Capital Leakage, House Prices, and Consumer Spending: Evidence from Asset Purchase Restriction Spillovers

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Abstract

We exploit plausibly exogenous spillovers from housing asset purchase restrictions in regulated cities on non-regulated cities in China to study the house price and real consequences of capital leakage across markets. Purchase restriction spillovers induce house price surges but not rent increases in the affected non-regulated cities. These spillover-driven house price surges lead to a causal increase in consumer automobile spending that (1) implies a larger housing MPC than in the United States and (2) implies a negative bias in the OLS due to saving propensities, and that is (3) important in the aggregate and (4) redistributive: Concentrated in the locals, zero for the non-locals, and U-shaped across age, consistent with predictions from Favilukis and Van Nieuwerburgh (2017).

JEL classification: D12, D14, E21, E31, E65, G12, G18, H23, H70, R10, R20, R28, R51

Keywords: house prices, out-of-town investors, consumer spending, purchase restrictions, spillover effects, quasi-experiment

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1 Introduction

The flow of capital is an increasingly important factor in asset markets and the real economy. Bernanke (2005) hypothesizes that the spillover of excess savings in developing countries raises asset prices and drives up aggregate demand abroad. Recent studies in the housing market also reveal the importance of the flow of capital. Badarinza and Ramadorai (2018), Cvijanovic and Spaenjers (2018) and Sá (2016) find that foreign capital is partially responsible for residential real estate price movements in global cities including London and Paris. Favilukis and Van Nieuwerburgh (2017) build a theoretical framework to analyze the effect of out-of-town demand on house prices, as well as the redistributive consequences on the consumption and welfare of city residents. Despite the strong interest in the capital flow channel, to the best of our knowledge, causal evidence is lacking on how external and out-of-town demand affects asset prices and aggregate demand in the same setting.

In this paper, we provide causal evidence on the asset pricing and real effects of external asset demand, by exploiting a quasi-experiment — the spillovers from local housing purchase restrictions in large cities in China, to neighboring non-regulated cities. Concerned with surging local house prices over the past decade, which grew as fast as 10% per annum in the country's 35 largest cities (Fang et al., 2016; Wu et al., 2016), large cities rolled out House Purchase Restriction measures in late 2016 and early 2017, targeted at cooling down and driving away investor demand, by raising the down payments on, and in some cases outright forbidding investment purchases. These locally-motivated policy changes appeared to introduce capital leakage across housing markets, to the nearby non-regulated cities, which are not close enough for commuting but convenient for investors to visit occasionally. This capital leakage causes arguably exogenous house price booms in the latter group of cities, and impacting the real economy by substantial increasing consumer spending.

China's vast group of cities, sizable housing boom characterized by strong investment demand (Glaeser et al., 2017), and shift towards a consumer economy offers unique advantages as a laboratory to study the investment demand, house prices and consumer spending link. We introduce a new administrative dataset to overcome data challenges especially on consumer spending, to undertake this research opportunity. We focus on consumer durable spending on new automobiles, an aspect of spending particularly important for the consumer responses to asset prices (Mian et al., 2013).[1] We utilize a unique dataset from CIITC, an agency under the China Insurance Regulatory Commission, of each private passenger automobile purchase in China.[2] Our administrative dataset addresses Aladangady's (2017) critiques of previous administrative datasets on automobile spending—that they rely on imputed car values, and cannot compare across household types, and offers important improvements upon existing similar datasets for the United States in Mian

[1]Automobile purchases are important, not only because they usually represent the largest portion of household spending besides housing, but also because they are closely related to firm production. In 2015, automobiles constituted 12% of China's manufacturing sector (Chen and Zhong, 2017).

[2]All automobiles are required by law to be covered by mandatory traffic accident liability insurance in China.

et al. (2013) and Di Maggio et al. (2017). This dataset provides comprehensive information on the timing, location, model and value of each purchase, and importantly also information on buyer's demographics (birth place and age). This allows for an unprecedented view into consumer spending in China across time, space and consumer types.[3] Combining this rich administrative data and the arguably exogenous impact of purchasing restriction spillovers to non-regulated cities, we are able to establish a causal chain from capital spillover to house prices and consumer spending.[4]

We first investigate the effects of spillovers from housing purchase restrictions in regulated cities, on house prices in non-regulated cities. The empirical strategy is a standard difference-in-difference design. Among the 336 prefectural cities in China, 22 large cities experienced House Purchase Restrictions. We split the remaining 314 non-regulated cities into two groups based on whether their distance from the closest regulated cities is larger than 250 km. We then estimate the differential changes in house prices between the treatment group (non-regulated cities closer than 250 km to regulated cities) and the control group (the remaining non-regulated cities).

Controlling for time-invariant city-level heterogeneity, seasonality, aggregate macroeconomic and housing market trends through city and time fixed effects, as well as time-varying city-level macroeconomic variables, including GDP per capita, population, road infrastructure, and public transportation, we show that House Purchase Restrictions in large cities have significant spillover effects on neighboring non-regulated cities and cause house prices to increase by an average of 6% to 10%. The increase in house prices is entirely accounted for by the increase in the price-rent ratio. We do not find significant differences in rent growth between the cities.

Next, we estimate the effects of the spillover-driven house price booms on consumer spending on new automobiles. We use the same difference-in-differences methodology. Again, we find large effects. According to our estimates, household spending on automobiles in the affected non-regulated cities increased by 11% to 17% compared with the control group of cities, following the plausibly exogenous house price booms, in response to the spillover of the purchase restriction shock. On the intensive margin. we find even larger increases in household spendings on luxury automobiles and SUVs. This suggests that households purchase more expensive cars in response to the shock.

To ensure the housing market and real consequences of spillovers we identify are not driven by pre-existing trends between the affected group of cities and the control group of cities, or by the choice of empirical specifications, we carry out several robustness checks. First, we show that before the purchase restrictions took effect in large regulated cities, both house prices and automobile purchases were not systematically diverging across the affected and the control group

[3]Two recent papers (Barwick et al., 2017; Chen and Zhong, 2017) use car registration data from China's Department of Transportation to achieve the same purpose. However, they do not have data on car prices and thus cannot calculate exact spending on car purchases. Nor do they have information on consumers. Our data overcome these challenges.

[4]By focusing on regional capital spillovers, we estimate house prices and spending responses holding the interest rate constant, equivalent to a small open economy setting.

of non-regulated cities, providing suggestive evidence for the parallel trend assumption. Post the shocks, local employment, population and output, except for residential investment, also do not systematically differ between treated and control cities, providing supporting evidence that the purchase restriction spillover shocks impact the treated cities solely through external effects on the local housing market of the treated cities. Second, we carry out alternative specifications, either allowing the exposure to purchase restriction spillovers decreases continuously with the distance from the nearest regulated city, or perturbing the cut-off distance to define the affected and control group of cities to 300 km, 200 km and 150 km, instead of 250 km, and the results under all specifications reassuringly remain quantitatively similar.

Next, we take full advantage of our administrative data to compare within-region, across demographic groups in consumers' spending response in affected cities. We first compare the spending responses of locals versus that of migrants and out-of-towners. We find a much larger increase in spending among the group of individuals born in the same city in which they reside (locals), who are more likely homeowners, consistent with the housing wealth effect. Moreover, we find no increase in average spending for individuals born outside of their current city (migrants and out-of-towners). That is, the overall spending response to the spillover-driven house price boom is entirely accounted for by the increase of spending among locals, and is not driven by the non-locals, either by the increase of spending among existing migrants, or by the spending of new migrants.

Taken together, the evidence we find is inconsistent with the possibility that a labor relocation channel alone can explain our results, whereby workers in the regulated cities migrate and find jobs in the nearby non-regulated cities, raising rents, spending, and house prices. This is because (1) we find no increase in rent in the cities affected by the spillover relative to the unaffected cities, and (2) we find no evidence that spending by migrants contributes to, let alone explain the overall increase in spending in the affected cities. What we find is consistent with the combined effects of the speculation channel and the wealth effect channel, whereby speculators facing increased transaction costs in the regulated cities turn to investing in nearby non-regulated cities, causing housing price there to grow in the absence of rent growth, and causing existing local homeowners to increase their spending.

We then compare the spending responses across individuals of different age groups, and find a U-shaped relationship between age and spending responses to the exogenous policy spillover and house price boom. The smallest spending response is found among individuals in the age ranges 35-39 and 40-44. This can be explained by the fact that people in these age groups are expected to upgrade their housing, whereas younger groups are expected to inherit housing from their parents, and older groups are expected to downsize.[5]

[5] The age heterogeneity results also suggest that the spending responses to house price increases that we find are consequences of forces beyond the loosening of borrowing constraints alone (Mian et al., 2017a,b). The market for HELOCs and mortgage refinancing is generally underdeveloped in China. Moreover, home equity borrowing is unavailable to individuals age 55 or above, while we find large spending responses for this age group. Expected downsizing (Sinai and Souleles, 2005; Buiter, 2010) is a plausible explanation for the estimated spending response

We impute homeownership for car buyers to make this point about homeownership and spending response more concrete. We combine homeownership data in household surveys with information on the birthplace status and age in our dataset. Using the imputed measure of the homeownership rate, we again find that spending is concentrated in owners as opposed to renters. Taken together, both the birthplace and age heterogeneity results are qualitatively consistent with the housing wealth effect — spending increases for existing homeowners in affected non-regulated cities, if the positive wealth effect from higher home values dominates the negative income effect from higher housing costs (Sinai and Souleles, 2005; Buiter, 2010).

Our estimates implies large redistributions in the real consequences of the spillover-induced housing boom and thus the welfare implications are unclear. The strong local-migrant heterogeneity in spending response and the U-shaped relationship between age and spending response suggest that even though spillover-driven house price increases led to large spending increase for some demographic groups, there are potentially negative effects for mid-age renters or owners looking to upgrade. In this way, our birthplace and age heterogeneity results also provide, for the first time to the best of our knowledge, empirical support for the model prediction in Favilukis and Van Nieuwerburgh (2017) on the redistributive effect of out-of-town demand on city residents.

We compare the marginal propensity to consume (MPC) out of housing wealth, corresponding to our quasi-experimental estimate, to estimates in the recent literature using regional data from the United States. According to the difference-in-differences (DID) estimate using purchasing restriction spillovers as a quasi-experimental variation in house prices, the elasticity of new automobile spending to house prices is 1.77: for every 10% increase in housing prices, other things equal, consumer spending on automobiles rises by 17.7%. Taking into account that the average ratio of annual automobile purchases to housing value for the DID sample period is 0.025, the causal estimate of the annual MPC out of housing wealth on automobiles is 0.044: for each *renminbi* of housing capital gains, consumers spend an average of 4.4 cents on automobile purchases. This is larger than the Mian et al. (2013) automobile MPC of 0.018, which suggests that housing wealth has a greater influence on consumer spending in China than in the United States.[6]

We next compare our causal estimate of the MPC from the purchasing restriction spillover setting with the OLS estimate. Using the same dataset on automobile spending by city and time, we report the OLS estimate of the automobile MPC out of housing wealth in the panel of cities from 2003 to 2017, with the appropriate controls and fixed effects, to be 0.016. The OLS estimate is lower than the causal estimate. This might be paradoxical at first glance.

We propose that the role of savings propensity in house prices explains why the OLS underestimate the MPC in this context. As documented in Wei et al. (2012) and Zhang (2017), under financial

for this group of consumers.

[6]This could be because housing wealth accounts for a much larger fraction of household wealth in China. Xie and Jin (2015) report that housing wealth accounts for 70% of total household wealth in China, whereas in the United States housing wealth accounts for for 30% to 45% of total household wealth according to Wolff (2016).

repression, housing assets act as a major (if not the most important) class of investment vehicles, and households' savings propensity plays an important role in determining residential real estate demand in China. This first leads house price growth to depart from permanent income growth, so that the traditional permanent income channel is weakened. Second, the role of savings propensity in housing asset demand causes the OLS estimate of MPC to be biased downwards. This is because savings propensity serves as an omitted variable, which negatively drives consumer spending and positively drives house prices.[7]

Turning to macroeconomic effects of the capital leakage induced by the policy spillover, we find that housing asset purchase restriction spillovers cause an average increase of 66 million yuan in household automobile spending in affected non-regulated cities per city per month. Aggregating over the affected cities and the event months, this translate to a total increase of 100.32 billion yuan in household automobile spending, or 717 thousand units in private passenger automobile sales. In comparison, the average annual increase in private passenger automobile sales in China in 2016 and 2017 is 1,754 thousand units. Taken together, the spillovers from the imposition housing asset purchase restrictions, which is a local policy, not only generated significant external impacts on non-regulated cities, but also lead to economically large macroeconomic effects, contributing to approximately 40.8% of the average annual increase in private passenger automobile sales in the automotive boom of 2016 and 2017.

This paper makes three major contributions to the literature. First, it contributes to the literature on the impact of capital flows and non-local asset demand on asset pricing and the real economy. In addition to estimating the spending and redistribution consequences, this paper presents a new mechanism that generates housing market spillover: asset purchase restrictions in a hot local housing market force the demand for housing to flow to neighboring colder markets, causing a house price boom in the colder markets. In this way, this paper adds to the literature on the effect of non-local factors in housing markets, including Badarinza and Ramadorai (2018), Cvijanovic and Spaenjers (2018), and Sá (2016), as well as DeFusco et al. (2018) and Bailey et al. (2016), but do not estimate spending or redistributive effects.[8] By providing an example of unintended external consequence of a local asset market policy in connected markets, this paper also contribute to the empirical literature on the effectiveness of macro-prudential policies (Acharya et al., 2017; DeFusco et al., 2019) and provides lessons to the theory of the design of efficient housing and

[7]This omitted variable concern implies that the researchers' estimates of the true value of the MPC out of housing wealth will ultimately depend on what source of variation drives house price movements. For example, Waxman et al. (2018) use credit and debit card transactions aggregated at the city level from 2011 to 2013 in China, focusing on the variation in house prices driven by differences in savings propensities, proxied by sexual imbalance, and find a negative spending elasticity of house prices. Throughout our analysis, we maintain the definition of the MPC out of housing wealth as the response of consumer spending to changes in housing wealth, holding everything else constant, including savings propensity.

[8]For instance, DeFusco et al. (2018) find that housing price in a given local market is affected by the geographically closest neighbor's housing price, and the effect does not act through changes in local market fundamentals, including speculation and migration. Bailey et al. (2016) show that housing price experiences within an individual's geographically distant social network can directly affect that individual's own expectations and housing market behavior in his local market.

macro-prudential policies (Fujita, 1989; Glaeser and Gyourko, 2008).

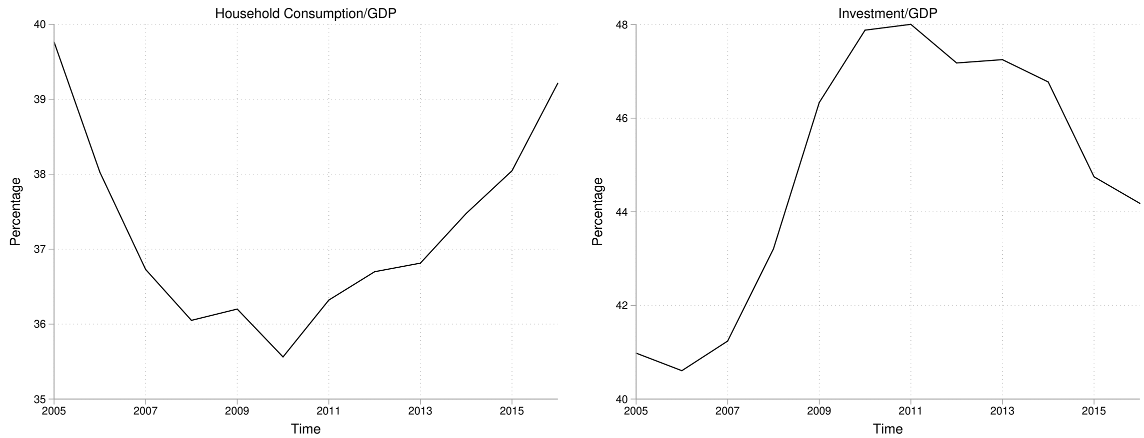
Second, this paper is among the first to provide an in-depth look into consumer spending in China and its determinants. After a high-growth episode characterized by strong external demand, savings, and investment, but weak household consumption, the importance of consumption in driving the country's growth is gradually rising, with the share of household consumption in GDP steadily increasing from 35.6% to 39.2% during 2010-2016 (Figure 1). China's shift towards a consumption-driven economy is coincident with its housing boom, but the causal relationship is unclear.[9] The difficulty in understanding the causal relationship arises from the lack of methods to address the endogeneity of housing prices (Waxman et al., 2018), which this paper contributes to solving by proposing a plausibly exogenous quasi-experimental variation using purchasing restriction spillovers.

Next, this paper improves upon the existing literature on the housing wealth effect, which currently either uses noisy survey data on consumption, card spending data (Gan, 2010; Agarwal and Qian, 2017), or automobile registration data without model or value information (Mian et al., 2013), which will not be able to provide an accurate or complete assessment of the consumption response. Specifically, this paper use comprehensive data on automobile purchases from the insurance domain to provide multi-dimensional estimates on the durable channel of the housing wealth effect across different demographic groups. In doing so, this paper also relates directly to the Aladangady (2017) in using geographically linked microdata with demographic information to estimate the causal effect of housing wealth on consumer spending. Both our paper and Aladangady (2017) seek to improve on existing work using regional data, for example Mian et al. (2013), Mian and Sufi (2014) and Benmelech et al. (2017), by considering variations within regions. Aladangady (2017) uses geographically identified Consumer Expenditure Survey (CEX) data and the interaction between interest rates and housing supply elasticity (Saiz, 2010; Chaney et al., 2012) as instruments to estimate the MPC in the United States. Our exercise complements the results in Aladangady (2017) and contributes by (1) introducing a new identification strategy for the housing wealth effect using spillovers from purchase restrictions on housing assets, (2) bringing a new rich administrative dataset that (a) offers useful demographic information and (b) minimizes misreporting by containing the universe of automobile purchases in China.

