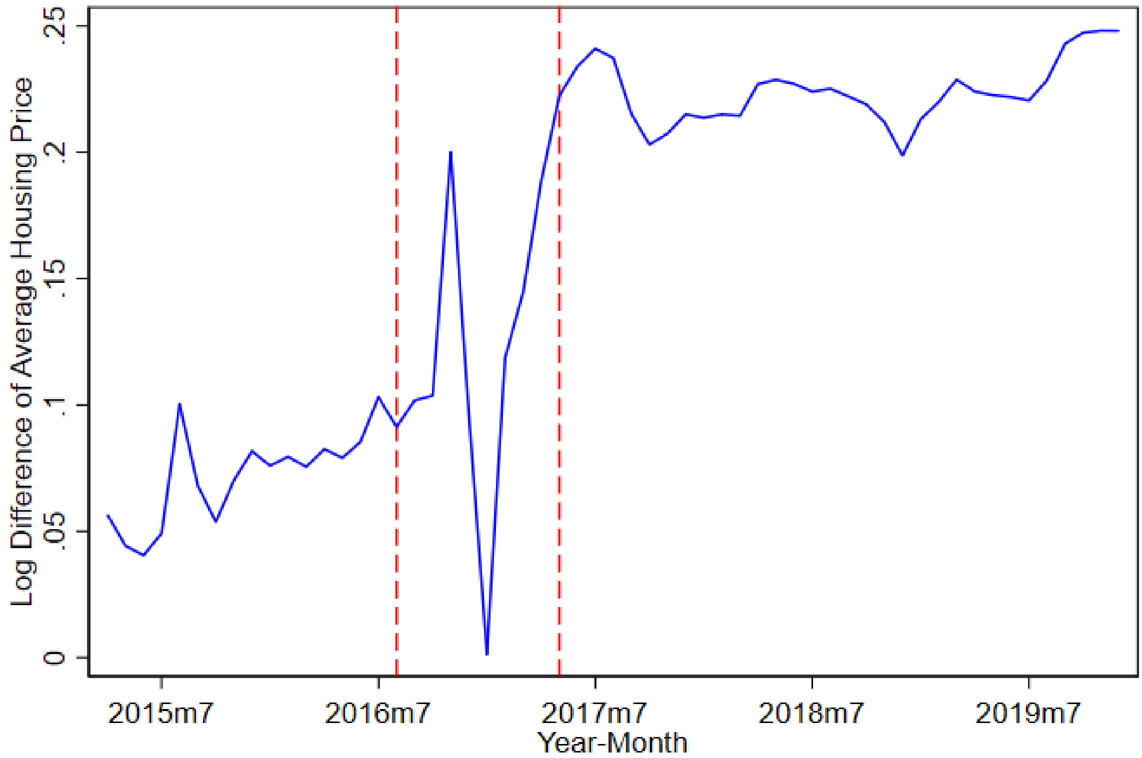

Figure 2: Log Difference of House Prices between Treatment and Control Group Cities: Anjuke 1

The fluctuation of house prices at the end of 2016 is partially induced by newly entering cities. However, the significant jump in the first half of 2017 is unlikely to be caused by new entries, as very few cities joined the dataset during that period. I do not intend to display a longer pre-treatment period because around 34% of the cities joined the dataset in April 2015, resulting in large fluctuations that are not meaningful and would distract from my focus.

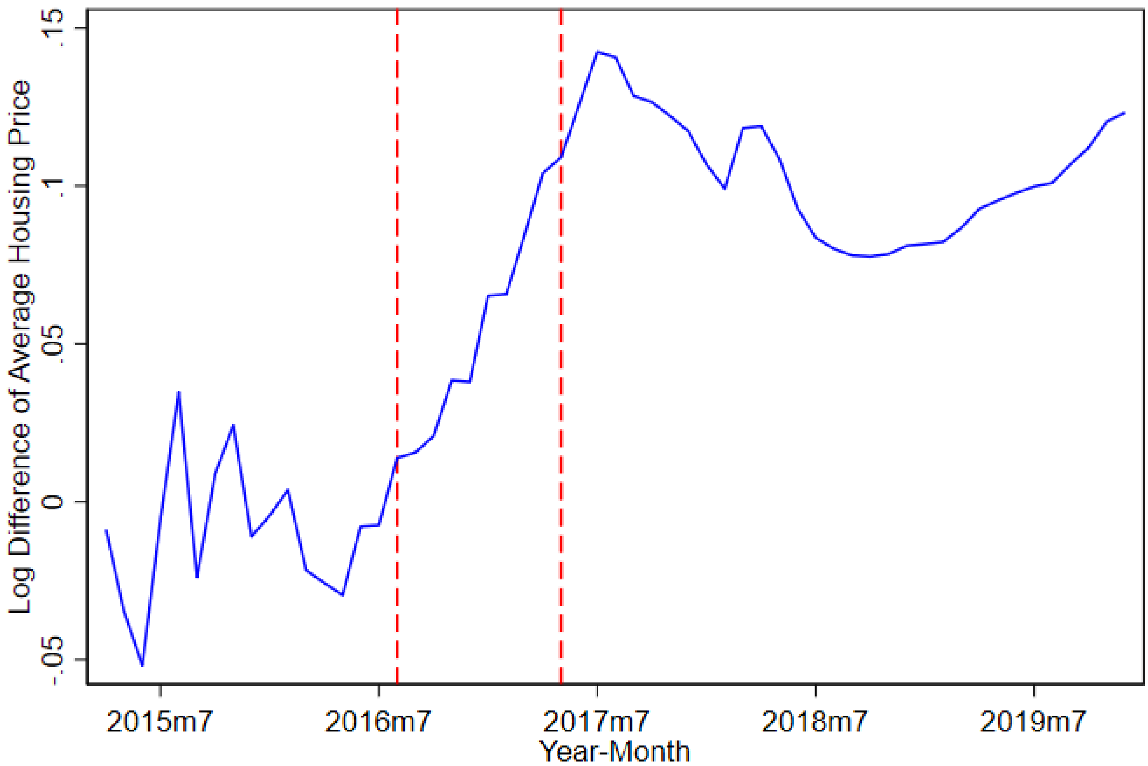

Figure 3: Log Difference of House Prices between Treatment and Control Group Cities: Anjuke 2

In this figure, I only retain cities that joined the dataset before April 2014, which provides us with two year of pre-treatment period unaffected by newly entering cities.

