Capital Leakage, House Prices, and Consumer Spending: Quasi-Experimental Evidence from House Purchase Restriction Spillovers

Yinglu Deng*, Li Liao†, Jiaheng Yu‡, Yu Zhang§

First draft: April 2018
This draft: March 2021

Abstract

Employing a unique quasi-experiment – spillovers caused by the imposition of local house purchase restrictions to nearby non-regulated cities, we study the house price and real spending effects of out-of-town housing demand and policy spillovers. The quasi-experiment induces sharp abnormal increases in house prices but not local fundamentals in nearby non-regulated cities, providing plausibly exogenous house price booms. The estimated spending response is positive in the aggregate, and is redistributive echoing Favilukis and Van Nieuwerburgh (forthcoming), negative for renters, but positive for homeowners. Our results suggest that the "pure" housing wealth effect can engineer powerful real consequences.

JEL classification: E21, G12, G18, H23, R21, R51
keywords: consumer spending, house prices, wealth effect, quasi-experiment, policy spillovers, out-of-town demand

We are grateful to Stijn Van Nieuwerburgh, two anonymous referees, Efraim Benmelech, Russell Cooper, Jefferson Duarte (discussant), Hanming Fang, Carola Frydman, Ruixue Jia, Jennifer Li (discussant), Wenli Li, John Mondragon, Jonathan Parker, Nancy Qian, Wenlan Qian, Michael Zheng Song, Emil Verner, Wei Xiong, Junfu Zhang, Li'An Zhou and conference and seminar participants at CICF 2019, CFRC 2019, CCER Summer Institute 2019, China Economics Summer Institute 2019, Econometric Society Asian Meeting 2019, the 2019 SJTU-UCL Joint Workshop on Macro-Finance, Jinan IESR and SHUFE for invaluable comments and discussions that help improve this research. All errors are our own.
*PBC School of Finance, Tsinghua University. Email: dengyl@pbcsf.tsinghua.edu.cn.
†PBC School of Finance, Tsinghua University. Email: liaol@pbcsf.tsinghua.edu.cn.
‡MIT Sloan School of Management. Email: yujh@mit.edu.
§Guanghua School of Management, Peking University. Email: yuzhang@gsm.pku.edu.cn.

1 Introduction

Our paper starts with three-pronged motivations. The first motivation is to study the effects of policy spillovers. Individual regions, countries within a union, or states and cities within a nation, often make locally-motivated policies. In terms of the housing market, these policies take the form of regulations on land supply, credit supply, price cap, zoning, and so on to achieve locally optimal outcomes. However, these local policies may be suboptimal in the aggregate if potential spillover effects are present but not considered.[1] Empirically, the nature and magnitude of the spillover of local policies need to be better understood.

The second motivation regards the role of external and non-local housing demand in not only asset prices, but also the real economy. Recent studies show that out-of-town investors contribute significantly to house price booms in global cities (Badarinza and Ramadorai, 2018; Cvijanovic and Spaenjers, 2018; Sá, 2016), and significantly drives the housing demand even in medium-sized cities (Chinco and Mayer, 2016).[2] Out-of-town investors potentially also have influence on the city residents through their housing market impact. For example, Favilukis and Van Nieuwerburgh (forthcoming) build and calibrate a theoretical framework to analyze the effects of out-of-town housing demand on house prices, local consumption, and city welfare. However, to the best of our knowledge, no empirical study so far seems to have causally estimated these real effects of out-of-town demand.

The third motivation is to revisit the housing wealth effect, of which both the identification and the mechanisms are at the center of recent academic debate. Such a revisit is essential, because in most economies, housing is the most significant form of private wealth (Yao and Zhang, 2005; Badarinza, Campbell, and Ramadorai, 2016b), and consumer spending is the largest component of aggregate demand. A key challenge to the identification of the housing wealth effect is the lack of suitable settings providing exogenous house price shocks unrelated to future economic fundamentals. A series of recent papers instrument for house prices using the land supply elasticity (Saiz, 2010)

[1]See Farhi and Werning (2017) and Rodrik (2019) for reasons to internalize the spillover effects of locally-motivated policies in a more general context.

[2]For example, research reports CRIC (2018a,b, 2019) reveal that the out-of-town investors' housing demand is recently substantially high across many medium-sized cities in China.

interacted with declines in mortgage rates (Aladangady, 2017). However, Davidoff (2016) shows that elasticity measure correlates with follow-on fundamental growth, and interacting with mortgage rates does not fully resolve the endogeneity issue. A new empirical approach is hence needed. Moreover, while the collateral view (Mian, Rao, and Sufi, 2013; Adelino, Schoar, and Severino, 2015; DeFusco, 2018) suggests the housing wealth effect potentially operates through home equity extraction and borrowing constraint relaxation in the United States, easy access to home equity borrowing may be conditional on more developed or less regulated financial markets (Calza, Monacelli, and Stracca, 2007). It is unclear whether the housing wealth effect is still important in economies with less prevalent use of home equity borrowing,[3] and if so, through what channels.

In this paper, we further the understanding of the three questions above. Specifically, we provide causal evidence on the asset pricing and real effects of out-of-town housing demand and policy spillovers, by exploiting a unique quasi-experiment — spillovers caused by the imposition of local house purchase restrictions (HPRs) in several Chinese cities, to the nearby non-regulated cities. Concerned with the surging local house prices over the past decade, which grew as fast as 10% annually (Fang, Gu, Xiong, and Zhou, 2016; Wu, Gyourko, and Deng, 2016), large cities rolled out two rounds of purchase restrictions in late 2016 and early 2017, to cool down investor demand on non-owner occupied homes. The measures include raising down payments on investment home purchases and, in some instances, outright forbidding these purchases. Effective in containing the local house price surge, these restrictions however appear to introduce significant capital leakage across markets – investors facing suddenly increased speculation cost in the regulated cities, shift demand to the nearby non-regulated cities. Our results show that the shift of speculation demand leads to a sharp and immediate abnormal increase in house prices in the nearby non-regulated cities, in a way that is unrelated to local fundamentals, providing a house price boom that is plausibly exogenous. The house price boom, in turn, spurs an abnormal increase in consumer automobile spending that is important in the aggregate, explaining 12% to 25% of the sample period average annual growth of automobile sales. The spending response is redistributive within the nearby non-regulated city, across the local-born versus the non-local-born population, two groups

[3] According to the China Household Finance Survey (CHFS), the percentage of homeowners with existing refinanced mortgage debt or HELOCs is 2.2% in 2015 in China, out of which only 0.4% (or equivalently, 0.01% of all homeowners) use the loan proceeds for consumption spending. The percentage of homeowners with existing refinanced mortgage debt or HELOCs reduces to 1.5% in 2017, in the background of a tightening of the credit regulation.

are empirically different in housing tenure status. Inferring from administrative and survey data combined, the spending response is significantly negative for renters and positive for homeowners. We show our results are best explained by the "pure" housing wealth effect (Sinai and Souleles, 2005; Buiter, 2010; Berger, Guerrieri, Lorenzoni, and Vavra, 2017; Kaplan, Mitman, and Violante, forthcoming), while alternative explanations including the permanent income channel, the labor relocation channel, and the collateral channel would struggle.

Our approach uses a difference-in-difference design in the context of spatially heterogeneous treatment effects in face of HPR spillovers. We exploit spatial variations in the exposure to the policy-induced spillovers of housing demand from the regulated cities, by splitting the non-regulated cities into two similar-sized groups based on whether their distance to the closest regulated city is within 250 km. All results are robust to choices of this cutoff distance, as well as an alternative continuous specification for designation of the treatment status. All else equal, non-regulated cities that are closer to the regulated cities are more likely to attract housing market investors in the latter.[4] On the other hand, though the treated cities (non-regulated cities within 250 km to regulated cities) are within the distance for occasional visits, they are empirically not within commuting distance to the regulated cities. This relatively large distance helps us eliminate confounding factors, including labor market adjustment, in response to the policy shocks.[5] Because the cities are inherently different in distance to the regulated cities, the urban literature has shown such initial conditions may predict growth rate differences (Glaeser, Scheinkman, and Shleifer, 1995; Blanchard and Katz, 1992; Glaeser and Nathanson, 2017), we additionally control for city-specific trends. The requirement then for a significant treatment effect is whether the treatment has made the outcome in the treatment group to increase abnormally more compared to the control group, more than if the pre-treatment city-specific trends had just continued. We estimate that house prices in the treated cities increase by 2% to 6% abnormally within a few months compared to the control cities, controlling for city-specific linear trends. We also provide results that suggest this increase is orthogonal to local economic conditions: there is no difference between treated and control cities in rent dynamics or other dimensions of local fundamentals, including post-treatment

[4] The closeness makes it easier for investors to acquire information on individual houses and monitor the status of the houses once purchased. Coordination among investors may also concentrate demand in closer non-regulated cities.

[5] For example, commuting choice decisions may adjust if the non-regulated cities are within commuting distance, where workers in the regulated cities may choose to live instead in the non-regulated cities and commute to work. Our setting is not subject to this confounding factor.

employment, population, and output growth.

What explains the increase in house prices if fundamentals and rents are unaffected in the nearby non-regulated cities? We find evidence that, taken as whole, suggests a capital leakage explanation, whereby investors facing increased transaction costs in the regulated cities turn to invest in nearby non-regulated cities, bringing in capital flows to the treated cities. We find that first, the reductions in transaction volumes in the regulated city are consistent with the increase in volumes in the treated cities. Second, we examine out-of-town web searches, from the regulated cities, for real estate in treated versus control cities. The data is from Baidu, the leading search engine company in China. Analyzing the out-of-town search data, we find a significant increase in attention from regulated cities on real estate in the treated city compared to the control cities after the HPR spillover shocks. Third, our estimates also indicate that bank deposits increase significantly in the treated cities realtive to control cities after the shock. In the absence of fundamental changes, this is consistent with a capital inflow, and the magnitude of this deposit effect is similar to the the house price rises.[6]

Next, we estimate the effects of the spillover-driven house price booms on consumer spending. We proxy for consumer spending by spending on private purchases of new automobiles, following Mian, Rao, and Sufi (2013) and Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru, and Yao (2017). Automobile purchasess makes up the third largest category of consumer spending in China, after housing and food, according to the 2015 wave of CHFS. To undertake this research opportunity, we introduce a new administrative dataset that provides precise and comprehensive information on all purchases of new consumer automobiles in China, allowing us to disaggregate by cities and also by local-born versus non-local born population with the city, two demographic groups that differ substantially in housing tenure statuses. The data is registry-based, which has the advantage of being relatively free of measurement errors compared to survey data (Koijen, Van Nieuwerburgh, and Vestman, 2014).[7]

[6] Anecdotally, government announcements by local housing bureaus cite out-of-town investment demand as being responsible for the house price rise. See an example of a local government's official notice regarding the matter at `http://pkulaw.cn/fulltext_form.aspx?Gid=18847064`.

[7] (Koijen, Van Nieuwerburgh, and Vestman, 2014) show that 35% of respondents to consumption survey in Sweden forget to report the car they bought according to registry data, leading to severe measurement error. Our registry-based data complements the card and survey data used in other existing housing wealth effect studies (Gan, 2010; Agarwal and Qian, 2017), which covers more than one category but can be incomplete or subject to misreporting. Our

Our baseline estimate suggests that in response to the spillover-driven house price booms, household spending on new automobiles increases abnormally by 6% to 14% in the nearby non-regulated cities relative to the control cities, relative the counterfactual that house prices and spending had just grown at the pre-event trends. To arrive at the baseline estimate, we control for time-invariant city-level unobservable heterogeneities, city-specific linear time trends, aggregate macroeconomic fluctuations, and other local factors that potentially influence automobile purchases including output per capita, population, road infrastructure, and public transportation. We also control for the exposure of local output growth to the nearest regulated city as the regional hub in a hub-and-spoke network, as well as any seasonality that could also be city-specific. On the intensive margin, we find a even larger response in household purchases of more expensive automobiles.

We take several additional steps to ensure the validity of our research design and the robustness of our estimates. Our estimated effects are robust to a "one-step-up" perturbation of the pre-trend assumption purposed in Bilinski and Hatfield (2019), where we control for city-specific trends using cubic splines instead of city-specific linear trends. We also exploit additionally the heterogeneity in the magnitude of the shock in the regulated city to improve identification, where we find that the estimated house price and spending effects in the treated cities are larger if, for the regulated city, the post-HPR decline in house price growth or transaction volumes is larger, supporting the spatially heterogeneous treatment effects. Furthermore, our estimated house market and spending effects are robust to alternative definitions of the treatment status, including using a continuous distance specification where the spatial treatment effect decays continuously, or perturbing the cut-off distance defining the treated cities, as well as defining the treatment status alternatively according to railroad travel times. Our estimated effects are robust to a matching specification, where we match control cities that are similar to treated cities in pre-event house prices, output per capita, and exposure of output growth to the nearest regulated city, but are different in the distance. Finally, we obtain an index of searches for big-ticket consumption goods by consumers in each city using data from Baidu as an alternative source of data on consumption, and find estimates that suggest our results is also present outside of car purchases.

data also addresses other critiques on existing non-registry based automobile data regarding imputation errors and lack of within-region demographic variations (Mian, Rao, and Sufi, 2013; Benmelech, Meisenzahl, and Ramcharan, 2017; Aladangady, 2017).

Our estimates across the baseline and various robustness specifications translates to a baseline point estimate of MPC on automobiles of 0.048, and a lowest point estimate of 0.023. Our point estimates for China are larger to the corresponding estimate of MPC on automobiles of 0.018 for the United States in Mian, Rao, and Sufi (2013), but we cannot rule out that this difference may be driven by statistical uncertainty. Our baseline and lowest estimate of the abnormal increase in spending on new automobiles is 60.9 billion RMB and 29.6 billion RMB, respectively. Compared to the aggregate growth of private passenger automobile sales during the event period, our estimated effect accounts for 12% to 25% of the average annual growth of private passenger automobile sales in 2016 and 2017. We take these numbers as suggesting that the unintended consequences of local policy spillovers can be non-negligible.

Next, we report that the spending responses to the quasi-experiment house price booms are heterogeneous, and similar to predictions of Favilukis and Van Nieuwerburgh (forthcoming). We analyze within-city the spending responses of the local-born and the non-local-born population, two groups for which we have separate data in the registry-based data source, and which differ substantially in the homeownership rate. In the survey data, the non-local-born population are significantly more likely to be renters, and the local-born population are significantly more likely to be homeowners. Subsample pre-trend tests suggest that these two groups do not exhibit different trends in the spending data before the event. After the HPR spillover shocks, we find a significantly larger spending response for the local-born group, while essentially a zero spending response for the non-local-born group. Moreover, applying survey measures of imputed housing tenure rates, we estimate that the spending response is indeed significantly positive for homeowners, significantly negative for renters.

The pattern of spending response heterogeneity within the city is consistent with a "pure" housing wealth effect (Sinai and Souleles, 2005; Buiter, 2010), where the part of renters who plan to climb up the housing tenure ladder and purchase homes cut back on consumption spending when house price rises, reducing the estimated spending response for the non-local-born group as well as renters as a whole,[8] and the part of homeowners who own more than the discounted value of

[8]Although renters in Chinese cities enjoy the residential utility of the house or apartment, they do not have rights equal to those of homeowners—whose rights allow *hukou* registration and hence access to local public services such as education and public health care. Chen, Shi, and Tang (2019) uses a regression-discontinuity design to estimate

future housing consumption raises spending, increasing the estimated spending response for the local-born group as well as homeowners as a whole. The aggregate housing wealth effect may be positive if there is a large presence of homeowners who own homes more than the discounted value of future housing consumption, and the house price change is non-fundamental. This maybe the case in China, because homes are important investment vehicles in China (Cao, Chen, and Zhang, 2018),[9] where according to survey data 18% of the urban households owns multiple homes.[10]

Compared to the "pure " wealth effect channel, alternative explanations, including the permanent income channel, the labor relocation channel, and the collateral channel, would struggle to explain our set of findings. The permanent income channel would predict relative increases in fundamentals in the treatment cities, which we do not detect, and similar increases in spending for the local-born and the non-local-born groups, which are counterfactual. The labor relocation channel involves workers in the regulated cities in the treatment cities migrate, find jobs and buy cars, and would predict spending increases from the non-local-born group and increases in fundamentals, both seem to be counterfactual. The collateral channel has the potential of explaining the positive estimated responses for homeowners but would not predict a negative spending response for renters. Via survey data, we also observe a low fraction of households in our economic setting that have had refinanced mortgage debt or had HELOCs, and an especially low fraction of households that use home-equity based borrowing for consumption spending.