The remainder of this paper is organized as follows. Section 2 further introduces the House Purchase Restrictions policy shock and describes the regression models. Section 3 explains data construction. Section 4 presents the empirical results and robustness tests. Section 6 concludes.

[9] There is an early empirical literature in China discussing the relationship between house prices and consumption; however, the results are mixed. One series of studies finds that a house price boom increases household consumption. Among them, Du et al. (2013) use household survey data in Shanghai and control for a rich vector of household characteristics., Yin and Gan (2010) use household survey data covering nine provinces. Another series of studies finds the direct opposite relationship. Among them, Chen et al. (2012) use a Hansen threshold model on province-level data, Xie et al. (2012) use household survey data covering 12 cities, and Waxman et al. (2018) uses city-level credit- and debit-card spending in 2011-2013. Gu et al. (2018) also use credit- and debit-card data, but find no significant effects on overall spending.

Figure 1: Decomposition of GDP



Notes: This figure plots a decomposition of the gross domestic product (GDP) in China. The share of household consumption in China's GDP has been increasing in the recent decade.

2 Empirical Strategy

2.1 House Purchase Restrictions

Over the past decade, there has been substantial heterogeneity in the housing markets of large and small cities in China. Table 1 summarizes the average annualized growth rate of house prices within each tier of cities during 2012-2016, using data from CityRE and Fang et al. (2016). We see that house prices grow at a high speed of 14.9% annually in Tier 1 cities, but at slower than 3% in Tier 3 cities prior to September 2016.[10]

Table 1: Average Annualized Percentage Growth of House Prices

Tier	count	population(million)	mean	sd	min	p50	max
1	4	15.71	14.85	5.23	9.0	14.66	21.07
2	28	8.02	3.44	5.07	-4.50	1.61	15.69
3	81	4.72	2.54	4.15	-6.29	2.0	18.49
Total	113	5.93	3.20	4.94	-6.29	1.99	21.07

Notes: By tier, this table reports summary statistics on average annualized percentage house price growth in 113 cities during 2012m1-2016m8, before the housing asset purchase restriction shocks in 2016m9 and 2017m3. City-level house price indices prior to 2013m3 are from Fang et al. (2016). City-level house price indices post 2013m3 are from CityRE.

At the end of September 2016 and in the middle of March 2017, two rounds of policy changes were implemented in all Tier 1 and many Tier 2 cities to contain surging house prices. These policy changes were called House Purchase Restrictions and targeted curbing housing market speculators,

[10]The same pattern of rapidly increasing house prices in Tier 1 and modestly increasing house prices in Tier 3 prior to the imposition of purchase restriction policy on housing assets in large cities is observed using the alternative National Bureau of Statistics 70-city house price index. CityRE and Fang et al. (2016)'s house price indices provides a more complete coverage of cities and time periods.

who are known to buy multiple homes and pay substantial amount of cash for the higher down payments required for the purchase of multiple homes (30% to 70% down for second homes and 50% to 100% down for third homes).

Table 2: First Round of House Purchase Restrictions

City	Policy Shock	Date Effective
Beijing	• Raise the down payment: from 35% to 40% for the 1st house; from 35% to 50%-70% for the 2nd house.	2016.9.30
Changsha	• Price-cap regulation: the average transaction price cannot increase further.	2016.11.25
Chengdu	• Raise the down payment: from 35% to 40% for the 2nd house.	2016.10.9
Fuzhou	• Raise the down payment: to 30% for the 2nd house .	2016.10.14
Guangzhou	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.10.1
Haikou	N/A	N/A
Hangzhou	• Restrictions on non-resident purchases: cannot own more than 1 house in city center areas. • Raise the down payment: from 30%-40% to 50% for the 2nd house.	2016.9.20
Hefei	• Restrictions on resident purchases: cannot own more than 2 houses. • Raise the down payment: to 40%-50% for the 2nd house.	2016.10.1
Huizhou	N/A	N/A
Jinan	• Raise the down payment: from 20% to 30% for the 1st house; from 20% to 30%-40% for the 2nd house.	2016.10.2
Nanchang	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.10.8
Nanjing	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.9.25
Qingdao	N/A	N/A
Sanya	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.10.1
Shanghai	• Decrease credit supply (by rationing).	2016.10.19
Shenzhen	• Restrictions on purchases: cannot own more than 1 house. • Raise the down payment: to 30%-50% for the 1st house.	2016.10.4
Shijiazhuang	• Raise the land tax: to 3% for the 2nd house.	2016.10.1
Tianjin	• Restrictions on non-resident purchases: cannot own more than 1 house. • Raise the down payment: to 40% for the 1st house purchased by nonresidents.	2016.9.30
Wuhan	• Restrictions on non-resident purchases: cannot own more than 1 house. • Raise the down payment: to 25% for the 1st house; to 50% for the 2nd house.	2016.10.3
Wuxi	• Raise the down payment: to 40% for the 2nd house.	2016.10.2
Xiamen	• Restrictions on non-resident purchases: those who own 1 house can only purchase additional houses with areas larger than 180 m^2. • Restrictions on resident purchases: those who own 2 houses can only purchase additional houses with areas larger than 180 m^2. • Raise down payment: to 30% for the 1st house; to 40% for the 2nd house.	2016.10.5
Zhengzhou	• Restrictions on non-resident purchases: those who own 1 house can only purchase additional houses with areas larger than 180 m^2. • Restrictions on resident purchases: those who own 2 houses can only purchase additional houses with areas larger than 180 m^2. • Raise down payment: to 30% for the 1st house; to 40% for the 2nd house.	2016.10.2

Notes: This table summarizes the first rounds of policy changes regarding restrictions on housing asset purchases. The policy information is collected from city government announcements and the China Index Academy(a company collecting information on China's real estate market). The resident and non-resident purchases mentioned above are at the household level instead of the individual level.

These House Purchase Restrictions measures included raising down payment requirement to even higher levels, increasing mortgage rates, and outright forbidding the purchase of more than two or three houses by one family. Among cities for which we have consistently reliable house price data, 19 implemented the first round of House Purchase Restrictions, and 22 implemented the second round of House Purchase Restrictions. Tables 2 and 3 summarize both rounds of policy changes for each city.

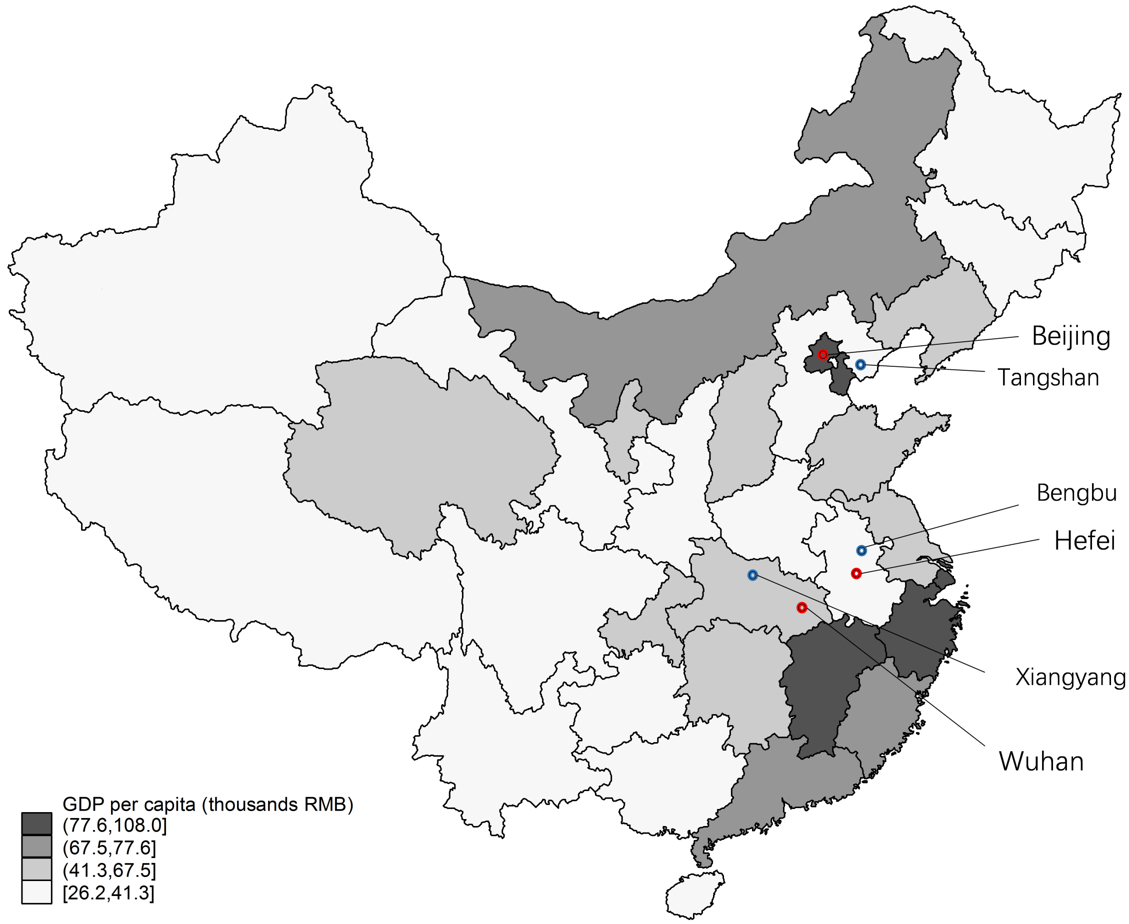
Table 3: Second Round of House Purchase Restrictions

City	Policy Shock	Date Effective
Beijing	• Raise the down payment: to 60%-80% for the 2nd house. • Decrease credit supply: stop providing mortgages lasting longer than 25 years.	2017.3.17
Changsha	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses. • Raise the down payment: to 30% for the 1st house; to 35%-40% for the 2nd house.	2017.3.18
Chengdu	• Restrictions on purchases: each family can only own 1 house.	2017.3.23
Fuzhou	• Raise the down payment: to 50% for the 2nd house. • Restrictions on resale: owner needs to hold a house for 2 years before resale.	2017.3.28
Guangzhou	• Raise the down payment: from 30% to 40%-70% for families that ever applied for mortgages.	2017.3.17
Haikou	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resale: owner needs to hold a house for 2 years before resale.	2017.4.14
Hangzhou	• Restrictions on non-resident purchases: cannot own more than 1 house in the city area. • Restrictions on resident purchases: cannot own more than 2 houses in the city area.	2017.3.3
Hefei	• Increase mortgage rate by 10%.	2017.3.20
Huizhou	• Increase mortgage rate by 10%.	2017.3.20
Jinan	• Raise the down payment: to 60% for the 2nd house . • Increase the mortgage rate by 10%. • Restrictions on resale: owner needs to hold a house for 2 years before resale.	2017.4.19
Nanchang	• Restrictions on non-resident purchases: raise the criteria for the purchases. • Restrictions on resident purchases: cannot own more than 1 house.	2017.3.8
Nanjing	• Restrictions on non-resident purchases: raise the criteria for the purchases. • Raise the down payment: from 30%-40% to 50% for the 2nd house.	2017.3.15
Qingdao	• Raise the down payment: from 20 to 30% for the 1st house; from 30 to 40% for the 2nd house.	2017.3.16
Sanya	• Raise the down payment: from 30%-40% to 50% for the 2nd house.	2017.3.11
Shanghai	• Decrease credit supply (by stricter rationing).	2017.3.17
Shenzhen	• Increase mortgage rate by 10%.	2017.3.20
Shijiazhuang	• Raise the down payment: to 30%-40% for the 1st house; to 50%-60% for the 2nd house.	2017.3.17
Tianjin	• Restrictions on non-resident purchases: raise the criteria for the purchases. • Restrictions on resident purchases: each individual cannot own more than 1 house. • Raise the down payment: to 40% for the 1st house purchased by nonresidents.	2017.3.31
Wuhan	• Increase mortgage rate by 10%.	2017.3.20
Wuxi	• Increase mortgage rate by 10%.	2017.3.20
Xiamen	• Restrictions on resident purchases: an individual can only own 1 house.	2017.3.24
Zhengzhou	• Restrictions on non-resident purchases: raise the criteria for the purchases.	2017.3.17

Notes: This table summarizes the second rounds of policy changes regarding restrictions on housing asset purchases. The policy information is collected from city government announcements and teh China Index Academy(a company collecting information on China's real estate market). The resident and non-resident purchases mentioned above are at the household level except for those otherwise indicated. The criteria for non-resident purchases usually include that the nonresidents must live in the city and pay taxes for a certain period of time.

These policy shocks were very effective in containing the growth of house prices in the regulated cities. In September 2016, the average monthly growth rate of house prices in the 19 regulated cities was 4%, but after the first round of policy changes that number dropped to 1.8% in October 2016 and subsequently remained below 1%. Similarly, the second round of policy changes decreased the average monthly growth rate of house prices in the 22 regulated cities from 0.7% in March 2017 to around 0.1% after that.

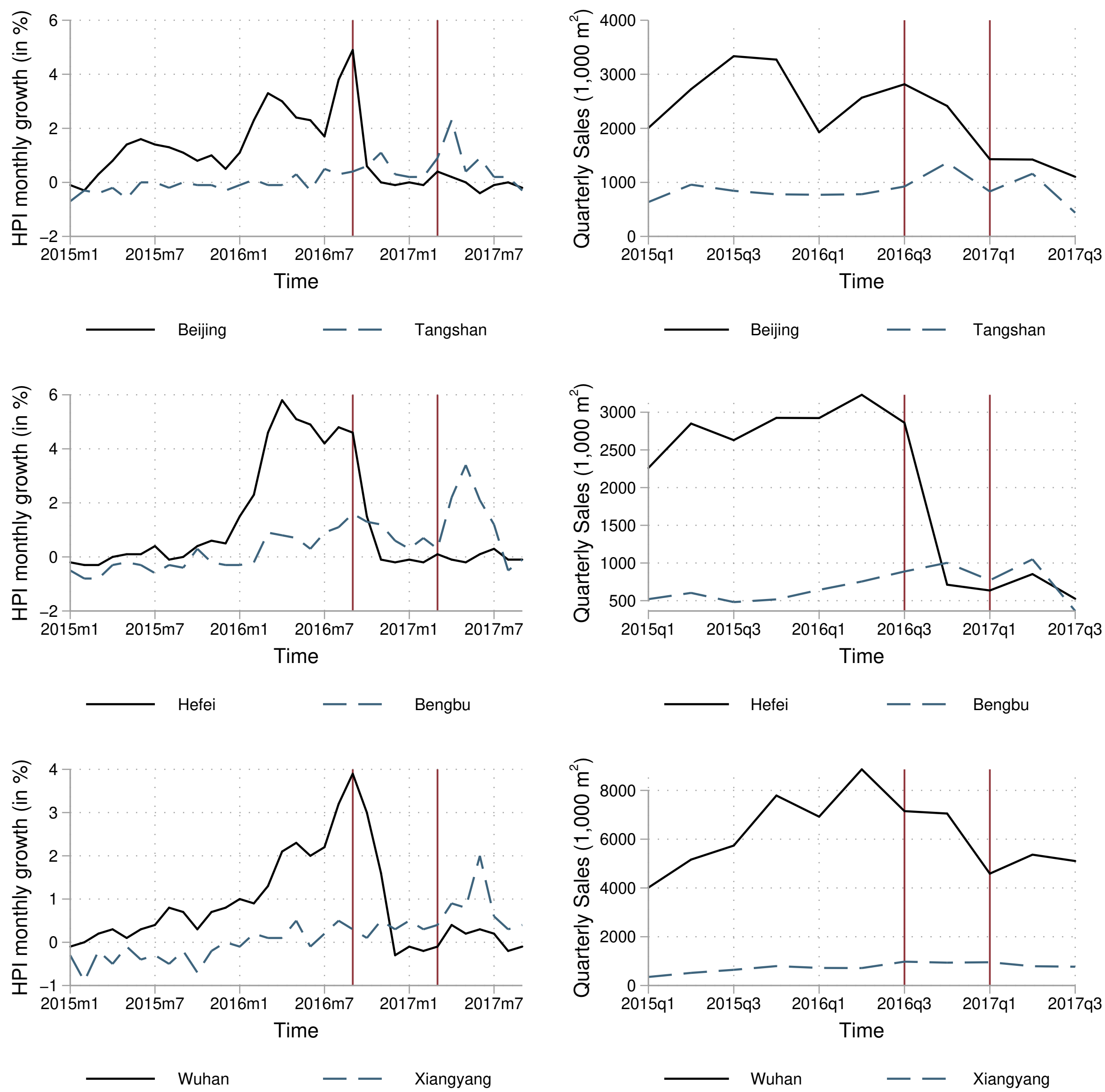
Figure 2: Locations of Three Pairs of Chinese Cities



Notes: This figure shows the locations of the three pairs of cities studied to illustrate the effect of the imposition of restrictions on housing asset purchases on regulated cities and neighboring non-regulated cities. Red dots indicate the three large cities where House Purchase Restrictions were implemented. Blue dots indicate the neighboring small cities that appeared to be affected by the capital inflow. As shown in Figure 3, the policy shocks in Beijing, Hefei and Wuhan appeared to affect the housing market in Tangshan, Bengbu and Xiangyang respectively.