Table 6: Regression Results: DiD

VARIABLES	(1) retire	(2) lfp	(3) hours
post_treated	-0.0537***	0.0530**	-0.814
	(0.0174)	(0.0224)	(2.183)
post_regulated	-0.0126	-0.00644	2.158
	(0.0180)	(0.0254)	(2.217)
Controls	✓	✓	✓
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	22,149	21,394	6,833
R-squared	0.826	0.717	0.670

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

dependent variables are retirement status, labor force participation, and weekly working hours. I report standard errors clustered at the city-year levels. The main interest is on coefficients β is of $\text{Treat}_i \times \text{Post}_t$, where $\text{Post}_t = 1$ iff $t \geq 2018$ and $\text{Treat}_i = 1$ for HPR-spillover cities, θ is $\text{Regulate}_i \times \text{Post}_t$, with $\text{Regulate}_i = 1$ for directly regulated cities.

Table 6 presents the regression results, which differ from the findings of the earlier IV regression. Respondents in cities affected by HPR spillover effects increased their labor force participation and delayed retirement. At the same time, it is worth noting that in cities where HPR was directly implemented, respondents' labor-supply decisions did not change significantly.

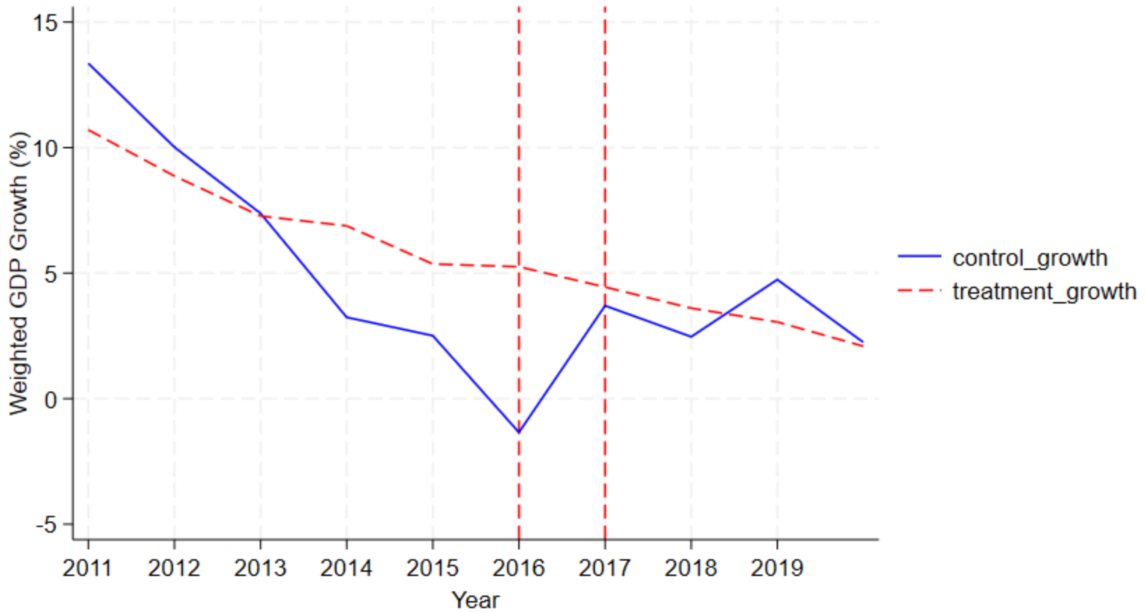

Figure 4: Output Growth in Treatment and Control Cities

3.3 Discussion

3.3.1 Region-Specific Shocks

One concern is that the observed treatment effect may be driven by region-specific shocks in macroeconomic fundamentals. To investigate this, I use annual city-level Gross Domestic Product (GDP) growth rates[10] to compare trends between treatment and control cities. In Figure 4 I can see that in 2016 and 2017, control cities experienced a positive shock in growth rates, which could have influenced their labor markets and, consequently, the labor supply of elderly workers. However, I believe that this effect does not alter the sign of the estimated treatment effect.

I explain the intuition using a simple Cobb-Douglas production function as follows. An increase in output may result from higher total factor productivity (TFP)

[10]Data are sourced from the China Statistical Yearbook and adjusted for city scale

or increased capital; in either case, labor demand would rise, as also supported by previous research (Cardenas and Bernal, 2003; Coile, 2016; Hunter and Gray, 2012; Winda et al., 2023). In this scenario, residents of control cities, benefiting from improved labor market conditions, may have increased their labor supply, potentially causing my estimator to be downward biased. Nonetheless, my findings indicate that elderly workers in treatment cities exhibit higher labor force participation than those in control cities. Therefore, even if the unbiased true estimator were larger, the sign of my estimated effect would remain unchanged. Additionally, I include city GDP as a control variable to mitigate its potential influence on my estimation.

Another possibility, under the Cobb-Douglas production function, is that labor supply in control cities could increase due to migration. Specifically, if workers move from treatment cities to control cities, those remaining in the treatment cities would experience a tighter labor market, encouraging them to remain in or re-enter the workforce. Conversely, residents in the control cities—facing a more competitive or slack labor market—may see older workers exiting the workforce.

To assess whether such migration actually occurred, I examine population data from the China Statistical Yearbook for both groups of cities. The results in Figure 5 indicate that differences in population growth trends between treatment and control cities did not change substantially before and after the HPR implementation, suggesting that migration is unlikely to drive my findings. Furthermore, by controlling for local macroeconomic fundamentals in the regression, I mitigate remaining worries that unobserved shocks or population shifts could account for the observed treatment effect.

Thus, I rule out the possibility that the treatment effect is driven by local macroe-