Finally, we note that the presence of investment demand for homes produces a new and scantly documented source of negative bias in the OLS estimate of the housing wealth effect. That is, investment demand may drive up house prices and simultaneously crowd out consumer spending, introducing a spuriously negative correlation between spending and house prices. Additionally, when the investment demand is driven by external and speculative factors, the traditional positive correlation between house prices and local fundamentals is weakened. Empirically, we find the OLS estimate of housing MPC to be indeed *lower* than the quasi-experimental estimate.

the part of renters' willingness to pay for homeownership purely for obtaining *hukou*.

[9]The role of housing as an investment vehicle is not only specific to China, but also to other economies where the direct finance market is less developed, for example, India (Badarinza, Balasubramaniam, and Ramadorai, 2018).

[10]Gan (2018) reports a multi-property ownership rate which is above 20%, suggesting even more widespread use of housing as an investment vehicle.

Our paper makes several contributions to the literature. First, it presents a new mechanism that generates housing market spillovers: asset purchase restrictions in a hot local housing market force the demand for housing to flow to neighboring colder markets, causing a house price boom in the colder markets. In this way, it adds to the literature on the effect of capital flows and non-local factors in housing markets, including Badarinza and Ramadorai (2018), Cvijanovic and Spaenjers (2018), and Sá (2016), as well as DeFusco, Ding, Ferreira, and Gyourko (2018) and Bailey, Cao, Kuchler, and Stroebel (2016).

Within this literature, the most related paper to us is Favilukis and Van Nieuwerburgh (forthcoming) (henceforth FV), which characterizes in a calibrated model the house prices, consumption, and welfare effects of out-of-town home buyers. Our paper empirically evaluates and qualitatively confirms predictions of FV, that there is substantial in-city redistribution: owners benefit, and renters are negatively impacted. Two distinctions arise. First, in our study, out-of-town demand induced by purchase restriction spillovers led to an increase in house prices but not rents, while in the baseline model of FV out-of-town investors leave homes vacant, thereby there are increases in both house prices and rents. Relatedly, the mechanism that may explain reduced spending of renters will be slightly different, where in FV it is primarily higher rents, whereas in our economic setting it is primarily the negative "pure" housing wealth effect for renters that are would-be homeowners.

Second, this paper contributes to the existing literature on the housing wealth effect and more generally the wealth effect literature including housing and stock wealth effect. We provide a new quasi-experimental strategy of addressing the endogeneity concerns in estimating the housing wealth effect. Our approach is useful because increasing evidence suggests that the Saiz (2010) supply elasticity instrument, one of the most widely-used instruments in the housing wealth effect studies (Saiz, 2010; Mian, Rao, and Sufi, 2013; Mian and Sufi, 2014; Aladangady, 2017) may correlate with follow-on fundamental growth (Davidoff, 2016). Importantly, we also provide the first quasi-experimental evidence for the "pure" housing wealth effect channel (Sinai and Souleles, 2005; Buiter, 2010; Kaplan, Mitman, and Violante, forthcoming). Our evidence complements that of Gan (2010), who shows that credit card consumption increases more for multi-property owners when house price increases, although the estimated effect is modest. The quasi-experimental nature of our strategy relates to Sodini, Van Nieuwerburgh, Vestman, and von Lilienfeld-Toal (2018), which also uses an exogenous shock to homeownership/housing wealth and finds evidence for the collateral effect in Sweden. Our study is also related to the stock wealth effect literature. House prices are more persistent (Glaeser and Nathanson, 2017; Guren, 2018) compared to stock prices (Lettau, Ludvigson, Steindel, et al., 2002; Lettau and Ludvigson, 2004), which may make homeowners more willing to consume out of wealth gains. Our approach using regional and within-region variations together with administrative data is also related to recent empirical work on consumption responses to stock wealth gains, including Chodorow-Reich, Nenov, and Simsek (Forthcoming), which use regional variations in administrative data, and Di Maggio, Kermani, and Majlesi (2020), which uses household-level variations in administrative data, to identify the stock wealth effect.

Finally, this paper is among the first to provide an in-depth look into consumer spending in China. The significance of consumption in driving the country's economic growth is gradually rising, with the contribution of household consumption in GDP growth out-taking that of investment in recent years. This shift coincides with the its sizable housing boom backed by a strong investment demand (Glaeser, Huang, Ma, and Shleifer, 2017). There is much attention on the link between investment demand, house prices, and consumer spending in China. However, the causal relationship is unclear,[11] and our paper fills the gap with the quasi-experimental evidence. The context for our finding is also potentially relevant for developing economies – where multi-property ownership and non-fundamental house price movements (Glaeser and Nathanson, 2017) are more pronounced, and for which the size and mechanisms of housing wealth effect are less understood. Our quasi-experimental also relates

The rest of this paper is organized as follows. Section 2 further introduces the institutional background and the purchase restriction spillover shock. Section 3 explains the quasi-experimental strategy, data construction, and presents the house price and spending results. Section 4 reports the heterogeneous spending response results, and discusses the "pure" housing wealth effect explanation

[11]There is an early empirical literature in China discussing the relationship between house prices and consumption; however, the results are mixed. One series of studies finds that a rise in house prices increases household consumption. Among them, Du, Shen, and Pan (2013) use household survey data in Shanghai and control for a rich vector of household characteristics., The same results are reached by Luo (2008) and Pei and Sun (2004). Another series of studies find the direct opposite relationship. Among them, Chen, Chen, and Gao (2012) use a Hansen threshold model on province-level data, Xie, Wu, Li, and Zheng (2012) use household survey data covering 12 cities, and Waxman, Liang, Li, and Barwick (2019) uses city-level credit- and debit-card spending in 2011-2013. Gu, He, and Qian (2018) also use credit- and debit-card data but find no significant effects on overall expenditure.

and alternative explanations. Section 5 addresses the methodological implications and external validity. Section 6 concludes.

2 Institutional Backgrounds

Over the past decade, there has been substantial heterogeneity in the housing markets of large and smaller cities in China – large cities witnessed surging growth while smaller cities remain relatively stagnant. For example, house prices grow at a high speed of 14.9% annually in Tier-1 cities[12], but at slower than 3% in Tier-3 cities from 2012 to September 2016[13]. At the end of September 2016 and in the middle of March 2017, two rounds of policy changes were implemented in all Tier-1 and many Tier-2 cities to contain the surging house prices. These policy changes were called house purchase restrictions (HPRs) and targeted at curbing the demand of housing market speculators, who typically hold multiple homes. The 2016 and 2017 round of home purchase restrictions can be contextualized as part of a longer-standing effort of both central and regional governments aimed to cushion the housing market against speculation-induced over-heating.[14]

In particular, the restrictions included raising the down payment requirement to even higher levels, and increasing mortgage rates on, and in several instances outright forbidding the investment purchases – identified by the purchase of more than two or three houses by one family. Among cities for which we have reliable house price data, 19 implemented the first round of house purchase restrictions, and 22 implemented the second round. Tables 1 and 2 summarize both rounds of policy changes for each city:

[Insert Table 1 near here]

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

[13]The house price growth is based on data from CityRE and Fang, Gu, Xiong, and Zhou (2016)

[14]Qian, Tu, Wu, and Xu (2019) examines a set of different policy experiments implemented around the year 2010 and find that investors constrained by house purchase restrictions increase new stock accounts openings and purchases of shares of listed real estate developers. Their analysis provide evidence of the redirection of investment demand from houses to other assets, which is related to our analysis but distinctively different. Relatedly, Lu, Zhang, and Hong (2021) study the response of local developers in the regulated cities to the reduction in local housing demand after the same set of HPRs. Different from these studies, we focus on the 2016 and 2017 round of HPRs because our automobile spending data better reflects household spending after 2010, when the automobile market in cities of all tiers are more developed.

[Insert Table 2 near here]

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

To illustrate the effect of these policy shocks on the regulated cities and the nearby non-regulated cities, we choose three pairs of cities as examples. They are (1) Beijing - Tangshan, (2) Hefei - Bengbu, and (3) Wuhan - Xiangyang. Figure 1 shows the locations of these three pairs of cities:

[Insert Figure 1 near here]

Within these three pairs, Beijing, Hefei, and Wuhan are large cities that implemented two rounds of house purchase restrictions, and Tangshan, Bengbu, and Xiangyang are nearby, smaller cities that are non-regulated and experienced no policy change. We also include three cities that are faraway from any of the regulated cities and for which we have data on home sales: Jilin, Jinzhou and Dali. These cities are all of a geographical scale similar to MSAs in the United States. Figure 2 plots the dynamics of house price growth and home sales in the three set of cities from 2015 to September 2017:

[Insert Figure 2 near here]

We see from Figure 2 that the policy changes were effective in containing house price growth in the regulated cities. In Beijing, Hefei, and Wuhan, house price almost stopped growing soon after the policy changes. The transaction volumes of houses also plummeted. In Beijing and Hefei, quarterly house sales dropped over 50% and 70% after the first round of house purchase restrictions.

In the meantime, the nearby non-regulated cities – which are the treated cities in our study, appeared to experience a sharp increase in home sales and house prices simultaneous with the imposition of HPRs in the regulated cities. The quarterly home sales in Tangshan increased by 47% and 39%, respectively, in the quarter following each round of house purchase restrictions implemented in Beijing. The reactions of house prices in Tangshan, Bengbu, and Xiangyang were also significant after the second round of restrictions. The growth rate of house prices in these three cities soon rose from less than 0.5% to 2.3%, 2.2%, and 2%, respectively, after March 2017. The house price and home sale reactions in the farway cities are less noticeable than the three nearby cities in the examples.

The house purchase restrictions aimed to curb the investment demand in several large cities. However, the increase in home sales and house prices we observed in the non-regulated cities led us to examine the hypothesis of a redirection of investment demand from the regulated cities to the non-regulated cities. The increase in investment demand soon seemed to become a concern of the local governments of the nearby non-regulated cities. Several local governments of the nearby non-regulated cities announced that they are closely monitoring the spillover of investment demand from Tier-1 and Tier-2 cities because this spillover can precipitate turmoil to housing markets in their jurisdictions.[15] In September 2017, following a period in which house price appreciation in these nearby non-regulated cities appeared to have become significant, and when the efficacy of the HPRs in the regulated cities has become evident, local governments of many nearby non-regulated cities also started to implement similar house purchase restrictions to cool down the housing market and to restrict out-of-town demand.[16] To avoid the effect of these later posted restrictions, our analysis focuses on the period before September 2017.

3 Difference-in-Differences: The HPR Spillovers Quasi-Experiment

In this section, we introduce the data used for the analysis, and first provide evidence of capital leakage channel. Then we discuss the existence of preexisting trends in the key variables, house

[15] This government's concern about housing policy spillovers is first observed in Government Announcement [2017 No. 10] from the Department of Housing and Urban-Rural Development of Hubei Province, available at `http://pkulaw.cn/fulltext_form.aspx?Gid=18847064`.

[16] For example, among other non-regulated cities, Xiangyang and Xiaogan (Tier 3 cities) started to implement house purchase restrictions specifically targeted at out-of-town buyers in September 2017, according to Government Announcement [2017 No. 10], ibid.

prices and automobile spending, before the treatment period. Because the cities are inherently different in distance to the regulated cities, the urban literature has shown such initial conditions may predict growth rate differences (Glaeser, Scheinkman, and Shleifer, 1995; Blanchard and Katz, 1992; Glaeser and Nathanson, 2017). We discuss our difference-in-differences specification that takes into account and explicitly controls for the preexisting trends. Then we report the response of house prices and consumer spending on automobiles to the house purchase restriction spillovers quasi-experiment. We also provide evidence that our estimation are robust to model specifications.

3.1 Data

To enable the estimation of the house price and real consequences of the house purchase restriction spillovers quasi-experiment, we assemble a novel dataset on house prices and consumer spending on automobiles. Table 3 reports the summary statistics, and we now introduce the variables in the table.

[Insert Table 3 near here]

Our primary source of house price data is CityRE, a leading national real estate information and data-service provider. The CityRE house price index provides comprehensive coverage of 307 cities from 2008 to 2017, allowing for a large sample for the difference-in-differences estimation. The CityRE house price index is a hedonic constant-quality index using home postings and transaction records that the company collects from national and local real estate brokerage platforms. We supplement the CityRE index with the index from Fang, Gu, Xiong, and Zhou (2016) (FGXZ), a semi-repeated sales house price index that covers 120 cities from 2003 to 2013, extending our ability to track the general pattern between house prices and consumer spending to 15 years, 2003–2017.[17] The CityRE index and the FGXZ index exhibit highly-synchronized comovements and similar growth in periods of overlap. We also obtain from CityRE for our sample cities a hedonic constant-quality rent index spanning 2008 to 2017.[18]

[17] We use the house price indices from CityRE and FGXZ and not the official NBS house price index in our regression analysis. As analyzed in FGXZ, the NBS index closely comoves with the FGXZ index and is more smoothed, but may slightly understate house price growth. The NBS house price index also covers a smaller set of only 70 cities.

[18] The CityRE rent index is for similar-quality houses and apartments as the owner-occupied units. The rental housing

We collect data on home transaction volumes from China Index Academy, a data vendor that records all completed real estate transactions registered at housing administration bureaus of municipalities. Home sales index is an index of the number residential homes purchased in each month benchmarked at the July 2016 (2016m7=100). Home sales index covers only 73 cities. We construct the Baidu house search index based on the Baidu search index data – which provides the intensity of web searches of a large set of documented keywords originated from web users in each individual city. We measures the intensity of web searches of keywords related to house price and house markets of a non-regulated city, originated from each regulated city, and then aggregate the searches for each non-regulated city.

Our administrative registry-based dataset on consumer automobile spending covers every automobile purchase in China from January 2003 to August 2017, and it comes from the China Insurance Information Technology Corporation (CIITC). Set up by the China Insurance Regulatory Commission (CIRC) – a commission overseeing the overall Chinese insurance products and services market, each automobile insurance application and registration must be reported to CIITC. Since automobiles are required to carry a form of mandatory liability insurance coverage, the CIITC data records purchase and subsequent information on all automobiles in China. Our dataset covers personal/commercial purchases of new passenger/non-passenger automobiles, and to measure consumer spending on automobiles, we retain exclusively personal purchases of new passenger automobiles. For each purchase, we observe its make, model, trim, vehicle identification number, registration place and date, as well as desensitized information on the purchaser. Figure 5 shows the number of all the automobiles purchased in China each month aggregated from this CIITC data:

[Insert Figure 5 near here]

Purchase price information in our CIITC data allows us to measure city-level and city-demographic-group level monthly consumer automobile spending with high precision. Automobiles carrying comprehensive or collision insurance coverage are required to report their purchase price in the insurance applications, which covers most of our sample. For the remaining purchases, we approximate the purchase price by the average of the same model and trim in the same city and

market in China lacks the type of rental apartment complexes commonly seen in the United States, and constitutes mostly of scattered homes mixed in buildings or complexes that are majority owner-occupied.

month. A great majority of the automobiles in our dataset (about 80%) have the exact purchase price available, and the approximate purchase price for the remaining ones is considerably accurate, aided by the detailed model information. For example, for Mercedes-Benz, we know whether an automobile is from the Mercedes-Benz SL-class with a 5.0L engine, which costs more than 150 thousand USD, or it is from the Mercedes-Benz C-class with a 2.0L engine and a much lower price of 40 thousand USD.

We begin by aggregating total spending on the purchases and the total number 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 also perform the aggregation for luxury cars separately. We also seasonally adjust all the automobile spending data, by excluding the month-specific effects from the automobile spending time series by each city.

Next, we obtain data on automobile purchases by different demographic groups. We first construct another dataset containing, for each city and each month, automobile purchases made by individuals born in the prefectural city they reside in ("the locally born"), and those made by individuals born outside of the prefectural city they reside in ("the non-locally born"). A desensitized part of the buyer's ID in the CIITC data provides the prefectural birthplace of the buyer.[19] The data also provides the city where each new car is registered, which is almost always the city where the car is purchased and used. Combining these two pieces of information, we can distinguish automobile purchases made by individuals that are born locally from those that are not – two groups of individuals that we later will show to distinguishably differ in housing tenure status. Table 3 indicates that the two birthplace groups are similar in the sample mean of the aggregated value of car purchases at the city-group-month level. In Section 4.1, we show that their spending responses differ significantly in treated cities following the house purchase restriction spillover shocks, but not before. Section 4.1 shows that consistent with a "pure" housing wealth effect, people born locally and those not born locally have substantially different rates of renting homes and owning homes, based on the survey data, which consequently may explain the difference in their spending

[19] More precisely, this variable provides the birthplace of individuals born after 1984, and the residence location in 1984 for individuals born earlier. This is because the national ID system was initiated in 1984. However, internal migration was extremely limited before 1984 (Liang and White, 1996), therefore our birthplace status measure is reasonably valid.

responses.

