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Capital Spillover, House Prices, and Consumer Spending: Quasi-Experimental Evidence from House Purchase Restrictions

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We use a unique quasi-experiment—spillovers from the imposition of purchase restrictions on local housing to nearby unregulated cities—to study the effects of out-of-town housing demand on house prices and consumer spending. While these restrictions effectively stymied the surge in local house prices, they induced capital flight and sharp abnormal increases in house prices in nearby unregulated cities. The effect of the house price increases on consumer spending is positive in the aggregate, but echoing Favilukis and Van Nieuwerburgh (2021), is redistributive, that is, negative for renters and positive for homeowners. (*JEL* E21, G18, R21)

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This paper studies the unintended consequences of regulating housing speculation at the local level. Specifically, we exploit a quasi-natural experiment

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in China, where local authorities imposed restrictions on investment home purchases in 2016 and 2017. While these restrictions were effective in containing the surge in local house prices, they triggered capital flight into nearby, unregulated housing markets. House prices in these unregulated cities rose sharply following the out-of-town home purchases despite no obvious improvement in local housing fundamentals. Consumption spending on automobiles increased following the housing wealth increase.

We use a difference-in-differences design to estimate the spillover effects of the purchase restrictions on housing. In our sample, we split the unregulated prefectural cities into two similarly sized groups, treated cities and control cities, based on each city's distance to the nearest regulated city. The treated cities are within 250 km of the nearest regulated cities. Since the closeness facilitates occasional visits to acquire information on individual houses and to monitor the status of the houses once purchased, treated cities are more likely to attract investors from the regulated cities, and they are more exposed to the capital flight induced by these restrictions.

We find that house prices in the treated cities increase abnormally by 4.0% compared to the control cities within a few months of the implementation of the purchase restrictions, while there is no significant response in rents. An increase in the volumes of home sales is observed in the treated cities with the same order of magnitude as the reduction in home sales in the regulated cities. The intensity of web searches from the regulated cities on the housing markets of the treated cities significantly rises. Bank deposits in the treated cities also significantly and abnormally increase, and the magnitude of the deposit effect is similar to the increases in house prices. We also exploit the heterogeneity in the strength of the shocks from the purchase restrictions in the regulated city that are represented by the magnitude of the decrease in the house price growth and volumes of home sales, and we find that regulated cities with a stronger shock generate stronger spillover effects to the nearby cities. These are the direct consequences and the evidence of the capital flight.

Next, we estimate the spending effects of the surge in house prices driven by the quasi-experiment. We focus on consumer spending on new automobiles, as in Mian, Rao, and Sufi (2013) and Di Maggio et al. (2017). Our baseline estimate indicates that consumer spending on new automobiles increases abnormally by 8.2% on average in the treated cities relative to the control cities in response to the shock. This increase translates to a marginal propensity to consume (MPC) of an automobile on housing wealth of 4.8 cents. Across various specifications, spending on new automobiles spurred by the increased house prices explains one-eighth to one-quarter of the average annual increase in automobile sales during the same period. To arrive at these estimates, we use a new administrative data set that provides precise and comprehensive information on all purchases of new consumer automobiles in China. The data set is registry-based which has the advantage of being free of measurement errors compared to survey data

(Koijen, Van Nieuwerburgh, and Vestman 2014).[1] On the intensive margin, we find an even larger response in household spending on expensive automobiles.

The spending response is strongly redistributive within the treated city and occurs across the locally and non-locally-born population; these two groups are empirically different in homeownership status. After the surge in houses price, we find a significantly large spending response for the locally born group, while essentially a zero spending response for the non-locally-born one. In the survey data, the non-locally-born group is significantly more likely to be renters, and the locally born group is significantly more likely to be homeowners. Subsample pre-trend tests show that these two groups did not have different trends in their automobile spending before the shocks from the purchase restrictions. We apply survey-data-based measures of the imputed likelihood of homeownership of the locally and the non-locally-born population, and find that the spending response to the surge in house prices is significantly positive for homeowners but significantly negative for renters.

The different patterns of spending responses across consumer groups within the treated cities is consistent with a "pure" housing wealth effect channel (Sinai and Souleles 2005; Buiter 2010; Berger et al. 2017; Kaplan, Mitman, and Violante 2020) for which the subset of homeowners whose housing assets are worth more than the discounted value of future housing consumption increase spending, while the subset of renters who plan to buy homes cut back on consumption spending. One possibility under which the "pure" housing wealth effect produces a positive aggregate spending response is that the change in house prices is nonfundamental; that is, house prices increase more than the present value of rents. The aggregate spending response is also enlarged by the already-significantly-large fraction of households that owned more homes than their housing consumption needs. Eighteen percent of Chinese urban households report owning multiple homes during the sample period.

Alternative explanations, such as the permanent income channel, the labor relocation channel, and the collateral channel, cannot explain our findings. The permanent income channel refers to the possibility that improvements in the growth prospects of the treated cities lead to a simultaneous increase in house prices and spending. It predicts relative increases in fundamentals in the treated cities and similarly sized increases in spending for the locally born and the non-locally-born groups, which we do not find. The labor relocation channel posits that the imposition of purchase restrictions leads workers to migrate to nearby cities that leads to increases in spending and house prices. Therefore, it predicts

[1] Koijen, Van Nieuwerburgh, and Vestman (2014) show that, according to registry data, 35% of respondents to a consumption survey in Sweden forget to report the car they bought. Our registry data on automobile spending accurately reflect the exact value of all new car purchases and provide buyer demographic information. These data allow us to complement the studies (Gan 2010; Agarwal and Qian 2017) that use debit and credit card data and cover multiple spending categories but do not cover the spending not through cards. They also address the Aladangady (2017) critique that data on car spending in other studies lack demographic information and require imputed car values.

increases in spending for the non-locally-born group and in fundamentals that we do not find. The collateral channel refers to that increasing house prices enable households to finance their consumption by pledging the more valuable housing assets. It does not predict a negative spending response for renters. Also, in surveys conducted before and after the quasi-experiment, we observe only a small fraction of households that have refinanced mortgages or have home equity lines of credit (HELOCs), and an even smaller fraction of households who have used them to buy cars.

We take several additional steps to ensure the validity of our research design and the robustness of our estimates. To do so, we control for city-specific linear trends in all estimations because the unregulated cities are inherently different, for example, in distances to the regulated cities; and the urban literature has shown that such different initial conditions predict different growth rates for the economy as well as for house prices (Glaeser, Scheinkman, and Shleifer 1995; Blanchard and Katz 1992; Glaeser and Nathanson 2017). Our estimated effects are robust to a "one-step-up" perturbation in the pre-trend assumption as proposed in Bilinski and Hatfield (2019), where we control for city-specific trends using cubic splines instead. We also control for other local factors that potentially influence automobile purchases, any seasonality that could be city specific, as well as the potential exposure of the growth in local output to the nearest regulated city as the regional hub in a hub-and-spoke network. Furthermore, our estimates are robust to alternative treatment designations based on different cutoff distances and railroad travel times, and to the spatial treatment effect that decays continuously. They are also robust to matching control cities to treated cities on criteria like house prices and levels of economic growth. Further, we show that our results hold outside of automobile purchases, by using indexes of web searches for big-ticket consumption goods by consumers in each of the treated and control cities, which serve as an alternative source of data on consumer spending.

Our study makes several contributions to the literature. Recent studies show that spillovers between housing markets are important (DeFusco et al. 2018; Bailey et al. 2018) and that out-of-town investors contribute significantly to surges in house prices (Badarinza and Ramadorai 2018; Cvijanovic and Spaenjers 2018; Sá 2016; Chinco and Mayer 2016; Sakong 2021). We empirically verify a new mechanism that generates spillovers between housing markets in which purchase restrictions in a "hot" local housing market force a capital flight and cause housing booms in nearby previously "cold" markets. Our analysis recognizes that policies designed to achieve locally optimal outcomes may generate unintended spillovers (Farhi and Werning 2017; Rodrik 2019).

We also provide the first evidence on the effect of out-of-town housing demand on consumer spending. Favilukis and Van Nieuwerburgh (2021) (henceforth FV) build and calibrate a model on the effect of out-of-town demand on house prices, consumption, and welfare that illustrates how out-of-town demand can lead to winners and losers among local residents. In concurrent work, Gorback and Keys (2020) and Li, Shen, and Zhang (2020) show that a

Chinese demand shock for homes in the United States raises construction and local nontradable employment in areas with a larger preexisting foreign-born Chinese population, but they do not study consumer spending nor different effects on consumers with different homeownership statuses. Our empirical setting fits nicely with the FV model. The strongly heterogeneous spending responses that we find in which owners benefit and renters lose provide evidence for their theoretical predictions.

We contribute to the literature by using a new quasi-experimental strategy to estimate the housing wealth effect. Estimating the housing wealth effect is important because in most economies, housing is the most significant form of private wealth (Yao and Zhang 2005; Badarinza, Campbell, and Ramadorai 2016). Studies have found that Saiz (2010)'s supply elasticity instrument, which has been widely used to estimate the effect of housing price shocks, does not satisfy the exclusion restriction (Davidoff 2016). Therefore, new methods to credibly estimate the housing wealth effect are needed. Guren et al. (2021) propose a Bartik instrument strategy based on systematic exposures to regional housing price cycles to replace the Saiz instrument. Sodini et al. (2018) apply a quasi-experimental strategy and use an exogenous shock to homeownership and housing wealth and find evidence for the collateral effect in Sweden. Together, our study and these concurrent works provide a new set of implementable strategies to credibly estimate the causal effect of house price shocks. Our empirical strategy is also related to recent work on consumption responses to stock price shocks, including Chodorow-Reich, Nenov, and Simsek (2021) and Di Maggio, Kermani, and Majlesi (2020).

While existing works (Mian, Rao, and Sufi 2013; Adelino, Schoar, and Severino 2015; Aladangady 2017; DeFusco 2018) show the collateral channel is operative in the United States, in less developed or more regulated financial markets like China (Calza, Monacelli, and Stracca 2007), easy access to home equity loans are not feasible, and causal evidence on the effect of house prices on consumer spending and its mechanism is still lacking. Our study fills that gap by providing quasi-experimental evidence for a significant "pure" housing wealth effect that is positive in the aggregate yet redistributive. Given the heterogeneity in the spending responses, the aggregate response in a population depends on the composition of renters and homeowners. Some studies find that a rise in house prices increases household consumption (Du, Shen, and Pan 2013; Painter, Yang, and Zhong 2021; Pan and Wu 2021), while others find the direct opposite relationship (Chen, Chen, and Gao 2012; Xie et al. 2012; Waxman et al. 2020) or no significant relationship (Gu, He, and Qian 2018). We reconcile the mixed results reported in these studies.

1. Institutional Background

Over the past decade, there has been substantial heterogeneity in the housing markets of large and small cities in China; large cities witnessed booming growth while small cities remained relatively stagnant. For example, house prices rose at a high speed of 14.9% annually in Tier-1 cities[2] but at slower than 3% in Tier-3 cities from 2012 to September 2016.[3] At the end of September 2016 and in the middle of March 2017, the government implemented two rounds of policy changes in all Tier-1 and many Tier-2 cities to contain the surging house prices. These policy changes were called house purchase restrictions (henceforth HPR) and were targeted at curbing the demand of housing speculators, who typically hold multiple homes. The HPR in 2016 and 2017 can be contextualized as a part of the long-standing effort by central and regional governments to cushion the housing market against over-heated speculative demand.

In particular, the restrictions included raising the down payment requirement to even higher levels, and increasing mortgage rates on, occasionally outright forbidding, investment purchases. These purchases were identified by the purchase of more than two or three houses by one family. Among cities for which we have reliable data on house prices, 19 implemented the first round of HPR, and 22 implemented the second round. Tables IA.1 and IA.2 in the Internet Appendix enumerate these policy changes.