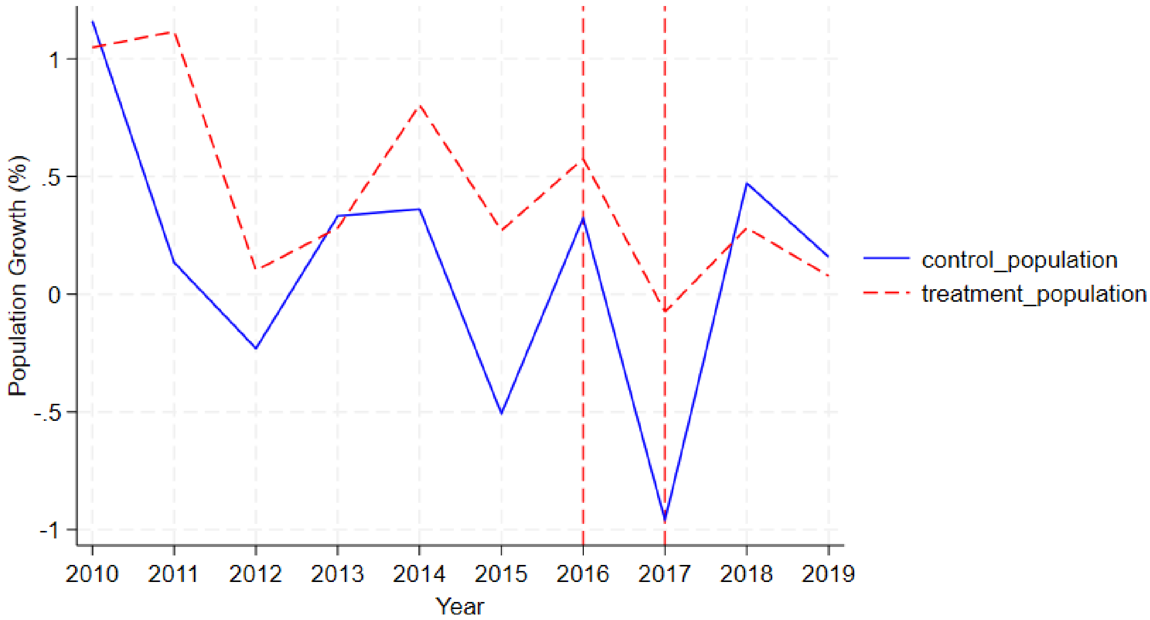

Figure 5: Population Growth in Treatment and Control Cities

conomic shocks or migration.

3.3.2 HPR Sample Cities

Similar to Deng et al. (2022), I exclude HPR events occurring before 2016 or after 2018. In fact, there was a wave of HPR implementations and cancellations between 2010 and 2014, and a few additional cities adopted HPR even after June 2017 (See Table 7 for cities implemented HPR after the second half of 2017, for the full implementation and cancellation history of HPR, there is a comprehensive table in Tian and Wang (2022)). One concern might be that excluding these events could lead to biased estimates. Here, I defend my DiD design.

Table 7: HPR Implementation after June 2017

City	**Implementation**
Jiujiang	2017.7.13
Xiaogan	2017.9.13
Beihai	2017.9.30
Yangzhou	2017.11.30
E'zhou	2017.12.27
Kunming	2018.3.1
Dalian	2018.3.21
Shenyang	2018.4.15
Harbin	2018.5.7
Taiyuan	2018.5.18
Ningde	2018.7.11
Pu'er	2018.7.19
Dandong	2019.4.30

Regarding HPR implementation before 2016, the primary reason for their exclusion is that they did not generate sufficiently exogenous variation in house prices. Using house prices data from Wind (see Figure 6), I observe that after the implementation of HPR in 2010, the difference in house prices between treatment and control groups actually shrinks[11]. This may be due to relatively weak speculative activity in the 2000s, which resulted in minimal spillover effects. This interpreta-

[11]Although the Wind house prices data is not perfect and has representativeness concerns, it is the best available dataset. The dataset from Anjuke starts from 2011

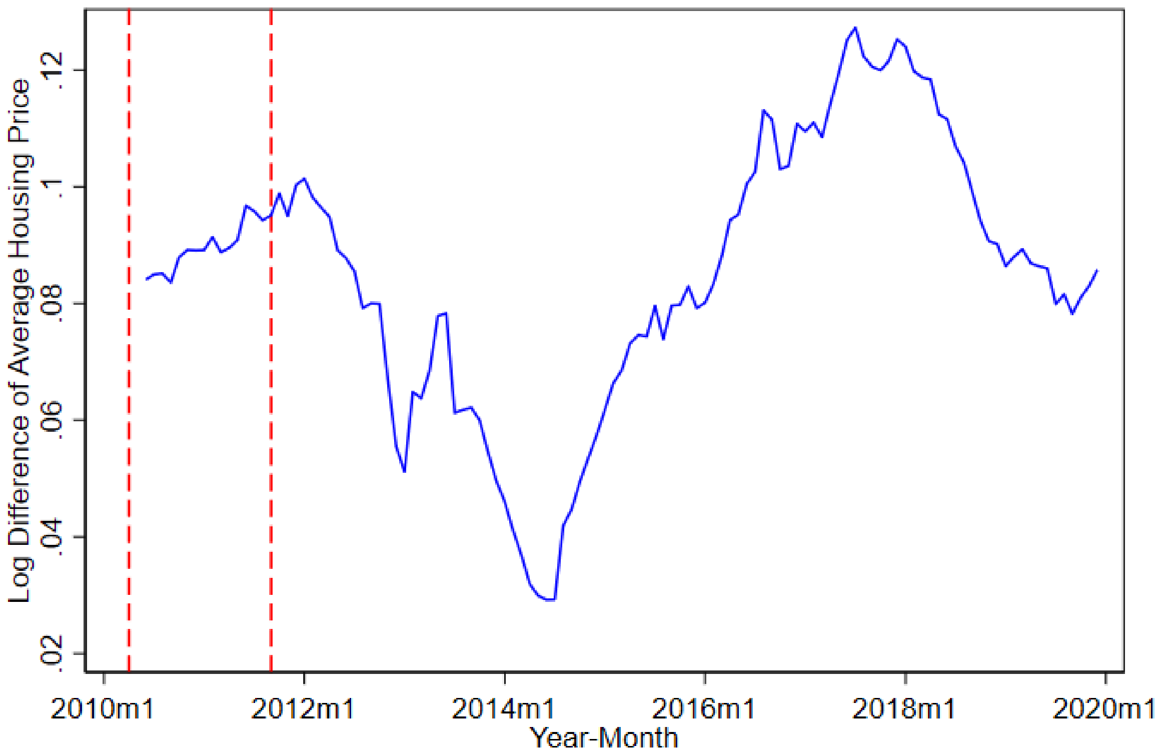

Figure 6: Log Difference of House Prices: Earlier Round of HPR

tion is supported by previous research; for instance, Carlos et al. (2024) argues that migration and income growth primarily explain the rise in house prices during the 2000s, and data from Rogoff and Yang (2020) shows that the proportion of first-time buyers did not decline until 2011. Thus, the initial round of HPR implementation in 2010 coincided with relatively weak speculative activity and limited spillover effects.

A second reason is that incorporating all these early HPR implementation and cancellation dynamics would necessitate a staggered DiD framework. However, one concern with a staggered DiD design is that it may introduce estimation bias when simultaneously comparing treatment versus control groups and early-treated versus late-treated units(Baker et al., 2022). Including all HPR events would leave nearly no clean control group (i.e., never-treated units), potentially compromising the reliability of my estimates.