There are reasons to think that car purchases are a good proxy for total spending in China. We analyze the China Household Finance Survey (CHFS), which has information on car purchases as well as categories of spending. In the 2017 wave of the CHFS (asking about spending in 2016), automobile purchases is the second largest category of urban consumer non-housing spending (10.6%), only lower than food expenses (27.5%), but higher than medical expenses, education, utilities, home improvement, clothing, and telecommunications. The spending share on automobile purchases is also large prior to the HPR spillover event — automobile purchases is also the second largest category of urban consumer non-housing spending in the 2015 wave of the CHFS (10.5%). The car purchases share of non-housing spending in China is similar to the United States, which is 9.5% in 2016 according to the Consumer Expenditure Survey (CEX).

To proxy for non-automobile consumption of households, we construct the Baidu non-car spending index based on the Baidu search index data. Specifically, we adopt and aggregate the Baidu search indexes measuring the intensity of searches of keywords related to a basket of consumption goods, originated from the web users in each city in each week. We let the basket consist of goods that are generally pricey to ordinary Chinese households, and have sufficient web search data at the city-week level. 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 including per capita gross regional product (GRP), residential population, square meters of road per capita, and the number of public buses per every ten thousand city residents, industrial output, real estate investment, and bank deposits. Some of the data come from the China City Statistical Yearbook composed by National Bureau of Statistics of China. For the cities where data are not available from the China City Statistical Yearbook, we manually collected the data from the statistical reports and yearbooks of each individual city.

The survey data we used to compute the rates of renting, and owning homes for automobile buyers in different demographic groups is based on nine waves of household surveys, including the 2010, 2011, 2012, 2013, 2015 waves of the Chinese General Social Survey (CGSS), and the 2010, 2012, 2014, 2016 waves of the China Family Panel Studies (CFPS). These surveys provide information on the birthplace of surveyed individuals, as well as whether she or her immediate co-residing family owns no home, owns one home, or owns multiple homes. To conserve space, the survey data is not summarized in Table 3.

3.2 Motivating Evidence of The Capital Leakage Channel

We now provide motivating evidence consistent with a "capital leakage" channel: real estate investors facing increased transaction costs in the regulated cities turn to invest in the nearby non-regulated cities, bringing in external capital, and pushing up the house prices.

We first examine whether out-of-town buyers' activity is more salient for cities nearby the regulated cities, relative cities distant from regulated cities. Figure 3 show that this is the case, where we use the Baidu house search index to proxy for the out-of-town buyers' demand and attention. In Panel (a) of the figure, for the city of Tangshan, a non-regulated city, nearby regulated city Beijing but distant from regulated city Hefei, receives dramatically more web searches after the HPR shocks on house price and housing market from web users in Beijing, but not Hefei. Panel (b) shows the exact same pattern for a different city triplet, Xiangyang, Wuhan and Beijing. In Panel (c), we show that patterns in Panel (a) and (b) are not just cherry-picking examples. Rather, treated cities on average receive much more house searches after the HPR shocks from close regulated cities, than from distant regulated cities. Meanwhile, control cities show much dampened responses in house searches at all. In Panel (d), we plot the difference in the house web searches received by treated cities and that by control cities, from all regulated cities. We see that web searches towards to homes in treated cities increase sharply after the two HPR policy spillover shocks. This provides some evidence that home buyers in the regulated cities cast their demand to the nearby non-regulated cities.

[Insert Figure 3 near here]

We also examine whether the reductions in volumes in the regulated city are consistent with the increase in volumes in the treated cities. Figure 4 shows the time series of total transaction volumes in regulated cities, treated cities, and control cities. We find that the volume drop in regulated cities is simultaneous with and similar in magnitude to the volume increase in treated cities. We also find that the volume increase in treated cities is greater than the volume increase in control cities.

[Insert Figure 4 near here]

3.3 Tests of Preexisting Trends

The quasi-experimental House Purchase Restriction shocks naturally lead us to a difference-in-differences estimation strategy, to estimate the effect of the capital inflow induced by policy spillovers on house prices and consumption in the affected cities. As shown in Section 2, 22 cities in our sample implemented house purchase restrictions, and the remaining cities during the sample period did not implement such restrictions. First, we calculate the distance from each non-regulated city to the nearest regulated city. Then we split the non-regulated cities into two approximately equal-sized groups based on the calculated distance. If a city is within 250 kilometers to any regulated city, then it belongs to the treatment group ("treated" by the capital inflow).[20] Otherwise, it belongs to the control group. The choice of 250 kilometers may seem arbitrary; however, it can be justified by the fact that with the aid of high-speed railways, traveling point-to-point between two cities closer than 250 kilometers takes less than 2 hours.[21] [22] We consider 250 kilometers between two cities to be "close" for investment purposes — this distance is relatively short and makes frequent visits possible for screening homes or monitoring tenants, but too far for daily commuting purposes. There are 152 treated cities and 151 control cities in the estimation sample according to this baseline treatment designation. The cities we study are prefectural cities of similar size to the MSAs in the United States. Even with the aid of high-speed trains, conventionally people rarely commute between these

[20]We calculate D_i as the distance of city i from the nearest of the 22 regulated cities listed in Table 1 and 2, although among them, three cities did not implement the first round of house purchase restrictions. The treatment group and our estimation results change little regardless of how the definition of the treatment group depends on these three cities.

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

[22]Section 3.6 shows that our results are robust to choices of the cutoff distance, an alternative continuous distance specification, as well as using railway travel time to designate treatment status.

cities.

We note that the major difficulty in difference-in-differences analyses involves separating out preexisting trends from the dynamic effects of a policy shock. To not confound the two, we first test whether there are preexisting trends in variables key to our analysis – house prices, automobile spending and rents. To carry out 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 that in the control cities, both before and after 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:

$$\begin{aligned} Y_{i,t} = & \sum_k \beta_k \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \\ & + \Gamma X_{i,t-1} + \sum_i \textit{City fixed effects}_i \\ & + \sum_t \textit{Time fixed effects}_t + \epsilon_{i,t} \end{aligned} \tag{1}$$

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 consumer spending on new automobiles, or other outcome variables appropriate for placebo tests (e.g., rents), in city i and at time t.

Figure 6 plots the coefficients of interests, β_k, measuring the dynamics of house prices in the treated cities relative to that in control cities before and after the shocks. As the figure makes clear, there is a persistent but stable differential trend in dynamics of house prices and automobile spending between the treated cities and control cities, in a long period before the first HPR spillover shock. The figure visually informs us that the shape of the preexisting trends is linear – that is, in five-year period before the HPR spillover shocks, house prices grows constantly by 2 percent more per year in the treatment than the control group, and automobile spending grows constantly by 4-5 percent more per year in the treatment than the control group. Albeit the preexisting trends, we observe statistically significant and economically sizable effect of the HPR spillover shocks. House price and automobile spending in treated cities increase immediately above the preexisting trends both at the first and the second round of HPR spillover shocks. The figure motivates us to explicitly control for the linear preexisting trends, when we quantify the effects of the HPR spillover shock in the following sections.

[Insert Figure 6 near here]

Figure 6 also presents the dynamics of rents in the treated cities relative to that in control cities — rents do not respond consistently differently in the treated cities than in the control group, not only before, but also long after the policy spillover shocks. Although there are a few months when the rent gap oscillates to become significantly positive, overall the rent gap does not substantially deviate from zero.[23] This provides additional indication that the factors that drive the house price and automobile spending responses appear orthogonal to local economic conditions, otherwise they would manifest themselves in rents changes. One limitation here is that we cannot fully rule out that households may have expectations about relative rent changes in the further future, which may also affect households' valuation of house prices.

3.4 Empirical Specifications

House Prices

We now estimate the response of house prices in treated cities relative to control cities, induced by the House Purchase Restriction spillover shocks. We recognize two rounds of spillovers shocks because the treated cities witnessed rises in house prices and home sales after the imposition of both rounds of restrictions in the regulated cities, as shown in Figure 2 and 6.

In order to separate out preexisting trends from the dynamic effects of a policy shock, we follow Wolfers (2006) and conduct slight modifications to standard difference-in-differences procedures.

[23] One casual observation is that 9 months after each individual wave of the policy, rents are 5-7 percent higher in the treatment than in the control group. However, the post-trend of rent also suggests that the rent gap between the treatment and the control group are statistically insignificant for 31 of the 36 post-event months, and are insignificant for all months after 2018m2, during which the post-trend oscillate around 0-2 percent for second half of the post-event period.

We first include city-specific linear time trends in 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 actual dynamics are more complex than a simple series break. Following his approach, we add variables that model the dynamic response of house prices and automobile spending explicitly. On that front, we pursue a specification that imposes very little structure on the response dynamics, including dummy variables for the first and second month after the shock, for months three, four, five, and so on. Thus, these variables should identify the entire response function allowing the estimated city-specific time trends to identify preexisting trends. Specifically, we estimate the following equation for the non-regulated cities.

$$\begin{aligned}
\log \mathrm{HPI}_{i,t} = & \sum_{0 \le k \le 5} \beta_{1k} \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \\
& + \sum_{0 \le k \le 5} \beta_{2k} \cdot Treat_i \times \mathbb{I}_{\{t=2017m3+k\}} \\
& + \Gamma X_{i,t-1} + \sum_i \textit{City fixed effects}_i \\
& + \sum_t \textit{Time fixed effects}_t \\
& \Big[+ \sum_i \textit{City}_i \times \textit{Time}_t + \epsilon_{i,t} \Big]
\end{aligned} \tag{2}$$

where $\mathrm{HPI}_{i,t}$ is the monthly house price index in city i at time t. $Treat_i$ is a dummy that equals 1 if the city belongs to treatment group. $Treat_i$ takes the value one if the distance of city i from the closest regulated city is less than 250 km. Similarly, the dummy variable $\mathbb{I}_{\{t=2016m9+k\}}$ or $\mathbb{I}_{\{t=2017m3+k\}}$) takes the value one if time t is k months after September 2016 or March 2017 . $X_{i,t}$ is a vector of time-varying city-level control variables for city i in time t. $\textit{City}_i \times \textit{Time}_t$ is the linear time trend of City i. Finally, $\epsilon_{i,t}$ is the error term.

The coefficients of interest are the averages of β_{1k}'s and β_{2k}'s, which measure the average treatment effect in a 6-months period right after the first HPR spillover shock, and the average treatment effect in a 6-months period right after the second shock. Our specification controls for city-level time-varying characteristics, city and time fixed effects and city specific linear trend. To account for serial correlation and region-specific random shocks, we cluster standard errors at the city level in all specifications.

Ideally, the control variables in $X_{i,t}$ should include economic fundamentals such as (1) local demand shifters such as the average income of potential buyers in the local market and migration flows; (2) buyer characteristics, such as the fraction of speculative buyers; and (3) credit market conditions measured by (for example) loan-to-value ratios of the purchased house over the time (DeFusco, Ding, Ferreira, and Gyourko, 2018). However, representative mortgage data are not accessible in China.[24] Also, even if representative data exists on all Chinese mortgages, they are not representative of all house purchases, since Chinese households have a low dependence on mortgages. According to statistics from the Urban Household Survey conducted by the National Bureau of Statistics (NBS), 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, which was much lower than in Japan (39%), the United States (72%), and the United Kingdom (86%) (Fan, Wu, and Yang, 2017). These data limitations make it difficult to control for all buyer characteristics and credit market conditions. Instead, we control for several city-level macroeconomic variables that may relate to house prices, including per capita gross regional product (GRP), residential population, road infrastructure, and public transportation. We note that cities may constitute a hub-and-spoke network, where economic shocks to the central city pass through to the spoke city and the effect fades as the distance increases. We also control for the exposure of the city to the economic activity (GRP) of the central city in all specifications. Specifically, for each non-regulated city we first calculated the beta of its GRP growth to the GRP growth of the nearest regulated city, using data from year 2003 to the end of the sample period, and then compute the economic exposure to closest regulated city as the non-regulated city's beta times the GRP of the regulated city.

We provide further evidence that the increase in house prices in the nearby non-regulated cities is consistent with a capital leakage channel, whereby investors facing increased transaction costs in the regulated cities turn to invest in nearby non-regulated cities, bringing in capital flows to the

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

treated cities. First, we adopt the same specification as in Equation (2), but use home sales index as the outcome variable, to show that the home sales increase more in the treated cities relative to the control cities. Second, we also use the Baidu house search index as the outcome variable, to show that intensity of web searches of keywords related to house price and house market of a city – proxy for attention of the out-of-town buyers, is higher in the treated cities relative to control cities. Third, to show that indeed economic fundamentals of treated cities relative to the control cities do not behave differently after the spillover shocks, we replace the variable in Equation (2) with a basket of macroeconomic variables. The basket of macroeconomic variables include city-level output, industrial output growth, output growth, employment growth, population, real estate investment, and bank deposits. These local macroeconomic variables are at the annual frequency and we assign years after 2016 as the post-treatment period.

Automobile Purchases

To investigate the real consequences of housing market capital inflows, we study how consumer spending on new automobiles in the treated cities respond to the house purchase restriction spillover shock. To do so, we re-estimate Equation (2) for automobile purchases, using consumer spending on automobiles as the outcome variable.

$$\begin{aligned}\log \text{Spending}_{i,t} = &\sum_{0\leq k\leq 5} \beta_{1k} \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \\ &+ \sum_{0\leq k\leq 5} \beta_{2k} \cdot Treat_i \times \mathbb{I}_{\{t=2017m3+k\}} \\ &+ \Gamma X_{i,t-1} + \sum_i \textit{City fixed effects}_i \\ &+ \sum_t \textit{Time fixed effects}_t \\ &\Big[+ \sum_i City_i \times Time_t + \epsilon_{i,t}\Big]\end{aligned} \tag{3}$$

where $\text{Spending}_{i,t}$ measures consumer spending on new automobiles in city i at time t. To further alleviate omitted variable concerns on top of utilizing a quasi-experimental design, we include city and year fixed effects, city-specific linear trends, and the same set of controls as in the house price response estimation. 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 of automobiles.

To estimate the elasticity of automobile spending to an increase in house price, we also utilize the spillover shock in an instrumental variable regression. Specifically, we instrument house prices with a rich set of interaction dummies $Treatment \times \mathbb{I}_{\{t=2016m9+k\}}$ and $Treatment \times \mathbb{I}_{\{t=2017m3+k\}}$ which takes the value one for treatment cities if 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 logarithm of automobile spending on logarithm of house prices, with the weight being the population of each city.

3.5 Difference-in-Differences Estimates of the Effects of HPR Spillovers

We now present our main results: the difference-in-differences estimate of the effects of the house purchase restriction spillovers quasi-experiment on house prices and consumer spending, controlling for city-specific trends, as described by Equations (2) and (3). Table 4 reports these results:

[Insert Table 4 near here]

The estimated house price and automobile purchase response to the House Purchase Restriction spillover shock in the treated cities (nearby non-regulated cities within 250 km from the nearest regulated city), relative to the control cities (non-regulated cities that are farther away), are highly significant statistically and economically. For the house price response, column (1) of Table 4 shows that the house prices in treated cities increased abnormally by 2.4% and 6.4% relative to control cities, following the two rounds of policy spillover shock, relative to if the pre-existing trends had just continued. In contrast, as suggested by column (2), there is no differential change in rents between treated and control cities, in response to the policy spillover shocks. According to column

(3), treated cities see an increase in home sales corresponding to 10% and 28% of the level in July 2016, relative to control cities. The effect is more statistically significant following the second spillover shock. According to column (4), treated cities see a significant increase in Baidu house search index, relative to control cities. The Baidu house search index is proportional to the number of searches. As we do not know the scaling factor used in the search index provided by Baidu, we do not attempt to interpret the economic magnitude of the coefficients, but just emphasize their statistical significance.

Consistent with a positive housing wealth effect, a rise in local consumer spending is observed to accompany the post-shock increase in house prices. Using the logarithm of consumer new automobile spending as the dependent variable, column (5) shows that following the two rounds of house purchase restriction spillover shock, consumer spending on new automobiles increases abnormally by 7.8% and 11.6% in the treated cities relative to the control cities. We also observe an intensive margin of the spending response. In columns (6) Table 4, we show that consumers in treated cities buy more expensive automobiles – luxury cars, after the spillover shocks. Specifically, consumer spending on luxury cars increases by 12.3% and 15.7% in the treated cities relative to the control cities, respectively after the two rounds of policy spillover shocks. We see that the estimated effects on luxury car spending are generally larger than those for all-model passenger automobiles, suggesting that the intensive margin of the response to the shocks is important. While the automobile spending data in columns (5) and (6) is not seasonally adjusted, the rest three columns use seasonally adjusted automobile spending data. Column (7) shows that seasonal adjustment only slightly changes the estimates, showing that automobile spending increases by 6.0% and 14.2% in the treated cities relative to the control cities. Column (7) reports results of regression weighted by city population, and suggests that the average person in treated cities would increase her automobile spending by 3.7% and 9.9% following the two spillover shocks.[25] Finally, column (8) shows that the elasticity of automobile spending to house price is 1.94.