These policy shocks were considerably effective in containing rising house prices in the regulated cities. In September 2016, the average monthly increase in house prices in the 19 regulated cities was 4%, but after the first round of HPR, dropped to 1.8% in the next month and subsequently remained below 1%. After the second round of HPR, the average monthly increase in house prices in the 22 regulated cities dropped from 0.7% in March 2017 to around 0.1% later on.[4]

To illustrate the effect of these policy shocks on the regulated cities and the nearby unregulated cities, we select three pairs of cities as examples: (1) Beijing and Tangshan, (2) Hefei and Bengbu, and (3) Wuhan and Xiangyang. Figure IA.1 in the Internet Appendix shows their locations. Within these pairs, Beijing, Hefei, and Wuhan are large cities that implemented two rounds of HPR, and Tangshan, Bengbu, and Xiangyang are nearby smaller cities that are unregulated. For comparison, we also include three unregulated cities that are farther away from any regulated cities: Jilin, Jinzhou, and Dali. Selected cities are similar to metropolitan statistical areas (MSAs) in the United States in geographical scale. Figure 1 plots the dynamics of house price growth rates and home sales in these cities from January 2015 to September 2017.

[2] Chinese cities are conventionally split into several tiers according to their population and economic size. Tier-1 cities are the largest ones, such as Beijing and Shanghai. Tier-2 cities are smaller, and Tier-3 cities are even smaller.

[3] The house price growth is based on data from CityRE and Fang et al. (2016).

[4] Inside the regulated cities, investors restricted from housing speculation turn to other assets. Qian et al. (2019) examine an earlier set of HPR from 2010 and find that the affected investors opened new stock accounts and purchased real estate stock that shows the redirection of investment demand to the stock market, a phenomenon that supports our analysis.

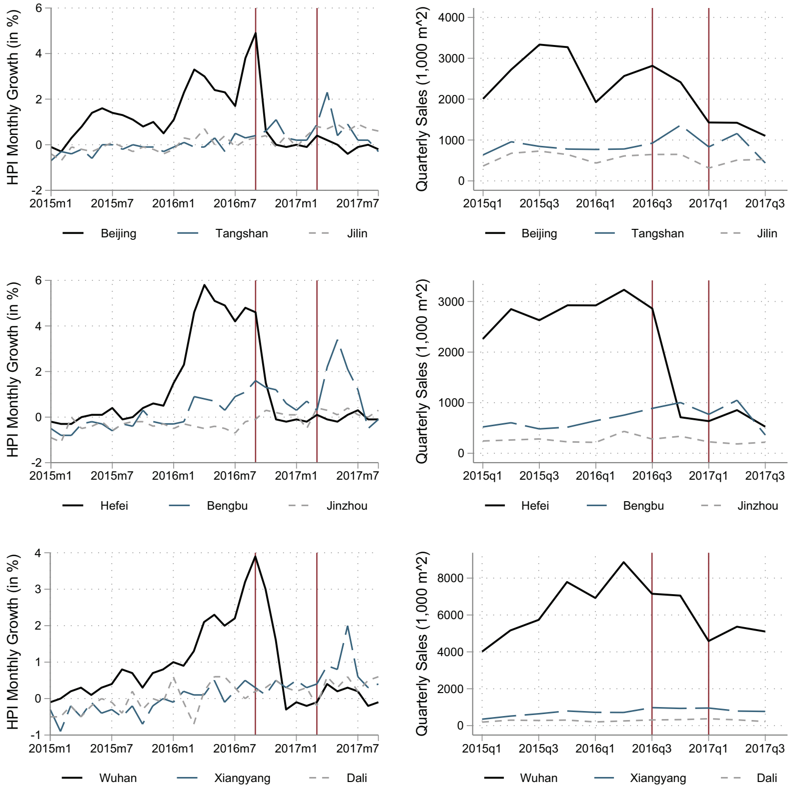

Figure 1
Reactions of house prices and home sales to house purchase restrictions: Some examples
This figure plots the dynamics of house price growth rates and home sales in three pairs of cities from January 2015 to September 2017. The figure illustrates the effect of the house purchase restrictions on regulated cities and neighboring unregulated cities. The three pairs of cities are (1) Beijing and Tangshan, (2) Hefei and Bengbu, and (3) Wuhan and Xiangyang. Within the pairs, Beijing, Hefei, and Wuhan are large cities that implemented two rounds of restrictions, and Tangshan, Bengbu, and Xiangyang are nearby smaller cities that are unregulated. For comparison, Jilin, Jinzhou, and Dali are unregulated cities farther away from any regulated cities. The three graphs on the left show the monthly growth rate of house prices, and the three graphs on the right show the quarterly volume of home sales. The figure uses the National Bureau of Statistics 70-city constant-quality sampling house price index, and the home sales data from China Index Academy. In regulated Beijing, Hefei, and Wuhan, house prices stopped rising soon after the restrictions, and the volume of home sales plummeted. In nearby unregulated Tangshan, Bengbu, and Xiangyang, house prices and home sales significantly increased, especially after the second round of the restrictions. The reactions of house prices and home sales in Jilin, Jinzhou, and Dali are barely noticeable.

The figure shows the effectiveness of the policy changes in containing rising house prices in the regulated cities. In Beijing, Hefei, and Wuhan, house prices almost stopped rising soon after the policy changes. The transaction volumes of houses also plummeted. In Beijing and Hefei, quarterly home sales dropped by over 50% to 70% after the first round of HPR.

The figure also shows that the nearby unregulated cities appeared to experience a sharp increase in home sales and house prices shortly after the regulated cities implemented HPR. For example, quarterly home sales in Tangshan increased, following each round of HPR implemented in Beijing by 47% and 39%, respectively. Also, the growth rate of house prices in Tangshan, Bengbu, and Xiangyang swiftly rose from less than 0.5% to 2.3%, 2.2%, and 2%, respectively, after the second round of HPR. In contrast, the reactions of house prices and home sales in Jilin, Jinzhou, and Dali, unregulated cities farther away from any regulated cities, are barely noticeable.

The HPR originally aimed to curb the investment demand in a few large cities. However, the increase in home sales and house prices in the unregulated cities manifests a redirection of investment demand from the regulated cities to the unregulated cities. Local governments of the nearby unregulated cities soon became concerned about this phenomenon and later announced that they were closely monitoring the spillover of investment demand which may precipitate turmoil in the housing markets in their jurisdictions. As an example, Hunan province announced that:

> "In order to prevent the spillover effect of Changsha's regulation and control from impacting and affecting the surrounding areas, it is necessary to effectively strengthen the coordination of the policies ... Zhuzhou and Xiangtan [nearby unregulated cities] should pay close attention to their real estate market. Once the market shows signs of overheating, timely targeted control measures should be introduced." (https://bit.ly/3gbphWw.)

As another example, Hubei province urged small cities as well to:

> "Prepare to implement purchase and loan restrictions for non-local residents, in areas with surging house prices and home purchases of non-local residents." (https://bit.ly/3g7Udqj.)

In September 2017, when housing appreciation in these nearby unregulated cities had become significant, local governments of many of these cities also started to implement HPR to cool down the housing market and to restrict out-of-town demand.[5] House prices in Tangshan, Xiangyang, Bengbu, and many other nearby unregulated cities continued to increase after September 2017 but at a much slower rate, and home sales declined to previous levels. To avoid the influence of these follow-on restrictions in the previously unregulated cities, we focus our analysis on the period before September 2017.

[5] See, for example, the Xiaogan Government [2017 No. 42], https://bit.ly/3d0jUqX and the Xiangyang Government [2017 No. 60], https://bit.ly/3d0jAbJ.

2. Difference-in-Differences: The HPR Quasi-Experiment

In this section, we introduce our data, provide evidence on the capital spillover, and discuss the preexisting trends in house prices and automobile spending before the policy shocks. Preexisting trends arise as unregulated cities are inherently different, for example, in distances from regulated cities, and the urban literature has shown that diversity in initial conditions predicts different rates of growth in the economy and in house prices (Glaeser, Scheinkman, and Shleifer 1995; Blanchard and Katz 1992; Glaeser and Nathanson 2017). We discuss our difference-in-differences specification that explicitly controls for the preexisting trends, report the estimated responses of house prices and automobile spending to the HPR spillovers, and provide evidence that our estimations are robust to different model specifications.

2.1 Data

To enable our analysis, we assemble a new data set on house prices and consumer spending on automobiles. Table 1 presents the summary statistics, and we now introduce the sources of the data variables.

Our primary source for house prices is CityRE, a leading national provider of real estate data. The CityRE provides comprehensive coverage of 307 cities from 2008 to 2017 that provides a large sample for estimations. The CityRE's index is a hedonic constant-quality index using home postings and transaction records from national and local real estate brokerages. We supplement the CityRE index with the index from Fang et al. (2016), a semi-repeated sales house price index that covers 120 cities from 2003 to 2013.[6] The CityRE index and the Fang et al. (2016) index have highly synchronized comovements in the overlapped period. We also obtain from CityRE a hedonic constant-quality rent index for our sample of cities that spans from 2008 to 2017.[7]

Data on the volumes of home transactions come from the China Index Academy. This data vendor records all completed real estate transactions registered at the housing administration bureaus of municipalities. The home sales index is an index of the number of residential units sold in each month and is benchmarked in July 2016 (2016m7=100) for 73 cities. The Baidu Index service allows measuring the intensity of web searches of specific keywords originated from web users in each individual city. Using this service, we construct the Baidu home search index that measures the intensity of web searches of keywords related to house prices and housing markets in each unregulated city that came from the regulated cities. The data provided by the Baidu Index service is proportional to the number of web searches of a

6 We use the house price indexes from CityRE and Fang et al. (2016), not the official National Bureau of Statistics (NBS) house price index. Fang et al. (2016) indicate that the NBS index closely comoves with their index and is smoother but may understate the rise in house prices. Besides, the NBS index covers only 70 cities.

7 The rent index is for houses and apartments of similar quality as owner-occupied units. Rental homes in China are mostly located in buildings or complexes in which the majority of homes are owner occupied.

Table 1
Summary statistics

	N	Mean	SD	10th	50th	90th
City-level data						
FGXZ house price index	13,641	2.05	1.02	0.99	1.82	3.46
CityRE house price index	31,373	1.55	0.52	1.02	1.43	2.23
Combined house price index	19,401	2.52	1.36	1.03	2.25	4.19
CityRE rent index	28,975	1.39	0.41	0.98	1.32	1.90
Home sales index	4,642	100.78	63.90	43.59	90.83	164.65
Baidu home search index	8,316	384.62	319.14	27.64	326.41	777.71
Automobile spending (¥ mil.)	59,130	281.30	534.20	14.85	108.22	662.97
Automobile purchases	59,130	2,124	3,437	141	982	5,162
Luxury automobile spending (¥ mil.)	59,130	54.56	150.17	0.51	10.69	113.16
Luxury automobile purchases	59,130	99	275	1	18	198
Baidu nonautomobile spending index	91,709	1,543	1,119	571	1,294	2,724
Per capita gross regional product (¥)	47,040	32,437	28,541	7,961	24,543	65,694
Residential population (1,000)	47,040	4,266	5,163	1,368	3,531	7,652
Square meters of road per capita	46,320	9.95	10.70	3.82	8.67	16.59
Public buses per 1,000 residents	46,344	0.67	0.63	0.21	0.58	1.17
GRP (annual, ¥ bil.)	2525	215.20	266.20	43.33	127.66	467.85
Real estate investment (¥ bil.)	2,542	27.54	44.92	2.77	12.41	64.10
Bank deposits (¥ bil.)	2,396	348.27	583.39	56.74	163.89	848.82
Employment growth	1,186	0.02	0.06	0.01	0.01	0.03
Residential population (mil.)	2,290	4.23	3.07	1.24	3.52	7.86
GRP growth	2,188	0.09	0.08	0.02	0.09	0.18
Industrial output growth	2,393	0.12	0.08	0.04	0.10	0.21
City demographic group-level data						
Aggregated automobile spending of birthplace groups (¥ mil.):						
Locally born	53,317	146.36	251.94	1.72	51.88	380.21
Nonlocally born	53,317	158.77	363.75	6.49	44.85	376.80

This table reports summary statistics for all the variables used in this paper. The constant-quality FGXZ 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.

keyword, but since it uses proprietary filtering and reweighting algorithms, our Baidu home search index only qualitatively reflects consumer demand.

Our data on automobile spending covers every purchase in China from January 2003 to August 2017. It comes from the administrative registry at the China Insurance Information Technology Corporation (CIITC) that is affiliated with the China Insurance Regulatory Commission (CIRC) which oversees the insurance products and service market. The CIITC requires insurers to report the compulsory liability insurance that each new automobile must register for at the time of purchase, so the data of CIITC represents all automobile purchases in China. They cover personal and commercial purchases of new passenger and nonpassenger automobiles; to measure consumer spending on automobiles, we exclusively use personal purchases of new passenger automobiles. For each purchase, we observe the automobile's manufacturer, model, trim, vehicle identification number, date and place of registration as well as desensitized information on the purchaser. Figure IA.2 in the Internet Appendix shows the aggregated number of automobile purchases in China in each month from the CIITC data.