I do not include all HPR events after 2016 in my baseline regression. Although some cities implemented HPR in the second half of 2017 and in 2018, I only include those events that occurred before June 2017. One reason is that the CFPS survey typically takes place in the second half of the year—for example, in CFPS 2018, interviews began in the fall. I want to allow sufficient time for the policy to take effect, so I restrict my sample to HPR implementations in or before the second quarter of 2017, leaving a one-year window for the policy effects to materialize.

Another concern is that many cities that implemented HPR after 2018 did so in response to spillover effects from the initially regulated cities. In these cases, a surge in house prices triggered policy adoption as a means to cool the market[12]. For instance, in November 2017, Hubei Province urged small cities to prepare for purchase and loan restrictions for non-local residents in areas experiencing surging house prices and high non-local buyer activity[13]. Many of these cities are smaller than the HPR cities in 2016, so the spillover effects of speculation are weaker or even absent, which could bias my estimates. Moreover, these cities had already experienced exogenous house price growth driven by spillover effects, and classifying them as regulated cities could further bias my estimation.

For my baseline regression, I did not classify these cities as regulated. However, to address these concerns, I conducted robustness checks using a staggered DiD

[12] One related example is Qingyuan, a small city that experienced a 70% increase in house prices in May 2017 compared to the same month in the previous year (data from Anjuke), followed by the implementation of a price cap policy (albeit different from HPR).

[13] Source: http://zjt.hubei.gov.cn/zfxxgk/zc/qtzdgkwj/202011/t20201102_2994943.shtml

approach that incorporates all cities with HPR implementations after 2016. Additionally, I constructed a "clean" sample by excluding not only all HPR cities after 2018 but also any cities that are within 300km from them, ensuring that the sample remains unaffected by the staggered implementation of HPR after 2016. I then reran the regression, and the results of these additional analyses are presented later in the robustness check section.

3.3.3 Parallel Trend

The DiD analysis in my study relies on the assumption that the control group and the treatment group should be homogeneous, and that the treatment effect should not be influenced by any pre-existing trend. Therefore, I use an event study approach to investigate the pre-trend and present my results in Figures 7, 8and 9. It can be seen that retirement status passes the pre-trend analysis (PTA) and shows a trend reversal before and after the treatment. LFP and Weekly working hours also pass the PTA, but I cannot completely rule out the possibility that the treatment effect may be driven by a pre-existing trend.

3.4 Intergenerational Transfers

For the intergenerational transfer channel to be valid, both parents and their children must be exposed to the same shock. In other words, even if parents reside in treated cities, they would have no incentive to delay retirement if their children live elsewhere.

According to a report by the Beike Research Institute[14], over 80% of elderly in-

[14] https://aimg8.dlssyht.cn/u/551001/ueditor/file/276/551001/1635127591254809.pdf