We now present the results of our placebo tests using a basket of macroeconomic variables to

[25] The size of the estimated house price and spending effects should be interpreted in our economic setting, where the average increase in house prices per year in the non-regulated cities for an earlier period of 2003-2013 in Fang, Gu, Xiong, and Zhou (2016) is 10-13%, and the annual increase in automobile spending during the baseline sample period is 9.6%. The size of the estimated effects in Table 4 are reasonable in comparison.

further establish the quasi-experimental assumption: the house purchase restriction spillover shocks affect the treated cities solely through external housing demand, but not other aspects of the local economy. Table 5 shows the results of these placebo tests.

We see that except for an increase in bank deposits (column 7), there is no change in total output, industrial output growth, output growth, employment growth, population, real estate investment (columns 1 through 6) after the policy spillover shocks. An increase in bank deposits in the treated cities would be consistent with the capital leakage channel, embodying the consequence of the capital inflow — when purchasing homes from the local homeowners, out-of-town investors inject funds into the treated cities.[26]

[Insert Table 5 near here]

3.6 Robustness Checks

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

Continuous Distance Specification One potential concern with the difference-in-differences results is the sensitivity to treatment group definition – for example, it may seem arbitrary to use 250 km as the cut-off distance to designate the treatment and control group in Equations (2) and (3). 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 house purchase

[26]The magnitude of the deposit effect could be consistent with the house price rises in treated cities. The deposit effect in Table 5 corresponds to an abnormal increase of 10.6 billion yuan in bank deposits per treated city on a pre-event average total bank deposits of 211.1 billion yuan in each treated city. In comparison, our baseline event-end abnormal house value increase is 6.4% (18.3 billion yuan) on a pre-event average total value of 285.0 billion yuan. The magnitude of the two are close. While the deposit effect is slightly smaller than the abnormal increase in the total value of homes, in other asset markets Gabaix and Koijen (2020) has also found the total asset value increase to exceed the value of the inflow.

restriction spillovers shocks. Specifically, we estimate the following equations:

$$
\begin{aligned}
Y_{i,t} = & \sum_{0\le k\le 5} \beta_{1k} \cdot \log(D_i) \times \mathbb{I}_{\{t=2016m9+k\}} \\
& + \sum_{0\le k\le 5} \beta_{2k} \cdot \log(D_i) \times \mathbb{I}_{\{t=2017m3+k\}} \\
& + \Gamma X_{i,t-1} + \sum_i \textit{City fixed effects}_i \\
& + \sum_t \textit{Time fixed effects}_t \\
& \left[+ \sum_i \textit{City}_i \times \textit{Time}_t + \epsilon_{i,t} \right]
\end{aligned} \tag{4}
$$

where $Y_{i,t}$ can be $\log \mathrm{HPI}_{i,t}$, $\log \mathrm{Spending}_{i,t}$ and other outcome variables of interest. D_i is the distance of a city to the closest regulated city. All the other variables are defined in the same way as in Equations (2) and (3). The coefficients of interest are the averages of β_{1k}'s and β_{2k}'s. They reflect how changes of the outcome variable in a non-regulated city after the spillover shocks relate to its distance from the nearest regulated city, on average during the 6-months periods right after the shocks, relative to if pre-existing trends had just continued. If the house price increases in the non-regulated cities are caused by capital inflow from the regulated cities, then we may hypothesize that as a city's distance from the regulated cities increases, the capital inflow should be weaker, hence the rise in house prices and consequently automobile spending should be on average smaller. Thus the averages of β_{1k}'s and β_{2k}'s are hypothesized to be negative. Table 6 reports the estimated averages of β_{1k}'s and β_{2k}'s based on this continuous distance specification.

[Insert Table 6 near here]

As shown in Table 6, we see that as the city's distance from the nearest regulated city continuously increases, the responses in house prices and automobile spending following the policy spillover shocks become weaker and weaker. The responses in home sales, Baidu house search index also become weaker. Meanwhile, the city's distance from the nearest regulated city does not affect changes in rents at all. As for the intensive margin, the responses in luxury automobile spending also become weaker as the city's distance from the nearest regulated city increases. The IV estimates using logarithm of distance $\times$ event-month dummies to instrument for house prices remain economically large and statistically significant. All results are in accordance with our main difference-in-differences results, suggesting the latter are unlikely to be driven by the specific choice of discrete cut-offs.

Figure 7 shows this continuous distance specification result in a graphical form. For each city, we first estimate the deviation in the two post-treatment periods in our key variables of interests from the city-specific preexisting time trends, controlling for month dummies to exclude the effect of seasonality. In Panel (a), we plot the post-treatment deviations of logarithm of house price against the city's distance from the closest regulated city. In Panel (b), we plot the post-treatment deviations of logarithm of automobile spending against the distance. In Panel (c), we plot the post-treatment deviations of Baidu house search index of a city. We see that the responses in house price, automobile spending and Baidu house search decay as distance from regulated city increases. Panel (b) suggests that the effect of the policy spillover shocks on automobile spending seem to decay to zero at a distance of around 500 kilometers.

[Insert Figure 7 near here]

We argued that we designate a city as treated if it is within 250km from the nearest regulated cities because the travel time from the regulated city is less than 2 hours. In Table A.1 in the appendix, we show the estimation results when we designate the treatment group by the shortest railway travel time to any regulated city. Specifically, utilizing data on time schedules of all trains and high-speed rail (HSR) operating in China in 2017, we compute for each non-regulated city the shortest rail travel time to any of the regulated cities. Then designate a city to be treated if this travel time is less than or equal to 2 hours. Figure A.1 in the appendix plots the shortest travel time against the distance to the physically nearest regulated city. We see that the shortest travel time specification does introduce differences to the treatment group designation. The correlation of this travel time treatment status and the baseline distance-based treatment status is 0.67. The largest distance for a city with a sub-2-hour travel time is 433km. The results in Table A.1 are reassuring.

Our last exercise to alleviate the concern about treatment group definition is to use alternative distance cut-offs to designate treatment and control groups. We perturb the cut-off distance – 250 km in the baseline specification, to be 300 km, 200 km, and 150 km and then verify that different choices of cut-off distance cause little change to our estimation results. Table A.2 in the appendix shows the estimation results when using alternative distance cut-offs. We see that compared with the baseline difference-in-differences estimates using a cut-off value of 250km, the estimated effects of the policy spillover shocks on all the variables of interests are quantitatively similar, regardless of the cut-off distance we use.

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 based on levels of economic development. We perform matching of the cities based on pre-treatment values of per capita GRP, exposure of output growth to nearest regulated city, and house prices. For each treated city, the closest matching control city will be chosen (with replacement), according to the Mahalanobis distance of the three variables we are matching on, to constitute the matched control group. Table 7 shows that matching based on levels of economic development gives quantitatively similar results compared to the pure non-matched analysis.

Table 8 shows a set of balance test results for the non-matched sample and the matched sample of cities. The non-matched sample shows that on average, cities closer to the regulated city have a higher per capita GRP and house prices, and lower economic exposure beta to regulated city. After matching, we are able to 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.

[Insert Table 7 near here]

[Insert Table 8 near here]

"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 policy spillover shocks, and may be too restrictive and prone to leading to bias. To alleviate this concern, we follow the "one step up" approach proposed by Bilinski and Hatfield (2019). This approach involves first specifying a base model that includes a linear trend difference, as we did with the city-specific linear time trends. This approach then suggests using a more complex trend difference to replace the linear trend difference in the baseline model, and compare to the linear trend specification. A restricted cubic spline is recommended for balance between flexibility and statistical power. If the estimates using restricted cubic splines is similar to the baseline model, then the baseline assumption of linear preexisting trends is more assured.

Table 9 reports the estimation results after controlling for the city-specific trends using restricted cubic splines instead of linear trends. The estimated effects of the policy spillover shocks on all the variables of interests are quantitatively similar to our baseline results.

[Insert Table 9 near here]

Heterogeneous treatment effects We also exploit additionally the heterogeneity in the magnitude of the policy shock in the regulated city to sharpen the identification. First, we note that regulated cities include 4 Tier 1 cities and 18 non Tier 1 cities. In Table 10, we show that regulations dampens the house price growth and home transaction volume growth for Tier 1 cities much more than the non Tier 1 cities. This is unsurprising as housing market speculative activities are more salient in Tier 1 cities, and the purpose of the policies is to cool down the speculative demand. We utilize this fact, and examine whether cities neighboring Tier 1 cities experience larger increase in house prices and automobile spending. We use the same difference-in-differences specification as in Equation 2 and 3, but add further interaction terms of the treat and post dummies with a dummy indicating whether a city's closest regulated city is a Tier 1 city. Table 11 shows the results. Indeed, the house price and spending responses are greater for the treated cities near Tier 1 cities, where the regulation dampened the house price growth and transaction volumes more.

[Insert Table 10 near here]

[Insert Table 11 near here]

Second, we directly measure the response in house prices and home sales of a city's closest regulated city to the House Purchase Restriction shocks. Then we adopt a heterogenous treatment effect estimation strategy, to relate the magnitude of the regulated city's response with the magnitude of treatment effect on the non-regulated city. Specifically, we use the same difference-in-differences specification as in Equation 2 and 3, but add further interaction terms of the treat and post dummies with the post-shock decline in house price growth, or the decline in home sales volumes of the closest regulated city.

Table 12 reports the estimation results. Indeed, if a regulated city has larger house price growth decline or home sale volume decline led by the House Purchase Restrictions, then its neighboring non-regulated cities on average experience higher increase in house prices and automobile spending. We also observe this pattern for the Baidu house search index, proxying for attention of out-of-town buyers, for the period of the first round of HPRs, but no significant differences in the second round. For regulated cities that have a larger home sale volume decline led by the House Purchase Restrictions, we observe their neighboring non-regulated cities on average experience higher increase in home sale volumes with a lower statistical power.

[Insert Table 12 near here]

Baidu non-automobile consumption measure Finally, we provide evidence that non-automobile spending in the treated cities also respond positively to the policy spillover shocks, to corroborate our main findings. Data sources on Chinese household consumption at monthly frequency are rather rare, and we rely on a novel measure of consumption – the Baidu non-car spending index based on Baidu search index data, that we described in Section 3.1.

In Figure 8, we first plot the relative differences in Baidu non-car spending index between treated and control cities, both before and after the policy spillover shocks. This is based on a regression using the same specification in Equation (1). As Figure 8 makes it clear, there is no difference in the Baidu non-car spending index between treated and control cities in a one-year period before the shock. Treated cities then see a significant increase in non-car spending index after the initiation of first shock, and such response fades away after September 2017. The pattern is consistent with that of the automobile spending.

[Insert Figure 8 near here]

We also re-conduct main difference-in-differences estimation and robustness check estimations, replacing automobile spending with Baidu non-car spending index. Table A.3 in the appendix tabulates the results from these estimations. Although we cannot interpret the economic magnitude of the coefficients, as the search index is proportional to the number of web searches and we do not know the scaling factor, the direction of the coefficients and their statistical significance are consistent with our results on automobile spending. In sum, our results are also present outside of car purchases.

4 Heterogeneity in Spending Responses and the "Pure" Housing Wealth Effect

What is the mechanism underlying the real spending effects of the quasi-experimental increase in house prices in the treated cities? We now provide evidence consistent with a "pure" housing wealth effect channel, by exploring the heterogeneous spending responses across consumer types. The varied spending responses will also manifest the substantial redistribution effect of the spillover shock. First, we show that automobile purchases of local-born population, who are more likely to own than rent, are more responsive to house price increases than the non-local-born population, who have a response close to zero. Second, we leverage the information from survey data in combination with the registry-based data to show that the homeowners are estimated to have a positive spending response and renters are estimated to have a negative spending response to the house price increases.

4.1 Combining Survey Information to Test the "Pure" Housing Wealth Effect

Local-Born Versus Non-Local-Born Individuals We first show that birthplace status at time of automobile purchase are good proxies for the housing tenure status. Utilizing household survey data on housing tenure status, we estimate how birthplace status elate to the likelihood of an individual being a renter or a homeowner. To that end, we combine nine waves of household surveys in China, including 2010, 2012, 2014, 2016 waves of the China Family Panel Studies (CFPS), and 2010, 2011, 2012, 2013, 2015 waves of the Chinese General Social Survey (CGSS), and test the relationship between birthplace 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 – whether a surveyed individual or her immediate co-residing family rents or owns, as well as her birthplace status.

Column (1) of Table 13 reports this first-stage regression's results. Compared to the non-local-born, the local-born surveyed subjects are 14.8% less likely to be a renter, equivalently 14.8% more likely to be a homeowner.[27] The pattern has its institutional causes, as the non-local-born have limited state transfer of wealth and intergenerational transmission of wealth (Wang, 2011; Cui, Geertman, and Hooimeijer, 2016).

[Insert Table 13 near here]

Our administrative registry-based automobile purchase data conveniently allows us to calculate separately the monthly purchases of new automobiles by local-born individuals (the "locals") and the non-local-born individuals (the "migrants" and the "out-of-towners") within each city. The local-born and the non-local-born are equally vital parts of the labor force and the consumer demand in Chinese cities. For example, Table 3 shows that on average, the local-born population and the non-local-born population contribute approximately equally towards total spending in the treated and the control cities.

According to the housing wealth effect, we should find a stronger spending response for the

[27] We also find that local-born surveyed subjects are not significantly more likely to be multi-property owners than the non-local-born subjects. This result is omitted to preserve space.

local-born population. We test these predictions by estimating the following equation:

$$\begin{aligned}
\log \text{Spending}_{j,i,t} = & \sum_{0\leq k\leq 5} \beta_{1k} \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \\
& + \sum_{0\leq k\leq 5} \theta_{1k} \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \times \textit{Born in Current City}_j \\
& + \sum_{0\leq k\leq 5} \beta_{2k} \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \\
& + \sum_{0\leq k\leq 5} \theta_{2k} \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \times \textit{Born in Current City}_j \\
& + \psi \cdot \textit{Born in Current City}_j \\
& + \Gamma X_{i,t-1} + \sum_i \textit{City fixed effects}_i \\
& + \sum_t \textit{Time fixed effects}_t \\
& \Big[+ \sum_i City_i \times Time_t + \epsilon_{j,i,t} \Big] \qquad (5)
\end{aligned}$$

where subscript j distinguishes the local-born and non-local-born population. The dummy variable *Born in Current City*$_j$ takes the value one for the local-born group. With j = born in current city, $\text{Spending}_{j,i,t}$ represents spending on new automobiles by the local-born group in city i at time t. Otherwise, it represents the automobiles purchased by the non-local-born group. The rest of the specification follows the main difference-in-differences specification in Equation (2) and (3).

The coefficients of interest are the averages of θ_{1k}'s and θ_{2k}'s, which measure the average responses during the 6-months period after the first and second policy spillover shock, in automobile spending of local-born population relative to the non-local-born population, both in treated cities. Columns (2) and (3) of Table 13 reports the estimation results. For illustration purposes, Figure 9 provides a bar chart of the estimated spending responses of the two populations.

[Insert Figure 9 near here]

The results shown in Table 13 and Figure 9 suggest that in treated cities, the local-born individuals on average increase the spending on automobiles by 14.5% and 18% in addition to the non-local-born individuals, following two rounds of policy spillover shock. Correspondingly, their number of automobile purchases also increases by 13% and 17.5%. However, we find effectively no increase in either spending or purchases for the non-local-born individuals, if anything, non-local-born individuals seem to decrease their auto purchases during September 2016 and February 2017.

In other words, we find a much larger spending response for the locally born group, who are more likely to own houses, while we find a spending response indistinguishable from zero for the non-locally born group, who in normal times spend as much as the former group, but in comparison to the local-born have a larger share of renters.

Here we provide two robustness checks of our estimates. First, although our estimates controlled for city-specific linear time trends, plotting the differences of the automobile spending between local-born and non-local-born individuals over time still provide useful information. Employing a similar regression as in Equation (1), Figure 11 plots such differences both before and after the policy spillover shocks. In Panel (a), we show that in the treated cities, local-born individuals and non-local-born individuals do not display significant trend differences in automobile spending before the policy spillover shocks, but local-born individuals significantly increase their automobile spending after the shocks. In Panel (b), we show that in the control cities, local-born individuals and non-local-born individuals show no consistent trend differences in automobile spending overall before and after the shocks.