The information on purchase prices in the CIITC data allows us to measure automobile spending with high precision. The CIITC requires insurers to report the purchase price of automobiles that are covered under comprehensive or collision insurance, which equals 80% of our sample. For the remaining automobiles, we approximate the purchase price with the average price of the same model in the same city and month. With detailed information on the car model, the approximation is accurate; for example, for a Mercedes-Benz, we know whether it is an SL-class with a 5.0L engine that costs more than US$150,000, or a C-class with a 2.0L engine that costs much less at US$40,000.

We aggregate the totals of spending and numbers of automobiles purchased at the city-month level. Also, to reflect the idea that different segments of the consumer automobile market may react differently, we aggregate luxury cars separately. We seasonally adjust all the automobile spending by excluding the month-specific effects from the automobile spending time series of each city.

Next, we obtain data on automobile purchases by different demographic groups. For each city and each month, we compile purchases made by individuals born in the prefectural city they reside in (the "locally born"), and those by individuals born outside of the prefectural city they reside in (the "nonlocally born"). This is possible because several digits of the buyer's ID in the CIITC data were preserved in the data desensitization process; these preserved digits give the birthplace of the buyer.[8] The insurance application also gives the city where an automobile was purchased and used. Combining these two pieces of information, we can distinguish automobile purchases made by locally born individuals and non-locally-born individuals.

Automobile spending is reflective of household spending in China. The China Household Finance Survey (CHFS) collects data on automobile purchases as well as other categories of spending. In the 2017 wave of the CHFS (asking about spending in 2016), automobile spending was the second largest category of urban consumer, nonhousing spending (10.6%); it was only lower than food at home (20.5%) and higher than other items, such as medical expenses, education, dining out, and home improvement. According to the 2015 wave of the CHFS, automobile spending was also the second-largest category of urban consumer, nonhousing spending (10.5%) prior to the HPR policy changes. Automobile spending occupies a similar share of nonhousing spending in the United States, namely, 9.5% in 2016, according to authors calculation using the Consumer Expenditure Survey.

As a proxy for the nonautomobile spending of households, we construct the Baidu nonautomobile spending index. Specifically, we measure the intensity

[8] Specifically, these digits of the ID record the birthplace if an individual was born after 1984 and the city of residence in 1984 otherwise, as the national ID system was initiated in 1984. Migration across cities was extremely limited before 1984 (Liang and White 1996), so our measure of birthplace status is reasonably accurate.

of searches through Baidu of keywords related to a basket of consumption goods that originated with web users in each city in each week. We let the basket consist of goods that are generally pricey for ordinary Chinese households and that have sufficient web searches. The basket of goods include smartphones (iPhone, Huawei phones, Vivo, OPPO), sportswear (Nike and Addidas), prestige cosmetics (Estée Lauder, Lancôme, Saint Laurent) as well as watches (no brand specified) and Moutai Wine (top liquor brand in China).

We collect time-varying macroeconomic variables for each city that comprise per capita gross regional product (GRP), residential population, square meters of road per capita, the number of public buses per 10,000 city residents, industrial output, real estate investment, and bank deposits. The data partly come from the China City Statistical Yearbook composed by the National Bureau of Statistics of China. When those data are not available, we manually collected the data from statistical reports and yearbooks of individual cities.

Finally, we compute the fraction of renters and homeowners in different demographic groups based on nine waves of household surveys: the 2010, 2011, 2012, 2013, and 2015 waves of the Chinese General Social Survey (CGSS); and the 2010, 2012, 2014, and 2016 waves of the China Family Panel Studies (CFPS). These surveys provide information on the birthplace, city of residence of surveyed individuals as well as whether their immediate co-residing family owns homes or not. To conserve space, we do not summarize the survey data in Table 1.

2.2 Motivating evidence for the capital spillover

We now present motivating evidence consistent with the "capital spillover": real estate investors facing increased transaction costs in regulated cities turn to invest in the nearby unregulated cities, brought in additional external capital and pushed up the house prices.

We first show that the reduction in the volume of home transactions in the regulated cities is consistent with the increase in volume in the nearby unregulated cities. Figure 2 shows that the volume drop in regulated cities is simultaneous with and similar in magnitude to the volume increase in unregulated cities. Also, the volume increase in nearby unregulated cities is greater than that in faraway unregulated cities.

The redirection of demand is also embodied by a surge in web searches for real estate in the nearby unregulated cities by users in the regulated cities. Figure 3 illustrates this surge from several angles. Panel A shows that Tangshan, an unregulated city, receives dramatically more web searches right after the HPR shocks from web users in Beijing, a regulated city close to it, than from Hefei, a regulated city farther away. Panel B shows the exact same pattern for a different city triplet, unregulated Xiangyang and regulated Wuhan and Beijing. Panel C shows that these triplets are not just cherry-picked examples. Unregulated cities on average receive much more web searches right after the HPR from close regulated cities than from faraway regulated cities. For

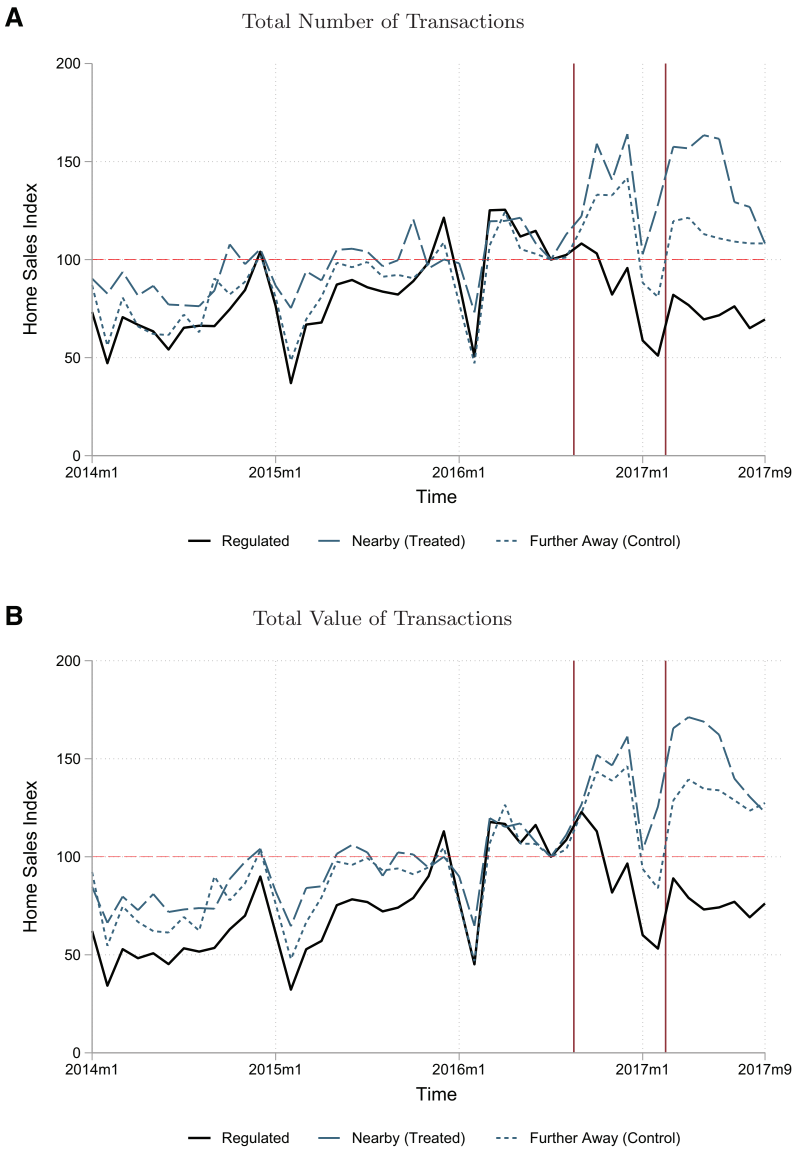

Figure 2
Home transaction volumes in regulated, treated, and control cities
This figure plots indexes of total volumes of residential home transactions in regulated cities, treated cities, and control cities. The data come from China Index Academy, a data vendor that records all completed real estate transactions registered at housing administration bureaus of municipalities. Panel A plots the average index of total number of homes transacted within each city group, where for each city the index is defined as monthly total number of homes transacted relative to that in 2016m7 (2016m7=100). Panel B plots the average index of total value of homes transacted within each city group, where for each city the index is defined as monthly total value of homes transacted relative to that in 2016m7 (2016m7=100).

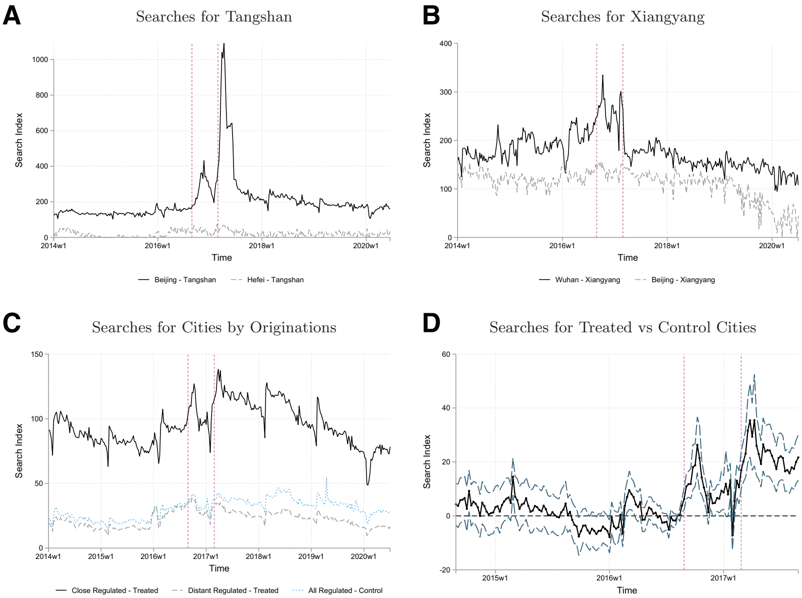

Figure 3
The Baidu home search index: Evidence of out-of-town buyers
This figure plots the Baidu home search index, which measures the intensity of web searches of keywords related to house prices and housing markets in each unregulated city that came from the regulated cities, to show evidence of out-of-town buyers. Panel A plots the Baidu home search index for Tangshan, considering web searches from Beijing and Hefei, respectively. Both Beijing and Hefei are regulated cities, but Beijing is close to Tangshan, and Hefei is distant. Panel B plots the Baidu home search index for Xiangyang, considering web searches from Wuhan and Beijing, respectively. Both Wuhan and Beijing are regulated cities, but Wuhan is close to Xiangyang, and Beijing is distant. Panel C plots the average Baidu home search index for treated cities, considering web searches from nearby (<250km) regulated cities and from distant (≥250km) regulated cities, respectively, and the average Baidu home search index for control cities, considering web searches from all regulated cities. Panel D plots the estimated differences in the Baidu home search index over time for treated cities minus control cities, considering web searches from all regulated cities, based on coefficients from a difference-in-differences regression. Treated cities are defined as unregulated cities within 250 km of the nearest regulated city, and the remaining unregulated cities are control cities.

unregulated cities far away from any regulated cities, we observe even smaller changes. Panel D shows a plot of the difference in the received web searches between unregulated cities nearby regulated cities and those far away from regulated cities. Web searches received by the former increase sharply after the HPR compared to the latter.

2.3 Tests of preexisting trends

The HPR shocks naturally lead us to a difference-in-differences estimation strategy to estimate the effect of the capital spillover on the nearby unregulated cities. Twenty-two prefectural cities in our sample period were under the HPR. Our designation of the treatment group relies on each unregulated city's distance to the nearest regulated city. We split the unregulated cities, which are also at the prefectural level, into two approximately equal-sized groups based on this distance. If a city is within 250 km of the nearest regulated city,[9] it belongs to the treatment group ("treated" by the capital inflow). Otherwise, it belongs to the control group. The choice of 250 km as the cutoff may seem arbitrary; however, we justify it with the fact that aided by high-speed railways, traveling point-to-point between two cities closer than 250 km takes less than 2 hr.[10] We consider two cities within 250 km to be "close" for investment purposes; the distance is acceptable for occasional visits to screen homes or monitor tenants, but too far for daily commuting. The cities we study are prefectural cities of similar size to the MSAs in the United States. Even with the aid of high-speed railways, people rarely commute between these cities. The designation entails 152 treated cities and 151 control cities. Treated cities and control cities are on average 136 km and 798 km away from the nearest regulated cities, respectively.