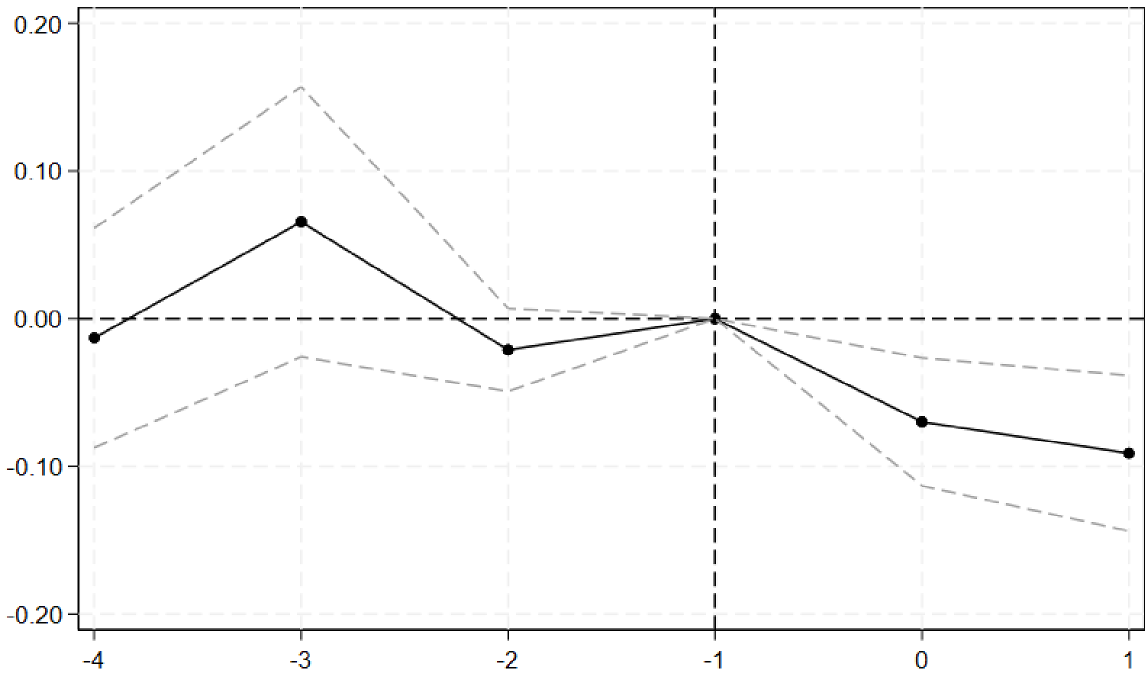

Figure 7: Parallel Trend Analysis: Retirement

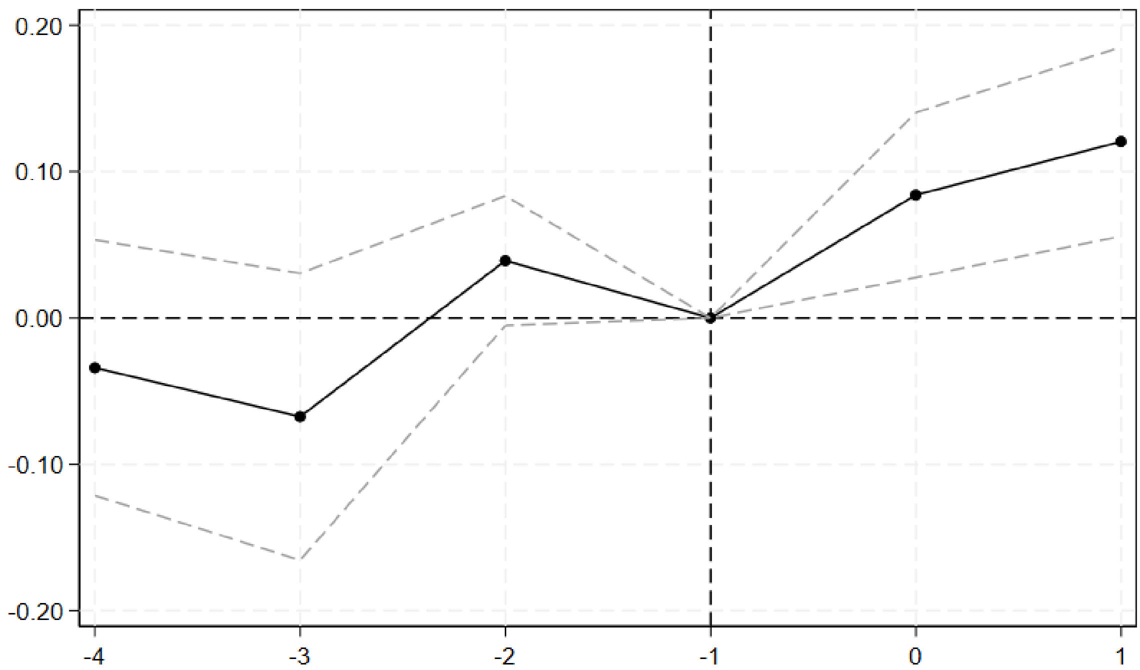

Figure 8: Parallel Trend Analysis: LFP

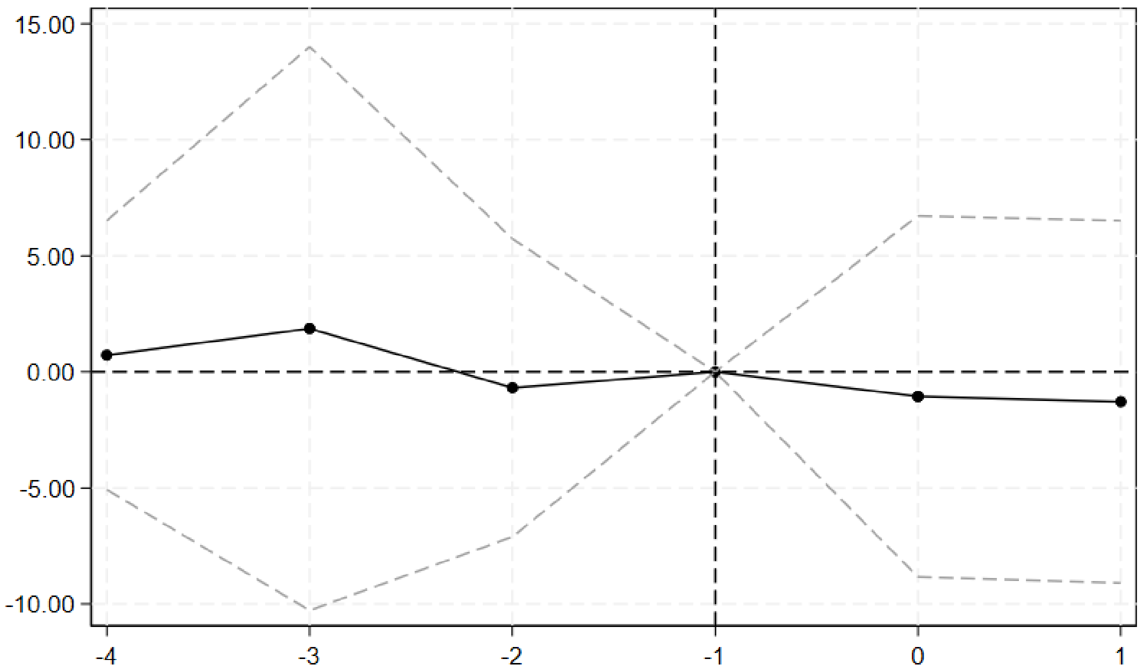

Figure 9: Parallel Trend Analysis: Weekly Working Hours

dividuals[15] live in the same city as their children. CFPS survey data further corroborates this pattern, showing a high prevalence of co-residence: approximately 40% of elderly workers[16] live with their children. These findings provide a foundational basis for the existence of the intergenerational transfer channel.

Following a similar approach to He et al. (2024), I extended the instrumental variable regression by incorporating total number of descendants and its interaction term with housing wealth. Unlike He et al. (2024), who used the number of male descendants as a proxy for intergenerational transfer motives, I used the total number of descendants.

The reason is simple, while Wei et al. (2017) suggests that in regions with im-

[15]Defined in the report as those aged 60 and above.
[16]In my research, I refer to people over 45 years old

balanced sex ratios, status competition may lead to stronger preferences for larger homes among male descendants, the 2010 Marriage Market Survey in China indicates that 71% of unmarried women prefer their future husbands to own a home, compared to 48% of unmarried men who express a similar preference for their future wives. This suggests that although the motivation may be slightly weaker, parents also have incentives to engage in intergenerational transfers for female descendants.

The results are presented in Table 8, where `house_child` represents the interaction term. The estimates indicate that for each additional descendant, the wealth effect of housing on labor supply decisions decreases by 25% to 50%. This pattern provides evidence of parents' intergenerational transfer or bequest motives. But is this effect causal?

Table 8: IV Regression Results: Intergenerational Transfer

VARIABLES	(1) retire	(2) lfp	(3) hours
house_net_asset	0.000425**	-0.000886***	0.00526
	(0.000187)	(0.000226)	(0.0273)
house_child	-0.000293***	0.000268**	-0.00846
	(9.31e-05)	(0.000114)	(0.0160)
Controls	✓	✓	✓
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	14,838	14,525	5,467
R-squared	-0.009	-0.002	0.004

Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

While causality cannot be definitively established, several considerations support this interpretation:

- Reverse causality is unlikely, as labor supply decisions are improbable to influence the number of descendants, particularly when income and wealth are controlled for.

- The observed effect is unlikely to be solely driven by increased expenditures from having more children, as such effects would be captured by the `child number` term rather than the interaction term.

In addition to this, I conducted three additional sets of regressions. First, I restricted the sample to individuals aged 60 and above. In China, workers over 60 typically have children who are over 30 and likely married, reducing their incentive for intergenerational transfers to support their children's home purchases. The regression results (column 1 to 4 in Table 9) indicate that individuals over 60 are not significantly affected by the HPR shock, suggesting that the observed effect is primarily driven by individuals aged 45 to 60.