[Insert Figure 11 near here]

The second robustness check we perform is to perturb the cut-off distance designating the treatment and control group to be 300 km, 200 km, and 150 km and then verify that different choices of cut-off distance cause little change to our estimation results. Table A.4 in the appendix shows our estimates are largely unchanged when using different values of the cut-off distance.

The "Pure" Housing Wealth Effect Based on the relationship between birthplace status and homeownership status in the survey data, and the birthplace status information in the administrative spending data, we compute the predicted share of renters and homeowners for birthplace groups in the spending data according to the survey information. And then we estimate whether the housing tenure differences explain the heterogeneous spending responses as predicted by the "pure" housing wealth effect channel. This is essentially another representation of the local-born test, with more direct account of the housing tenure differences across the local-born and the non-local-born group. In particular, we estimate the following equation:

$$\begin{aligned}
\log \text{Spending}_{j,i,t} = & \sum_{0 \le k \le 5} \beta^R_{1k} \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \times Renter_j \\
& \sum_{0 \le k \le 5} \beta^O_{1k} \cdot Treat_i \times \mathbb{I}_{\{t=2016m9+k\}} \times Owner_j \\
& + \sum_{0 \le k \le 5} \beta^R_{2k} \cdot Treat_i \times \mathbb{I}_{\{t=2017m3+k\}} \times Renter_j \\
& + \sum_{0 \le k \le 5} \beta^O_{2k} \cdot Treat_i \times \mathbb{I}_{\{t=2017m3+k\}} \times Owner_j \\
& + \psi^R \cdot Renter_j + \psi^O \cdot Owner_j + \Gamma X_{i,t-1} \\
& + \sum_i \textit{City fixed effects}_i \\
& + \sum_t \textit{Time fixed effects}_t \\
& \Big[+ \sum_i City_i \times Time_t + \epsilon_{i,t} \Big]
\end{aligned} \tag{6}$$

where $\text{Spending}_{j,i,t}$ is the consumer spending on automobiles for individuals in birthplace group j, city i and time t. The treated and post dummies are same as defined in Equation (3). The interactive variables $Renter_j$ and $Owner_j$ are the predicted share of renters, home owners for individuals in birthplace group j. The coefficients of interest are the averages of β^R_{1k}'s, β^O_{1k}, β^R_{2k}, and β^O_{2k}, which represents the responses in spending on new automobiles for renters and home owners, respectively, on average during the 6-months period after the first and second policy spillover shock. Since $Renter_j$ and $Owner_j$ are imputed variables which comes with estimation errors, we use bootstrapped standard errors for coefficients of interests to correct for the first-stage estimation errors.[28]

[28] Specifically, we bootstrap both the first-stage estimation of $Renter_j$ and $Owner_j$ and the second stage estimation of spending responses.

The "pure" housing wealth effect (Sinai and Souleles, 2005; Buiter, 2010) refers to the channel where the spending response to house price changes depends on the gap between the value of owned housing assets and the discounted value of housing consumption. If renters plan to climb up the housing tenure ladder and purchase homes, they would cut back on consumption spending when house price rises even if rents are unchanged, reducing the estimated spending response for the non-local-born group as well as renters as a whole. There are reasons to believe that at least some renters in our economic setting are prospective homeowners. Although renters in Chinese cities enjoy the residential utility of the house or apartment, they do not have rights equal to those of homeowners—whose rights allow *hukou* registration and hence access to local public services such as education and public health care. Chen, Shi, and Tang (2019) uses a regression-discontinuity design to estimate a significant part of renters' willingness to pay for homeownership comes purely for obtaining *hukou*. On the other hand, the part of homeowners who own more than the discounted value of future housing consumption raises spending when house price rises, which would increase the estimated spending response for the local-born group as well as homeowners as a whole. In Appendix Table A.7 we estimate that the survey multi-home ownership rate is around 18.0%, with only minor regional variations, which suggests that there could be a non-negligible fraction of owners in our setting that own more than the discounted value of future housing consumption. Therefore, in our economic setting, the "pure" housing wealth effect may predict a significant positive spending response on average for homeowners, and a significant negative spending response for renters.

Columns (4) and (5) of Table 13 report the results of estimating Equation (6). Figure 10 provides a graphical summary of the estimated spending responses across survey-predicted housing tenure statuses. The results suggest that renters on average significantly decrease their automobile spending, and homeowners on average significantly increase their automobile spending, in response to the policy spillover shocks. In sum, the results in columns (4) and (5) of Table 13 are consistent with the predictions of the "pure" housing wealth effect channel.

[Insert Figure 10 near here]

4.2 Alternative Explanations

Other than the "pure " wealth effect channel, alternative explanations for the positive response in automobile spending in treated cities, include the permanent income channel, the labor relocation channel, and the collateral channel.

The permanent income channel refers to that the improvements of the growth prospects of the treated cities (concurrent with the shocks) may lead to a simultaneous increase in spending and house prices. The permanent income channel would predict relative increases in fundamentals in the treatment cities, which we do not detect in Table 5 of Section 3.5. Furthermore, the permanent income channel would predict similar increases in spending for the local-born and the non-local-born groups, and for homeowners and for renters, or at least some positive response for each group. This is inconsistent with our findings in Section 4.1, where we see no effect for the non-local-born group and significantly negative for renters.

The labor relocation channel is the possibility that the imposition of house purchase restrictions (even though mainly curbing investment purchases) may lead workers to migrate to nearby cities, which leads to spending and house prices increases. Similar to the permanent income channel, the labor relocation channel would also predict relative increases in fundamentals in the treatment cities that we do not observe. Perhaps more counterfactual than the permanent income channel, the labor relocation channel tend to predict that the spending increase comes more from the non-local-born group, which are not consistent with results in Section 4.1.

The collateral channel refers to that an increase in house prices enable households finance their consumption by pledging the more valuable housing assets. The collateral channel does not require an increase in fundamentals, and moreover has the potential of explaining the positive spending response for the local-born group and for homeowners. However, the collateral channel does not predict a negative spending repsonse for renters in our economic setting when house price increases and no rent increase is observed, unless there are strong expectation of rent increases in the further future, which we cannot test. Furthermore, analyzing the household surveys in our economic setting, we observe found a low prevalence of refinancing uses especially for consumer spending. The statistics are summarized in Table A.7 in the appendix. The data is the 2015 wave of CHFS, which asks about refinancing and its uses. There, only 2.2% of all homeowners report to have existing refinanced mortgage debt or HELOCs. The most prevalent use of refinanced funds are (1) to buy another home (87.2%), (2) to support personal business (5.6%), and (3) to lend in informal markets (2.7%). Only 0.4% of the refinancing and HELOC users (0.01% of all homeowners) report using the funds to buy cars.[29] This small percentage may be because of consumer culture. The CFPS asked respondents in 2018 and found that 66.7% disagree and 23.6% somewhat disagree with the action of "borrowing to consume".[30]

That the spendings of the non-local-born individuals do not increase also helps us rule out other confounding effects – for example, other policy shocks that may affect automobile spendings in the treated cities. One policy shock that we find the most relevant is that several of the regulated cities have restrictions on car purchases by rationing license plates – leading some car buyers to purchase and register new cars in the nearby cities to partially circumvent this rationing. Although the timing of the imposition of the car ownership restrictions in these regulated cities are all before 2014, and clearly do not overlap with the period of our quasi-experiment, such activities may still artificially inflate car spending in the nearby cities. However, these activities will show up as an increase in automobile spending by out-of-towners, which we do not observe at all. Overall, our results in Section 3.5 on the house price and spending increases with no observable changes in fundamentals, and our results in Section 4.1 on the local/non-local difference in spending responses to the quasi-experiment are best explained by the "pure" housing wealth effect channel.

5 Discussions

In this section, we further discuss the estimates in Section 3 and 4 focusing on three issues. First, a source of *negative* OLS bias in the estimation of MPC in the presence of investment demand. Second, the external validity of our MPC estimate. Third, the economic magnitude of the real

[29] In the 2017 wave of the CHFS, a lower 1.5% of all homeowners have outstanding refinanced mortgages, possibly because of a tightening of the credit regulation. The 2017 wave of CHFS did not ask about the uses of the refinanced debt.

[30] Somewhat relatedly, the share of car purchases on installment loans (23.9% in 2017) is also lower than in the United States (85% in 2017).

effects of the policy spillover shocks and macroeconomic implications.

5.1 Negative OLS Bias in the MPC with Investment Demand

We convert our baseline quasi-experiment estimate of the spending elasticity in Section 3.5 to the marginal propensity to consume (MPC). To compute the MPC, note that:

$$\text{MPC} = \text{Elasticity} \times (\text{Spending/Housing Wealth}). \tag{7}$$

Hence we need information on the value of housing wealth. There is no existing measure of housing wealth or the household balance sheet for our sample period and cities, so we construct such an measure using the methodology in Zhang (2019), utilizing a perpetual inventory method. The baseline estimate of the elasticity of consumer spending on new automobiles to house prices is 1.94 (column 9 in Table 4). The average ratio of annual automobile spending to housing wealth for the difference-in-differences estimation period is 0.025. Therefore, the baseline quasi-experimental point estimate of the MPC out of housing wealth on new automobiles is 0.048.

We also examine the OLS estimate of the MPC, by estimating a panel OLS regression of automobile spending on house prices with city and time fixed effects as below.

$$\log \text{Spending}_{i,t} = \alpha_i + \lambda_t + \beta \cdot \log \text{HPI}_{i,t} + \Gamma X_{i,t-1} + \epsilon_{i,t} \tag{8}$$

where $\text{HPI}_{i,t}$ is the monthly house price index in city i at time t, and other variables similarly defined as before. Appendix Table A.5 shows the OLS estimates. Columns (1), (3), (5) use the CityRE house price index used in the quasi-experimental estimation. Columns (2), (4), (6) use a house price index combining the CityRE one and the one in Fang, Gu, Xiong, and Zhou (2016) covering a longer period. The OLS estimates of the elasticity is positive and significant, generally similar across periods, and slightly higher in the treatment and control cities in the quasi-experimental estimation than if we also include the regulated cities.[31] The OLS esimate of the elasticity for the

[31] We also report a dynamic panel VAR estimate of the relationship between automobile spending and house prices in Appendix Table A.6. The estimates also show a significant relationship between automobile spending and house prices while correcting for the complicated time series variations that may be at play in our economic setting. Appendix Figure A.2 shows the impulse response functions from the dynamic panel VAR estimates.

quasi-experimental estimation sample (column 5) is 0.64. The corresponding OLS estimate of the MPC is 0.016. Compared with the quasi-experimental estimation in Section 3.5, the panel OLS estimation appears to underestimate the MPC.

A negative bias in the OLS estimation of the housing wealth effect may be counterintuitive. Conventional wisdom (Attanasio and Weber, 1994; Campbell and Cocco, 2007) suggests that the shared positive loadings of consumer spending and house prices on local growth prospects should generate a positive bias in the OLS estimation.

However, the presence of investment demand in real estate challenges this conventional wisdom and helps explain this negative bias. As documented in Wei, Zhang, and Liu (2012) and Zhang (2019), when financial markets are underdeveloped, housing assets are the major investment instruments, and a large fraction of household savings are in the form of housing assets. In this context, an increase in the saving propensity lowers consumption spending, but at the same time raises the investment demand in real estate and consequently house prices. In other words, the saving propensity becomes an omitted variable that positively correlate with house prices and negatively correlates with consumer spending. This omitted variable issue lead to a negative bias when estimating the housing wealth effect.

Our prescription to future studies on the housing wealth effect is that we should be wary of the bias caused by omitting the saving propensity (investment demand in real estate) when estimating the housing MPC, especially in economies where real estate serves as important investment vehicles.

5.2 MPC Comparison and External Validity

Next, we compare the baseline quasi-experimental point estimate of the automobile MPC out of housing wealth on new automobiles of 0.048 in our economic setting to existing estimates for the United States. We note that Mian, Rao, and Sufi (2013) report an automobile MPC of 0.018 out of housing wealth for the United States, which constitutes approximately 40% of the overall MPC. Aladangady (2017) use the Saiz (2010) instrumental variable strategy and estimate an overall housing MPC of 0.047; if we take the 40% number from Mian, Rao, and Sufi (2013), this corresponds to an automobile MPC of 0.19.

The baseline point estimate of the automobile MPC for the economic setting of our analysis in China then appears to be higher than point estimates of of the automobile MPC in the United States. We caution that first, this comparison is subject to statistical uncertainty. The standard error of our baseline automobile MPC estimate and the automobile MPC estimate in Mian, Rao, and Sufi (2013) is 0.018 and 0.001, respectively. A one-sided statistical test on that the baseline automobile MPC in the current analysis being larger than the baseline automobile MPC in Mian, Rao, and Sufi (2013) assuming independence of the two studies' samples has a T-value of 1.67 and a one-sided p-value of 4.8%. We caution that second, this comparison could also be subject to variations across specifications in the current analysis. Across the various robustness specifications, the set of tests using a smaller cutoff distance of 200 km gives rise to a lowest MPC estimate of 0.023, which would be statistically indistinguishable from the Mian, Rao, and Sufi (2013) estimate, and the set of tests using a matching specification gives rise to a highest MPC estimate of 0.064. This indicates that we obtain a range of MPC estimates across specifications, but we did not obtain a point estimate that is lower than the Mian, Rao, and Sufi (2013) estimate for the United States.

Why would the MPC be possibly higher in our economic setting in China than in the United States? One potential contributor is that the prerequisites for the "pure" housing wealth effect channel to drive the housing MPC may be present in China. For the "pure" housing wealth effect channel to drive an aggregate spending response to house price gains, it requires that first, there should be a non-fundamental component of the house price increase, i.e. house price should increase more than the present value of rent costs, and second, there is a significant fraction of households owning homes more than their consumption needs. There are reasons to believe that these prerequisites hold in China. Our empirical analysis provides a case study in which house prices seem to at least have a non-fundamental comonent. Furthermore, multi-property ownership seems to be more prevalent in China than in the United States.

The ownership rate of investment homes, as well as households' portfolio share on investment homes, seems lower in survey data for the United States than for China. We define investment home ownership as the ownership of single-family and multiple-family housing units, apartments, and condos that is beyond the main residence and not occupied by the owner. The ownership rate of investment homes in the United States is 7%, while 18% in China.[32] Households' portfolio share of residential housing units other than the main residence units is lower than 4% in the United States(Badarinza, Campbell, and Ramadorai, 2016b) but 26% in China,[33]. These contrasting numbers may help rationalize why the "pure" housing wealth effect is more salient in China than in the United States.

Besides, during the sample period of our study, we observe not only a high investment home ownership rate by Chinese households, but also a substantial increase in the value of the investment homes in the household portfolio, regardless of measured by the portfolio share or the value-to-income ratio (see Figure 12). These patterns are consistent with a salient presence of investment demand in the Chinese housing market during the sample period of our study. Furthermore, they highlight the substantial spending power these investment homes can bestow, during the time when their value increases.

[Insert Figure 12 near here]

Our MPC estimates may be relevant to many economies where investment purchases of real estate assets are prevalent. Beyond the United States and China, the ownership rate of investment real estate assets – real estate assets aside from the main residence, varies substantially around the world. Across Europe, Australia, and Canada, the lowest ownership rate of investment real estate assets is 8.1% in Netherlands, and the highest is 46% in Cyprus.[34] In India, although we do not have data on ownership rate of the investment real estate assets, Badarinza, Balasubramaniam, and Ramadorai (2016a) have shown that housing and land are the most prevalent investment vehicles there, even more prevalent than in China.[35] Figure 13 plots the ownership rate of the investment real estate assets versus the financial development index for the 23 countries in Badarinza, Campbell, and Ramadorai (2016b). It presents a noticeable pattern – when an economy has more underdeveloped financial markets, it tends to have higher investment real estate ownership rate.

[32] Authors' calculation based on SCF (2016 wave) for the United States and CFPS (2016 wave) for China.

[33] Authors' calculation based on CFPS (2016 wave).

[34] We use statistics from HFCS for Europe except the UK, and Badarinza, Campbell, and Ramadorai (2016b) for Australia, Canada and the UK, where the real estate assets include land and commercial real estate.

[35] The average Indian household holds 77% of its total assets in real estate, while the average Chinese household holds 62% of its total assets in real estate according to Badarinza, Balasubramaniam, and Ramadorai (2016a).