To illustrate the effect of these policy shocks on regulated cities and neighboring cities, we choose three pairs of cities as examples. They are (1) Beijing - Tangshan, (2) Hefei - Bengbu, and (3) Wuhan - Xiangyang. Beijing, Hefei and Wuhan are large cities where two rounds of House Purchase Restrictions were implemented, and Tangshan, Bengbu and Xiangyang are neighboring cities that experienced no policy change. Figure 2 shows the locations of these three pairs of cities.

Figure 3: Reactions of House Prices and House Sales to the Restrictions - Some Examples



Notes: This figure plots the dynamics of house price growth rates and house sales in three pairs of cities from 2015 to September 2017 to illustrate the effect of the imposition of housing asset purchase restrictions on regulated cities and on neighboring non-regulated cities. For the three pairs of cities, the three graphs in the left panel show the monthly growth rate of house prices, and the three graphs in the right panel show the quarterly sales of houses. House Purchase Restrictions implemented in September 2016 significantly decreased the growth of house prices and the number of sales in large cities such as Beijing, Hefei and Wuhan, and increased the sales in small cities such as Tangshan. The restrictions implemented in March 2017, although stricter, had little effect on the housing markets in Beijing, Hefei and Wuhan. However, they significantly increased house prices in Tangshan, Bengbu and Xiangyang. They also increased the sales of houses in Tangshan and Bengbu.

Figure 3 plots the dynamics of house price growth rates and house sales in the three pairs from 2015 to September 2017. We see from Figure 3 that the policy changes were effective in containing surges in house prices in the regulated large cities. House prices in Beijing, Hefei and Wuhan almost stopped growing soon after the policy changes. The transaction volumes of houses also plummeted. In Beijing and Hefei, quarterly house sales dropped over 50% and 70% after the first round of House Purchase Restrictions.

In the meantime, the neighboring small cities appeared to be significantly affected by the House Purchase Restrictions. The quarterly transaction volume in Tangshan increased 47% and 39%, respectively in the quarter that followed each round of House Purchase Restrictions implemented in Beijing. The reactions of house prices in Tangshan, Bengbu and Xiangyang were also significant after the second round of restrictions. The growth rate of house prices soon rose from less than 0.5% to 2.3%, 2.2%, and 2%, respectively, in the three cities after March 2017. The first round of restrictions did not lead to such a large rise in the house prices in these three cities. On the one hand, this is because the second round of policy changes came on top of the first round, and thus posed tougher restrictions on house purchases. On the other hand, it is likely that households took a period of time to learn about the policy changes and form expectations of their actual effect. For example, households may have questioned whether the changes would be strictly enforced. The second round of House Purchase Restrictions was much stricter and made the households realize that the policy changes would pose real restrictions on their future house purchases.

The spillovers of investor demand from the housing market in cities regulated by Housing Purchase Restrictions soon become a concern of the government.[11] The increase in house price in these non-regulated small cities was so significant that in September 2017 the government started to implement similar Housing Purchase Restrictions in these small cities to cool down the housing markets.[12] The government announced that the spillover of housing demand from Tier 1 and Tier 2 cities to Tier 3 cities is a strong concern, since it is potentially able to precipitate turmoil in the economy of the small cities.[13] So our analysis focuses on the time period before September 2017.

2.2 Regression Models

2.2.1 House Prices

To estimate the effect of the capital inflow, induced by policy spillover, on house prices in the affected cities, our empirical design uses a difference-in-differences estimation strategy. As shown in Section 2.1, 22 cities in our sample implemented House Purchase Restrictions, and the remaining

[11] In addition to affecting house prices in neighboring cities, Qian et al. (2019) find that investors constrained by Housing Purchase Restrictions increase new stock accounts openings and purchases of shares of listed real estate developers, providing further evidence of the spillover effect.

[12] Tier 3 cities Xiangyang and Xiaogan started to implement Housing Purchase Restrictions especially targeted at out-of-town buyers in September 2017 according to the Department of Housing and Urban-Rural Development.

[13] This government concern about housing policy spillovers is first observed in the Government Announcement [2017 No. 10] from the Department of Housing and Urban-Rural Development of Hubei Province.

cities during the sample period did not implement such restrictions. First, we calculate the distance from each city to the nearest city where House Purchase Restrictions were implemented. Then we split the non-regulated cities into two approximately equal-sized groups, based on the calculated distance. If a city is within 250 kilometers of a regulated city, then this city belongs to the treatment group ("treated" by the capital inflow), otherwise it belongs to the control group. The choice of 250 kilometers seems arbitrary. However, it can be justified by the fact that with high-speed railways, traveling from a smaller city to a large city takes less than 1 hour if they are closer than 250 kilometers.[14] Note that we consider the treated prefectural cities to be close to the regulated cities for investment purposes, as the relative short distance make frequent visit possible for screening homes or monitoring tenants, but the treated cities are still conventional considered too far for daily commuting to the regulated cities. Third, we estimate the differential change in house prices between the treatment group and the control group before and after each round of the House Purchase Restrictions shock, controlling for a vector of time-varying city-level variables. We recognize two rounds of House Purchase Restrictions because the treated cities witnessed rises in house prices and house sales after both rounds of restrictions, as shown in Figure 3, and because the second round of House Purchase Restrictions was much stricter than the first round.[15]

Specifically, we estimate the following equation for the non-regulated cities.

$$\begin{aligned}\log \mathrm{HPI}_{i,t} = \alpha_i + \lambda_t &+ \beta_1 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2016m9 \le t < 2017m3\}} \\ &+ \beta_2 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2017m3 \le t \le 2017m6\}} + \Gamma X_{i,t-1} + \epsilon_{i,t} \end{aligned} \tag{1}$$

where $\mathrm{HPI}_{i,t}$ is the monthly house price index in city i at time t, α_i are city fixed effects, λ_t are time fixed effects at monthly frequency, D_i is the distance (in kilometers) from the closest city that implemented House Purchase Restrictions.[16] The dummy variable $\mathbb{I}_{\{D_i<250\}}$ is a treatment indicator that takes the value one if the distance of city i from the closest regulated city (i.e., D_i) is less than 250 km. Similarly, the dummy variable $\mathbb{I}_{\{2016m9 \le t < 2017m3\}}$ ($\mathbb{I}_{\{2017m3 \le t \le 2017m6\}}$) takes the value one if time t falls between September 2016 and February 2017 (between March 2017 and June 2017). $X_{i,t}$ is a vector of city-level control variables for city i in time t, and $\epsilon_{i,t}$ is the error term.

[14] This 1-hour travel radius is given the speed of the high-speed trains in China, which run at 250km-350km/hour. Most, if not all cities in our samples are connected or are planned to be connected to China's high-speed rail system.

[15] For example, the down payment in Beijing for the 2nd house purchased by a household was 50%-70% after the first round of restrictions, yet rose to 60%-80% after the second round of restrictions. And several cities increased mortgage rates in the second round as a new restriction method.

[16] Three cities did not implement the first round of House Purchase Restrictions but implemented the second round. This means a city's distance from the regulated cities actually depends on the time. However, it is difficult to define such a distance that varies with time. For example, if we want to compare the outcome variable for the treatment group before and after the second round of restrictions effective in March 2017, then the treatment group within a time window around March 2017 should be defined based on the distance from the 22 cities where the second round of restrictions took effect. It is nonetheless unclear what the appropriate time window would be. Although not an ideal solution, we calculate D_i as the distance of city i from the nearest city among all the 22 cities where the second round of restrictions took effect, which does not vary across time. The constitution of the treatment group changed little across different "choices" of the regulated cities. And the results are robust across these choices.

The coefficients of interest are β_1 and β_2, which measure the differential change in house prices for cities closer to regulated cities relative to cities farther from regulated cities? following two rounds of House Purchase Restrictions, and holding constant city-level time-varying characteristics, as well as city and time fixed effects. To account for serial correlation and region-specific random shocks, we cluster standard errors at the city level in all specifications.

Ideally, the control variables in $X_{i,t}$ should include economic fundamentals such as (1) local demand shifters such as the average income of potential buyers in the local market and migration flows; (2) buyer characteristics, such as the fraction of speculative buyers; and (3) credit market conditions measured by (for example) loan-to-value ratios of the purchased house over the time (DeFusco et al., 2018). However, representative mortgage data are not accessible in China. [17] Also, even if we had data on all Chinese mortgages, they are not representative of all house purchases, since Chinese households have a low dependence on mortgages. According to the statistics from the Urban Household Survey conducted by the National Bureau of Statistics (NBS), only 17% of home buyers in urban China received mortgage loans between 2002 and 2009. In 2012, the outstanding balance of residential mortgages made up only 14.5% of GDP in China, which was much lower than in Japan (39%), the United States (72%), and the United Kingdom (86%) (Fan et al., 2017). This makes it difficult to control for buyer characteristics or credit market conditions. Instead of controlling for all the aforementioned factors, we control for several city-level macroeconomic variables that might be related to house prices, including per capital gross regional product (GRP), resident population, road infrastructure, and public transportation.

Another potential concern with the specification in Equation 1 is that the choice of 250 km as the cut-off value for distance to determine the treatment group and the control group is arbitrary. To address this concern, we conduct two robustness tests. First, we estimate how the distance from the nearest regulated city affects the response of a city's house prices following the House Purchase Restriction spillover. Specifically we estimate the following equation.

$$\begin{aligned}\log \mathrm{HPI}_{i,t} = \alpha_i + \lambda_t + \beta_1 \cdot \log(D_i) \times \mathbb{I}_{\{2016m9 \leq t < 2017m3\}} \\ + \beta_2 \cdot \log(D_i) \times \mathbb{I}_{\{2017m3 \leq t \leq 2017m6\}} + \Gamma X_{i,t-1} + \epsilon_{i,t} \qquad (2)\end{aligned}$$

where all the variables are defined in the same way as in Equation 1, and the coefficients of interest are β_1 and β_2. They reflect how the house prices in a non-regulated city after each one of the two rounds of House Purchase Restrictions are related to the city's distance from the closest regulated city, while keeping all else equal. If the increase in house prices in the non-regulated cities is caused by capital inflow from the regulated ones, then we may hypothesize that as a city's distance from the regulated cities increases, the capital inflow should be weaker, and the rise in house prices should be smaller. Thus β_1 and β_2 should be hypothesized to be negative.

Secondly, we set the cut-off value to be 300 km, 200 km and 150 km, and then verify that different

[17] Mortgage data are available from only one or two mortgage lenders, which is only a small part of all the mortgages.

choices of cut-off value produce little change in our estimation results.

2.2.2 Automobile Purchases

To show that as a general pattern and motivating fact, house prices and automobile purchases are positively correlated, we estimate a simple OLS regression with city and time fixed effects as below.

$$\log \text{Car Sale}_{i,t} = \alpha_i + \lambda_t + \beta \cdot \log \text{HPI}_{i,t} + \Gamma X_{i,t-1} + \epsilon_{i,t} \tag{3}$$

where $\text{Car Sale}_{i,t}$ is the household automobile purchases in city i at time t. We study both the number of automobiles purchased each month and household spending on automobiles each month. $\text{HPI}_{i,t}$ is the monthly house price index in city i at time t. $X_{i,t}$ is a vector of city-level control variables for city i in time t, and $\epsilon_{i,t}$ is the error term. Ideally $X_{i,t}$ should include variables affecting auto production and consumption, especially those correlated with the housing market, such as changes in uncertainty about future local growth. However these data are not available. $X_{i,t}$ currently include only city-level macroeconomic variables that potentially affect automobile purchases, including GDP per capita, population, road infrastructure and public transportation.

To investigate the effect of housing market capital inflow on household automobile purchases, we study how automobile purchases in the non-regulated cities respond to the House Purchase Restrictions policy spillover shock. We use the same difference-in-differences specification used in the house price regressions in Section 2.2.1. We re-estimate Equation 1 for automobile purchases, by changing the outcome variable from log house price to log car sale. Specifically, we estimate the following equation.

$$\begin{aligned} \log \text{Car Sale}_{i,t} = {} & \alpha_i + \lambda_t + \beta_1 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2016m9 \le t < 2017m3\}} \\ & + \beta_2 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2017m3 \le t \le 2017m6\}} + \Gamma X_{i,t-1} + \epsilon_{i,t} \end{aligned} \tag{4}$$

To address the concern that the choice of 250 km as the cut-off value for distance to determine the treatment group and the control group is arbitrary, we conduct two robustness tests. First, we estimate how the distance from the nearest regulated city affects the response in a city's automobile purchases following the House Purchase Restrictions. Specifically we estimate the following equation.

$$\begin{aligned} \log \text{Car Sale}_{i,t} = {} & \alpha_i + \lambda_t + \beta_1 \cdot \log(D_i) \times \mathbb{I}_{\{2016m9 \le t < 2017m3\}} \\ & + \beta_2 \cdot \log(D_i) \times \mathbb{I}_{\{2017m3 \le t \le 2017m6\}} + \Gamma X_{i,t-1} + \epsilon_{i,t} \end{aligned} \tag{5}$$

Second, we set the cut-off value to be 300 km, 200 km, and 150 km, and then verify that different choices of cut-off value cause little change in our estimation results.

2.2.3 Homeownership Heterogeneity

To understand to what extent the increase in automobile purchases is caused by an increase in housing wealth, we test whether people who are more likely to own a house purchase more automobiles. Although our dataset does not include information on homeownership status, it offers information on birthplace and age of car buyers, both of which strongly correlate with homeownership status. We proxy homeownership by whether car buyers are born in the city they reside in, and by the age of the car buyer. In particular, for each city, we collect data on automobile purchases made by individuals born in that city and by individuals born outside the city, as well as data on automobile purchases by buyers in different age groups. Since individuals born locally are more likely than migrants to own a house there, then if the housing wealth channel exists, and if migrant's purchases are not the main cause of increased automobile spending, we expect to see a higher increase in automobiles purchased by individuals born locally than in those purchased by migrants, in response to the House Purchase Restriction shocks.

Similarly, certain age groups are more likely to own a house than other age groups. If the housing wealth channel exists, then we expect to see certain age groups buy more automobiles than others in response to the House Purchase Restriction shocks. Specifically, we estimate the following two equations.

$$\begin{aligned}
\log \text{Car Sale}_{j,i,t} = {} & \alpha_i + \lambda_t + \beta_1 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2016m9\leq t<2017m3\}} & (6)\\
& + \theta_1 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2016m9\leq t<2017m3\}} \times \mathbb{I}_{\{j=\text{ born in current city}\}} \\
& + \beta_2 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2017m3\leq t\leq 2017m6\}} & (7)\\
& + \theta_2 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2017m3\leq t\leq 2017m6\}} \times \mathbb{I}_{\{j=\text{ born in current city}\}} \\
& + \Gamma X_{i,t-1} + \epsilon_{i,t} & (8)
\end{aligned}$$

where subscript j takes values from born in current city and migrants. With $j =$ born in current city, Car Sale$_{j,i,t}$ represents automobiles purchased by individuals born locally in city i at time $t.\alpha_i$ are city fixed effects, λ_t are time fixed effects at monthly frequency, D_i is the distance (in kilometers) from the closest city that implemented House Purchase Restrictions. The dummy variable $\mathbb{I}_{\{D_i<250\}}$ is a treatment indicator that takes the value one if distance of city i from the closest regulated city (i.e., D_i) is less than 250 km. Similarly, the dummy variable $\mathbb{I}_{\{2016m9\leq t<2017m3\}}$ ($\mathbb{I}_{\{2017m3\leq t\leq 2017m6\}}$) takes the value one if time t falls between September 2016 and February 2017 (between March 2017 and June 2017). The dummy variable $\mathbb{I}_{\{j=\text{born in current city}\}}$ takes the value one if j takes the value of born in current city. $X_{i,t}$ is a vector of city-level control variables for city i in time t, and $\epsilon_{i,t}$ is the error term.

The coefficients of interest are θ_1 and θ_2, which measure the difference between automobile spending responses of individuals born locally and of migrants in treated cities (those closer than 250 km to regulated cities), in response to two rounds of House Purchase Restrictions, holding constant city-level, time-varying characteristics, as well as city and time fixed effects.

$$\begin{aligned}
\log \text{Car Sale}_{Age,i,t} = {} & \alpha_i + \lambda_t + \beta_1 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2016m9 \leq t < 2017m3\}} && (9) \\
& + \sum_k \theta_{1k} \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2016m9 \leq t < 2017m3\}} \times \mathbb{I}_{\{Age=k\}} && \\
& + \beta_2 \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2017m3 \leq t \leq 2017m6\}} && (10) \\
& + \sum_k \theta_{2k} \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{2017m3 \leq t \leq 2017m6\}} \times \mathbb{I}_{\{Age=k\}} && \\
& + \Gamma X_{i,t-1} + \epsilon_{i,t} && (11)
\end{aligned}$$

where subscript Age represents age group. We split age at time of automobile purchases into eight groups, so Age takes values from one to eight. For example, if $Age = 1$, then Car $\text{Sale}_{Age,i,t}$ represents the automobile purchases made by age group 1 in city i at time t.

The coefficients of interest are θ_{1k} and θ_{2k}, which measure the difference in automobile purchases among different age groups in treated cities (those closer than 250 km to regulated cities), in response to two rounds of House Purchase Restrictions, holding constant city-level, time-varying characteristics, as well as city and time fixed effects.