Table 9: DiD Regression Results: Intergenerational Transfer

VARIABLES	(1) retire	(2) lfp	(3) retire	(4) lfp	(5) retire	(6) lfp	(7) retire	(8) lfp
post_treated	-0.0907***	0.0675*	-0.0168	0.0158	-0.0306	0.0455*	-0.0197	0.00504
	(0.0249)	(0.0346)	(0.0235)	(0.0246)	(0.0197)	(0.0268)	(0.0229)	(0.0282)
single-house*post_treated					-0.0291**	0.00952		
					(0.0120)	(0.0170)		
child_number*post_treated							-0.0168**	0.0237**
							(0.00674)	(0.00901)
45-60 years old	✓	✓						
over 60 years old			✓	✓				
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	11,678	11,505	9,760	9,175	22,149	21,394	22,149	21,394
R-squared	0.773	0.652	0.848	0.725	0.826	0.718	0.826	0.718

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The second set of regressions provides more compelling evidence. I introduced an interaction term for single-property owners, which equals 1 if the family owns only one home. Families with a single property are expected to have a stronger incentive to financially support their children. The results in column 5 and 6 in Table 9 show that single-property owners have, on average, a 4.1% lower probability of retirement compared to other groups, providing strong support for the intergenerational transfer channel.

In the third set of regressions, I included an interaction term between the treatment effect (treat_post) and the number of children. If the intergenerational transfer channel holds, families with more children should experience greater financial pressure, leading to a stronger treatment effect. The results in column 7 and 8 in Table 9 confirm this prediction: in treated cities, each additional child increases labor force participation by 2.37% after the shock.

Using a categorical variable for the number of children (Table 10), I find that, following the HPR shock, families with two or more children exhibit higher labor force participation and a lower probability of retirement compared to single-child households.

Although the regressions above do not establish a causal relationship, they provide strong evidence supporting the existence of the intergenerational transfer channel.

4 Robustness Checks

For the regression using instrumental variables, one concern is that individuals residing in the same district may share similar characteristics, or that interactions among them could influence their labor supply decisions. To address this issue, rather than using the average housing wealth within the same community, I em-

Table 10: Categorical Child Number

VARIABLES	(2) retire	(3) lfp	(4) hours
post_treated	-0.0185	0.00921	-0.176
	(0.0212)	(0.0245)	(2.385)
twokid	0.0277	-0.0620**	-1.102
	(0.0211)	(0.0293)	(1.967)
multkid	0.00391	-0.0789*	-1.993
	(0.0295)	(0.0427)	(3.562)
twokid_post_treated	-0.0427***	0.0695***	0.417
	(0.0162)	(0.0190)	(1.851)
multkid_post_treated	-0.0585***	0.0608**	-1.684
	(0.0184)	(0.0259)	(2.186)
Controls	✓	✓	✓
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	21,699	20,958	6,715
R-squared	0.825	0.718	0.670

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

ploy the average housing wealth in the same city (excluding the same county) as the instrumental variable[17]. The results in Table 11 indicate that my conclusions remain robust.

Table 11: Robustness Checks: Instrumental Variable

VARIABLES	(1) retire	(2) lfp	(3) hours
house_net_asset	0.000698**	-0.00160***	0.0112
	(0.000345)	(0.000429)	(0.0356)
Controls	✓	✓	✓
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	4,964	4,862	1,528
R-squared	-0.102	-0.247	0.004

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

I conducted some placebo tests (shown in Figure 10, 11, 12, 13) to verify the robustness of the findings [18]. I conducted in-space placebo test, where observations were randomly assigned as treated and mixed placebo test, where treatment and control groups were randomly assigned, and the treatment time was shifted backward to create a fake treatment. The results demonstrate that the observed effects are unlikely to be driven by random chance, providing strong support for the validity of the empirical strategy.

I conducted additional robustness tests, the first of which involves modifying the classification of treatment and control cities. While my main specification defines treatment cities as those are within 300 from regulated cities, this robustness check classifies cities bordering with regulated areas as treatment cities [19]. As

[17]In China a county is smaller than a city

[18]I did not conduct placebo test for weekly working hours as the effect in the baseline regression are not statistically significant.

[19]This way of classification generates a more balanced number of treatment and control cities.