We caution that the "pure" housing wealth effect channel we emphasize may not apply to all countries – for example, it may not apply to countries with highly-developed financial markets, where households no longer need to use housing as a storage of value. However, it may be immediately relevant in many developing countries and medium-income countries. In these countries, the "pure" housing wealth effect channel can translate fluctuations in house prices to important impact on the real economy.

[Insert Figure 13 near here]

5.3 Economic Significance of the Overall Spending Response

What is the macroeconomic magnitude of the automobile spending increase caused by the house purchase restriction spillovers quasi-experiment? We conduct back-of-the-envelope calculations as follows. According to column (8) of Table 4, during the 12-month period following the policy spillover shocks, the treatment group of cities experience a weighted average of 7.8% increase (the mean of 3.7%, the Treat $\times$ Post1 coefficient, and 9.9%, the Treat $\times$ Post2 coefficient) in consumer spending on new automobiles, relative to the control group of cities. The lowest weighted spending response estimate in our robustness specifications is in the railroad travel time specification in Table A.1, where the weighted average increase in spending across treatment cities is 3.3%. The total consumer spending on new automobiles in the 12-month period before the policy spillover shocks in the treatment group of cities in our dataset is 896 billion RMB (approximately 130 billion USD). In sum, we observe a causal increase of 29.6 to 60.9 billion RMB (approximately 4.5 to 9.3 billion USD) in consumer spending on new automobiles.

The following comparison helps to further put into context the size of the effects of the spillover shocks. According to the China Association of Automobile Manufacturers, the average annual growth of automobile sales during 2016 and 2017, the two years covering our event window, is 1,754 thousand units, or 245.6 billion RMB (approximately 37.8 billion USD) with a mean car value of 140,000 RMB (approximately 20,000 USD). In comparison, our estimated spillover effects during the event window help explain 12% to 25% of the average annual growth of private passenger automobile sales in 2016 and 2017. Thus, the above back-of-envelop calculation suggests that the impositions of house purchase restrictions, originally locally-motivated for containing the local house price growth in the regulated cities, has led to sizable spillover effects.[36]

6 Conclusion

This paper provides a causal evaluation of the asset pricing and real consequences of out-of-town housing demand, and the impact of policy spillovers. In doing so, this paper illustrates how the "pure" housing wealth effect channel can transform the external-driven house price increase into substantial overall and redistributive consumer spending effects.

Our identification scheme relies on a unique quasi-experiment – the spillovers from the imposition of local house purchase restrictions in regulated cities to nearby non-regulated cities in China. We show that these spillovers incur non-negligible impact on the non-regulated cities and provide important implications for future policy design and regulatory coordination.

The quasi-experiment economic setting allows for a transparent empirical approach that creates house price variations that are plausible exogenous to local fundamentals. These house price variations in turn enable new findings on the housing wealth effect. For future studies, it provides a new laboratory to study the impact of housing market on other real outcomes.

The presence of investment demand in the housing market substantially influences the findings of this paper and leads to results different from the literature focusing on the housing market in the United States. As we find that the presence of investment demand in the housing market is especially prevalent in economies where financial markets are under-developed, many countries share the defining characteristics of the housing market in our setting. For these countries, further research is needed to better understand the interaction of investment demand in the housing market, household financial decisions and financial market dynamics.

[36] Relatedly, the magnitude of the redistribution in the real effects across groups in these cities are of the same order as the overall positive effect, as Figures 9 and 10 graphically illustrates.

References

Adelino, M., Schoar, A., Severino, F., 2015. House prices, collateral, and self-employment. Journal of Financial Economics 117, 288–306.

Agarwal, S., Qian, W., 2017. Access to home equity and consumption: Evidence from a policy experiment. Review of Economics and Statistics 99, 40–52.

Aladangady, A., 2017. Housing wealth and consumption: Evidence from geographically-linked microdata. American Economic Review 107, 3415–46.

Attanasio, O. P., Weber, G., 1994. The UK consumption boom of the late 1980s: Aggregate implications of microeconomic evidence. The Economic Journal 104, 1269–1302.

Badarinza, C., Balasubramaniam, V., Ramadorai, T., 2016a. The Indian household finance landscape. Working paper, Imperial College London.

Badarinza, C., Balasubramaniam, V., Ramadorai, T., 2018. The household finance landscape in emerging economies. Working paper, Imperial College London.

Badarinza, C., Campbell, J. Y., Ramadorai, T., 2016b. International comparative household finance. Annual Review of Economics 8, 111–144.

Badarinza, C., Ramadorai, T., 2018. Home away from home? Foreign demand and London house prices. Journal of Financial Economics 130, 532 – 555.

Bailey, M., Cao, R., Kuchler, T., Stroebel, J., 2016. Social networks and housing markets. Working paper, National Bureau of Economic Research.

Benmelech, E., Meisenzahl, R. R., Ramcharan, R., 2017. The real effects of liquidity during the financial crisis: Evidence from automobiles. Quarterly Journal of Economics 132, 317–365.

Berger, D., Guerrieri, V., Lorenzoni, G., Vavra, J., 2017. House prices and consumer spending. The Review of Economic Studies 85, 1502–1542.

Bilinski, A., Hatfield, L. A., 2019. Nothing to see here? non-inferiority approaches to parallel trends and other model assumptions. arXiv preprint arXiv:1805.03273 .

Blanchard, O. J., Katz, L. F., 1992. Regional evolutions. Brookings papers on economic activity 1992, 1–75.

Buiter, W. H., 2010. Housing wealth isn't wealth. Economics: The Open-Access, Open-Assessment E-Journal 4, 1–29.

Calza, A., Monacelli, T., Stracca, L., 2007. Mortgage markets, collateral constraints, and monetary policy: Do institutional factors matter? CEPR Discussion Papers 6231, C.E.P.R. Discussion Papers.

Campbell, J. Y., Cocco, J. F., 2007. How do house prices affect consumption? Evidence from micro data. Journal of monetary Economics 54, 591–621.

Cao, Y., Chen, J., Zhang, Q., 2018. Housing investment in urban china. Journal of Comparative Economics 46, 212–247.

Chen, J., Chen, J., Gao, B., 2012. Credit constraints, house price and household consumption: Evidence from a Hansen panel model estimation. Journal of Financial Research 04, 45–57.

Chen, Y., Shi, S., Tang, Y., 2019. Valuing the urban hukou in china: Evidence from a regression discontinuity design for housing prices. Journal of Development Economics 141, 102381.

Chinco, A., Mayer, C., 2016. Misinformed speculators and mispricing in the housing market. The Review of Financial Studies 29, 486–522.

Chodorow-Reich, G., Nenov, P., Simsek, A., Forthcoming. Stock market wealth and the real economy: A local labor market approach. American Economic Review .

CRIC, 2018a. Home buyer survey report(2017): Youth buyer slows, investment buyer keeps pace. [Online; accessed 31-October-2019].

CRIC, 2018b. White paper of China real estate market 2017. [Online; accessed 31-October-2019].

CRIC, 2019. 2019 Tier 3-4 city real estate market survey report. [Online; accessed 31-October-2019].

Cui, C., Geertman, S., Hooimeijer, P., 2016. Access to homeownership in urban China: A comparison between skilled migrants and skilled locals in Nanjing. Cities 50, 188–196.

Cvijanovic, D., Spaenjers, C., 2018. "We'll always have Paris": Out-of-country buyers in the housing market. Working paper, Kenan Institute of Private Enterprise.

Davidoff, T., 2016. Supply constraints are not valid instrumental variables for home prices because they are correlated with many demand factors. Critical Finance Review 5, 177–206.

DeFusco, A., Ding, W., Ferreira, F., Gyourko, J., 2018. The role of price spillovers in the American housing boom. Journal of Urban Economics 108, 72–84.

DeFusco, A. A., 2018. Homeowner borrowing and housing collateral: New evidence from expiring price controls. The Journal of Finance 73, 523–573.

Di Maggio, M., Kermani, A., Keys, B. J., Piskorski, T., Ramcharan, R., Seru, A., Yao, V., 2017. Interest rate pass-through: Mortgage rates, household consumption, and voluntary deleveraging. American Economic Review 107, 3550–88.

Di Maggio, M., Kermani, A., Majlesi, K., 2020. Stock market returns and consumption. The Journal of Finance 75, 3175–3219.

Du, L., Shen, J., Pan, C., 2013. Housing price rise and average household MPC: Evidence from Shanghai survey data. Journal of Financial Research 03, 44–57.

Fan, Y., Wu, J., Yang, Z., 2017. Informal borrowing and home purchase: Evidence from urban China. Regional Science and Urban Economics 67, 108–118.

Fang, H., Gu, Q., Xiong, W., Zhou, L.-A., 2016. Demystifying the Chinese housing boom. NBER macroeconomics annual 30, 105–166.

Farhi, E., Werning, I., 2017. Fiscal unions. American Economic Review 107, 3788–3834.

Favilukis, J., Van Nieuwerburgh, S., forthcoming. Out-of-town home buyers and city welfare. Journal of Finance .

Gabaix, X., Koijen, R. S., 2020. In search of the origins of financial fluctuations: The inelastic markets hypothesis. Available at SSRN 3686935 .

Gan, J., 2010. Housing wealth and consumption growth: Evidence from a large panel of households. Review of Financial Studies 23, 2229–2267.

Gan, L., 2018. Survey report on home vacancy rate in Chinese cities 2017. Survey report, Survey and Research Center for China Household Finance.

Glaeser, E., Huang, W., Ma, Y., Shleifer, A., 2017. A real estate boom with Chinese characteristics. Journal of Economic Perspectives 31, 93–116.

Glaeser, E. L., Nathanson, C. G., 2017. An extrapolative model of house price dynamics. Journal of Financial Economics 126, 147–170.

Glaeser, E. L., Scheinkman, J., Shleifer, A., 1995. Economic growth in a cross-section of cities. Journal of monetary economics 36, 117–143.

Gu, Q., He, J., Qian, W., 2018. Housing booms and shirking. Working paper, National University of Singapore.

Guren, A. M., 2018. House price momentum and strategic complementarity. Journal of Political Economy 126, 1172–1218.

Kaplan, G., Mitman, K., Violante, G. L., forthcoming. The housing boom and bust: Model meets evidence. Journal of Political Economy .

Koijen, R., Van Nieuwerburgh, S., Vestman, R., 2014. Judging the quality of survey data by comparison with" truth" as measured by administrative records: evidence from sweden. In: *Improving the Measurement of Consumer Expenditures*, University of Chicago Press, pp. 308–346.

Lettau, M., Ludvigson, S., Steindel, C., et al., 2002. Monetary policy transmission through the consumption-wealth channel. FRBNY Economic Policy Review 5, 117–133.

Lettau, M., Ludvigson, S. C., 2004. Understanding trend and cycle in asset values: Reevaluating the wealth effect on consumption. american economic review 94, 276–299.

Liang, Z., White, M. J., 1996. Internal migration in china, 1950–1988. Demography 33, 375–384.

Lu, Z., Zhang, S., Hong, J., 2021. Examining the impact of home purchase restrictions on china's housing market. China Economic Review p. 101620.

Luo, Z., 2008. Financial and housing wealth effects in China: A comparative study. China Soft Science 2008/04, 40–47.

Mian, A., Rao, K., Sufi, A., 2013. Household balance sheets, consumption, and the economic slump. Quarterly Journal of Economics 128, 1687–1726.

Mian, A., Sufi, A., 2014. House price gains and US household spending from 2002 to 2006. Working paper, National Bureau of Economic Research.

Pei, C., Sun, S., 2004. Precautionary savings and liquidity constraints in China. Journal of Financial Research 10, 26–32.

Qian, W., Tu, H., Wu, J., Xu, W., 2019. Unintended consequences of demand-side housing policies: Evidence from capital reallocation. Working paper.

Rodrik, D., 2019. Putting global governance in its place. Working paper 26213, National Bureau of Economic Research.

Sá, F., 2016. The effect of foreign investors on local housing markets: Evidence from the UK. Working paper, King's College London.

Saiz, A., 2010. The geographic determinants of housing supply. Quarterly Journal of Economics 125, 1253–1296.

Sinai, T., Souleles, N. S., 2005. Owner-occupied housing as a hedge against rent risk. Quarterly Journal of Economics 120, 763–789.

Sodini, P., Van Nieuwerburgh, S., Vestman, R., von Lilienfeld-Toal, U., 2018. Identifying the benefits from home ownership: A swedish experiment. Tech. rep., National Bureau of Economic Research.

Svirydzenka, K., 2016. Introducing a new broad-based index of financial development. International Monetary Fund.

Wang, S.-Y., 2011. State misallocation and housing prices: theory and evidence from China. American Economic Review 101, 2081–2107.

Waxman, A., Liang, Y., Li, S., Barwick, P. J., 2019. Tightening belts to buy a home: Consumption responses to rising housing prices in urban China. Journal of Urban Economics .

Wei, S.-J., Zhang, X., Liu, Y., 2012. Status competition and housing prices. Working paper, National Bureau of Economic Research.

Wolfers, J., 2006. Did unilateral divorce laws raise divorce rates? a reconciliation and new results. American Economic Review 96, 1802–1820.

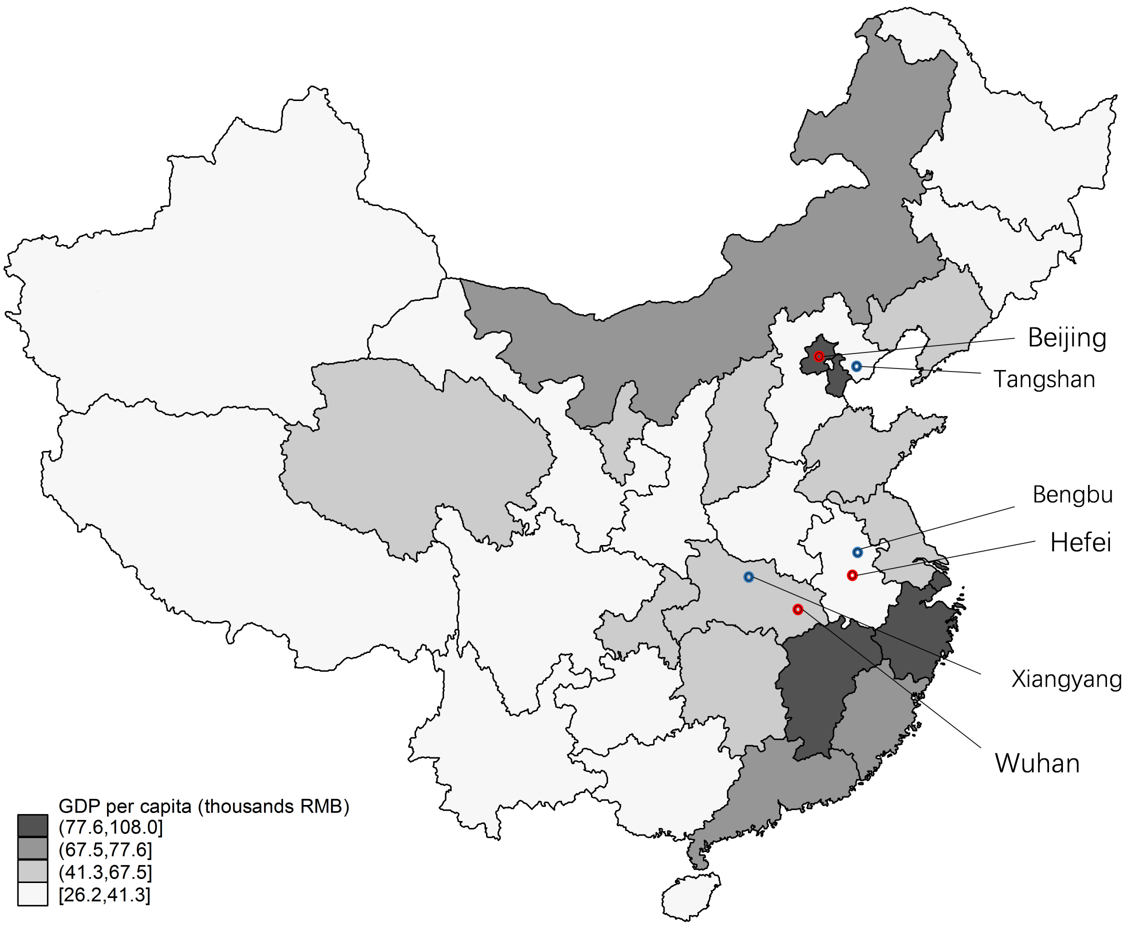
Wu, J., Gyourko, J., Deng, Y., 2016. Evaluating the risk of Chinese housing markets: What we know and what we need to know. China Economic Review 39, 91–114.