The major difficulty in difference-in-differences analyses involves separating out preexisting trends from the dynamic effects of a policy shock. To avoid confounding the two, we first test for preexisting trends in variables key to our analysis, namely, house prices, automobile spending, and rents. To perform the test, we interact a series of time indicators with the treatment designation indicator to estimate the dynamics of the dependent variables in the treated cities relative to those in the control cities, both before and after the treatment. We then check whether the response of the treated cities diverges from that of the control cities before the treatment. Specifically, we estimate the following equation:

$$Y_{i,t} = \sum_k \beta_k \, Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} + City\ FE_i + Time\ FE_t + \epsilon_{i,t}, \tag{1}$$

where $\mathbb{I}_{\{t=2016m9+k\}}$ is an indicator of whether time t is exactly $2016m9+k$, and $Treat_i \times \mathbb{I}_{\{t=2016m9+k\}}$ is the treatment-time interaction. The dependent variable $Y_{i,t}$ can be the logarithm of the house price index, the logarithm of automobile spending, and the logarithm of rent in city i and at time t. *City FE* and *Time FE* are city fixed effect and time fixed effect. And, $\epsilon_{i,t}$ is the error term.

Figure 4 displays the coefficients of interest, β_k, that measure the dynamics of the dependent variable of the treated cities relative to the control cities before and after the shocks. The figure shows a persistent but stable differential trend in the dynamics of house prices and automobile spending between the treated cities and control cities over a long period before the first round of HPR. Further, the figure shows that the shape of the preexisting trends is linear: within 5 years before the HPR, house prices rise 2% per year and automobile spending rises

[9] We use the distance of a city from the nearest of the 22 regulated cities. Although three of the regulated cities only implemented the 2017 round of HPR, the treatment group and our estimation results change little regardless of how the treatment group depends on these three cities.

[10] This 2-hr travel radius is given by the speed of the high-speed trains in China, which run at 250 km–350 km/hr, and that each within-city trip to the train station takes on average 30 minutes. Most (if not all) cities in our sample are connected or are planned to be connected to China's high-speed rail system.

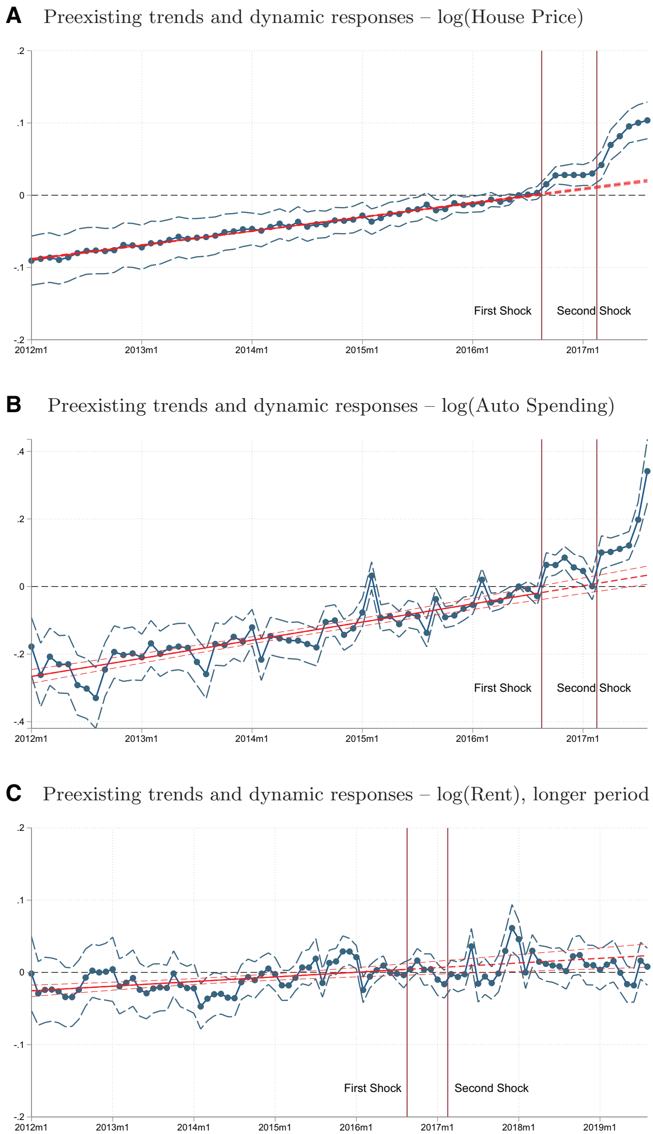

Figure 4
Preexisting trends and dynamic responses: House prices, auto spending, and rents
The figure plots the estimated responses of house prices, automobile spending, and rents in treated cities relative to control cities, both before and after the house purchase restrictions. The responses are estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. All responses are relative to the level of responses in July 2016. City fixed effects and time (year-month) fixed effects are added, and 95% confidence interval is drawn based on standard errors clustered at the city level. Automobile spending is seasonally adjusted. Two rounds of house purchase restrictions, in September 2016 and March 2017, are represented by vertical red lines. Red upward-sloping lines represent the pretreatment trend of the relative responses and the trend's 95% confidence interval, based on linear regression of the estimated responses on time. Data on automobile spending and house prices are from January 2012 to August 2017. We rely on rent data from January 2012 to August 2019, in order to examine the longer-period effects on rent.

by 4%–5% per year more in the treated cities than in the control cities. Despite the preexisting trends, the effect of the HPR shocks is statistically significant and economically sizable. House prices and automobile spending in treated cities increase immediately above the preexisting trends after both rounds of the HPR.[11] The figure indicates that we should explicitly control for the linear preexisting trends when we quantify the effects of the HPR shocks.

Figure 4 also shows that rents do not respond differently in the treated cities than in the control cities, both long before and long after the HPR. The rent gap between treated and control cities oscillates around the preexisting trend and is significantly positive in very few months. But overall, it does not substantially deviate from the preexisting trend or from zero. Despite scattered blips in the rent data, the rent gap between treated and control cities is statistically insignificant from zero for 31 of the 36 post-treatment months, and is statistically insignificant from the preexisting trend for 35 of the 36 post-treatment months. This result provides additional evidence that the HPR shocks that drive the responses of house prices and automobile spending are orthogonal to confounding factors, for example, local economic productivity and city-to-city migration. One limitation here is that we cannot fully rule out the possibility that households expect rent changes in the future that may affect their valuations of houses.

2.4 Empirical specifications

2.4.1 House prices. We now estimate the response of house prices in treated cities relative to that of control cities as induced by the unintended spillovers from HPR in regulated cities. To separate the preexisting trends from the dynamic effects of the policy shocks, we follow Wolfers (2006) and slightly modify the standard difference-in-differences procedures. We first add city-specific linear time trends to the regression. Also, as Wolfers (2006) suggests, reduced-form or structural analysis that assumes an immediate constant response to a policy shock may be misspecified if the actual dynamics are more complex. Following his approach, our specification models the dynamic response explicitly and imposes very little structure on it. Specifically, we include dummy variables for the first and second month after the shocks and for months three, four, five after the shocks, and so on. These variables should identify the entire response function while allowing the estimated city-specific time trends to identify the preexisting trends. Our specification is

$$\log \mathrm{HPI}_{i,t} = \sum_{0\leq k\leq 5} \alpha_k Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} + \sum_{0\leq k\leq 5} \beta_k Treat_i \times \mathbb{I}_{\{t=2017m3+k\}}$$
$$+\Gamma X_{i,t-1} + City\ FE_i + Time\ FE_t + \theta_i t + \epsilon_{i,t} \tag{2}$$

[11] The gap in house prices between treated and control cities remains in parallel above the preexisting trend for 1.5 years after August 2017 and starts to slowly reduce in 2019. We lack data on automobile spending after August 2017.

where $\text{HPI}_{i,t}$ is the monthly house price index in city i at time t. $Treat_i$ is a dummy that equals one if the city belongs to the treatment group. Dummy variables $\mathbb{I}_{\{t=2016m9+k\}}$ and $\mathbb{I}_{\{t=2017m3+k\}}$ equal one if time t is k months after September 2016 and March 2017, respectively. $X_{i,t}$ is a vector of time-varying city-level control variables for city i at time t. $\theta_i t$ is the linear time trend of city i.

The coefficients of interest are the averages of α_k's and β_k's that measure the average treatment effect in the 6-month period right after the first round of HPR and the second round of HPR, respectively. Our specification controls for city-level time-varying economic fundamentals, city and time fixed effects, and the city-specific linear trend. To account for serial correlation and region-specific random shocks, we cluster the standard errors at the city level in all specifications.

Ideally, the control variables in $X_{i,t}$ should include local demand shifters, such as the average income of potential buyers in each local market and the migration flows; buyer characteristics, such as the fraction of speculative buyers; and measures of credit market conditions, such as the loan-to-value ratios of homes purchased over the sample period (DeFusco et al. 2018). However, this inclusion is impeded by the fact that representative mortgage data are not accessible in China.[12] Even if data on all mortgages were available, they would not be representative of home purchases in China, where households have a low dependence on mortgages. The Urban Household Survey conducted by the National Bureau of Statistics shows that only 17% of homebuyers 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 and is much lower than in Japan (39%), the United States (72%), and the United Kingdom (86%) (Fan, Wu, and Yang 2017). Given the data limitations, we instead control for several city-level macroeconomic variables that may relate to house prices, such as per capita gross regional product (GRP), residential population, road infrastructure, measured as the per capita area of roads and freeways, and public transportation, measured as the per capita number of public buses. We also control for the exposure of a city to the economic activity of the nearest regulated city, as cities may constitute a hub-and-spoke network in which any economic shocks to the central city pass through to the spoke city, and the effect fades with distance. Specifically, for each unregulated city we first calculate the beta of its GRP growth to the nearest regulated city's by using data from year 2003 to the end of the sample period. We then compute the economic exposure to the nearest regulated city as the beta times the regulated city's GRP.

We provide further evidence on the capital spillover induced by the HPR by using the same specification but by replacing the outcome variable with the

12 Proprietary mortgage data are available from only one or two mortgage lenders, which account for only a small part of all mortgages.

home sales index to show that the home sales increase more in the treated cities relative to the control cities; with the Baidu home search index to show that out-of-town buyers' interest and attention are higher in the treated cities relative to the control cities; and with rent index and a basket of macroeconomic variables to look for confounding factors to the unintended spillover from HPR. The basket of macroeconomic variables includes city-level output, industrial output growth, output growth, employment growth, population, real estate investment, and bank deposits. They are at an annual frequency, and we assign years after 2016 as the post-treatment period.

2.4.2 Automobile purchases. To investigate the influence of capital spillover to the housing market on the consumer spending of city residents, we study how consumer spending on new automobiles in the treated cities responds to the HPR shocks. To do so, we reestimate Equation (2) for automobile purchases with the logarithm of consumer spending on new automobiles in each city and each month as the outcome variable. We add city and year fixed effects, city-specific linear trends, and the same set of controls used in the estimation of the response by house prices. Specifically, we control for income via GRP per capita, residential population, road infrastructure measured as the per capita area of roads and freeways, as well as public transportation measured as the per capita number of public buses, all of which may affect household demand for automobiles.

To estimate the elasticity of automobile spending to changes in house prices, we use the HPR shocks in an instrumental variable regression. Specifically, we instrument house prices with the interaction dummies $Treatment \times \mathbb{I}_{\{t=2016m9+k\}}$ and $Treatment \times \mathbb{I}_{\{t=2017m3+k\}}$ which equal one for treated cities and when time t is k months after September 2016 or March 2017 for $0 \leq k \leq 5$ and carry out a weighted 2SLS regression of the logarithm of automobile spending on the logarithm of house prices, with the weight being the population of each city. In both stages of the IV regression, we include the same control variables, fixed effects, and city-specific linear trends used in Equation (2). And we use standard errors clustered at the city level.