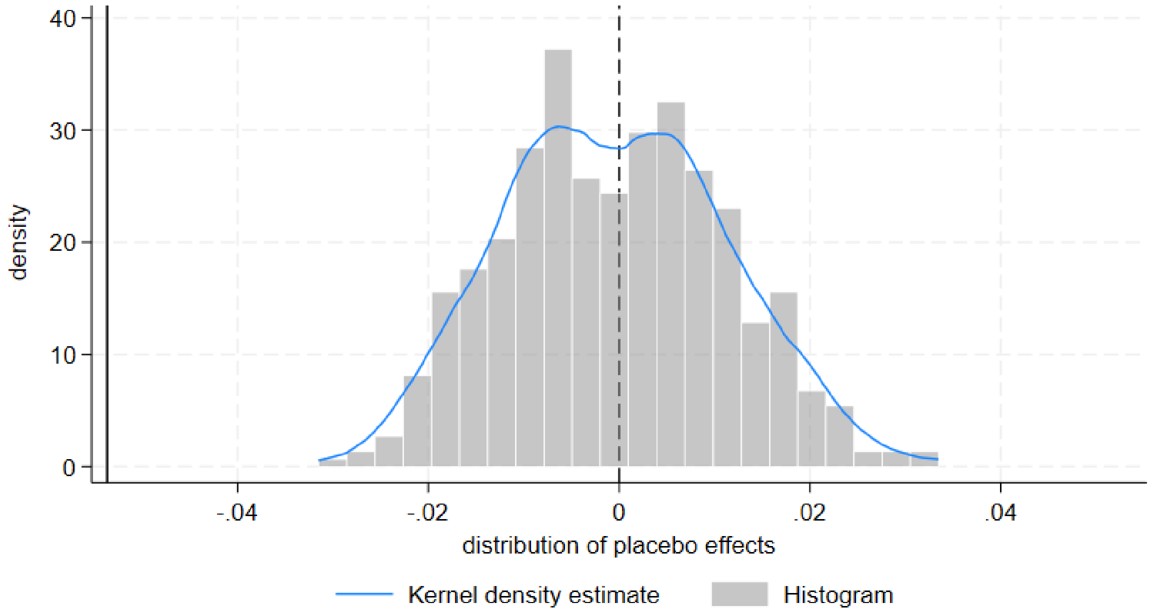

Figure 10: In-space Placebo Test: Retirement

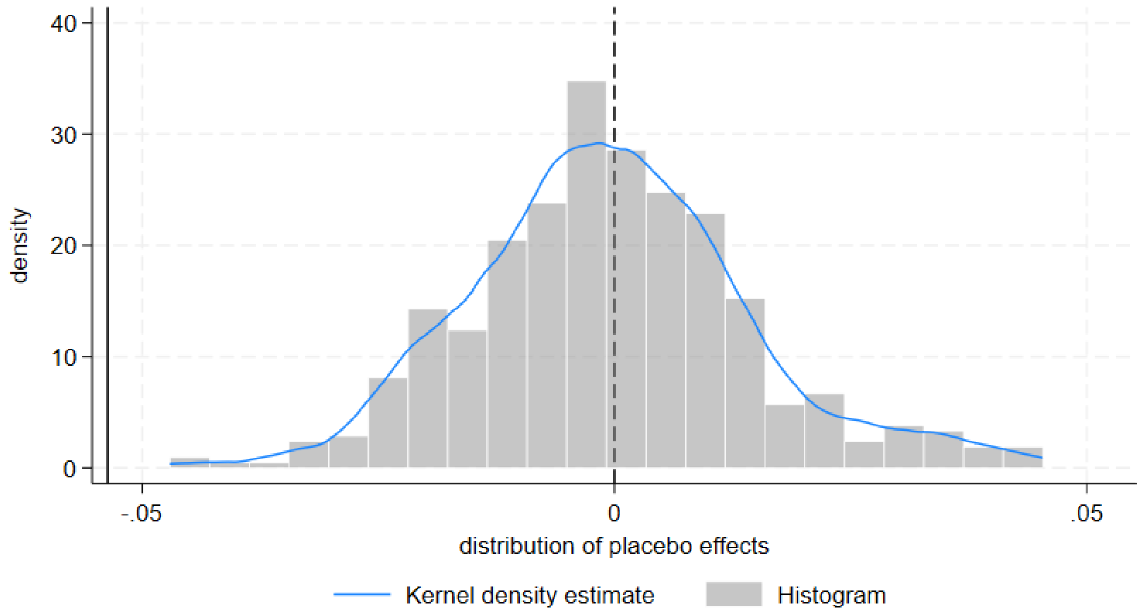

Figure 11: Mixed Placebo Test: Retirement

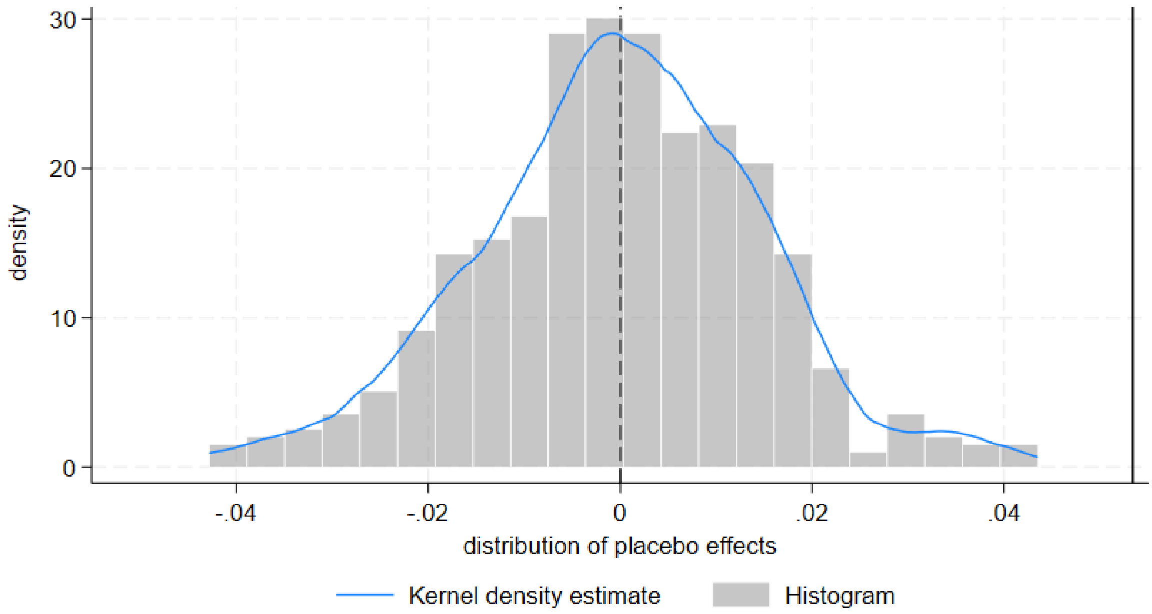

Figure 12: In-space Placebo Test: LFP

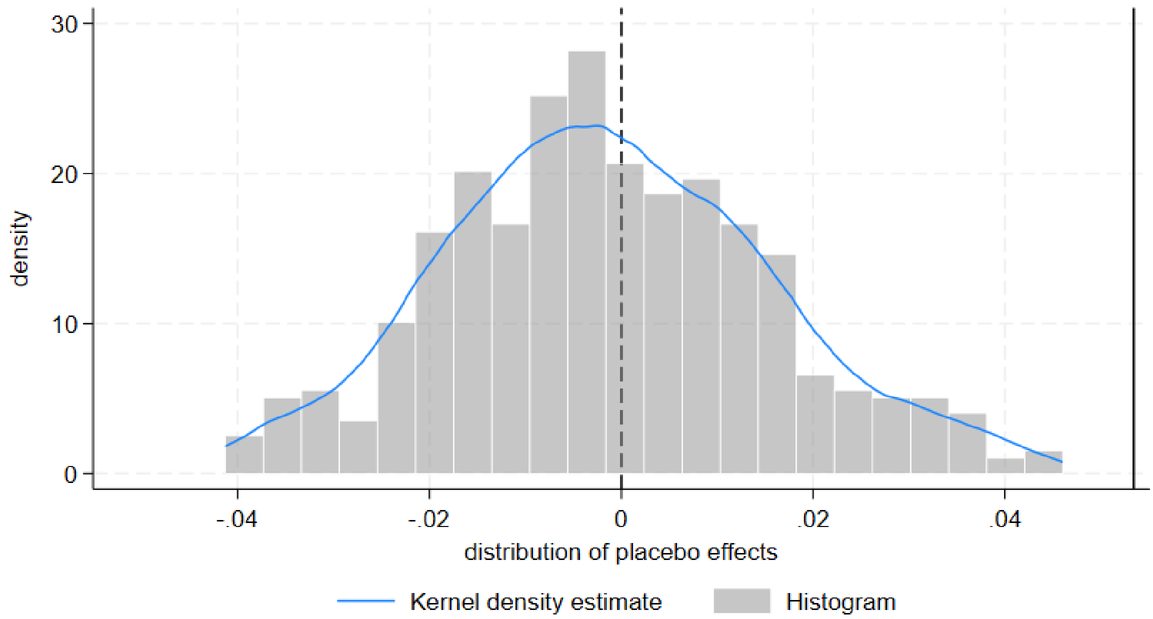

Figure 13: Mixed Placebo Test: LFP

shown in Table 12, this alternative classification does not significantly affect the main results, further supporting the robustness of my findings.