Xie, J., Wu, B., Li, H., Zheng, S., 2012. House price and household consumption in Chinese cities. Journal of Financial Research pp. 13–27.

Yao, R., Zhang, H. H., 2005. Optimal consumption and portfolio choices with risky housing and borrowing constraints. The Review of Financial Studies 18, 197–239.

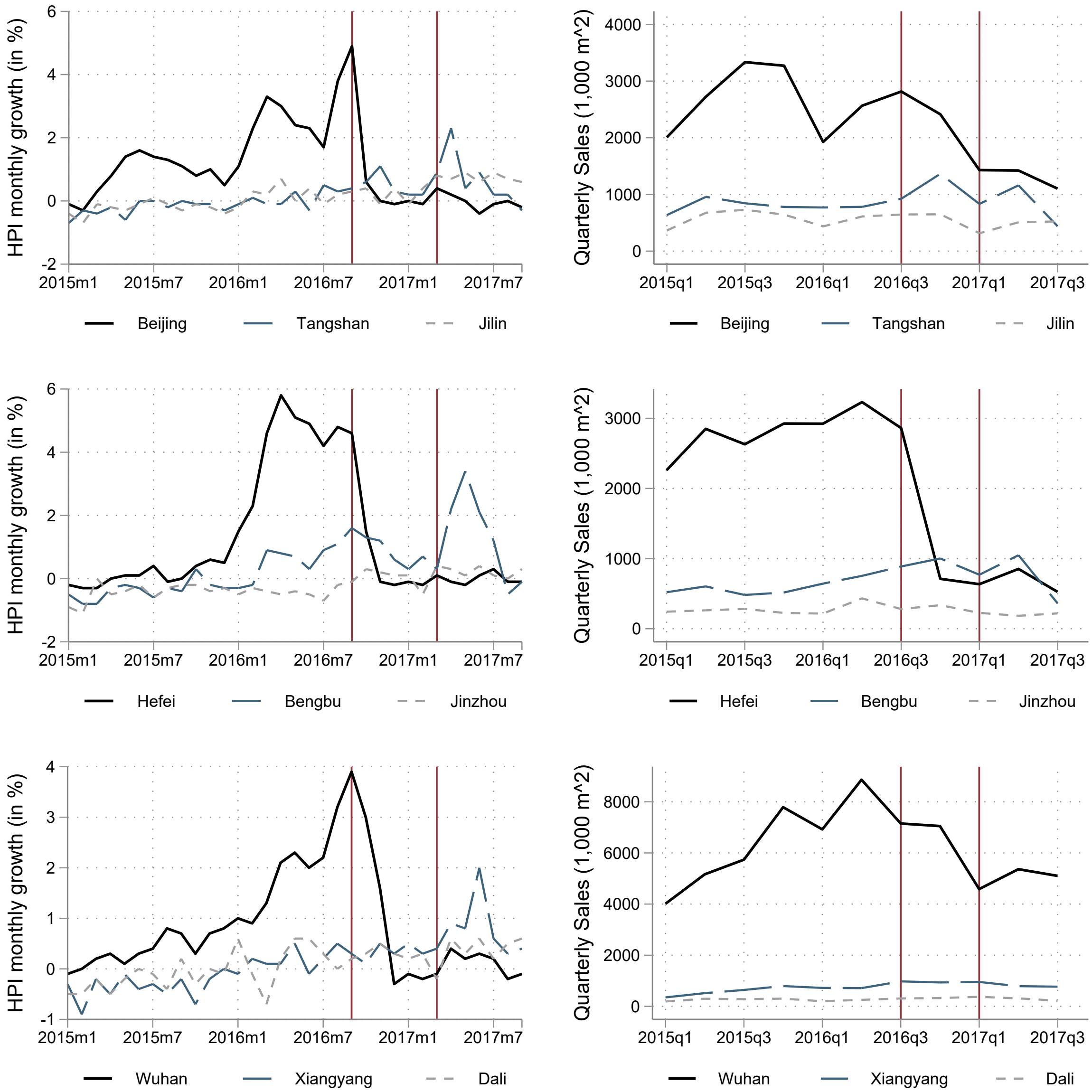
Zhang, Y., 2019. Wealth dynamics and the Chinese housing boom. Working paper, Peking University.

Figure 1: Locations of Example Pairs of Policy Cities and Treated Cities



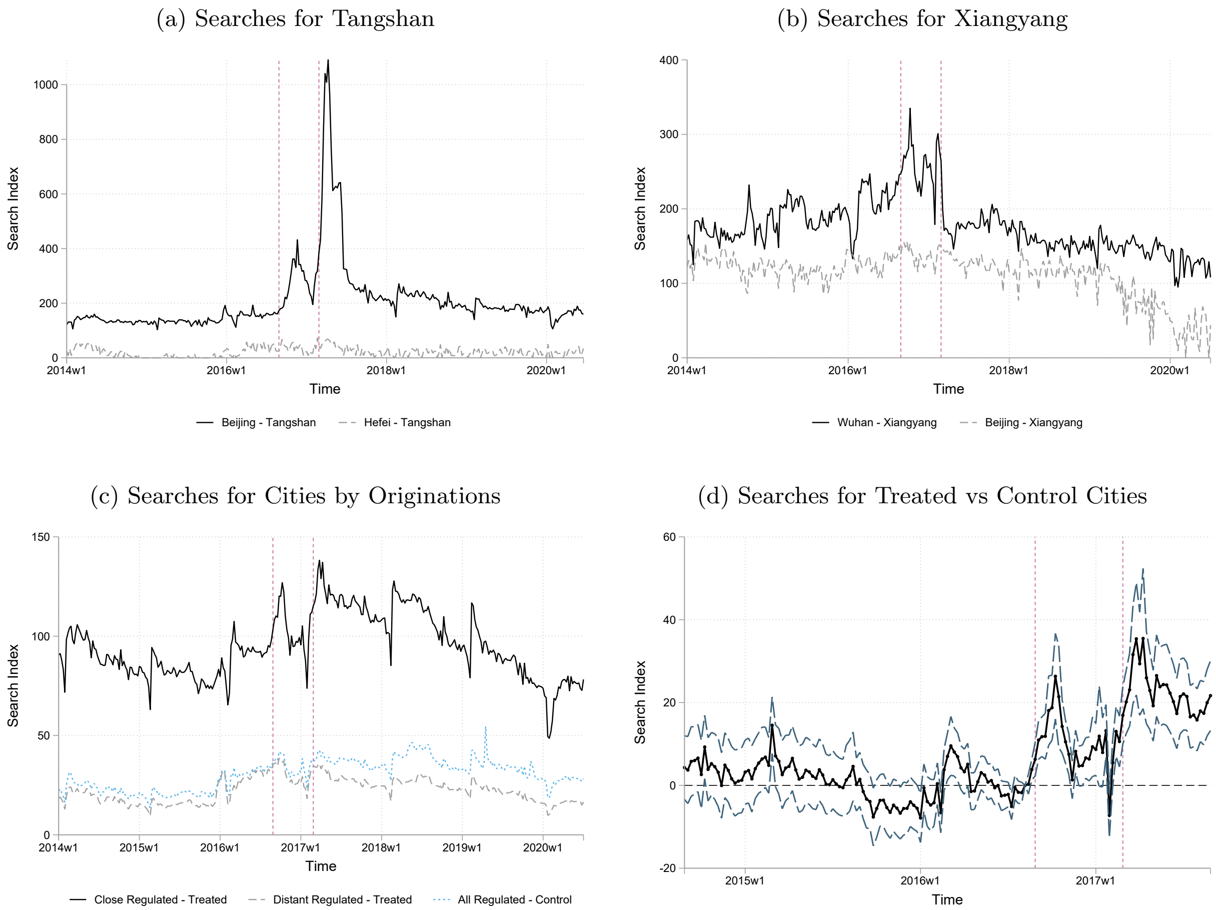
Notes: This figure shows the locations of three example pairs of cities that illustrate the effect of the imposition of restrictions on home purchases on regulated cities and neighboring non-regulated cities. Red dots indicate the three large example cities where house purchase restrictions were implemented. Blue dots indicate the neighboring small cities that appeared to be affected by the policy spillover. As shown in Figure **??**, the policy shocks in Beijing, Hefei and Wuhan appeared to affect the housing market in Tangshan, Bengbu and Xiangyang respectively.

Figure 2: Reactions of House Prices and Home Sales to the Restrictions - Some Examples



Notes: This figure plots the dynamics of house price growth rates and house sales in three pairs of cities from 2015 to September 2017 to illustrate the effect of the imposition of house purchase restrictions on regulated cities and on neighboring non-regulated cities. For the three pairs of cities, the three graphs in the left panel show the monthly growth rate of house prices, and the three graphs in the right panel show the quarterly home sales. House Purchase Restrictions implemented in September 2016 significantly decreased the growth of house prices and home sales in large cities such as Beijing, Hefei and Wuhan, and increased the sales in small cities such as Tangshan. The restrictions implemented in March 2017, although stricter, had much smaller effect on the housing markets in Beijing, Hefei and Wuhan. However, they significantly increased house prices in Tangshan, Bengbu and Xiangyang and also increased home sales in Tangshan and Bengbu. Meanwhile, Jilin, Jinzhou and Dali, distant from any regulated cities, shows little responses in house price and home sales. The figure uses National Bureau of Statistics 70-city house price index, which comes from constant-quality sampling but covers much fewer cities and home sales data from China Index Academy. The responses of house price and house sales in the non-regulated cities seem to be more pronounced after the second round of the impositions of house purchase restrictions in the regulated cities, which is consistent with the larger causally estimated policy spillover effect on house price and automobile spendings after the second round of house purchase restrictions.

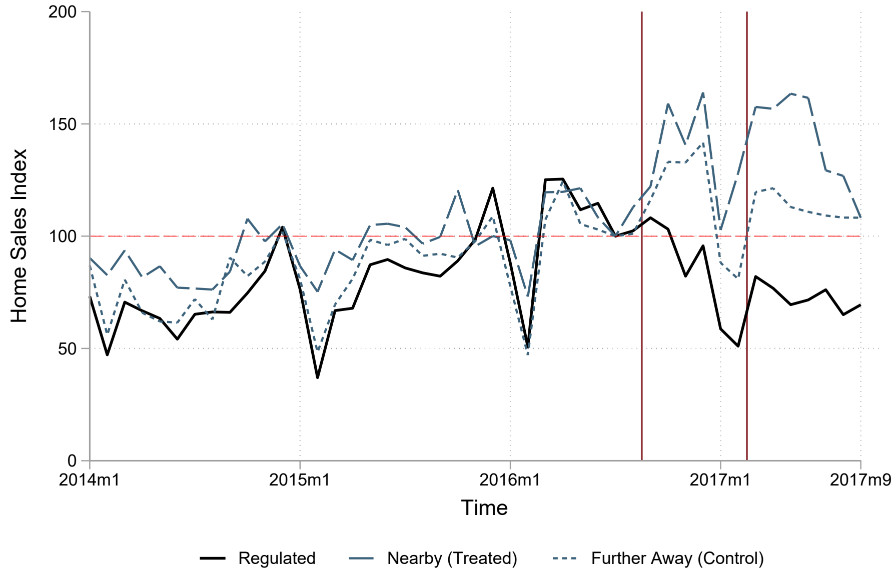
Figure 3: Web Searches of House Prices – Evidence of Out-of-Town Buyers



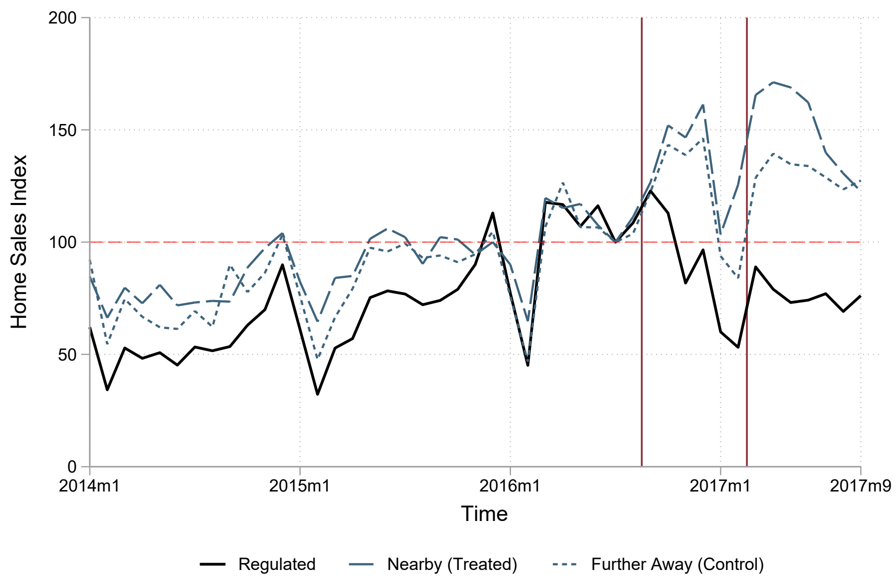
Notes: This figure plots the intensity of web searches of keywords related to house price and house market of the non-regulated cities originated from regulated cities, to show evidence of out-of-town buyers. We use Baidu search index to measure the intensity of web searches from one city to another. Panel (a) plots the intensity of searches for Tangshan originated from Beijing and Hefei. Both Beijing and Hefei are regulated cities, but Beijing is close to Tangshan and Hefei is distant. Panel (b) plots the intensity of searches for Xiangyang originated from Wuhan and Beijing. Both Wuhan and Beijing are regulated cities, but Wuhan is close to Xiangyang and Beijing is distant. Panel (c) plots the average intensity of searches for treated cities originated from close (<250km) regulated cities, from distant (≥250km) regulated cities, and average intensity of searches for control cities originated from all regulated cities. Treated cities are defined as non-regulated cities within 250km from the closest regulated city and the rest non-regulated cities are control cities. Panel (d) plots the estimated difference in intensity of searches for treated cities and control cities originated from all regulated cities, based on coefficients from a difference-in-differences regression.

Figure 4: House Transaction Volumes in Regulated, Treated and Control Cities

(a) Total Number of Transactions

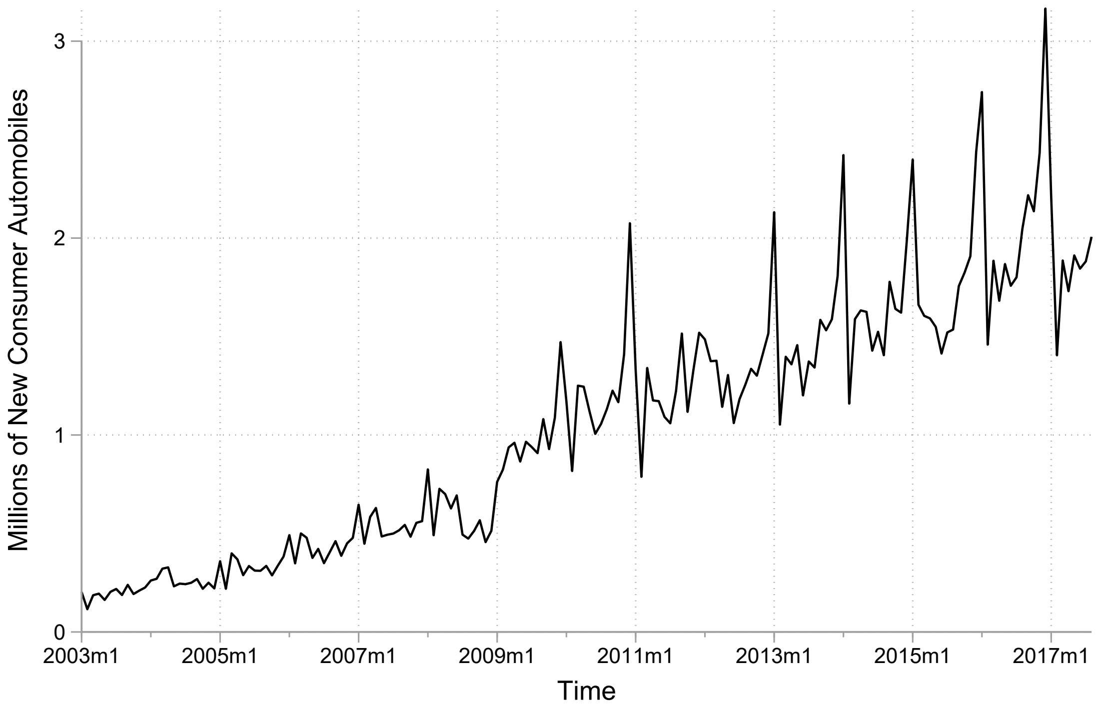


(b) Total Value of Transactions



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

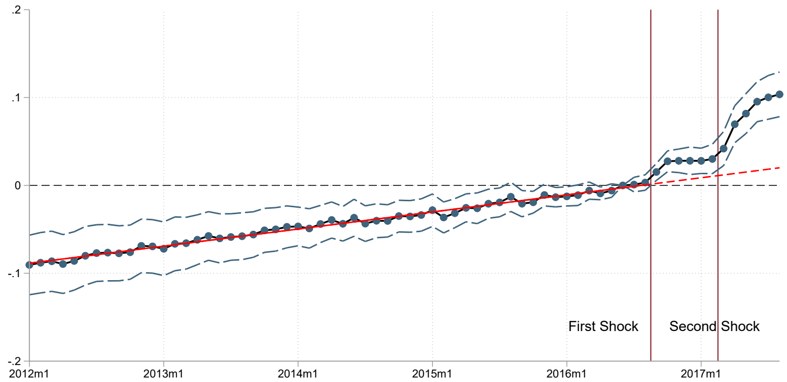
Figure 5: National Automobile Purchases in the CIITC Data



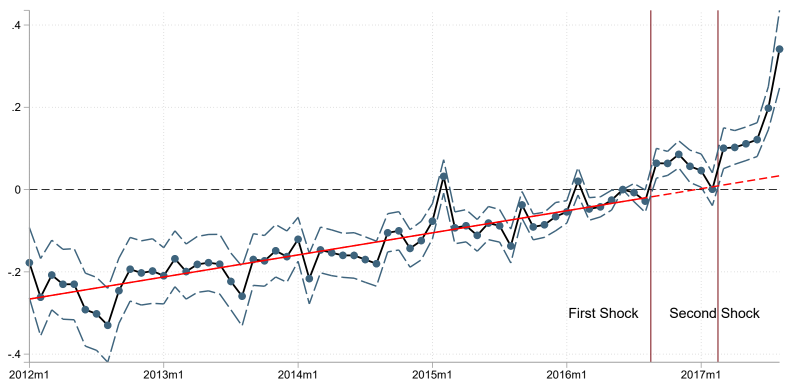
Notes: This figure shows the number of all the automobiles purchased in China each month aggregated from our CIITC data, from January 2003 to August 2017.