2.5 Difference-in-differences estimates of the effects of HPR shocks

We now present our main results: the difference-in-differences estimate of the effects of the HPR shocks on house prices and consumer spending, while controlling for city-specific trends, as presented in Equation (2). Table 2 displays the results.

The estimated responses of house prices and automobile spending to the HPR shocks in the treated cities are statistically significant and economically sizable. Column 1 of the table shows that house prices in treated cities increased by 2.4% and 6.4% following the two rounds of HPR relative to control cities and after controlling for the preexisting trends. In contrast, column 2 indicates there is no differential change at all in rents between treated and control cities, and column

3 shows that treated cities experience an increase in home sales of 10% and 28% of the level in July 2016 relative to control cities. The effect is more statistically significant following the second round of HPR. Column 4 shows the response in the Baidu home search index. This index qualitatively reflects out-of-town buyers' demand. We do not attempt to interpret the economic magnitude of the coefficients, but just emphasize their statistical significance. According to column 4, treated cities also experience a significant increase in that index relative to control cities.

Consistent with a positive housing wealth effect, a major rise in automobile spending accompanies the increase in house prices. Column 5 shows that following the two rounds of HPR, consumer spending on new automobiles increases by 7.8% and 11.6% in the treated cities relative to the control cities. Column 6 shows the intensive margin of the spending response, that is, consumers in treated cities buy more expensive automobiles as well after the shocks. Specifically, consumer spending on luxury cars increases by 12.3% and 15.7% in the treated cities relative to the control cities. The increase in spending on luxury cars is generally larger than that on all-model passenger automobiles that indicates the intensive margin of the response is sizable and important. While the data on automobile spending in columns 5 and 6 are not seasonally adjusted, the remaining columns use seasonally adjusted data. Column 7 shows that the adjustment only slightly changes the estimates: seasonally adjusted automobile spending increases by 6.0% and 14.2% in the treated cities relative to the control cities. Column 8 presents the results of the regression that is weighted by city population and shows that the average person in treated cities increases their automobile spending by 3.7% and 9.9%. To comprehend the size of the estimated effects, the average annual growth in house prices in the unregulated cities is 10%–13% from 2003 to 2013, according to Fang et al. (2016), and the annual growth in automobile spending is 9.6% from 2012 to 2017, both of which are similar in magnitude to our estimates. And column 9 shows that the IV estimate of the elasticity of automobile spending to house prices is 1.94.

We now present the results of another set of tests using a basket of macroeconomic variables to further look for confounding factors to the unintended spillovers from HPR. Table 3 shows the results of this set of tests.

We see that, except for an increase in bank deposits (column 7), there is no obvious improvement in local fundamentals that could confound the quasi-experiment of the unintended spillovers from HPR. The increase in bank deposits in the treated cities appears to be the consequence of the capital spillover—when purchasing homes from the local homeowners, out-of-town buyers inject funds into the treated cities. Consistent with this, the increase in bank deposits is similar in magnitude to the increases in house prices. Specifically, the estimated increase in total home value per treated city is 18.3 billion yuan (6.4% on a pretreatment level of 285.0 billion), which is 1.7 times the 10.6 billion yuan increase in bank deposits (5.0% on a pretreatment level

Table 2
DID estimated effects of house purchase restrictions on house prices and automobile spending: Main results

	(1) log(House Price)	(2) log(Rent Index)	(3) Home Sales	(4) Baidu Home Search	(5) log(Auto Spending)	(6) log(Luxury Auto) Spending)	(7) log(Auto Spending) (sea. adj.)	(8) log(Auto Spending (weighted)	(9) log(Auto Spending) (IV)
Treat× Post1	0.024***	−0.005	10.270	81.677***	0.078***	0.123***	0.060***	0.037**	
	(3.188)	(−0.607)	(1.237)	(6.893)	(4.033)	(4.533)	(3.407)	(2.292)	
Treat× Post2	0.064***	−0.008	28.083**	128.594***	0.116***	0.157***	0.142***	0.099***	
	(5.151)	(−0.665)	(2.022)	(6.619)	(4.103)	(3.886)	(5.150)	(3.723)	
log(House price)									1.940***
									(2.676)
Observations	20,331	19,483	3,637	8,052	21,012	20,749	21,012	20,944	20,263
R^2	.983	.944	.566	.941	.979	.944	.987	.986	
First-stage F									40.326
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the difference-in-differences regressions of variables related to housing markets and automobile spending with respect to the spillovers from the imposition of house purchase restrictions. The sample consists of all unregulated cities, and the data span from 2012m1 to 2017m8. The dependent variables are log CityRE house price index in each city in each month in column 1, log CityRE rent index in column 2, home sales index (2016m7=100) in column 3, Baidu home search index in column 4, log household automobile spending in column 5, log household spending on luxury automobiles in column 6, and seasonally adjusted log automobile spending in column 7. In column 8, the regression is weighted by city population. Column 9 reports IV estimates of the elasticity of automobile spending to house price, instrumenting house price by policy spillover shocks. Automobile spending is also seasonally adjusted in columns 8 and 9. Treat is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post1 is a dummy that takes the value of one if the time is after the first round of the house purchase restrictions and before the second round of the restrictions. Post2 is a dummy that takes the value of one if the time is after the second round of the restrictions. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to the nearest regulated city's GRP. City trend is a city-specific linear trend. Standard errors are clustered at the city level. t-statistics are in parentheses. *p <.1; **p <.05; ***p <.01.

Table 3
Post-treatment responses of macroeconomic variables

	(1) log(GRP)	(2) Industrial output growth	(3) GRP growth	(4) Emp. growth	(5) log(Pop)	(6) log(RE investment)	(7) log(Bank deposits)
Treat × Post	−0.002	−0.004	−0.002	0.004	0.002	0.173	0.050***
	(−0.084)	(−0.368)	(−0.203)	(0.960)	(0.060)	(1.123)	(3.415)
Observations	1,551	1,501	1,484	1,031	1,463	1,549	1,449
R^2	.998	.735	.373	.856	.976	.934	.998
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the difference-in-difference regressions of several key annual macroeconomic variables with respect to the spillovers from the imposition of house purchase restrictions. The sample consists of all unregulated cities except those 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 of one if the city is within 250 km of the nearest regulated city. Post is a dummy that takes the value of one if the time is after or equal to year 2017. GRP abbreviates for city-level gross regional product. City trend is a city-specific linear trend. Standard errors are clustered at the city level. t-statistics are in parentheses. *p <.1; **p <.05; ***p <.01.

of 211.1 billion); while the former is slightly larger than the latter, Gabaix and Koijen (2021) also find that the total asset value increases more than the capital inflow. Local real estate investment increases by 17.3% but is statistically insignificant that indicates construction is slow in responding to the increase in out-of-town housing demand. The no responses of the other macroeconomic variables, population, total output, output growth, industrial output growth, and employment, refute the possibility that migration, improvement in total factor productivity, or increases in physical capital lead to the increase in house prices and automobile spending and confound our quasi-experiment.

However, the no response does not refute that the increase in house prices also increases consumption in locally produced nontradables and services and employment and wages in these sectors. Our estimated responses for employment and output do not reflect these changes for the following reasons: in China, workers in the local nontradable sectors, such as in restaurants, are routinely informally employed and therefore are not properly captured in our employment data. The HPR shocks may also be absorbed by adjustment in working hours, profits, and wages rather than the number of workers employed. The increase in nontradable spending should be small relative to the total annual output; hence the estimated no response in output.

2.6 Robustness checks

We take several additional steps to ensure the validity of our research design and the robustness of our estimates.

2.6.1 Continuous distance specification. One potential concern with the difference-in-differences results is the sensitivity to the definition of the treatment group; for example, it may seem arbitrary to use 250 km as the cutoff distance to designate the treatment and control group in Equation (2). To address this concern, we use a "continuous distance" specification to estimate how the distance from the nearest regulated city affects the response of a city following the unintended spillovers from HPR. Specifically, we estimate the following equation:

$$Y_{i,t}=\sum_{0\leq k\leq 5}\alpha_k \log(D_i)\times\mathbb{I}_{\{t=2016m9+k\}}+\sum_{0\leq k\leq 5}\beta_k \log(D_i)\times\mathbb{I}_{\{t=2017m3+k\}}$$
$$+\Gamma X_{i,t-1}+City\ FE_i+Time\ FE_t+\theta_i t+\epsilon_{i,t} \quad (3)$$

where $Y_{i,t}$ can be the logarithm of house prices, the logarithm of automobile spending, and the logarithm of other outcome variables of interest. D_i is the distance of a city to the nearest regulated city. All the other variables are defined in Equation (2). The coefficients of interest are the averages of α_k's and β_k's. They reflect how changes in the outcome variable of an unregulated city relate to its distance from the nearest regulated city after controlling for preexisting trends. If the increases in house prices in the unregulated cities are caused by the capital inflow from the regulated cities, then we can hypothesize that as a city's distance from the regulated cities increases, the capital inflow will be weaker, hence the rise in house prices and consequently automobile spending should be smaller. Thus, the averages of α_k's and β_k's should be negative.

Table IA.3 in the Internet Appendix presents the estimation results. The table shows that as the city's distance from the nearest regulated city continuously increases, the responses of house prices, automobile spending, luxury automobile spending, home sales, and the Baidu home search index following the shocks become weaker and weaker. Meanwhile, the city's distance from the nearest regulated city does not affect changes in rents at all. The IV estimates using the logarithm of distance $\times$ event-month dummies to instrument for house prices remain economically large and statistically significant. All results are consistent with our main difference-in-differences results that indicate the latter are unlikely to be driven by the specific choice of discrete cutoffs.

Figure 5 shows these results in graphical form. For each city, we first estimate the deviations in the two post-treatment periods in our key variables of interest from the city-specific preexisting time trends while also controlling for month dummies to exclude seasonality. Panel A shows the deviations in the logarithm of house prices against the city's distance from the nearest regulated city. Panel B displays the deviations in the Baidu home search index against the distance. Panels C and D show the deviations in the logarithms of automobile spending and luxury automobile spending against the distance. We see that the responses of house prices, Baidu home search, automobile spending, and luxury automobile spending all decay with the distance from the regulated city. The effect of the HPR shocks on automobile spending and luxury automobile spending decay to zero at a distance of around 500 km.

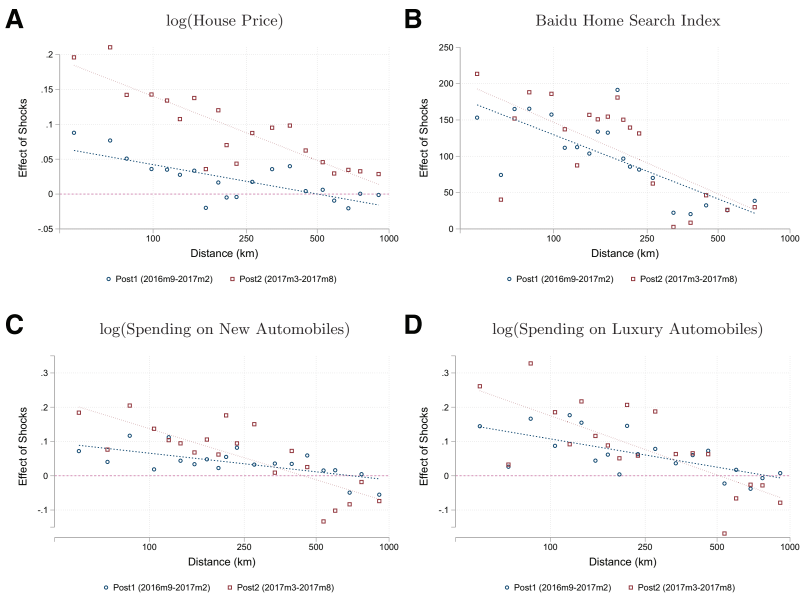

Figure 5
Spillover effects of house purchase restrictions on house prices and automobile spending
This figure plots the spillover effects of house purchase restrictions on the unregulated city as the distance from the nearest regulated city varies. The spillover effect on each city is defined as deviations in the variable of interest in post-shock periods (2016m9–2017m2 and 2017m3–2017m8) from city-specific trend estimated using the preshock period data. Panel A plots the spillover effect on log house price. Panel B plots the spillover effect on the Baidu home search index, measuring the intensity of out-of-town web searches for real estate. Panels C and D plot the spillover effect on log spending on new automobiles and on luxury automobiles (seasonally adjusted), respectively.