To show that the automobile purchases of individuals born locally are generally more affected by house prices than those of migrants, we presents evidence from a simple OLS regression with city and time fixed effects. This evidence corroborates the difference-in-differences regression result, that house price affects locals more than migrants, which further supports the housing wealth channel.

$$\log \text{Car Sale}_{j,i,t} = \alpha_i + \lambda_t + \beta \cdot \log \text{HPI}_{i,t} + \theta \cdot \log \text{HPI}_{i,t} \times I_{\{j=\text{ born in current city}\}} + \Gamma X_{i,t-1} + \epsilon_{i,t} \tag{12}$$

Similarly, to show that the automobile purchases of certain age groups generally correlate more strongly with house prices, we presents evidence from the following simple OLS regression with city and time fixed effects.

$$\log \text{Car Sale}_{Age,i,t} = \alpha_i + \lambda_t + \beta \cdot \log \text{HPI}_{i,t} + \sum_k \theta_k \cdot \log \text{HPI}_{i,t} \times I_{\{Age=k\}} + \Gamma X_{i,t-1} + \epsilon_{i,t} \tag{13}$$

2.3 Parallel Trends and Placebo Test

One potential concern with the difference-in-differences specification 1 is that the estimates of β_1 and β_2 may just be picking up an overall divergence in house prices between the treatment and control groups that has nothing to do with the implementation of House Purchase Restrictions but begins prior to implementation. It is therefore necessary to perform a parallel trend test as a robustness test. One way to perform this test is to estimate the response of the treatment group relative to that of the control group both before and after treatment, and see whether the response of the treatment group diverges from that of the control group before the treatment. Specifically, we estimate a version of Equation 1 that includes treatment group – time interactions.

$$\log \mathrm{HPI}_{i,t} = \alpha_i + \lambda_t + \sum_k \beta_k \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{t=2016m9+k\}} + \epsilon_{i,t} \tag{14}$$

where $\mathbb{I}_{\{t=2016m9+k\}}$ is an indicator of whether time t is exactly 2016m9 + k. For example, if $k = 1$, then it is an indicator of whether t is October 2016. In this specification, we omit the dummy for $k = 0$, so that the coefficients β_k estimate the response of house prices in the treatment group relative to that in the control group and also relative to the level of response in September 2016. In order for the parallel trend assumption to hold, we need to have the estimate β_k only significant for those $k \geq 1$.

As a placebo test, we show that House Purchase Restriction shocks did not affect other housing market variables, such as rents. This indicates that the increase in house prices in the non-regulated cities is not due to a fundamental change in growth perspectives, but is more likely caused by the speculative activities of out-of-town buyers. Otherwise, rents should react to the shocks as well. This mitigates the concern that the rise in automobile purchases is caused by an permanent income effect rather than a housing wealth effect; i.e., that the shocks improve the growth perspectives of the non-regulated cities and thus expectations for household income.

Further, we test the parallel trend assumption for car purchase regression specification 4 by estimating

$$\log \mathrm{Car\ Sale}_{i,t} = \alpha_i + \lambda_t + \sum_k \beta_k \cdot \mathbb{I}_{\{D_i<250\}} \times \mathbb{I}_{\{t=2016m9+k\}} + \epsilon_{i,t} \tag{15}$$

Our analysis of homeownership and the housing wealth channel hinges on how well birthplace status and age at automobile purchase proxy for homeownership. We provide two robustness checks. First, we utilize household survey data on homeownership and test how birthplace status and age predict homeownership. Second, we impute a measure of homeownership based on birthplace status and age, and then show that this measure of homeownership is positively correlated with the response of automobile purchases to house price appreciation.

3 Data

Table 4 reports summary statistics of all data used in this paper. We have two sources for the house price data. The first is from CityRE, a leading national real property information and data-service provider. The second is from Fang et al. (2016) (FGXZ). We use the house price indices from CityRE and FGXZ and not the NBS in our regression analysis, because the NBS house price index covers only 70 cities. House price indices from CityRE and FGXZ together cover many more cities. We first use first the FGXZ house price index, which is a constant quality index constructed using a semi-repeated sales method, and covers 120 cities from 2003 to 2013. We supplement it with the CityRE house price index, which covers 307 cities from 2008 to 2017. We also combine house price indices from CityRE and FGXZ into a new set of indices to have continuous coverage of the house price index for 113 cities from 2003 to 2017. We also use the rent index from CityRE spanning 2008 to 2017.

Ideally we would use constant quality house price data throughout, as developed by Fang et al. (2016) for selected time periods and (Wu et al., 2016) for Tier 1 and Tier 2 cities. However, such data are not generally available for all cities and all time periods. We use constant quality house price indices from FGXZ whenever they are available, and house price indices from CityRE in other cases.

For the data on automobile purchases, we first aggregate total spending on the purchases for each city each month, and the number of automobiles purchased. Then we look at purchases of different models of automobiles. In particular, we compute the number of luxury cars and SUVs purchased and the amount spent on these purchases. This proprietary automobile purchase data comes from the China Insurance Information Technology Corporation (CIITC), a company set up by the China Insurance Regulatory Commission (CIRC). Each automobile insurance application and registration must be reported to CIITC. Since all automobiles in China are required to purchase a type of mandatory insurance[18] every year, all Chinese automobiles are recorded in this data. For each automobile, we observe its unique ID of automobile (VIN code), license plate number, model, and owner information. We observe all automobiles: passenger and non-passenger automobiles, and personal and commercial. To measure household spending on automobile, we retain only personal purchases of passenger vehicles. From the first two digits of the license plate number we know in which city an automobile was registered, which is usually the city where the automobile was purchased.

[18]The insurance is called Compulsory Traffic Accident Liability Insurance and covers liabilities in traffic accidents.

Table 4: Summary Statistics

	Count	Mean	Std. Dev.	10th	50th	90th
City-level data						
Fang et al. (2016) house price index	13641	2.05	1.02	0.99	1.82	3.46
CityRE house price index	30709	1.54	0.51	1.02	1.42	2.21
Combined house price index	19179	2.49	1.33	1.03	2.23	4.14
CityRE rent index	28311	1.39	0.41	0.98	1.31	1.90
Total automobile spending (¥ mil.)	58458	280.60	534.19	14.74	107.65	659.97
Total automobile purchases	58458	2123	3443	140	980	5157
Luxury automobile spending (¥ mil.)	58458	54.33	149.66	0.50	10.59	112.41
Luxury automobile purchases	58458	98	274	1	18	197
SUV spending (¥ mil.)	58458	68.98	159.48	1.48	18.89	159.05
SUV purchases	58458	336	743	9	91	843
Per capita gross regional product (¥)	47040	32437	28541	7961	24543	65694
Resident Population (1,000)	47040	4266	5163	1368	3531	7652
Square meters of road per capita	46320	9.95	10.70	3.82	8.67	16.59
Public buses per 1,000 residents	46344	0.67	0.63	0.21	0.58	1.17
City-demographic group-level data						
Automobile spending of birthplace groups (¥ mil.):						
Born locally	105402	146.00	252.28	1.72	51.51	378.69
Migrants and out-of-towners	105402	158.42	363.35	6.48	44.72	375.31
Automobile spending of age groups (¥ mil.):						
Age Group 1 (18-24)	430632	30.71	52.55	2.60	12.80	75.38
Age Group 2 (25-29)	430632	54.37	109.11	3.01	18.26	130.74
Age Group 3 (30-34)	430632	52.63	103.57	3.70	19.73	119.80
Age Group 4 (35-39)	430632	49.89	89.82	3.69	21.29	115.81
Age Group 5 (40-44)	430632	44.55	80.42	2.80	19.27	103.21
Age Group 6 (45-49)	430632	29.99	55.45	1.19	12.43	71.45
Age Group 7 (50-54)	430632	15.17	30.17	0.55	5.56	36.08
Age Group 8 (55-64)	430632	8.39	18.28	0.21	2.90	18.98

Notes: This table reports summary statistics for all the variables used in this paper. The constant-quality house price index from Fang et al. (2016) covers 2003m1 to 2013m3. The CityRE house price index covers a broader set of cities from 2008m1 to 2017m12. The combined house price index takes data from Fang et al. (2016) and CityRE for the set of cities in Fang et al. (2016), using the Fang et al. (2016) data whenever available. City-level and city-demographic group-level automobile spending and purchase data are aggregated at a monthly frequency from transaction-level data provided by the CIITC.

To obtain data on automobile purchases by different groups, we construct another dataset that distinguishes between automobile purchases made by individuals born in the prefectural city they reside in, and those made by individuals born outside of the prefectural city they reside in. We do this for each city and each month. Based on our CIITC data, we know the birthplace of the buyer from the first four digits of the buyer's ID. By combining these two pieces of information, we can distinguish the automobile purchases made by individuals born locally from those made by migrants.

Finally, we also determine the automobile purchases made by different age groups in each city and each month. From the buyer's ID in our CIITC data, we know her date of birth, and thus can infer her age at the time of automobile purchase. We split ages at purchase into 8 groups: 18 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44, 45 to 49, 50 to 54, and 55 to 64.

For each birthplace category, each age group, each city, and each month, we again aggregate the automobile purchases. *Born in current city* is a dummy variable labeling birthplace groups. *Age Group 1 - Age Group 8* are dummy variables labeling the 8 age groups defined above. To help understand the structure of the dataset, note that for each purchase measure, each city and each month, we have 16 observations representing the interactions of 2 birth place groups and 8 age groups. The averages and standard deviations of the birth place group and age group dummies in the summary statistics in Table 4 do not correspond to the averages and standard deviations of birth place status and age group status in the disaggregated data.

It is worthwhile to mention that the automobile model information in our CIITC data is very detailed, which allows us to know the price of automobiles at purchase. For example, for Mercedes-Benz, we know whether an automobile is from the Mercedes-Benz SL-class with a 5.0L engine, which costs more than 150 thousand USD, or it is from the Mercedes-Benz C-class with a 2.0L engine and a much lower price of 40 thousand USD. Since for each automobile model there will always be at least several cars that are insured by the comprehensive insurance each month, and since the insured amount is required to be the automobile's purchase price, we know the average purchase price of each model of automobiles in each month in each city. This allows us to calculate the exact household spending on automobiles over time.[19] Using information from the CIITC data, we further exclude passenger vehicles purchased by corporations, as well as all non-passenger vehicles, to focus exclusively on consumer automobile spendings.

Figure 4 shows the number of all the automobiles purchased in China each month aggregated from our CIITC data.

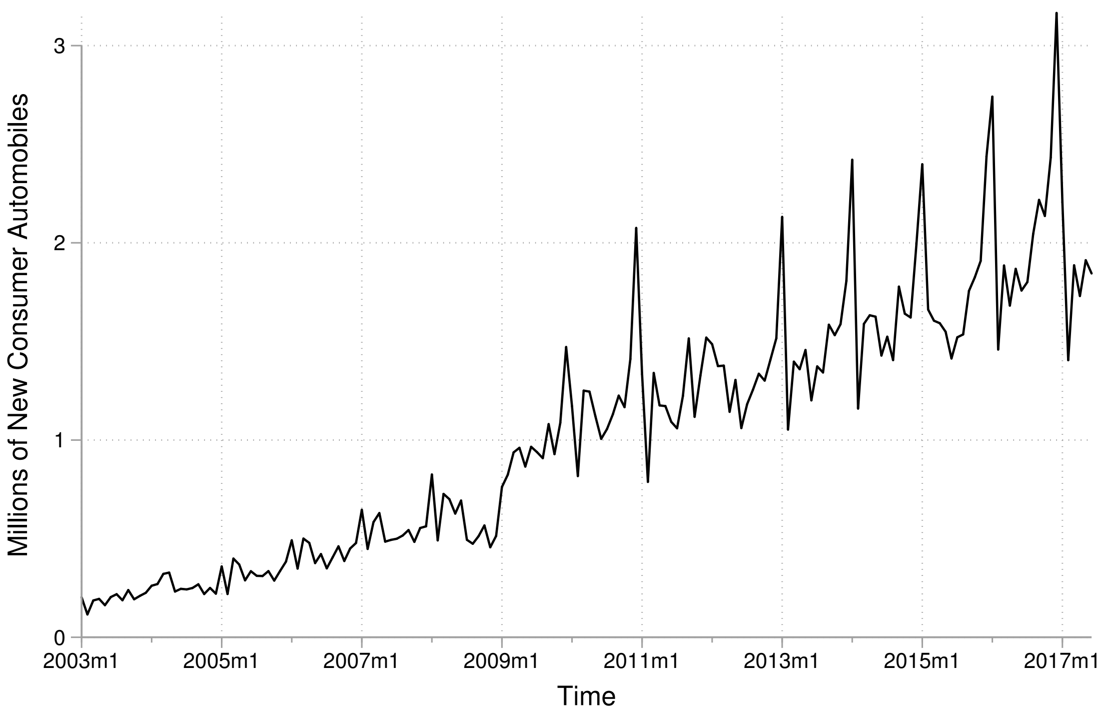
Time-varying macroeconomic controls for each city include per capita gross regional product (GRP), resident population, square meters of road per capita, and number of public buses per every ten thousand city residents. They come from National Bureau of Statistics of China published in the China City Statistical Yearbook. For the set of cities where statistical data are missing from the China City Statistical Yearbook, we manually collected the required data from the statistical reports and yearbooks of the respective cities.

We also impute a measure of homeownership rate using birthplace status and age, based on data from three household surveys in eleven waves, including the 2000 Census, the Chinese General Social Survey (CGSS), and the China Family Panel Studies (CFPS), which provides information

[19]The majority of the automobiles in the dataset are covered by comprehensive insurance. Alternatively, we use the exact purchase price for these covered automobiles and use the average purchase price for those remaining; this does not change our results in any meaningful way.

on birthplace and age, as well as homeownership status of individuals across the sample years. Combining information from these household surveys, we regress homeownership rates on birthplace status and age group dummies. The predicted value from the regression using the birthplace status and age group information in the CIITC data is the imputed homeownership rate.

Figure 4: National Automobile Purchases in the CIITC Data



Notes: This figure shows the number of all the automobiles purchased in China each month aggregated from our CIITC data.

4 Empirical Results

We report our empirical results in this section. We first report fixed effects regression results to motivate the strong sample association between house prices and spending, controlling for aggregate time trend and city-level time-invariant heterogeneity. Next, we present our main results: the difference-in-differences estimate of the impact of Housing Purchase Restriction policy spillovers on house prices and automobile spending in affected cities. Then, we examine mechanism for the main results and report results on heterogeneity in spending responses across birthplace types, age groups, and imputed rates of homeownership. Lastly, we present robustness checks for our main results, including parallel trend tests and alternative treatment assignment specifications.

4.1 Fixed Effects Regressions Results

Table 5 reports the association between automobile purchases and house prices estimated from panel regressions with city and time fixed effects (Equation 3).

In Panel A, we report fixed effects regression results using all cities in our sample. After controlling for aggregate time trend and time-invariant city-level heterogeneity, there is a strong association between house prices and household spending on automobiles. Depending on whether city-level macroeconomic control variables are added, and on the house price index we use, the result suggests that household spending on automobiles increase by 3.8% - 5.9% if housing price increases by 10%.

In Panel B, we report fixed effects regression results from a subset of smaller cities. Since Tier 1 and Tier 2 cities are fundamentally different from the other smaller cities (Table 1), and since they are usually regulated by House Purchase Restrictions (Table 2 and Table 3), while our focus in the difference-in-differences design is on non-regulated cities, we present fixed effects regression results excluding Tier 1 and Tier 2 cities to coincide with the sample choice of the difference-in-differences design.

Table 5: Spending on Automobiles vs House Price (OLS)

log(Automobile Spending)	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Spending, All Cities					
log(FGXZ house price index, 03-13)	0.463***			0.591***		
	(3.616)			(4.139)		
log(CityRE house price index, 08-17)		0.495***			0.375***	
		(5.700)			(4.179)	
log(Combined house price index, 03-17)			0.527***			0.529***
			(3.991)			(3.859)
Observations	13392	30703	18931	11854	26869	17179
R^2	0.967	0.963	0.961	0.971	0.962	0.963
	Panel B: Spending, Excluding Tier 1 and Tier 2 Cities					
log(FGXZ house price index, 03-13)	0.512***			0.671***		
	(3.209)			(3.921)		
log(CityRE house price index, 08-17)		0.639***			0.508***	
		(8.028)			(6.229)	
log(Combined house price index, 03-17)			0.685***			0.705***
			(4.630)			(4.653)
Observations	9773	26729	13782	8630	22985	12464
R^2	0.954	0.950	0.951	0.959	0.947	0.951
log(Automobile Purchases)	(1)	(2)	(3)	(4)	(5)	(6)
	Panel C: Number of Purchases, All Cities					
log(FGXZ house price index, 03-13)	0.359***			0.499***		
	(3.063)			(3.961)		
log(CityRE house price index, 08-17)		0.358***			0.251**	
		(3.898)			(2.583)	
log(Combined house price index, 03-17)			0.377***			0.398***
			(2.828)			(2.889)
Observations	13392	30703	18931	11854	26869	17179
R^2	0.961	0.958	0.950	0.966	0.956	0.953
Controls				YES	YES	YES
City FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the panel fixed effect regressions of household automobile spending and automobile purchases on three house price indices covering different time periods in China. The sample consists of all cities and the data span from 2003m1 to 2017m6. The dependent variable is log household spending on automobiles in each city in each month in Panels A & B, and the log number of automobiles purchased by households in Panel C. The main explanatory variable is the constant-quality house price index from Fang et al. (2016), covering 2003m1 to 2013m3 in columns (1) and (4), the CityRE house price index, covering a broader set of cities for 2008m1 to 2017m12 in columns (2) and (5), and the combined house price index, covering 2003m1 to 2017m12 in columns (3) and (6), taking data from Fang et al. (2016) and CityRE for the set of cities in Fang et al. (2016), using the Fang et al. (2016) data whenever available. All cities are included in Panel A and C. Only Tier 3 cities are included in Panel B, because these are non-regulated cities and better coincide with the sample choice of the difference-in-differences design. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

The results in Panel B suggest that in smaller cities household spending on automobiles increases by

5.1% to 7.1% if house prices increase 10%. It is worthwhile to note that the response of automobile purchases to higher house prices is larger in smaller cities. This may suggest the importance of the housing wealth channel, given the fact that the homeownership rate is higher in smaller cities than in Tier 1 and Tier 2 cities.