Table 12: Robustness Checks: Alternative Classification of Treatment and Control Groups

VARIABLES	(1) house_net_asset	(2) retire	(3) lfp	(4) hours
post_treated	6.182***	-0.0246*	0.0240	-1.321
	(2.730)	(0.0142)	(0.0208)	(1.641)
post_regulated	0.0160	-0.0348	2.126	
	(0.0133)	(0.0212)	(1.570)	
Controls	✓	✓	✓	
Individual FE	✓	✓	✓	
Time FE	✓	✓	✓	
Observations	22,142	22,142	21,386	6,835
R-squared	0.826	0.826	0.717	0.670

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In Table 13, following the approach of Wolfers (2006), I add linear time trend for each city, $\gamma_i t$, to separate the treatment effect from the pre-existing trend of each city. The results remain consistent.

Table 13: Robustness Checks: DiD with City Specific Time Trend

VARIABLES	(1) retire	(2) lfp	(3) hours
post_treated	-0.0453**	0.0472	-1.238
	(0.0216)	(0.0307)	(3.375)
post_regulated	-0.00694	-0.0103	1.877
	(0.0215)	(0.0306)	(3.373)
Controls	✓	✓	✓
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	22,143	21,384	6,832
R-squared	0.826	0.718	0.669

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Additionally, while the main regression clusters standard errors at the city-year level to account for correlation of respondents in the same city and time, I also conducted robustness checks by clustering at the city-ownership level to account for potential correlated behaviors among individuals within the same city and with the same property ownership status (one property, multiple property)(Han et al., 2020). As shown in the column 1 and 2 of Table 14, clustering at the city-ownership level does not significantly alter the results, providing further confirmation of the robustness of my findings. I also follow a similar approach of Cameron et al. (2006), I introduce two way cluster, I cluster at city-year and individual level at the same time, as shown in column 3 and 4 of Table 14, this approach also does not significantly alter my estimation.

Table 14: Robustness Checks: Alternative Clustering of Standard Errors

VARIABLES	(2) retire	(3) lfp	(4) retire	(5) lfp
post_treated	-0.0578***	0.0531**	-0.0534***	0.0535**
	(0.0182)	(0.0240)	(0.0165)	(0.0215)
Controls	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	17,727	17,071	22,146	21,384
R-squared	0.836	0.729	0.826	0.717

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In the baseline DiD specification, I exploit all available individual-level observations—including multiple respondents per household—to maximize precision. To account for intra-family correlation, I cluster standard errors at the city-year level. However, since spouses often make joint labor-supply decisions, Table 15 re-estimates the model using only the household head in each family. The results are consistent, confirming that the main results are not driven by intra-household decision processes.

Table 15: Robustness Checks: DiD with Household Head

VARIABLES	(1) retire	(2) lfp	(3) hours
post_treated	-0.0551*	0.0929***	-2.460
	(0.0313)	(0.0355)	(2.846)
post_regulated	0.00393	-0.0168	1.870
	(0.0312)	(0.0396)	(2.880)
Controls	✓	✓	✓
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
Observations	8,780	8,590	3,674
R-squared	0.812	0.728	0.666

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

To address the concern that excluding HPR cities after 2018 might lead to biased estimates, I conducted two tests. First, I applied a staggered DiD approach that incorporates all HPR implementations after 2016. The results, shown in columns 1 and 2 of Table 16, are consistent with my baseline regression. Additionally, I constructed a ”clean” sample by excluding not only all HPR cities after 2018 but also any treatment cities that share a boundary with these cities. This ensures that the sample remains unaffected by the staggered implementation of HPR after 2016. The results, presented in columns 3 and 4 of Table 16, confirm that my findings remain robust.

Table 16: Robustness Checks: HPR Implementation after 2018

VARIABLES	(1) retire	(2) lfp	(3) retire	(4) lfp
post_treated	-0.0505***	0.0278	-0.0537***	0.0532**
	(0.0126)	(0.0195)	(0.0174)	(0.0224)
post_regulated	-0.0192	0.0133	-0.0127	-0.00626
	(0.0170)	(0.0234)	(0.0180)	(0.0254)
Controls	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	22,329	21,571	22,150	21,391
R-squared	0.826	0.718	0.826	0.717

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5 Conclusion

This thesis investigates the impact of housing wealth on labor supply decisions among elderly workers in China by leveraging the quasi-natural experiment created by the implementation of House Purchase Restrictions (HPR) in 2016-2017. The analysis employs a two-pronged approach: a difference-in-differences (DiD) regression, utilizing the variation in house prices between regulated and unregulated cities, and an instrumental variable (IV) approach to address potential endogeneity concerns.

The IV regression reveals a negative effect of housing wealth on labor supply, suggesting that increased housing wealth incentivizes earlier retirement among elderly workers. However, the DiD analysis, leveraging exogenous house price increases caused by HPR spillovers, shows the opposite effect: individuals experiencing house price increases exhibit higher labor supply and delayed retirement.To reconcile these seemingly contradictory findings, this thesis introduces a intergenerational transfer channel as a key explanatory mechanism. I identify a significant intergenerational transfer motive, rooted in the widespread practice of parents' financially supporting their children in purchasing homes. This mechanism helps explain how rising house prices not only impact elderly workers' labor supply directly but also influence their decisions indirectly through the financial pressures faced by the younger generation.

My findings suggest that the impact of housing wealth on elderly workers' labor supply is straightforward and intuitive: increased housing wealth negatively affects labor supply. However, rising house prices not only increase elderly workers' housing wealth but also impose additional financial burdens on the younger generation, making it more difficult for them to afford homes. This, in turn, motivates elderly workers to delay retirement in order to provide financial support to their children. Therefore, I conclude that China's recent housing boom is not the primary reason behind the declining average retirement age among elderly workers.

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