2.6.2 Alternative distance cutoffs. We argued that we designated an unregulated city within 250 km from the nearest regulated cities as treated since in that case the travel time from the regulated city would be less than 2 hr. We now verify this idea by designating the treatment group with the travel time to regulated cities. Specifically, using data on time schedules of all trains and high-speed rail (HSR) that operated in China in 2017, we compute for each unregulated city the shortest travel time by rail to each regulated city. Then we designate a city as treated if the travel time to any regulated city is less than or equal to 2 hr. Figure IA.3 in the Internet Appendix plots the shortest travel time against the distance to a city's physically nearest regulated city. The treatment group defined this way includes a set of different cities from the baseline designation. The correlation of the treatment status with the 250 km distance-based treatment status is 0.67, and the largest distance of a treated city to the nearest regulated city is 433 km. Table IA.4 in the Internet Appendix gives the estimation results of which all are consistent with the main results.

Our last exercise to alleviate the concern about the definition of the treatment group is to use alternative distance cutoffs to designate the treatment and control groups. We perturb the cutoff distance from 250 km to 300 km to 200 km to 150 km and then verify that different choices of a cutoff distance cause little change in our estimation results. Table IA.5 in the Internet Appendix shows the estimation results.

2.6.3 Matching specification. Another concern of our estimates is that cities in the treatment and control groups might be very different in levels of economic development and many other characteristics. To alleviate this concern, we conduct a matched sample approach that is based on levels of economic development. We match the cities based on pretreatment values of per capita GRP, exposure of output growth to the nearest regulated city, and house prices. For each treated city, the closest matching control city is chosen (with replacement) according to the Mahalanobis distance of the three variables we are matching on, to constitute the matched control group. Table IA.6 in the Internet Appendix shows a set of balance test results for the nonmatched sample and the matched sample of cities. In the nonmatched sample, treated cities on average have higher per capita GRP and house prices, and higher beta for economic exposure to the regulated city. After matching, we are able to bring the differences in the means down to below 12% of the sample's standard deviations, and bring the differences in the means down to be all below 12% of the sample standard deviations, and bring the variances to be almost the same. Table 4 shows that the matched sample yields quantitatively similar results compared to the pure nonmatched sample.

2.6.4 "One-step-up" perturbation in modeling the preexisting trends. Another concern of our estimates is that linear preexisting trends may not fully capture the differences between treated cities and control cities before the HPR shocks and that they may be too restrictive and induce bias. To alleviate this concern, we follow the "one step up" approach proposed by Bilinski and Hatfield (2019). This approach involves first specifying a baseline model that includes a linear difference in the trends as we did with the city-specific linear time trends. It then uses a more complex difference in the trends to replace the original difference; a restricted cubic spline is recommended here for the balance between flexibility and statistical power. If the estimates using restricted cubic splines are similar to the baseline model, then the baseline assumption of linear preexisting trends is more assured. Table IA.7 in the Internet Appendix has the estimation results after controlling for the restricted cubic spline form of city-specific trends. The estimated responses of all the variables of interest are quantitatively similar to our baseline results.

2.6.5 Heterogeneous treatment effects. We also exploit the heterogeneity in the magnitude of the policy shocks in the regulated city to sharpen the identification. Regulated cities subject to stronger HPR shocks should generate larger spillover effects. Although measuring the strength of HPR shocks based

Table 4
DID estimated effects of house purchase restrictions on house prices and automobile spending: Matching specification

	(1) log(House Price)	(2) log(Rent Index)	(3) Home Sales	(4) Baidu Home Search	(5) log(Auto Spending)	(6) log(Luxury Auto) Spending)	(7) log(Auto Spending) (sea. adj.)	(8) log(Auto Spending (weighted)	(9) log(Auto Spending) (IV)
Treat× Post1	0.021***	0.002	7.144	73.056***	0.069***	0.108***	0.054***	0.046***	
	(2.931)	(0.331)	(1.075)	(6.068)	(4.050)	(4.289)	(3.374)	(2.881)	
Treat× Post2	0.058***	0.005	18.991*	103.626***	0.094***	0.154***	0.118***	0.096***	
	(4.513)	(0.418)	(1.712)	(5.305)	(3.621)	(3.969)	(4.668)	(3.776)	
log(House price)									2.548*
									(1.926)
Observations	20,193	19,739	3,200	9,240	20,604	20,493	20,604	20,536	20,125
R^2	.983	.954	.542	.943	.980	.950	.987	.987	
First-stage F									15.143
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the robustness of the main difference-in-differences estimates to using matched treated and control cities. We perform the matching on city per capita GRP, economic exposure to the nearest regulated city, and house price. The sample consists of all unregulated cities, and the data span from 2012m1 to 2017m8. The dependent variables are log CityRE house price index in each city in each month in column 1, log CityRE rent index in column 2, home sales index (2016m7=100) in column 3, Baidu home search index in column 4, log household automobile spending in column 5, log household spending on on luxury automobiles in column 6, and seasonally adjusted log automobile spending in column 7. In column 8, the regression is weighted by city population. Column 9 reports IV estimates of the elasticity of automobile spending to house price, instrumenting house price by policy spillover shocks. Automobile spending is also seasonally adjusted in columns 8 and 9. Treat is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post1 is a dummy that takes the value of one if the time is after the first round of the house purchase restrictions and before the second round of the restrictions. Post2 is a dummy that takes the value of one if the time is after the second round of the restrictions. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to the nearest regulated city's GRP. City trend is a city-specific linear trend. Standard errors are clustered at the city level. t-statistics are in parentheses. *p <.1; **p <.05; ***p <.01.

on the textual descriptions of the policy changes is difficult, we represent the strength using two methods. First, we note that regulated cities include 4 Tier-1 cities and 18 non-Tier-1 cities. HPR dampens the rise in house prices and the growth in home sales for Tier-1 cities much more than for the non-Tier-1 cities. Table IA.8 in the Internet Appendix tabulates this observation. This is unsurprising because speculation in the housing market was more salient in Tier-1 cities, and the purpose of the HPR was to cool down speculative demand. We use this fact, and examine whether cities neighboring Tier-1 cities experience a larger increase in house prices and automobile spending. We use the same difference-in-differences specification used in Equation (2) but add further interaction terms for the treatment and post dummies with a dummy that indicates whether a city's nearest regulated city is a Tier-1 city.

Second, we measure the strength of HPR shocks with the response of house prices and home sales in the regulated city where the restrictions were applied. We adopt a heterogenous treatment effect estimation strategy that relates the magnitude of the treatment effect on the unregulated city to the strength of HPR shocks in its nearest regulated city. Specifically, we use the same difference-in-differences specification from Equation (2) but add further interaction terms for the treatment and post dummies with the local post-treatment decrease in the growth rate of house prices or with the local post-treatment decrease in home sales in the nearest regulated city.

Table 5 reports these estimation results. Indeed, in treated cities near Tier-1 cities, and in treated cities whose nearest regulated city had a larger decrease in the growth rate of house prices or in home sales after the HPR, the increases in house prices and automobile spending are significantly greater. In these treated cities, the increase in home sales also appears larger but with a lower statistical power due to insufficient data (covering only 73 cities). In these treated cities, the increase in the Baidu home search index is also larger whenever the estimated coefficient is statistically significant.

2.6.6 Baidu measure of nonautomobile spending. In this subsection, we provide evidence that the nonautomobile spending in treated cities also responds positively to the HPR shocks, which corroborates the increase in automobile spending. Data sources on Chinese household consumption at a monthly frequency are rare, and we rely on the search-based measure of consumption we constructed, namely, the Baidu nonautomobile spending index.

Figure 6 plots the relative differences in the Baidu nonautomobile spending index between treated and control cities, both before and after the HPR shocks. This is based on a regression using the same specification from Equation (1). The figure shows no difference in the index between treated and control cities in a 1-year period before the first round of HPR. After that, the treated cities see a significant increase in the nonautomobile spending index, and the response

Table 5
Heterogeneous treatment effects by policy effectiveness in the closest regulated city

	(1) log(House price)	(2) log(Auto spending)	(3) Home sales	(4) Baidu home search
Treat × Post1	0.014**	0.058***	9.065	84.855***
	(2.053)	(3.182)	(1.131)	(7.457)
Treat × Post1	0.097***	0.020	−0.313	34.166
× Tier 1 city neighbors	(2.782)	(0.650)	(−0.028)	(0.659)
Treat × Post2	0.053***	0.132***	18.025	140.894***
	(4.348)	(4.710)	(1.357)	(7.908)
Treat × Post2	0.109**	0.102**	14.226	−5.545
× Tier 1 city neighbors	(2.471)	(2.105)	(1.180)	(−0.070)
Observations	20,331	20,331	2,505	8,025
R^2	.984	.987	.554	.942
Treat × Post1	0.018***	0.037**	8.769	66.984***
	(2.661)	(2.379)	(1.035)	(6.347)
Treat × Post1	0.024***	0.017	0.710	24.382**
× Closest regulated city HPG decline	(3.532)	(1.559)	(0.134)	(2.581)
Treat × Post2	0.054***	0.106***	21.615	136.113***
	(4.650)	(4.075)	(1.650)	(7.131)
Treat × Post2	0.042***	0.038**	−7.287	−10.028
×Closest regulated city HPG decline	(4.206)	(2.580)	(−0.899)	(−0.729)
Observations	20,331	20,331	2,505	8,052
R^2	.984	.987	.554	.941
Treat × Post1	0.021***	0.037**	5.411	66.335***
	(2.820)	(2.291)	(0.705)	(5.866)
Treat × Post1	0.010**	0.029***	4.286	20.728**
× Closest regulated city volume decline	(2.023)	(2.977)	(1.286)	(2.435)
Treat × Post2	0.058***	0.112***	12.703	134.494***
	(4.697)	(4.204)	(1.060)	(6.819)
Treat × Post2	0.020**	0.037***	5.451	−12.426
× Closest regulated city volume decline	(2.281)	(2.663)	(0.902)	(−0.965)
Observations	19,583	19,583	2,469	7,744
R^2	.983	.987	.555	.941
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
City trend	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

This table reports the heterogeneity treatment effect results comparing cities neighboring regulated cities differently affected by the house purchase restrictions. The sample consists of all unregulated cities, and the data span from 2012m1 to 2017m8. Regressions are at the city and month levels. The dependent variables are log house price in column 1, log seasonally adjusted automobile spending in column 2, home sales index (2016m7=100) in column 3, and Baidu home search index in column 4. Treat is a dummy that takes the value of one if the city is within 250 km of the nearest city regulated by house purchase restrictions. Post1 is a dummy that takes the value of one if the time is after the first round of the house purchase restrictions and before the second round of the restrictions. Post2 is a dummy that takes the value of one if the time is after the second round of the restrictions. Tier 1 city neighbors is a dummy that takes the value of one if the city's closest regulated city is a Tier 1 city. Closest regulated city HPG decline is the decline in house price growth rate from 2015m8 to 2016m8 and 2016m8 to 2017m8, of the closest regulated city. Closest regulated city volume decline is the decline in volume of home sales from 2015m8 to 2016m8 and 2016m8 to 2017m8, of the closest regulated city. The control variables are per capita GRP, resident population, square meters of road per capita, the number of public buses per capita, and exposure to the nearest regulated city's GRP. City trend is a city-specific linear trend. Standard errors are clustered at the city level. t-statistics are in parentheses. *p <.1; **p <.05; ***p <.01.

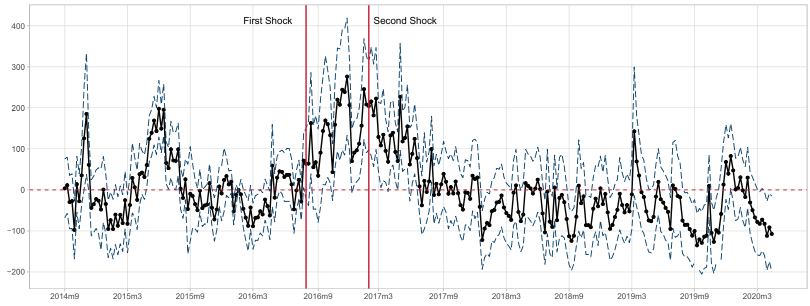

Figure 6
Spillover effect of house purchase restrictions on Baidu nonautomobile spending index
The figure plots the estimated responses of an alternative household consumption measure, namely, the Baidu nonautomobile spending index, in treated cities relative to control cities, both before and after the house purchase restrictions. The index measures weekly intensity of Baidu web searches of keywords related to a basket of pricey consumption goods, including smartphones (iPhone, Huawei Phone, Vivo, OPPO), sportswear (Nike and Addidas), prestige cosmetics (Estée Lauder, Lancôme, Saint Laurent), as well as watches (no brand specified) and Moutai Wine (top liquor brand in China). The responses are estimated using a difference-in-differences regression replacing post-treatment dummies with time dummies. City fixed effects and time fixed effects are added. The 95% confidence interval is drawn based on standard errors clustered at the city level. Two rounds of house purchase restrictions, in September 2016 and March 2017, are represented by vertical red lines.

fades after September 2017. The pattern is consistent with that of automobile spending.