In Panel C, we use all cities in our sample, but focus instead on the number of automobiles purchased. The results show that not only household spending on automobiles, but also the number of automobiles purchased, increases in response to higher house prices. In particular, the number of automobiles purchased increases 2.5% to 5% if house prices rise 10%.

Table 6: Spending on Luxury Cars and SUVs vs House Price (OLS)

Panel A: Luxury Car and SUV Spending									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		log(Automobile Spending)			log(Luxury Automobile Spending)			log(SUV Spending)	
log(FGXZ house price index, 03-13)	0.591*** (4.139)			0.671*** (3.494)			0.670*** (4.718)		
log(CityRE house price index, 08-17)		0.375*** (4.179)			0.715*** (6.778)			0.602*** (4.990)	
log(Combined house price index, 03-17)			0.529*** (3.859)			0.737*** (4.335)			0.651*** (4.437)
Observations	11854	26869	17179	11739	26850	17064	11851	26869	17176
R^2	0.971	0.962	0.963	0.929	0.929	0.933	0.953	0.944	0.955
Panel B: Luxury Car and SUV Purchases									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		log(Automobile Purchases)			log(Luxury Automobile Purchases)			log(SUV Purchases)	
log(FGXZ house price index, 03-13)	0.499*** (3.961)			0.598*** (3.180)			0.661*** (4.575)		
log(CityRE house price index, 08-17)		0.251** (2.583)			0.617*** (5.941)			0.481*** (3.705)	
log(Combined house price index, 03-17)			0.398*** (2.889)			0.627*** (3.606)			0.590*** (3.763)
Observations	11854	26869	17179	11739	26850	17064	11851	26869	17176
R^2	0.966	0.956	0.953	0.939	0.943	0.942	0.954	0.948	0.958
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the panel fixed effect regressions of household spending on luxury automobiles sand SUVs on three house price indices covering different time periods in China. The sample consists of all cities, and the data spans from 2003m1 to 2017m6. The dependent variables are are log household spending on all automobiles in columns (1) to (3), on luxury cars in columns (4) to (6) and on SUVs in columns (7) to (9) in each city in each month in Panel A, and log number of purchases by households of all automobiles in columns (1) to (3), of luxury cars in columns (4) to (6) and of SUVs in columns (7) to (9), in each city in each month. The main explanatory variables are the constant-quality house price index from Fang et al. (2016), covering 2003m1 to 2013m3 in columns (1), (4) and (7), the CityRE house price index, covering a broader set of cities for 2008m1 to 2017m12 in columns (2), (5) and (8), and the combined house price index covering 2003m1 to 2017m12 in columns (3), (6) and (9), taking data from Fang et al. (2016) and CityRE for the set of cities in Fang et al. (2016), using the Fang et al. (2016) data whenever available. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

Having motivated that on the extensive margin there is a positive correlation between automobile purchases and house prices, we now demonstrate that on the intensive margin, households buy more expensive cars when house prices increase.

Table 6 reports the association between purchases of luxury cars and SUVs and housings prices.

In Panel A, the results in Columns (4) to (6) suggest that household spending on luxury cars increases by 6.7% to 7.4% when house prices increase by 10%. Results in Columns (7) to (9) suggest that household spending on SUVs increases by 6%-6.7% when house prices increase by 10%. This estimated association is larger than that of housing price on all-model purchases, as shown in Columns (1) to (3).

In Panel B, instead of looking at household spending on automobiles, we focus on the number of automobiles purchased. Results in Columns (4) to (6) suggest that household purchases of luxury cars increase by 6.0% to 6.3% when house prices rise by 10%. Results in Columns (7) to (9) suggest that household purchases of SUVs increase 4.8% to 6.6% when house prices rise 10%. Again, this effect is larger than the effect of house prices on all-model purchases, as shown in Columns (1) to (3).

4.2 Main Results: Difference-in-Differences using Asset Purchase Restriction Spillovers

Table 7 reports the effects of the House Purchase Restrictions implemented in large cities on house prices and automobile purchases in the neighboring small cities, estimated by our difference-in-differences design described by Equations 1 and 4. The estimated effects are highly statistically significant and economically large.

Results in Column (1) show that the house prices in treated cities (cities less than 250 km from the nearest cities regulated by the House Purchase Restrictions) increases by 6.0% and 9.8% relative to those in cities that are farther away, following the two rounds of regulations, respectively. In contrast, as suggested by Column (5), there is no differential change in rents between treated and control cities, in response to such policy spillover shocks .

Results in Columns (2) and (3) show that, following the two rounds of regulations, household spending on automobiles increases by 17.4% and 10.7% and the number of automobiles purchased increases by 17.9% and 11.8% in treated cities, relative to control cities.

Results in Column (4) suggest that household spending increases by an average of 88 million and 33 million *renminbi* per month, or about US$12 million and US$5 million per month in each treated cities relative to control cities, an economically-sizable increase.

Table 7: DID Estimated Effects of Asset Purchase Restriction Spillovers on House Prices and Automobile Spending

	(1) log(CityRE House Price Index)	(2) log(Automobile Spending)	(3) log(Automobile Purchases)	(4) Automobile Spending	(5) log(CityRE Rent Index)
Treat × Post1	0.060***	0.174***	0.179***	88306569.994***	0.006
	(5.006)	(6.561)	(6.779)	(5.679)	(0.477)
Treat × Post2	0.098***	0.107***	0.118***	32921196.345*	0.006
	(6.243)	(4.045)	(4.288)	(1.746)	(0.378)
Observations	16658	16716	16716	16716	16601
R^2	0.957	0.963	0.959	0.924	0.917
Controls	YES	YES	YES	YES	YES
City FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

t statistics in parentheses

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

Notes: This table reports the difference-in-differences regressions of house prices, household automobile spending, household automobile purchases and rents with respect to shocks from the imposition of housing asset purchase restrictions in neighboring regulated cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m6. The dependent variables are log CityRE house price index in column (1), log household spending on automobiles in each city in each month in column (2), log number of automobiles purchased by households in each city in each month in column (3), raw values of household spending on automobiles in each city in each month in column (4), and log CityRE rent index in column (5). Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest cities regulated by the House Purchase Restrictions. Post1 (Post2) is a dummy that takes the value 1 if the time is after the first (second) round of the House Purchase Restriction shock. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

In Table 8, we show that on the intensive margin, households in treated cities buy more expensive cars in response to the House Purchase Restriction shocks.

In Panel A, results in Columns (2) and (3) show that, household spending on luxury cars increases by 17.4% and 11.5%, and household spending on SUVs increases by 26.7% and 16.3%, in treated cities relative to control cities, following the two rounds of regulations.

In Panel B, results in Columns (2) and (3) show that, the number of luxury cars purchased increases by 17.9% and 14.6%, and the number of SUVs purchased increases by 28.9% and 17.8%, in treated cities relative to control cities, following the two rounds of regulations.

These effects are generally larger than those on purchases of all car models, as shown in Column (1) of Panels A and B, especially during the spillover shocks from the second round of asset purchase restrictions.

Table 8: DID Estimated Effects of Asset Purchase Restriction Spillovers on the Intensive Margin of Automobile Spending

Panel A: Spending on Luxury Cars and SUVs			
	(1) log(Automobile Spending)	(2) log(Luxury Automobile Spending)	(3) log(SUV Spending)
Treat × Post1	0.174***	0.174***	0.267***
	(6.561)	(4.964)	(8.477)
Treat × Post2	0.107***	0.115***	0.163***
	(4.045)	(2.790)	(5.290)
Observations	16716	16715	16716
R^2	0.963	0.929	0.944
Panel B: Purchases of Luxury Cars and SUVs			
	(1) log(Automobile Purchases)	(2) log(Luxury Automobile Purchases)	(3) log(SUV Purchases)
Treat × Post1	0.179***	0.179***	0.289***
	(6.779)	(5.254)	(8.933)
Treat × Post2	0.118***	0.146***	0.178***
	(4.288)	(3.706)	(5.610)
Observations	16716	16715	16716
R^2	0.959	0.945	0.945
Controls	YES	YES	YES
City FE	YES	YES	YES
Time FE	YES	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the difference-in-differences regressions of household spending on luxury automobiles and SUVs with respect to shocks from the imposition of housing asset purchase restrictions in neighboring regulated cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m6. The dependent variables are log household spending on all-model automobiles in column (1), on luxury cars in column (2) and on SUVs in column (3) for each city and each month in Panel A, and log number of all-model automobiles in column (1), of luxury cars in column (2) and of SUVs in column (3), purchased by households in each city in each month in Panel B. Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest cities regulated by the House Purchase Restrictions. Post1 (Post2) is a dummy that takes the value 1 if the time is after the first (second) round of the House Purchase Restriction shock. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

4.3 Heterogeneity in Spending Responses Across Consumer Types

4.3.1 Individuals Born Locally Versus Migrants

To demonstrate that the housing wealth effect is significant in influencing household automobile purchases, we first present evidence that the automobile purchases of individuals born in the cities where they live, who are more likely to be homeowners, are more responsive to house prices than those of migrants, who are less likely to be homeowners. We then show that the automobile purchases of certain age groups are more affected by house prices than others.

Table 9 reports fixed effects OLS regression results from the specification in Equation 12. Columns (1),(2),(4),(5),(7) and (8) are fixed effects regressions based on all cities in our sample. Column (3),(6), and (9) are fixed effects regressions based on the sample excluding Tier 1 and Tier 2 cities.

Table 9: OLS Heterogeneity in Spending Responses: Individuals Born Locally vs. Migrants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					log(Automobile Spending)				
	All	All	Tier 3	All	All	Tier 3	All	All	Tier 3
log(FGXZ HPI, 03-13)	0.239*	0.379**	0.282						
	(1.725)	(2.514)	(1.626)						
log(FGXZ HPI, 03-13)	0.343**	0.262*	0.510***						
× Born in Current City	(2.198)	(1.870)	(3.171)						
log(CityRE HPI, 08-17)				0.418***	0.293**	0.320*			
				(3.725)	(2.514)	(1.880)			
log(CityRE HPI, 08-17)				0.120	0.146	0.371			
× Born in Current City				(0.625)	(0.724)	(1.276)			
log(Combined HPI, 03-17)							0.341**	0.366***	0.333**
							(2.455)	(2.683)	(2.118)
log(Combined HPI, 03-17)							0.290*	0.196	0.482***
× Born in Current City							(1.954)	(1.538)	(3.024)
Observations	27015	23654	17172	59548	52103	44333	38019	34237	24773
R^2	0.794	0.814	0.763	0.561	0.551	0.440	0.784	0.798	0.747
Controls		YES	YES		YES	YES		YES	YES
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Immigrants FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the heterogeneity results comparing individuals born locally and migrants in the panel fixed effect regressions of household automobile spending on house prices. The sample consists of all cities, and the data span from 2003m1 to 2017m6. Regressions at at the city, birthplace group, and month level. The dependent variables are log household spending on automobiles, measured for each city, each birthplace group in each month. The explanatory variable is the constant-quality house price index from Fang et al. (2016), covering 2003m1 to 2013m3 in columns (1) to (3), the CityRE house price index, covering a broader set of cities for 2008m1 to 2017m12 in columns (4) to (6), and the combined house price index, covering 2003m1 to 2017m12 in columns (7) to (9), taking data from Fang et al. (2016) and CityRE for the set of cities in Fang et al. (2016), using the Fang et al. (2016) data whenever available. All cities are included in columns (1), (2), (4), (5), (7), and (8). Only Tier 3 cities are included in columns (3), (6), and (9), because these are non-regulated cities and better coincide with the sample choice of the difference-in-differences design. Born in Current City is a dummy variable that takes the value 1 if the automobile purchase is made by individuals born in the city they reside in. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

The OLS birthplace heterogeneity results motivates that the spending of individuals born locally, who are more likely to be homeowners, is significantly more responsive to house prices than the spending of migrants, who are less likely to be homeowners. The coefficients on the interaction term between house prices and Born in Current City across all nine specifications in Table 9 are economically large: Individuals born locally have a 29% to 180% higher estimated association between automobile spending and house prices than migrants. The coefficients on the interaction term between house prices and Born in Current City are larger in the sample excluding Tier 1 and Tier 2 cities, which is more consistent with the main difference-in-differences design. When using only the CityRE housing price index, the coefficient estimates on the interaction term are not statistically significant; however the point estimates are generally large.

Table 10 reports the main difference-in-differences heterogeneity results on the spending response of individuals born locally versus that of migrants from the specification in Equation 8. Figure 5 provides a bar chart of the effect of house prices on the automobile spending of individuals born locally versus that of migrants from the DID specification. The results suggest that in treated cities, individuals born locally increase their spending on automobiles by 32.5% and 38.6%, relative to that of individuals born outside, following the two rounds of House Purchase Restrictions.

Table 10: DID Heterogeneity in Spending Responses: Individuals Born Locally vs. Migrants

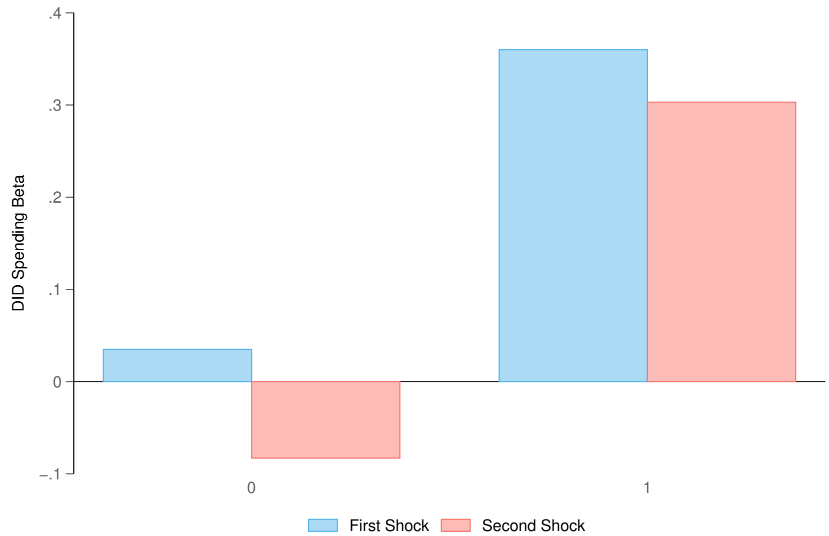
	(1) log(Automobile Spending)	(2) log(Automobile Purchases)
Treat × Post1	0.035	0.037
	(0.416)	(0.434)
Treat × Post1 × Born in Current City	0.325^{**} (2.307)	0.305^{**} (2.122)
Treat × Post2	-0.083	-0.067
	(-1.073)	(-0.885)
Treat × Post2 × Born in Current City	0.386^{***} (2.819)	0.365^{***} (2.624)
Observations	32220	32220
R^2	0.472	0.447
Controls	YES	YES
City FE	YES	YES
Time FE	YES	YES
Immigrants FE	YES	YES

t statistics in parentheses

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

Notes: This table reports the heterogeneity results comparing individuals born locally and migrants in the difference-in-differences regressions of household automobile spending with respect to shocks from the imposition of restrictions on housing asset purchases in neighboring regulated cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m6. Regressions are at the city, birthplace group, and month level. The dependent variables are log household spending on automobiles in column (1) and log number of automobiles purchased by households in column (2), measured for each city, each birthplace group in each month. Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest cities regulated by the House Purchase Restrictions. Post1 (Post2) is a dummy that takes the value 1 if the time is after the first (second) round of the House Purchase Restriction shock. Born in Current City is a dummy variable that takes the value 1 if the automobile purchase is made by individuals born in the city they reside in. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

Figure 5: DID Heterogeneity in Spending Responses: Individuals Born Locally Versus Migrants



Notes: This figure plots the differential average spending responses estimated in Table 10, where we compare difference-in-differences estimates of spending responses to housing booms following the spillover shock for individuals born in the current city they reside in and for individuals born outside of the city they reside in.

Furthermore, across individuals with different birthplaces, we find a significant increase in average spending only among those born in the same city where they live. We find no increase in average spending for individuals born outside of the city where they currently live. This suggests that capital leakage from the housing market policy spillovers affects automobile spending through increasing the spending of locals, instead of through re-classifying the spending of out-of-town buyers or new migrants.

4.3.2 Age Groups

Next, we compare across consumers belonging to different age groups to sheds further light on the source of the consumer spending increase. The results for heterogeneity in spending responses to the house price boom in affected non-regulated cities across age groups are presented below. Table 11 reports results from the OLS age heterogeneity specification in Equation 13, and Table 12 reports the results from the difference-in-differences age heterogeneity specification in Equation 11. Figures 6 and 7 provide a convenient graphical summary of the OLS and DID estimated age profiles in Tables 11 and 12 of the spending responses of different age groups to higher house prices.