We also redo the main difference-in-differences estimation and the robustness checks but replace automobile spending with the Baidu nonautomobile spending index. Table IA.9 in the Internet Appendix tabulates the regression results. Although we cannot interpret the economic magnitude of the coefficients, the sign of the coefficients and their statistical significance are consistent with our results on automobile spending, and indicates the local consumer spending responses to the increase in house prices are not limited to automobile spending.

3. Heterogeneity in Spending Responses and the "Pure" Housing Wealth Effect

What is the mechanism underlying the increase in automobile spending when the house prices increase in treated cities? We now provide evidence consistent with a "pure" housing wealth effect (Sinai and Souleles 2005; Buiter 2010); that is, an individual's spending response to house prices depends on the gap between the value of owned housing assets and the discounted value of housing consumption. On the one hand, renters who plan to purchase homes, for example, to obtain *hukou* and access to local public services, such as education and public health care (Chen, Shi, and Tang 2019), would cut back on consumption spending when house prices rise even if rents are unchanged. On the other hand, homeowners who own more than the discounted value of future housing consumption would increase spending.

The evidence comes from the heterogeneous spending responses across consumers. We focus on two consumer groups that we are able to measure with the data: the locally born population and the non-locally-born population. We find that first, the locally born population, who are more likely to be homeowners than renters, increases automobile spending when house prices increase. The non-locally-born population, instead, does not increase automobile spending. Second, leveraging homeownership data from household surveys, we show that homeowners increase their automobile spending, while renters decrease their automobile spending in reaction to the rise in house prices. The varied spending responses also indicate the substantial redistributions caused by the HPR shocks.

3.1 Combining survey information to test the "pure" housing wealth effect

3.1.1 Locally born versus nonlocally born individuals. We first show that being locally born or nonlocally born at the time of an automobile purchase is a good proxy for the buyer's homeownership status. We combine the nine waves of household surveys in China to estimate the relationship between the locally born/non-locally-born status and homeownership status. To the best of our knowledge, these are the only nationally representative surveys in China that provide the data we need, specifically whether a surveyed individual or their immediate coresiding family rents or owns the home, as well as whether they are locally born.

Column 1 of Table 6 presents this first-stage regression's results. Compared to the nonlocally born, the locally born surveyed subjects are 14.8% less likely to be a renter, or equivalently 14.8% more likely to be a homeowner. This is as expected and has institutional reasons: the non-locally-born group has more limited state transfer of housing assets during the economic reform and benefits less from the intergenerational transmission of wealth (Wang 2011; Cui, Geertmen, and Hooimeijer 2016).

The housing wealth effect predicts a stronger spending response for the locally born population. We test this prediction by estimating Equation (2), while adding a dummy to the interactions of the treatment and post dummies to indicate whether automobiles are purchased by the locally born or the non-locally-born population. The coefficients on these interaction terms are denoted by θ_{1k}'s and θ_{2k}'s, for $0 \leq k \leq 5$. Columns 2 and 3 of the table give the averages of θ_{1k}'s and θ_{2k}'s that measure the average responses during the 6-month periods after the two HPR shocks of the locally born population relative to the non-locally-born population in the treated cities. They indicate that the locally born individuals on average increase automobile spending by 14.5% and 18% and the number of automobiles purchased by 13% and 17.5% more than the non-locally-born individuals. The columns also indicate essentially no change in either spending or purchases from the non-locally-born individuals and, if anything, a reduction in automobile purchases. Figure IA.4 in the

Table 6
DID heterogeneity in spending responses: Locally born versus nonlocally born, survey-predicted renters versus homeowners

	(1) Home Ownership	(2) log(Auto Spending)	(3) log(Auto Purchases)	(4) log(Auto Spending)	(5) log(Auto Purchases)
Constant	0.812***				
	(58.675)				
Born in current city	0.148***				
	(4.219)				
Treat × Post1		−0.032	−0.040*		
		(−1.385)	(−1.768)		
Treat × Post1 × Born in current city		0.145***	0.130***		
		(5.688)	(5.110)		
Treat × Post2		−0.018	−0.027		
		(−0.568)	(−0.898)		
Treat × Post2 × Born in current city		0.180***	0.175***		
		(7.519)	(7.048)		
Treat × Post1 × Renter				−0.723***	−0.660***
				(−2.722)	(−2.625)
Treat × Post1 × Owner				0.252***	0.216***
				(3.854)	(3.428)
Treat × Post2 × Renter				−0.874***	−0.861***
				(−2.784)	(−2.642)
Treat × Post2 × Owner				0.335***	0.317***
				(4.091)	(3.696)
Observations	62,554	33,185	33,185	33,185	33,185
R^2	.818	.986	.989	.986	.989
Controls		Yes	Yes	Yes	Yes
City× Born in current city		Yes	Yes	Yes	Yes
City trend		Yes	Yes	Yes	Yes
Time FE		Yes	Yes	Yes	Yes

In this table, column 1 shows the first stage relationship between home ownership and locally born/non-locally-born status based on survey data. The relationship is used to impute rentership and homeownership. Standard errors are clustered at the survey-year-group level in column 1. Columns 2 and 3 show the difference-in-differences estimates of the responses of automobile spending and number of automobile purchases, of locally born buyers relative to non-locally-born buyers. The regression specification is $\log \text{Spending}_{j,i,t} = \sum_{0\leq k\leq 5} \beta_{1k} \cdot Treat_i \times \mathbb{I}_{\{t=2017m3+k\}} + \sum_{0\leq k\leq 5} \theta_{1k} \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \times \text{Born in Current City}_j + \sum_{0\leq k\leq 5} \beta_{2k} \cdot Treat_i \times \mathbb{I}_{\{t=2017m3+k\}} + \sum_{0\leq k\leq 5} \theta_{2k} \cdot Treat_i \times \mathbb{I}_{\{t=2017m3+k\}} \times \text{Born in Current City}_j + \psi \cdot \text{Born in Current City}_j + \Gamma X_{i,t-1} + \text{City FE}_i + \text{Time FE}_t + \theta_i \quad t + \epsilon_{i,j,t}$, where j,i,t denotes population group, city, and time. Columns 4 and 5 show the responses of renters and homeowners. Automobile spending and number of automobiles purchased are seasonally adjusted. The sample consists of all unregulated cities, and the data span from 2012m1 to 2017m8. Treat is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post1 is a dummy that takes the value of one if the time is after the first round of the house purchase restrictions and before the second round. Post2 is a dummy that takes the value of one if the time is after the second round of the restrictions. Born in current city is a dummy variable that takes the value of one if the automobile purchase is made by individuals born in the city they reside in. Renter and Owner are the imputed rentership and homeownership rates. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to the nearest regulated city's GRP. City trend is a city-specific linear trend. Standard errors are clustered at the city level in columns 2 and 3, and are bootstrapped in columns 4 and 5 after taking into account both first- and second-stage errors. t-statistics are in parentheses. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Internet Appendix provides a graphical summary of the estimated spending responses of the two populations. The size of the redistribution that these estimates indicate is substantial, since consumption of the locally born and the nonlocally born are equally important and sizable. For example, the summary statistics table shows that on average, the automobile spending of the locally born population and the non-locally-born population are 146 million and 159 million yuan per city per month, respectively.

We provide two robustness checks for the findings. First, we use a regression similar to Equation (1). Figure 7 displays the differences in automobile spending between locally born and non-locally-born groups, both before and after the HPR shocks that visually depict any preexisting differences within a city that could confound the subgroup estimates. Ideally, there should not be any. Panel A shows that within the treated cities, there are indeed no significant preexisting differences between the subgroups before the house price shocks. Right after the shocks, however, locally born individuals significantly increase their automobile spending relative to the non-locally-born group. Panel B shows that in the control cities, such differences are not significant at all, either before or after the shocks. The second robustness check is to perturb the cutoff distance that designates the treatment and control group to be 300 km, 200 km, or 150 km. Table IA.10 in the Internet Appendix reports the estimation results. Different choices of the cutoff distance do not change the findings.

3.1.2 The "pure" housing wealth effect. The locally born and the non-locally-born populations are no different from each other with respect to the house price shocks, besides the former are more likely to be homeowners. From the different responses of the two populations, we can thus infer the average spending response of renters and homeowners, respectively, to the house price shocks. Based on the first-stage relationship between the locally born/non-locally-born status and homeownership status in the survey data, and the locally born/non-locally-born status in the automobile spending data, we compute the predicted likelihood of an automobile buyer being a renter (otherwise a homeowner). And then we study how much the spending responses of homeowners and renters differ. Specifically, we estimate the following equation:

$$\begin{aligned}\log \text{Spending}_{j,i,t} &= \sum_{0\le k\le 5} Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \times \left[\alpha_k^o Own_j + \alpha_k^r Rent_j\right] \\ &+ \sum_{0\le k\le 5} Treat_i \times \mathbb{I}_{\{t=2017m3+k\}} \times \left[\beta_k^o Own_j + \beta_k^r Rent_j\right] + \psi^o Own_j \\ &+ \psi^r Rent_j + \Gamma X_{i,t-1} + City\ FE_i + Time\ FE_t + \theta_i t + \epsilon_{i,j,t} \end{aligned} \tag{4}$$

where $\text{Spending}_{j,i,t}$ is the automobile spending for a demographic group (local vs. nonlocal) j of city i at time t. $Rent_j$ and Own_j are the predicted likelihood of being renters and homeowners. Other variables are defined in Equation (2). The coefficients of interest are the averages of α_k^r's, α_k^o's, β_k^r's, and β_k^o's, which represent the responses in spending on new automobiles for renters and homeowners, respectively, on average during the 6-months period after the two rounds of HPR. We use bootstrapped standard errors to correct for the first-stage

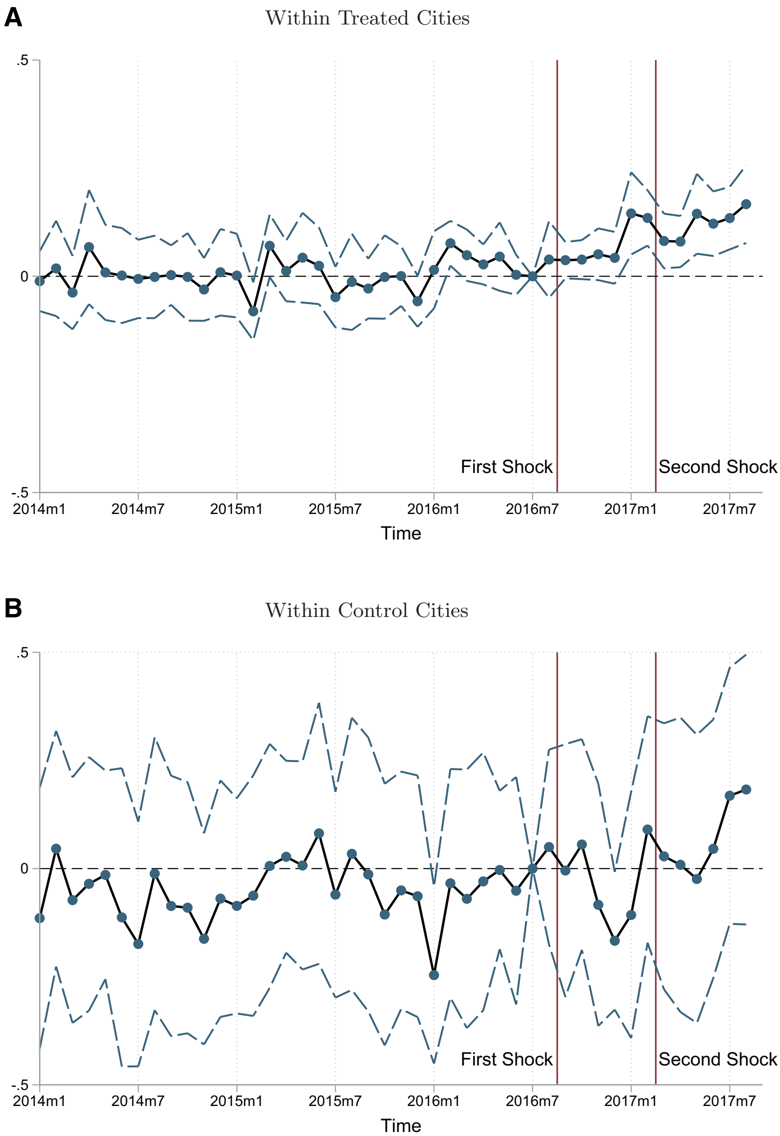

Figure 7
Parallel trends in automobile spending: Locally born versus non-locally-born city residents
The figure plots the estimated responses in automobile spending of the locally born residents relative to the non-locally-born residents, both before and after the house purchase restrictions. The responses are estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. All responses are relative to the level of responses in July 2016. Panel A plots the relative responses of locally born residents estimated within the treated cities, that is, those within 250 km of the nearest regulated cities. Panel B plots the relative responses estimated within the control cities. City fixed effects, time (year-month) fixed effects, city-level controls and city-specific linear trends are controlled for. Automobile spending is seasonally adjusted. The 95% confidence interval is drawn based on standard errors clustered at the city level. Two rounds of house purchase restrictions, in September 2016 and March 2017, are represented by vertical red lines.

estimation errors. Specifically, we bootstrap both the first-stage estimation of the predicted values of $Rent_j$ and Own_j and the second-stage estimation of spending responses.

Columns 4 and 5 of Table 6 present the results of estimating Equation (4). Figure IA.5 in the Internet Appendix provides a graphical summary of the estimated spending responses across survey-predicted housing tenure statuses. The results show that renters on average significantly decrease their automobile spending and homeowners on average significantly increase their automobile spending in response to the policy shocks. The results are consistent with the predictions of the "pure" housing wealth effect channel.

3.2 Alternative explanations

Alternative explanations for the increase in automobile spending in treated cities are the permanent income channel, the labor relocation channel, and the collateral channel. We examine these explanations and explain why they do not fit our findings.

The permanent income channel refers to the possibility that improvements in the growth prospects of the treated cities may lead to a simultaneous increase in spending and house prices. This explanation would predict the changes in economic fundamentals in the treated cities, which we do not detect in Table 3. Furthermore, the channel would predict similar increases in spending for the locally and the non-locally-born populations, and for homeowners and renters. This is inconsistent with what we find.

The labor relocation channel, that is, the imposition of HPR leads workers to migrate to nearby cities, which leads to spending and house price increases, would also predict relative increases in economic fundamentals in the treatment cities, which we do not observe. This explanation also predicts that the spending increase comes more from the non-locally-born group, which we do not find. One form of migration is to live in the nearby unregulated city and commute to work in the regulated city. But our continuous distance specification indicates that unregulated cities that are >100 km but <500 km away from the regulated cities still observe increases in house prices and spending, and evidently it is too far to commute from these unregulated cities to the regulated cities.

The collateral channel, that is, the increase in house prices enables households to finance their consumption by pledging the more valuable housing assets, does not predict a negative spending response for renters, since house prices actually increase and rents do not change unless there is strong expectation of rent increases in the future, which is hard to test. Furthermore, based on household survey data, we observe a low prevalence of home equity borrowing for the purpose of consumer spending. Table IA.11 in the Internet Appendix shows that only 2.2% of all homeowners reported existing refinanced mortgage debt or HELOCs in 2015. In 2017, even less of the homeowners (1.5% of all homeowners) had outstanding refinanced mortgages. Besides, only 0.4% of the refinancing and HELOC users (0.01% of all homeowners) report using the funds to buy cars. The most prevalent uses of the funds are to buy another home (87.2%), to support personal business (5.6%), and to lend in informal markets (2.7%). Besides,"borrowing to consume" is generally less accepted in China's consumer culture. The 2018 CFPS survey found that 66.7% of respondents disagree and 23.6% somewhat disagree with the action of "borrowing to consume". Relatedly, the share of automobile purchases on installment loans (23.9% in 2017) is also lower than in the United States (85% in 2017).

That the spending of the non-locally-born population does not increase helps us rule out other confounding effects, for example, other policy shocks that may affect automobile spending in the treated cities. One policy shock that we find relevant is that several regulated cities restricted automobile purchases for road capacity and environmental reasons by rationing license plate registrations. This restriction led some buyers to purchase and register new cars in nearby cities to partially circumvent this rationing. Although this restriction was implemented before 2014, much earlier than the HPR, these cross-city purchases may still artificially inflate automobile spending in the nearby cities. However, these purchases would have shown up as an increase in automobile spending by non-locally-born individuals, which we did not find.

4. Discussions

Here we provide additional discussions regarding our quasi-experimental estimate of the house price and spending effect of the HPR shocks.

4.1 MPC comparison and external validity

We convert our quasi-experimental estimate of the spending elasticity to the MPC by using the following formula.

$$\text{MPC} = \text{Elasticity} \times (\text{Spending}/\text{Housing Wealth}). \tag{5}$$

where the elasticity of automobile spending to house prices is 1.94 (column 9 in Table 2). There is no existing flow-of-funds-based measure of the household balance sheet for our sample period and cities, so we construct a housing wealth measure using the perpetual inventory method in Zhang (2019). The average ratio of annual automobile spending to housing wealth in our sample period is 0.025. Our quasi-experimental estimate of the automobile MPC out of housing wealth is thus 0.048. Mian, Rao, and Sufi (2013) report an automobile MPC of 0.018 out of housing wealth, which constitutes about 40% of the overall MPC. Aladangady (2017) find an overall housing MPC of 0.047, for which applying the same 40% percentage corresponds to an automobile MPC of 0.019.

Our MPC for this period in China appears to be larger than the corresponding MPC estimates in Mian, Rao, and Sufi (2013) and Aladangady (2017) for the United States. The difference remains after accounting for the standard errors of our MPC estimate and Mian, Rao, and Sufi's (2013) MPC estimate, which are 0.018 and 0.001, respectively: a one-sided test of our estimated MPC being larger would yield a p-value of 4.8%. This difference also remains after accounting for uncertainty in specifications: our lowest MPC estimate across all robustness specifications is 0.023.

A large MPC of housing wealth may be possible when (1) the increase in house prices reflects speculative demand with no obvious changes in housing fundamentals and (2) there are a large presence of multiple-property owners. The presence of investment homes in the household portfolio in China, as measured by the portfolio share or by the value-to-income ratio, all increased substantially during the sample period of our study; a pattern we show in Figure IA.6 in the Internet Appendix. According to the 2016 waves of the Survey of Consumer Finances (SCF) for the United States, the ownership rate of investment homes in the United States is 7%. In contrast, Table IA.11 in the Internet Appendix shows that the survey multi-home ownership rate in China is around 18.0% with little regional variation that indicates a large presence of households who would be able to increase consumption under the "pure" housing wealth effect.

Our MPC estimates may be relevant to many economies where investment demand for houses is prevalent. The role of housing as an investment vehicle is integral not only to China (Cao, Chen, and Zhang 2018) but also to other economies in which the direct finance market is less developed (Badarinza, Balasubramaniam, and Ramadorai 2018). According to the Household Finance and Consumption Survey (HFCS), in the euro area, while the lowest ownership rate of investment real estate is 8.1% in the Netherlands, the highest ownership rate of investment real estate reaches 46% in Cyprus. For India, Badarinza, Balasubramaniam, and Ramadorai (2016) show that houses and land are the most prevalent investment vehicles, even more prevalent than in China. Figure 8 plots the ownership rate of the investment real estate assets versus the financial development index for the 23 countries in Badarinza, Campbell, and Ramadorai (2016). It shows the high investment real estate ownership rate in countries with underdeveloped financial markets for which our analysis may be applicable.

4.2 Economic significance of the overall spending response

What is the macroeconomic magnitude of the automobile spending increase caused by the HPR shocks? We conduct back-of-the-envelope calculations as follows: column 8 of Table 2 shows that the treated cities experience a weighted average increase of 6.8% in automobile spending that can be attributed to the unintended spillovers from HPR in the regulated cities. Accounting for that the total annual automobile spending in treated cities immediately before the HPR is US$136 billion, and that the lowest estimate of the weighted average increase in automobile spending caused by the HPR is 3.3%, we observe a causal increase of 4.5 to US$9.3 billion in automobile spending in nearby unregulated cities. Also, the average annual increase in automobile sales in the event window is US$37.8 billion, according to the industry association. Compared to this statistic, our estimated spending response to the house price

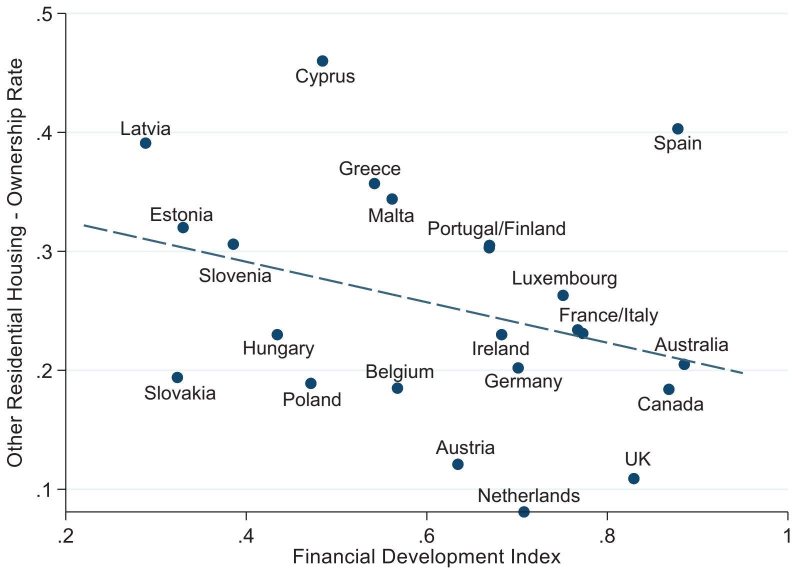

Figure 8
Cross-country comparison of multiproperty ownership rate
The figure plots the participation rate of households that hold investment housing assets, defined as housing assets other than the main residence, against the financial development index, of the 23 countries in Badarinza, Campbell, and Ramadorai (2016). The participation rate data are based on the Household Finance and Consumption Survey (HFCS) for Europe, except the United Kingdom, and Badarinza, Campbell, and Ramadorai (2016) for Australia, Canada, and the United Kingdom. The financial development index is the IMF financial development index (Svirydzenka 2016). The dashed line plots the fitted values from an ordinary least squares (OLS) regression.

shocks helps explain 12% to 25% of the aggregate annual increase in automobile sales, a non-negligible amount. Further, the size of redistribution is substantial: the difference in the spending responses across consumer groups in treated cities is of the same order as the positive effect in the aggregate.

5. Conclusion

In this study, we provide a causal evaluation of the unintended consequences of regulating housing speculation at the local level. Specifically, we exploit a quasi-natural experiment in China, where local authorities imposed restrictions on investment home purchases in 2016 and 2017. While these restrictions were effective in containing the surge in local house prices, they triggered capital flight into nearby, unregulated housing markets. House prices in these unregulated cities rose sharply following the out-of-town home purchases despite no obvious improvements in the local housing fundamentals. The increased housing wealth raised the consumption spending on automobiles in the aggregate.

The locally born population, who are more likely to be homeowners than renters, increased automobile spending with the increase in housing wealth. The non-locally-born population instead, did not increase automobile spending. The heterogeneous spending responses across demographic groups show the substantial redistribution caused by the capital flight and out-of-town home purchases. The HPR shocks also provide a quasi-natural experiment that allows us to estimate the housing MPC. Our housing MPC is higher than that in the literature and indicates the importance of investment demand in the housing market in affecting the MPC. We hope our findings have implications for the future design of policies that regulate housing speculation. In future studies, the HPR shocks could be used to study the impact of the housing market on other real outcomes. For many developing countries, further research is needed to better understand the interaction of investment demand in the housing market, household financial decisions, and financial market dynamics.

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