Table 11: OLS Heterogeneity in Spending Responses: Age Groups

	(1)	(2)	(3)
		log(Automobile Spending)	
	HPI = FGXZ house price index (03-13)	HPI = CityRE house price index (08-17)	HPI = Combined house price index (03-17
log(HPI)	0.129	0.277**	0.046
	(1.033)	(2.599)	(0.347)
log(HPI)×Age Group 2 (25-29)	0.298***	0.234***	0.475***
	(10.176)	(7.219)	(12.011)
log(HPI)×Age Group 3 (30-34)	0.210***	0.145***	0.289***
	(6.613)	(3.813)	(7.738)
log(HPI)×Age Group 4 (35-39)	0.254***	-0.007	0.220***
	(6.086)	(-0.167)	(5.260)
log(HPI)×Age Group 5 (40-44)	0.386***	-0.041	0.330***
	(7.327)	(-0.884)	(6.457)
log(HPI)×Age Group 6 (45-49)	0.755***	0.016	0.712***
	(11.224)	(0.334)	(10.783)
log(HPI)×Age Group 7 (50-54)	0.513***	0.128**	0.665***
	(8.120)	(2.241)	(9.829)
log(HPI)×Age Group 8 (55-64)	0.845***	0.191***	0.830***
	(10.133)	(3.039)	(9.820)
Observations	94911	214978	137511
R^2	0.944	0.928	0.940
Controls	YES	YES	YES
City FE	YES	YES	YES
Time FE	YES	YES	YES
Age Group FE	YES	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the heterogeneity results comparing individuals in different age groups in the panel fixed effect regressions of household automobile spending on house prices. The sample consists of all cities, and the data span from 2003m1 to 2017m6. Regressions are at the city, age group, and month level. The dependent variable is log household spending on automobiles, measured for each city, each age group in each month. The explanatory variables are the constant-quality house price index from Fang et al. (2016), covering 2003m1 to 2013m3 in column (1), the CityRE house price index, covering a broader set of cities for 2008m1 to 2017m12 in column (2), and the combined house price index, covering 2003m1 to 2017m12 in column (3), taking data from Fang et al. (2016) and CityRE for the set of cities in Fang et al. (2016), using the Fang et al. (2016) data whenever available. Age Group 2 - Age Group 8 are dummy variables labeling which age group the automobile buyers are in. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

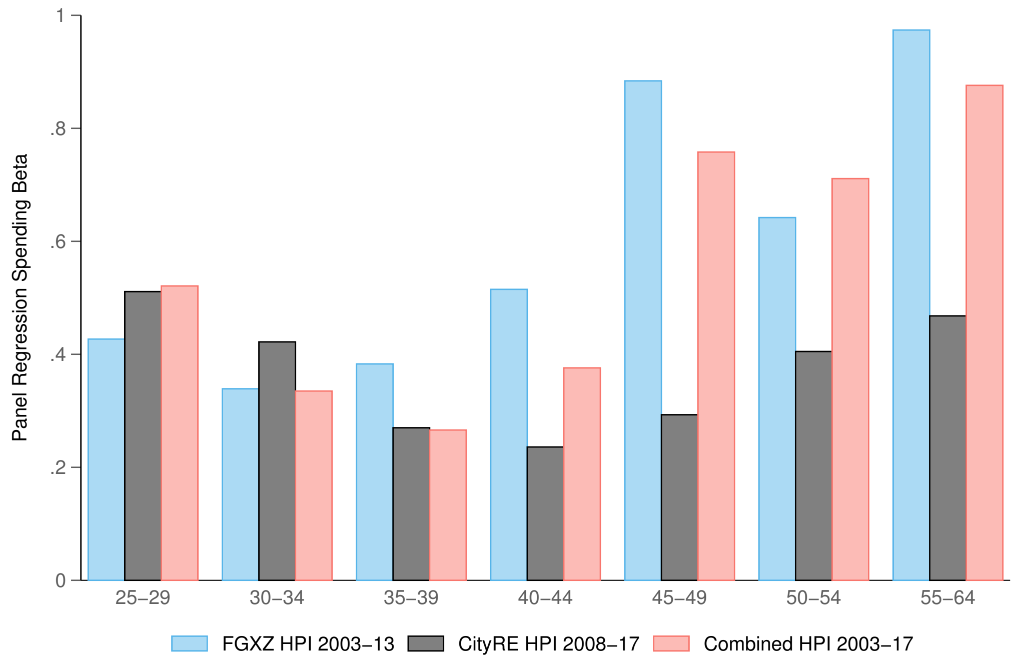
Table 12: DID Heterogeneity in Spending Responses: Age Groups

	(1) log(Automobile Spending)	(2) log(Automobile Purchases)
Treat×Post1	0.433***	0.408***
	(11.404)	(10.805)
Treat×Post2	0.154***	0.148***
	(4.589)	(4.422)
Treat×Post1×Age Group 2 (25-29)	-0.005	0.003
	(-0.299)	(0.199)
Treat×Post1×Age Group 3 (30-34)	-0.228***	-0.216***
	(-9.578)	(-9.208)
Treat×Post1×Age Group 4 (35-39)	-0.406***	-0.373***
	(-16.125)	(-15.102)
Treat×Post1×Age Group 5 (40-44)	-0.468***	-0.407***
	(-19.247)	(-16.759)
Treat×Post1×Age Group 6 (45-49)	-0.325***	-0.258***
	(-12.396)	(-9.779)
Treat×Post1×Age Group 7 (50-54)	-0.258***	-0.198***
	(-8.171)	(-6.285)
Treat×Post1×Age Group 8 (55-64)	-0.481***	-0.471***
	(-11.893)	(-11.667)
Treat×Post2×Age Group 2 (25-29)	0.137***	0.133***
	(7.706)	(7.559)
Treat×Post2×Age Group 3 (30-34)	0.013	0.008
	(0.571)	(0.355)
Treat×Post2×Age Group 4 (35-39)	-0.160***	-0.145***
	(-6.707)	(-6.097)
Treat×Post2×Age Group 5 (40-44)	-0.297***	-0.250***
	(-12.106)	(-10.318)
Treat×Post2×Age Group 6 (45-49)	-0.091***	-0.038
	(-3.499)	(-1.453)
Treat×Post2×Age Group 7 (50-54)	0.006	0.056*
	(0.210)	(1.817)
Treat×Post2×Age Group 8 (55-64)	0.028	0.024
	(0.771)	(0.651)
Observations	133728	133728
R^2	0.925	0.930
Controls	YES	YES
City FE, Time FE, Age Group FE	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

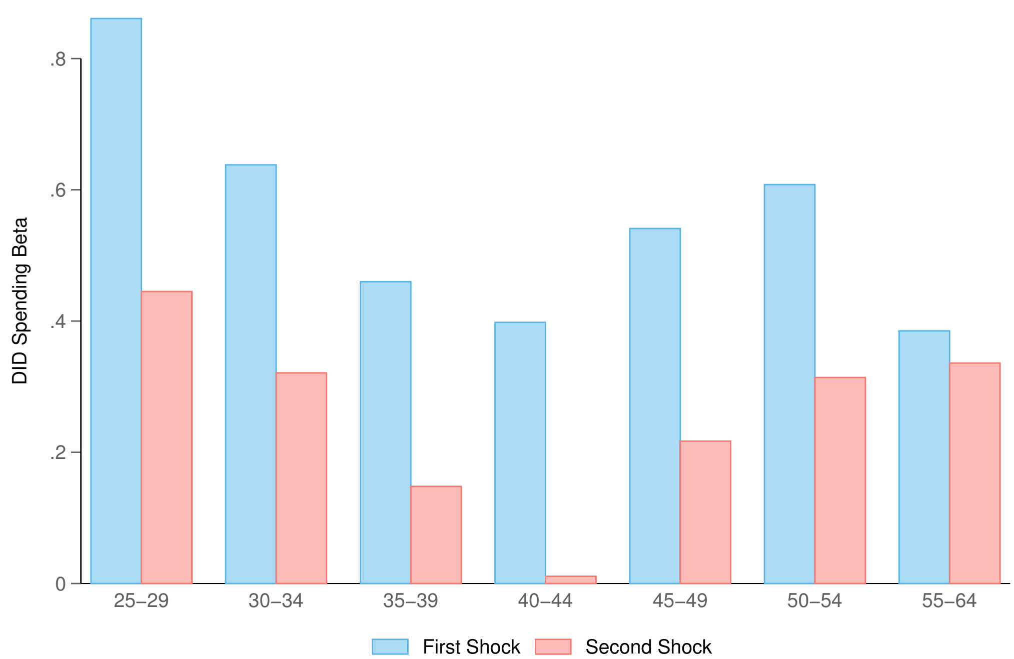
Notes: This table reports the heterogeneity results comparing individuals in different age groups in the difference-in-differences regressions of household automobile spending with respect to shocks from the imposition of housing asset purchase restrictions in neighboring regulated cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m6. Regressions at at the city, age group, and month level. The dependent variables are log household spending on automobiles in column (1) and log number of automobiles purchased by households in column (2), measured for each city, each age group in each month. Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest cities regulated by the House Purchase Restrictions. Post1 (Post2) is a dummy that takes the value 1 if the time is after the first (second) round of the House Purchase Restriction shock. Age Group 2 - Age Group 8 are dummy variables labeling which age group are the automobile buyers in. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

Figure 6: OLS Heterogeneity in Spending Responses: Age Groups



Notes: This figure plots the differential average spending responses across the age groups estimated in Table 11, where we compare the estimated coefficients from panel regressions for different age groups between spending and house prices.

Figure 7: DID Heterogeneity in Spending Responses: Age Groups



Notes: This figure plots the differential average spending responses across the age groups estimated in Table 12, where we compare the difference-in-differences estimate of spending responses to house price booms following the Housing Purchase Restriction policy spillover shock for individuals in different age groups.

Across individuals of different age groups, we find a U-shaped relationship between age and spending responses to the exogenous house price shock. The smallest spending response is found among individuals in the age ranges of 35 to 39 and 40 to 44. This can be explained by the fact that these age groups are expected to upgrade in their housing, whereas younger groups are expected to inherit housing from their parents, and older groups are expected to downsize.

Given the U-shaped relationship between age and spending response, the welfare implication of the spillover-induced surge in house prices is unclear, in the sense that these price surges led to large redistributions across individuals in the affected cities, with potentially negative effects for mid-age renters or owners looking to upgrade. In this way, our birthplace and age heterogeneity results also provide for the first time, to the best of our knowledge, empirical support for the model prediction in Favilukis and Van Nieuwerburgh (2017) on the redistributive effect of out-of-town demand on city residents.

Furthermore, the age heterogeneity in spending responses that we find is unlikely to be driven entirely by loosening borrowing constraints. The market for HELOCs and mortgage refinancing is underdeveloped in China overall. Moreover, home equity borrowing is unavailable to individuals age 55 or above, while we find large spending responses for this age group. Expected downsizing better explains the estimated spending responses.

4.3.3 Imputed Homeownership Rates

Our analysis of homeownership and the housing wealth channel hinges on how well birth place status and age at time of automobile purchase are a proxy for homeownership. To further validate our choice of birthplace status and age as a proxy for homeownership, we provide two robustness checks. Firstly, we utilize household survey data on homeownership, and test how well birthplace status and age predict homeownership. Next, we impute the rate of homeownership, by combining information from surveys with birthplace status and age-group information in the CIITC data, and then test the association between the imputed homeownership rate and spending responses to house prices.

In the first stage, we combine eleven waves of household surveys in China, including the 2000 Census, the 2008, 2010, 2011, 2012, 2013, 2015 waves of the Chinese General Social Survey (CGSS), and the 2010, 2012, 2014, 2016 waves of the China Family Panel Studies (CFPS), and test the relationship between birthplace status, age and homeownership. To the best of our knowledge, these are the only publicly accessible, nationally representative surveys in China that provides information on birthplace and age, as well as homeownership status of individuals across the sample years.

Table 13: First Stage: Survey-Imputed Homeownership

	(1) Homeownership		
Born in Current City	0.161*** (22.465)	Born in Current City × Survey Year	0.002** (2.448)
Age Group 2 (25-29)	-0.044*** (-3.122)	Age Group 2 (25-29) × Survey Year	0.007*** (5.707)
Age Group 3 (30-34)	-0.015 (-1.067)	Age Group 3 (30-34) × Survey Year	0.009*** (7.058)
Age Group 4 (35-39)	-0.033** (-2.414)	Age Group 4 (35-39) × Survey Year	0.013*** (10.733)
Age Group 5 (40-44)	-0.014 (-0.996)	Age Group 5 (40-44) × Survey Year	0.013*** (10.831)
Age Group 6 (45-49)	0.021 (1.553)	Age Group 6 (45-49) × Survey Year	0.013*** (10.509)
Age Group 7 (50-54)	0.016 (1.126)	Age Group 7 (50-54) × Survey Year	0.015*** (11.896)
Age Group 8 (55-64)	0.108*** (8.395)	Age Group 8 (55-64) × Survey Year	0.009*** (8.486)
Survey Year	-0.010*** (-10.778)		
Observations		233887	
R^2		0.070	

t statistics in parentheses

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

Notes: The sample consists of individuals surveyed in the 2000 Census, the 2008, 2010, 2011, 2012, 2013, 2015 waves of the Chinese General Social Survey (CGSS), and the 2010, 2012, 2014, 2016 waves of the China Family Panel Studies (CFPS). Homeownership is a dummy that takes the value of 1 if the surveyed individual or her immediate family (parents, spouse, children) owns a home in the current city. Born in Current City is a dummy variable that takes the value 1 for individuals born in the city they currently reside in. Survey is the year of the survey, capturing time trend. Age Group 2 - Age Group 8 are dummy variables labeling which age group the individual is in. Regression is weighted by sample weights provided by the surveys, normalized by sample size of each survey.

Table 13 reports the results from the first stage regression of homeownership status on whether an individual is born in the city she currently resides in, age, the sample year, and interaction of the sample year with birthplace status and age. The homeownership rate of locally-born households is 16.1% higher than that of migrants. Except for age group 2 (between 25 and 30), which seems slightly anomalous, age groups 4 and 5 (between 35 and 45) do indeed have the lowest homeownership rate. The first-stage estimates suggests that birthplace status and age do indeed significantly predict homeownership status, providing verification for our use of birth place status and age to proxy for homeownership in the CIITC data.

In the second step, we combine estimates from first-stage regression and the birthplace status and age-group information in the CIITC data to impute homeownership rates for consumer groups in the CIITC data, and then test whether this imputed measure of homeownership is positively associated with automobile spending responses to house price appreciation. Table 14 reports the fixed effects OLS regression results of automobile spending responses to house prices using our survey-imputed homeownership measure. Columns (1), (2), (4), (5), (7) and (8) are regressions based on all cities in our sample. Columns (3), (6) and (9) are regressions based on the sample excluding Tier 1 and Tier 2 cities, which are more aligned with the sample choice of the difference-in-differences design.

Table 14: OLS Heterogeneity in Spending Responses: Survey-Imputed Homeownership

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					log(Automobile Spending)				
	All	All	Tier 3	All	All	Tier 3	All	All	Tier 3
Ownership	-2.520***	-1.876***	-1.617**	0.242	0.503	-7.130	-2.077***	-1.420**	-1.271
	(-3.947)	(-2.921)	(-2.135)	(0.051)	(0.103)	(-1.068)	(-3.097)	(-2.169)	(-1.649)
log(FGXZ HPI, 03-13)	-0.700	-0.272	-0.905*						
	(-1.594)	(-0.592)	(-1.735)						
log(FGXZ HPI. 03-13) × Ownership	1.637***	1.189*	2.032***						
	(2.766)	(1.880)	(2.873)						
log(CityRE HPI, 08-17)				0.473	0.355	-0.228			
				(1.196)	(0.878)	(-0.393)			
log(CityRE HPI, 08-17) × Ownership				-0.166	-0.163	0.773			
				(-0.306)	(-0.291)	(0.990)			
log(Combined HPI, 03-17)							-0.373	-0.136	-0.649
							(-0.944)	(-0.346)	(-1.422)
log(Combined HPI, 03-17) × Ownership							1.188**	0.813	1.631***
							(2.252)	(1.519)	(2.667)
Observations	201686	177506	126478	456731	395545	333457	288648	261322	186438
R^2	0.516	0.510	0.393	0.424	0.411	0.288	0.548	0.537	0.421
Controls		YES	YES		YES	YES		YES	YES
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the heterogeneity results with respect to homeownership rates imputed using birthplace and age information in the panel fixed effect regressions of household automobile spending on house prices. The sample consists of all cities, and the data span from 2003m1 to 2017m6. Regressions at at the city, birthplace group, age group, and month level. Ownership is the imputed homeownership rate for each birth place by age group, predicted by the first stage regression in Table 13. The dependent variable is log household spending on automobiles, measured for each city, each birthplace by age group in each month. The explanatory variable is the constant-quality house price index from Fang et al. (2016), covering 2003m1 to 2013m3 in columns (1) to (3), the CityRE house price index, covering a broader set of cities for 2008m1 to 2017m12 in columns (4) to (6), and the combined housing price index, covering 2003m1 to 2017m12 in columns (7) to (9), taking data from Fang et al. (2016) and CityRE for the set of cities in Fang et al. (2016), using the Fang et al. (2016) data whenever available. All cities are included in columns (1), (2), (4), (5), (7), and (8). Only Tier 3 cities are included in columns (3), (6), and (9), because these are non-regulated cities and better coincide with the sample choice of the difference-in-differences design. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

We see that, the automobile spending responses to rising house prices is generally stronger the higher the imputed homeownership rate. Excluding Tier 1 and Tier 2 cities, the result suggests that a 10% rise in house prices is associated with a 5.4% to 11.3% increase in automobile spending for homeowners. In contrast, the same rise in house prices is associated with a 2.3% to 9.1%

decrease in automobile spending for renters. These results are consistent with the hypothesis that the housing wealth effect drives the response of spending to house prices in our sample.

4.4 Robustness: Continuous Distance Specification

To address the concern that the choice of 250 km as the cut-off value for distance to determine the treatment group and the control group is arbitrary, we conduct two robustness tests. First, we estimates how the distance from the nearest regulated city affects the response in a city's house prices and automobile purchases following the House Purchase Restrictions, according to Equations 2 and 5. Tables 15 and 16 report results from these estimations.

Table 15: DID Estimated Effects of Asset Purchase Restriction Spillovers on House Prices and Automobile Spending: Continuous Distance Specification

	(1) log(CityRE House Price Index)	(2) log(Automobile Spending)	(3) log(Automobile Spending)	(4) Automobile Spending	(5) log(CityRE Rent Index)
log Distance × Post1	-0.040***	-0.096***	-0.097***	-4.529e+07***	0.005
	(-5.123)	(-6.433)	(-6.493)	(-4.636)	(0.812)
log Distance × Post2	-0.064***	-0.056***	-0.059***	-4781984.077	0.012
	(-6.821)	(-3.538)	(-3.512)	(-0.455)	(1.501)
Observations	16658	16716	16716	16716	16601
R^2	0.957	0.963	0.959	0.924	0.917
Controls	YES	YES	YES	YES	YES
City FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the robustness of the main difference-in-differences estimates of the house price and household spending responses to using distance from the nearest regulated city as a continuous variable for assigning the treatment status of cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m6. The dependent variables are log CityRE house price index in column (1), log household spending on automobiles in each city in each month in column (2), log number of automobiles purchased by households in each city in each month in column (3), raw values of household spending on automobiles in each city in each month in column (4), and log CityRE rent index in column (5). Distance is the distance of each city from the nearest city regulated by the House Purchase Restrictions. Post1 (Post2) is a dummy that takes the value 1 if the time is after the first (second) round of the House Purchase Restriction shock. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

Table 16: DID Estimated Effects of Asset Purchase Restriction Spillovers on the Intensive Margin of Automobile Spending: Continuous Distance Specification

Panel A: Luxury Car and SUV Spending			
	(1)	(2)	(3)
	log(Automobile Spending)	log(Luxury Automobile Spending)	log(SUV Spending)
log Distance × Post1	-0.096***	-0.110***	-0.143***
	(-6.433)	(-5.596)	(-7.997)
log Distance × Post2	-0.056***	-0.074***	-0.086***
	(-3.538)	(-3.264)	(-4.884)
Observations	16716	16715	16716
R^2	0.963	0.929	0.944
Panel B: Luxury Car and SUV Purchases			
	(1)	(2)	(3)
	log(Automobile Purchases)	log(Luxury Automobile Purchases)	log(SUV Purchases)
log Distance × Post1	-0.097***	-0.113***	-0.154***
	(-6.493)	(-5.909)	(-8.263)
log Distance × Post2	-0.059***	-0.094***	-0.096***
	(-3.512)	(-4.395)	(-5.169)
Observations	16716	16715	16716
R^2	0.959	0.945	0.945
Controls	YES	YES	YES
City FE	YES	YES	YES
Time FE	YES	YES	YES

t statistics in parentheses

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

Notes: This table reports the robustness of the main difference-in-differences estimates of the intensive margin of the household spending responses to using distance from the nearest regulated city as a continuous variable for assigning the treatment status of cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m6. The dependent variables are log household spending on all-model automobiles in column (1), on luxury cars in column (2) and on SUVs in column (3) for each city and each month in Panel A, and log number of all-model automobiles in column (1), of luxury cars in column (2) and of SUVs in column (3), purchased by households in each city in each month in Panel B. Distance is the distance of each city from the nearest city regulated by the House Purchase Restrictions. Post1 (Post2) is a dummy that takes the value 1 if the time is after the first (second) round of the House Purchase Restriction shock. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

According to the continuous distance specification results in Tables 15 and 16, we see that as distance from the nearest regulated city increases, the responses in house prices and automobile purchases following the House Purchase Restrictions become weaker and weaker. This suggests that our difference-in-differences results are unlikely to be driven by arbitrary choices of discrete cut-offs.

4.5 Robustness: Alternative Distance cut-offs

Second, we set the cut-off value to be 300 km, 200 km, and 150 km and then verify that different choices of cut-off value indeed cause little change in our estimation results.

According to Tables 17 and 18, different choices of the cut-off value results in no change to the estimation results. Compared with the baseline difference-in-differences estimates using a cut-off value of 250km, the estimated effects of spillovers from the housing asset purchase restriction on house prices, rents, spendings on all models of automobiles, luxury automobiles and SUVs are all quantitatively unchanged, and confirms that the spillover shock leaves rents unchanged in the affected non-regulated cities, but significantly increases house prices and the extensive and the intensive margins of automobile purchases.

The difference-in-differences heterogeneity results are also largely unchanged using different values of cut-off distance, as shown in Table 18. The robustness results for age heterogeneity are omitted due to space, and are available upon request. For the heterogeneity in spending responses to spillover shocks between individuals born locally (who are more likely homeowners) and migrants (who are more likely to be renters), the coefficient estimates on automobile spending for the interaction term between the spillover shock and Born in Current City is, in fact, even larger under the alternative cut-off distances.

Table 17: DID Estimated Effects of Asset Purchase Restriction Spillovers on House Prices and Automobile Spending: Alternative Distance Cutoffs

	(1) log(CityRE House Price Index)	(2) log(Automobile Spending)	(3) Automobile Spending	(4) log(CityRE Rent Index)	(5) log(Luxury Automobile Spending)	(6) log(SUV Spending)
			Cutoff Distance=300 km			
Treat × Post1	0.059***	0.183***	88250457.139***	0.000	0.187***	0.279***
	(5.012)	(6.739)	(5.528)	(0.004)	(5.160)	(8.664)
Treat × Post2	0.096***	0.109***	20991440.153	-0.007	0.130***	0.161***
	(6.278)	(4.075)	(1.075)	(-0.455)	(3.019)	(5.068)
			Cutoff Distance=200 km			
Treat × Post1	0.059***	0.151***	77549219.925***	0.005	0.198***	0.235***
	(4.529)	(5.507)	(4.839)	(0.399)	(5.889)	(7.268)
Treat × Post2	0.100***	0.090***	19728512.654	0.001	0.145***	0.145***
	(5.908)	(3.297)	(1.082)	(0.062)	(3.679)	(4.663)
			Cutoff Distance=150 km			
Treat × Post1	0.056***	0.106***	52948074.832***	-0.009	0.172***	0.170***
	(3.567)	(3.462)	(2.863)	(-0.746)	(4.639)	(4.812)
Treat × Post2	0.093***	0.076**	5763666.660	-0.017	0.132***	0.112***
	(4.633)	(2.465)	(0.283)	(-1.154)	(3.115)	(3.278)
Controls	YES	YES	YES	YES	YES	YES
City FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the robustness of the main difference-in-differences estimates of the house price and household spending responses to using alternative distance cutoffs of 150 km, 200 km and 300 km for assigning the treatment status of cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m6. The dependent variables are log CityRE house price index in column (1), log household spending on automobiles in each city in each month in column (2), raw values of household spending on automobiles in each city in each month in column (3), log CityRE rent index in column (4), log household spending on luxury automobiles and SUVs, respectively, in each city in each month, in columns (5) and (6). Treat is a dummy that takes the value 1 if the city is closer than 300 km, 200 km, or 150 km from the nearest cities regulated by the House Purchase Restrictions. Post1 (Post2) is a dummy that takes the value 1 if the time is after the first (second) round of the House Purchase Restriction shock. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

Table 18: DID Heterogeneity in Spending Responses: Individuals Born Locally Versus Migrants, Alternative Distance Cutoffs

	(1)	(2)
	log(Automobile Spending)	log(Automobile Purchases)
	Cutoff Distance=300 km	
Treat × Post1 × Born in Current City	0.346***	0.331**
	(2.628)	(2.464)
Treat × Post2 × Born in Current City	0.401***	0.381***
	(3.142)	(2.945)
	Cutoff Distance=200 km	
Treat × Post1 × Born in Current City	0.430**	0.403**
	(2.582)	(2.363)
Treat × Post2 × Born in Current City	0.506***	0.478***
	(3.166)	(2.941)
	Cutoff Distance=150 km	
Treat × Post1 × Born in Current City	0.462**	0.433**
	(2.180)	(1.985)
Treat × Post2 × Born in Current City	0.527**	0.500**
	(2.577)	(2.412)
Controls	YES	YES
City FE	YES	YES
Time FE	YES	YES
Immigrants FE	YES	YES
Treat × Post1	YES	YES
Treat × Post2	YES	YES

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

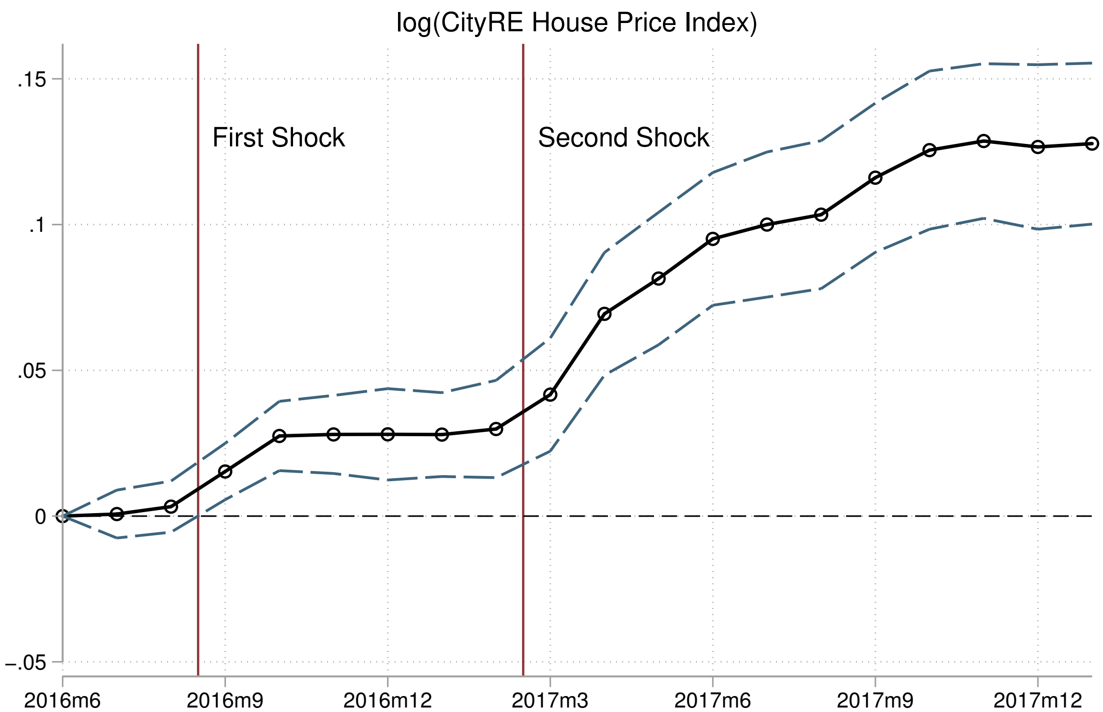
Notes: This table reports the robustness of the difference-in-differences estimates of the heterogeneity in household spending responses between individuals born locally and migrants to using alternative distance cutoffs of 150 km, 200 km and 300 km for assigning the treatment status of cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m6. Regressions at at the city, birthplace group, and month level. The dependent variables are log household spending on automobiles in column (1) and log number of automobiles purchased by households in column (2), measured for each city, each birthplace group in each month. Treat is a dummy that takes the value 1 if the city is closer than 300 km, 200 km, or 150 km from the nearest cities regulated by the House Purchase Restrictions. Post1 (Post2) is a dummy that takes the value 1 if the time is after the first (second) round of the House Purchase Restriction shock. Born in Current City is a dummy variable that takes the value 1 if the automobile purchase is made by individuals born in the city they reside in. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita. Standard errors are clustered at the city level.

4.6 Placebo Tests and Parallel Trends

To show that the difference-in-differences estimates are not driven by underlying differences between treated cities and control cities that existed well before the House Purchase Restrictions were implemented, we conducted the parallel trend tests described by Equation 14.

Figure 8 plots the estimated response of house prices in the treated cities relative to that in control cities before and after the regulations took effect. As the figure makes clear, the increase in house prices following each round of House Purchase Restrictions does not seem to be the result of a pre-treatment trend.

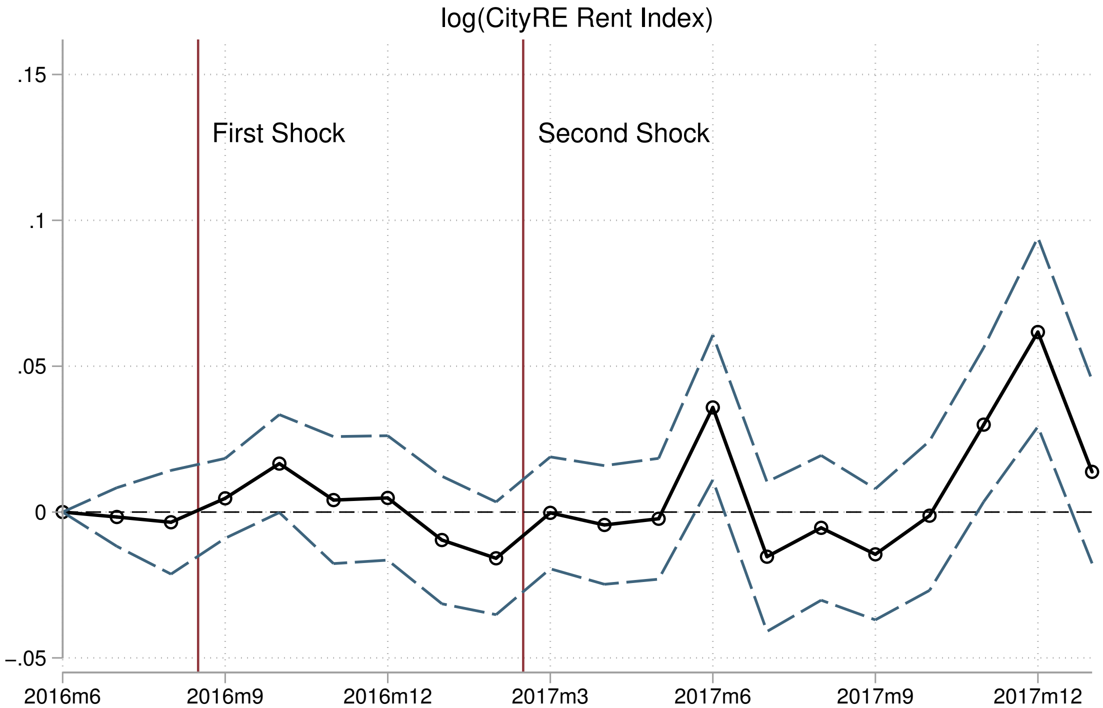
Figure 8: Parallel Trends: House Prices



Notes: This graph plots the coefficient estimates of Equation 14 with $k \in [-15, 15]$, and the outcome variable is log of the CityRE house price index, at the city by month level. It shows the estimated response of house prices in treated cities relative to those in control cities before and after the policy spillover shocks took effect. All the responses are relative to the level of response in June 2016 (i.e. $k = -3$). The second round of restrictions happened in March 2017, which is labeled by a vertical red line. City fixed effects, time(year-month) fixed effects and city-level controls are added. The 95% confidence interval is drawn based on standard errors clustered at the city level.

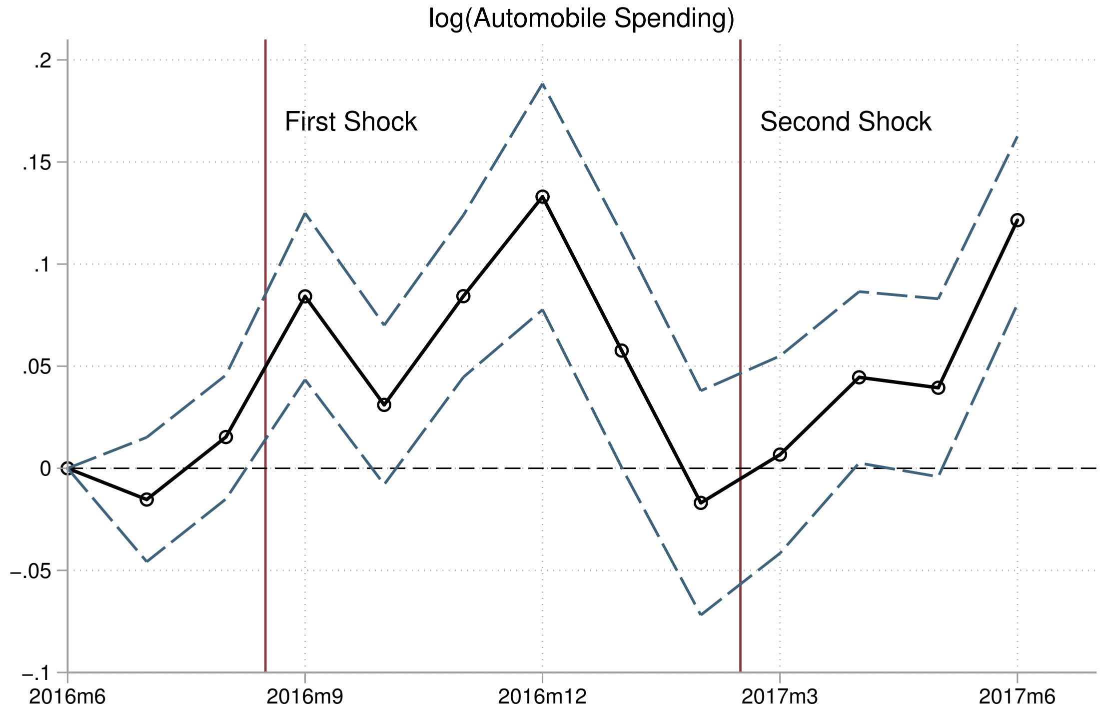
Figure 9 plots the placebo test result for rents. We see that rents respond no differently in the treated cities than in the control group, not only before, but also long after the House Purchase Restriction shocks.

Figure 9: Parallel Trends: Rents



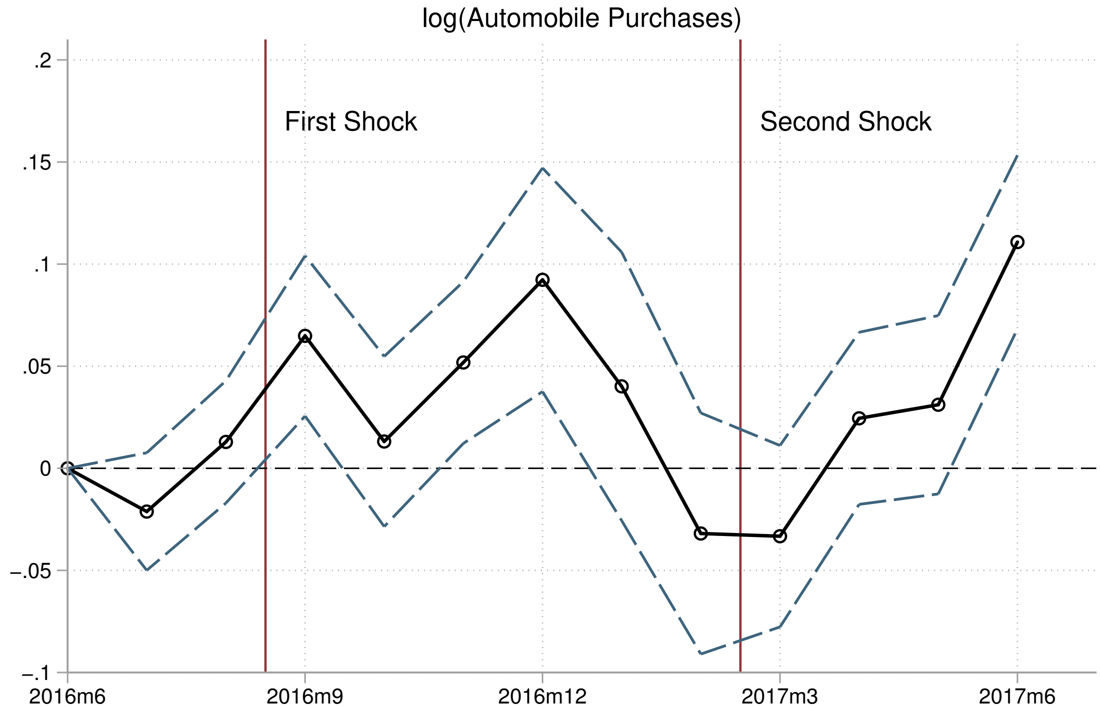
Notes: This graph plots the coefficient estimates of Equation 14 with $k \in [-15, 15]$, and outcome variable is log of the CityRE rent index, at the city by month level.. It shows the estimated response of rents in treated cities relative to those in control cities before and after the policy shocks took effect. All the responses are relative to the level of response in June 2016 (i.e. $k = -3$). The second round of restrictions happened in March 2017, which is labeled by a vertical red line. City fixed effects, time(year-month) fixed effects and city-level controls are added. The 95% confidence interval is drawn based on standard errors clustered at the city level.

Figure 10: Parallel Trends: Value of Automobile Spending



Notes: This graph plots the coefficient estimates of Equation 14 with $k \in [-15, 9]$, and the outcome variable is log of household automobile spending at the city by month level. It shows the estimated response of household automobile spending in treated cities relative to that in control cities before and after the policy shocks took effect. All treatment-control difference estimates are relative to the level of difference in June 2016 (i.e. $k = -3$). The second round of restrictions happened in March 2017, which is labeled by a vertical red line. City fixed effects, time(year-month) fixed effects and city-level controls are added. The 95% confidence interval is drawn based on standard errors clustered at the city level.

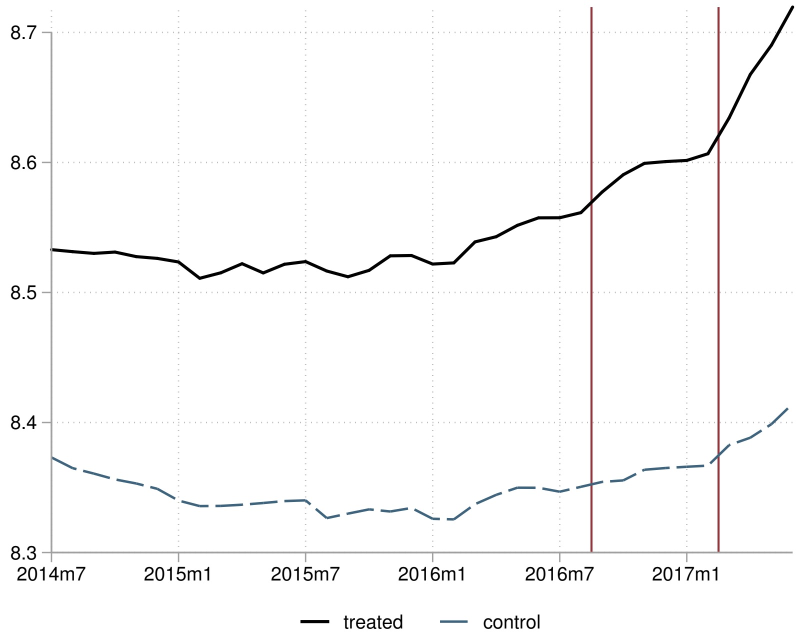
Figure 11: Parallel Trends: Number of Automobile Registrations



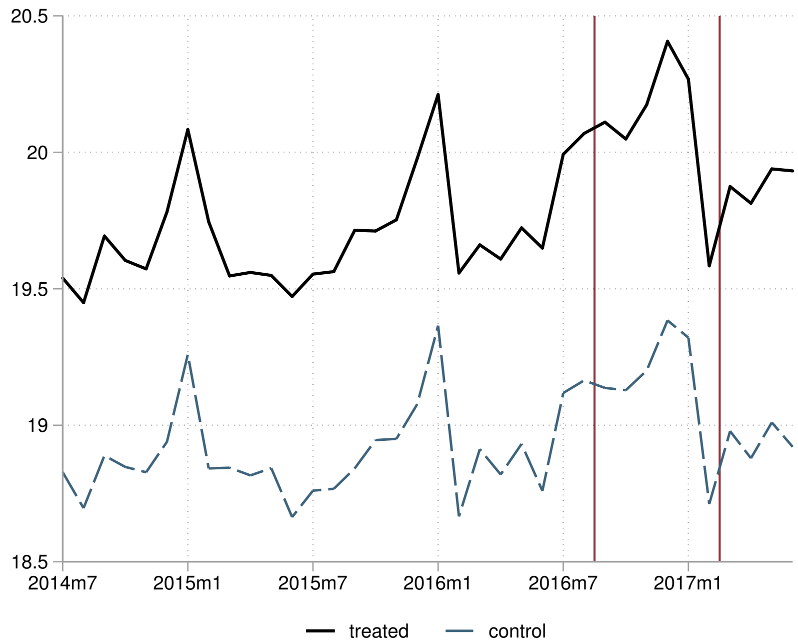
Notes: This graph plots the coefficient estimates of Equation 14 with $k \in [-15, 9]$, and the outcome variable is log of the number of automobiles purchased by households at the city by month level. It shows the estimated response of household automobile purchases in treated cities relative to those of control cities before and after the policy shocks took effect. All treatment-control difference estimates are relative to the level of difference in June 2016 (i.e. $k = -3$). The second round of restrictions happened in March 2017, which is labeled by a vertical red line. City fixed effects, time(year-month) fixed effects and city-level controls are added. The 95% confidence interval is drawn based on standard errors clustered at the city level.

Figure 12: Parallel Trends: Long-Term Past Differences Between Treated and Control Cities

(a) House Prices



(b) Household Spending on Automobiles



Notes: This graph plots for the treated cities and in the control group of cities the long-term grouped means of house prices, in Panel (a), and household spending on automobiles, in Panel (b), for the 36-month period from July 2014 to June 2017. The first round of restrictions happened in September 2016, which is labeled by the first vertical red line from left to right. The second round of restrictions happened in March 2017, which is labeled by second vertical red line from left to right.

Figures 10 and 11 show the results of the parallel trend test for household automobile purchases, described by Equation 15. No significant differences were found prior to the first spillover shock in house prices, spending on automobiles, and the number of automobile purchases. Panels (a) and (b) of Figure 12 shows for a longer period, which includes twenty-four months prior to the first spillover shock, the grouped means of log house prices and log spending, respectively, for the treated and the control groups of cities. Departures between trends of the two groups of cities are only observed after the spillover shocks even after taking into account of longer periods in the past.

Again, as the parallel trends figures make clear, the increase in household automobile purchases following each round of House Purchase Restrictions does not seem to be the result of any pre-treatment trend. Furthermore, the increase in the value of automobile spending, as shown in Figure 10, is larger than the increase in the number of automobile purchases in Figure 11, confirming our finding that consumers increase both the extensive and the intensive margin of automobile purchases.

Finally, we provide evidence consistent with the DID assumption that the purchase restriction spillover shocks impact the treated cities solely through external effects on the local housing market. To do so, we collect local statistics on city-level employment, population and output, and conduct difference-in-differences tests of whether there are differential changes in local employment, population and output between the treatment and control groups of cities coinciding with the timing of the purchase restriction spillover shocks. The local statistics are at the annual level and we assign years after 2016 as the post period. Table 19 shows the results, that after the purchase restriction spillover shocks, local employment, population and output, except for real estate investments, do not systematically differ between treated and control cities, confirming that purchase restriction spillover shocks are solely external shocks to the local housing market in non-regulated cities.

Table 19: Post Treatment Trends of Macroeconomic Variables

	(1) log (GRP)	(2) log (GRP excluding Real Estate Investment)	(3) log (Real Estate Investment)
Treat × Post	-0.010	-0.015	0.217*
	(-0.667)	(-0.908)	(1.698)
Observations	1551	1500	1549
R^2	0.994	0.991	0.887
	(4) New Employment/ Residential Population	(5) log(Residential Population)	(6) Growth of GRP excluding Real Estate Investment
Treat × Post	0.006	0.007	-0.018
	(0.805)	(0.233)	(-1.160)
Observations	1031	1463	1451
R^2	0.660	0.953	0.279
City FE	YES	YES	YES
Time FE	YES	YES	YES

t statistics in parentheses

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

Notes: This table reports the difference-in-difference regressions of several key annual macroeconomic variables with respect to shocks from the imposition of housing asset purchase restrictions in neighboring regulated cities. The sample consists of all non-regulated cities excluding cities in four provinces that were involved in output and investment statistics scandals around the treatment event. The data span from 2012 to 2018. Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest cities regulated by the House Purchase Restrictions. Post is a dummy that takes the value 1 if the time is after or equal to year 2017. GRP abbreviates for city-level gross regional product. Standard errors are clustered at the city level.

5 Discussions

In this section, we further discuss the information contained in the estimates in Section 4. We focus on two issues. First, the value of the marginal propensity to consume (MPC) out of housing wealth implied by our estimates. Second, how the Chinese setting provides new insights into the mechanisms responsible for differences between OLS and quasi-experimental estimates of the MPC.

5.1 Calculation of the MPC

Converting the estimates in our main results to MPC facilitates comparisons with the estimates in the literature regarding the strength of the housing wealth effect, and also gives another concrete view of the economic magnitude of the spending effect of capital leakages in the housing market.

To do so, we first extend the measure of housing wealth in Zhang (2017), constructed using a perpetual inventory method, to our sample period.[20] We then use that information, together with the elasticity and value of automobile spendings, in our main results to compute the MPC.

The difference-in-differences (DID) estimates using purchasing restriction spillovers as a quasi-experiment point to a spending elasticity of 1.77: for every 10% increase in housing prices, other things equal, consumer spending on automobiles rises by 17.7%. Taking into account that the average ratio of annual automobile purchases to housing value for the DID sample period is 0.025, this maps to a causal estimate of the MPC out of housing wealth on automobiles of 0.044: for each 100 *yuan* of housing capital gains, consumers spend an average of 4.4 *yuan* on automobile purchases.

Considering that automobile spending is only part of consumer spending, and that Mian et al. (2013) report that the MPC for automobiles is 43% of the total MPC, our MPC estimate is slightly larger than that of Mian et al. (2013) and Aladangady (2017), which suggests that housing wealth has more influence on consumer spending in China than in the US. This difference could be because housing wealth accounts for a much larger fraction of household wealth in China. Xie and Jin (2015) report that housing wealth accounts for 70% of total household wealth in China, whereas housing wealth accounts for only 30% to 45% of total household wealth in the United States according to Wolff (2016).

5.2 Savings Propensity and Differences in Quasi-Experimental versus OLS Estimates of the MPC

How does the DID estimate of the MPC obtained using spillovers from asset purchase restrictions as quasi-experimental variations compare with the OLS estimate? The OLS estimate of the automobiles MPC is 0.016. This is obtained by multiplying the OLS spending elasticity of house prices of 0.53, reported in Column 3 of Panel (a), Table 5 with the average ratio of annual automobile purchases to housing value for the OLS sample period 0.03. The OLS estimate of the automobile MPC, which is 0.016, when compared with the DID estimate of the automobile MPC, which is 0.044, points to an underestimation of the MPC under the OLS.

What accounts for the underestimation of the MPC under the OLS? Aside from the differences in sample periods, there are two major reasons why the OLS can underestimate the MPC. First, it is widely known that house price indices in China contain substantial measurement errors (Fang et al., 2016). Using a noisy house price index as a dependent variable leads to attenuation bias in

[20] Chian differs from the U.S. in that there are no officially reported household balance sheet aggregates at any level, and researchers must compute their own estimate of the aggregate household balance sheet.

OLS and biases the MPC downward.

More importantly, the OLS can underestimate the MPC because households' savings propensity and housing's asset role as an investment vehicle are important determinants of house prices in China, as in Wei et al. (2012) and Zhang (2017) for example. This causes growth in house prices to depart from permanent income growth. When such a departure is severe, as is potentially the case at hand, instead of the usual permanent income considerations, the OLS estimate contains an omitted variable bias with a negative sign. Savings propensity is an omitted variable that negatively correlates with consumer spending and positively correlates with house prices, biasing the OLS estimate of the MPC downward.

Aligning precisely with our expectation, our quasi-experimental estimate of the MPC contrasts with other work in which the main variation in house prices comes from the savings propensity. For example, Waxman et al. (2018) use city-level credit and debit card data from 2011 to 2013 in China, focusing on differences in house prices driven by differences in savings propensities proxied by sexual imbalance, and find a negative spending elasticity of house prices. Ultimately, the conclusion of the researchers on the true value of the MPC out of housing wealth will depend on its definition, or equivalently, will depend on what variation of house prices to focus on in studying consumer spending. Throughout our analysis, we maintain the definition of the MPC out of housing wealth as the response of consumer spending to changes in housing wealth, holding everything else constant including savings propensity, which is more aligned with the previous literature on the estimation of MPC out of housing wealth.

5.3 Economic Significance of the Overall Spending Response

How large is the overall external spillover effect on spending of the housing purchase restriction policy that is local and motivated only at cooling the internal housing market?

According to a back-of-the-envelope calculation based on our estimates, the spillovers from the imposition of housing asset purchase restrictions have lead to a significant contribution to the increase in automobile purchases during the sample years of 2016 and 2017.

The back-of-the-envelope calculation proceeds as follows. We find that affected non-regulated cities experience an average 66 million yuan increase in household automobile spending per city per month relative to the control group of cities. We have 152 treated cities in our sample, and the length of the treatment window in our estimation is 10 months. This translates to a causal increase of 100.32 billion yuan in household automobile spending. For an average selling price of 140 thousand yuan for private passenger vehicles in the treatment period, this is a causal increase of 717 thousand units in automobile sale.

To further put the size of this effect into perspective, the average annual increase in automobile sale in China in the automotive boom of 2016 and 2017 is 1754 thousand units. Given this calculation, our estimates suggest that the spillover effects from the imposition of housing asset purchase restrictions can explain approximately 40.8% of the average annual increase in private passenger automobile sales in 2016 and 2017. This is a sizable overall spillover effect on spending in external areas, of a local policy that is originally targeted only at internal housing markets in the regulated cities.

6 Conclusion

Using plausibly exogenous spillovers from the imposition of restrictions on housing asset purchases in nearby large cities as quasi-experimental variations, and comparing cities with different exposures to such spillover shocks, we find that the capital leakage from large regulated cities causes a significant rise in house prices in the neighboring non-regulated cities. Furthermore, these plausibly exogenous house price booms in the non-regulated cities lead to a substantial increase in household spending on automobiles.

The capital inflow from the spillover of asset purchase restrictions is estimated to cause an increase of 6% to 9.8% in the house prices of the affected non-regulated cities. Household spending on automobiles in the affected non-regulated cities are also estimated to increase by 10.7% to 17.4% in response to the capital inflow. The quasi-experimental estimates of MPC from housing wealth is larger than the OLS estimate because saving propensities drives simultaneously spending and house prices in China, introducing a negative omitted variable bias in the OLS estimate. This also suggests that the source of house price variations matters in understanding housing wealth effects.

The spending responses to capital leakage from housing market policy spillovers are highly heterogeneous across household types, suggesting strong redistributive effects, consistent with predictions from Favilukis and Van Nieuwerburgh (2017). These results help to explain how capital inflows, caused by the spillover of asset purchase restrictions, lead to house price booms that are arguably exogenous to the affected cities, and how such house price booms differentially impact city residents.

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