Figure 6: Pre-existing Trends and Dynamic Responses: House Prices, Auto Spending and Rents

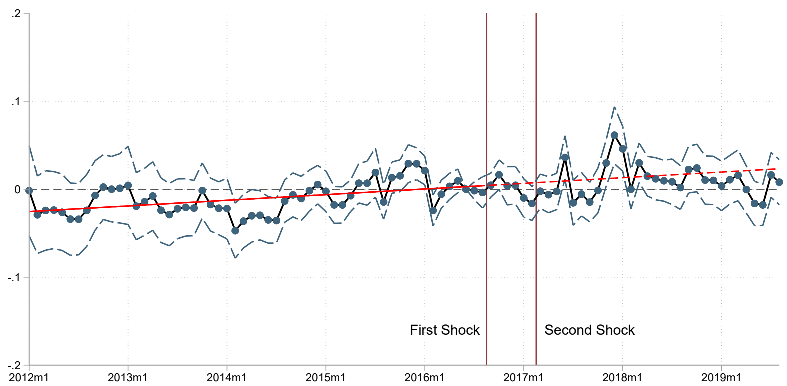
(a) Pre-existing trends and dynamic responses – log(House Price)



(b) Pre-existing trends and dynamic responses – log(Auto Spending)



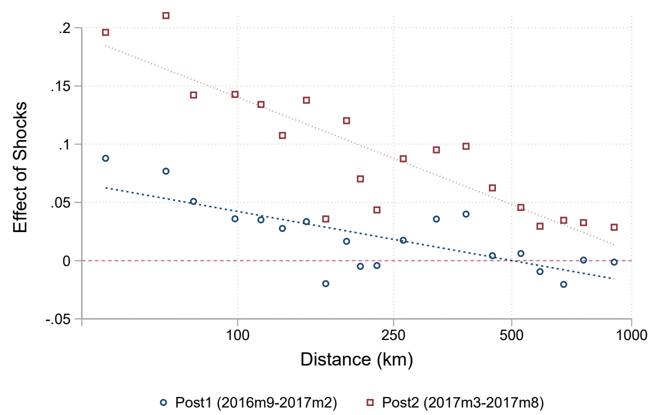
(c) Pre-existing trends and dynamic responses – log(Rent), longer period



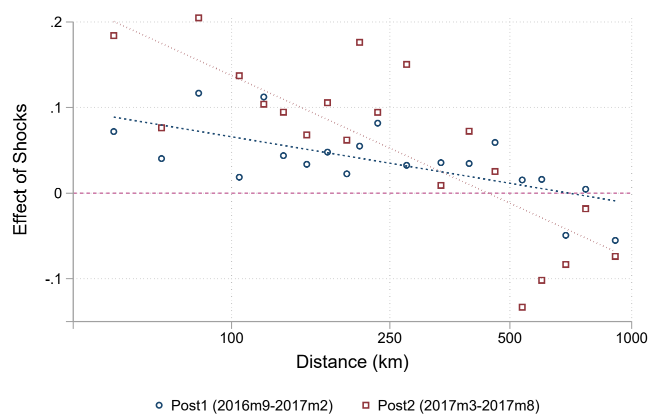
Notes: The figure plots the estimated responses of house prices, automobile spending and rents in treated cities relative to control cities, both before and after the policy spillover shocks took effect. The responses are estimated using difference-in-differences regressions replacing post-treatment 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. Automobile spending is seasonally adjusted. The 95% confidence interval is drawn based on standard errors clustered at the city level. Two rounds of policy spillover shocks, in September 2016 and March 2017, are labeled by vertical red lines. Red solid and dashed line is the pre-shock trend of the relative responses. Data on automobile spending and house prices are from January 2012 to August 2017. Data on rents are from January 2012 to August 2019, in order to examine longer-period effects on rent.

Figure 7: Effects of House Purchase Restriction Spillover on House Prices and Automobile Spending

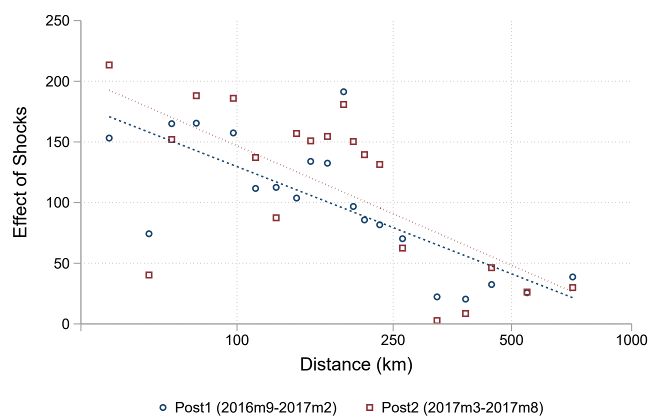
(a) log(House Price)



(b) log(Auto Consumption)

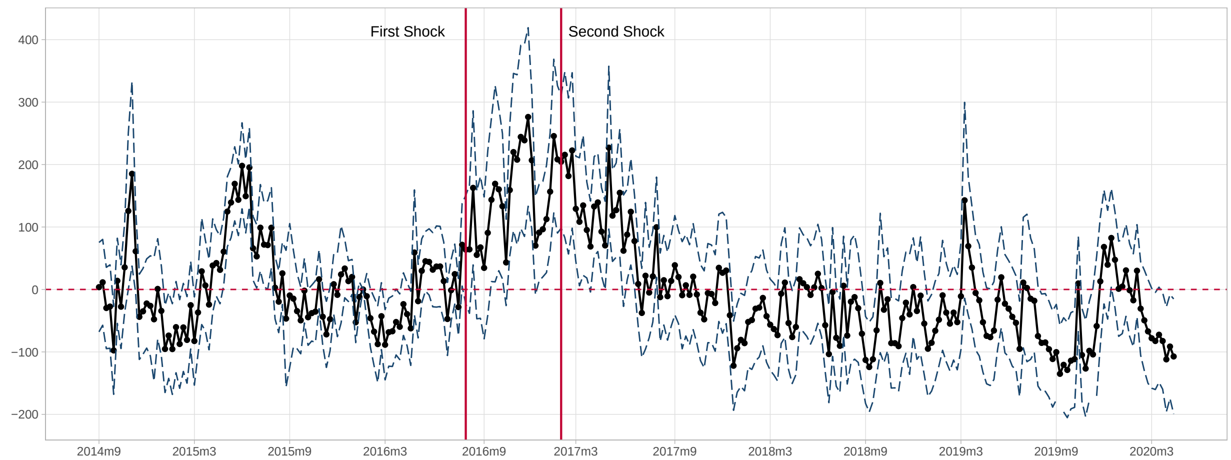


(c) Baidu Search



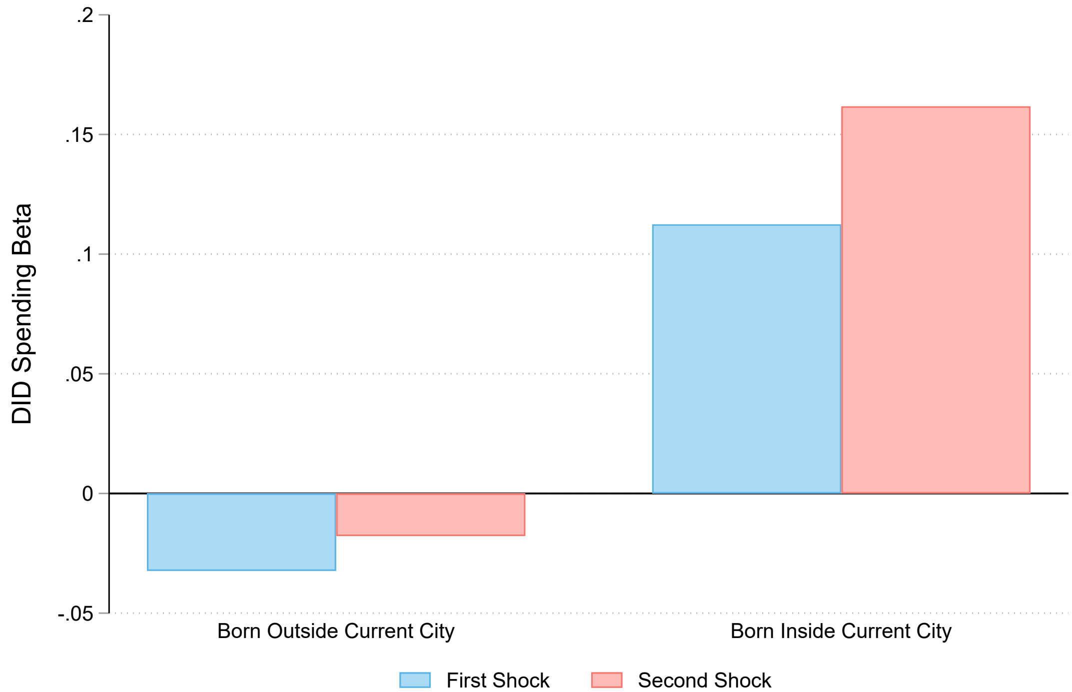
Notes: This figure plots the effect of House Purchase Restriction Spillover on the non-regulated city as the distance from the closest regulated city varies. The effect of spillover on each city is defined as deviations of three variables in post-shock periods (2016m9-2017m2 and 2017m3-2017m8) from city specific trend estimated using pre-shock period data. Panel (a) plots the effect of spillover on log house price. Panel (b) plots the effect of spillover on log automobile spending (seasonally adjusted). Panel (c) plots the effect of spillover on Baidu Search index measuring intensity of web searches of keywords related to house price and house market of each city.

Figure 8: Effects of House Purchase Restriction Spillover Baidu Non-Car Consumption



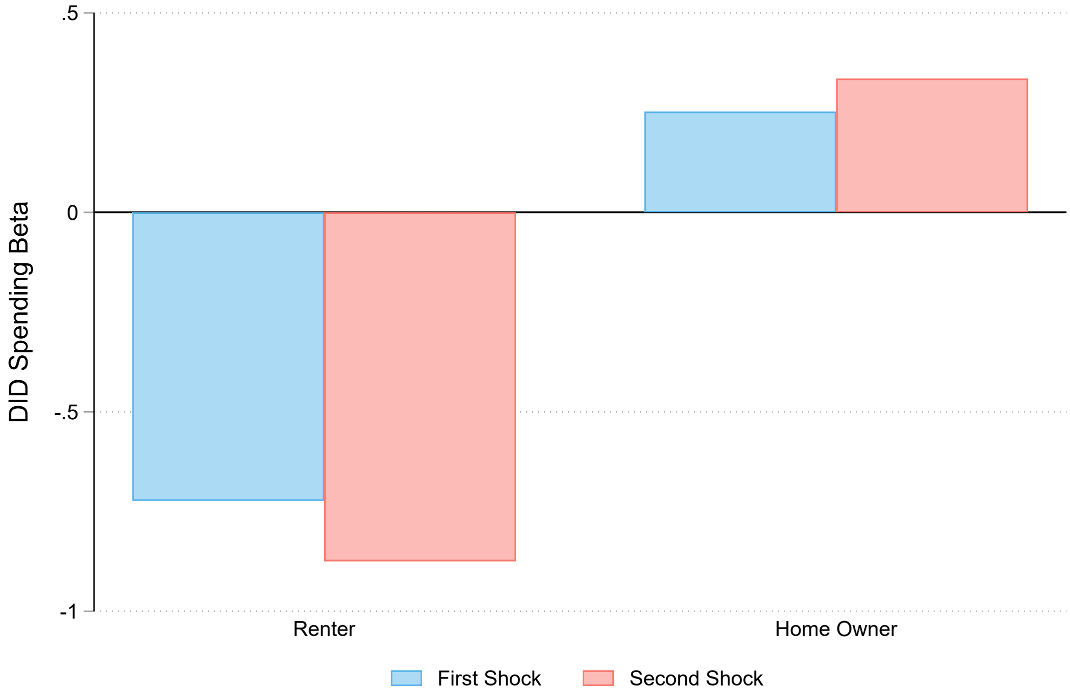
Notes: The figure plots the estimated responses of an alternative household consumption measure in treated cities relative to control cities, both before and after the policy spillover shocks took effect. The alternative household consumption measure is weekly intensity of Baidu web searches of keywords related to a basket of pricey consumption goods excluding automobiles, 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 (No. 1 liquor brand in China). The responses are estimated using difference-in-differences regressions 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 city level. Two rounds of policy spillover shocks, in September 2016 and March 2017, are labeled by vertical red lines.

Figure 9: DID Heterogeneity in Spending Responses: Individuals Born Locally Versus Migrants



Notes: This figure plots the differential average spending responses estimated in Table 13, where we compare difference-in-differences estimates of spending responses to housing booms following the spillover shock for individuals born in the current city they reside in and for individuals born outside of the city they reside in.

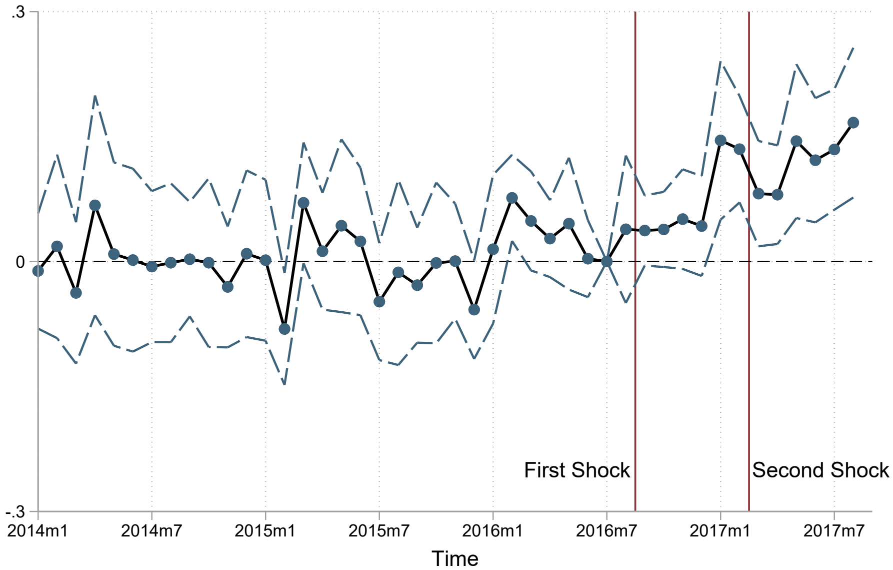
Figure 10: DID Heterogeneity in Spending Response: Survey-Predicted Homeownership



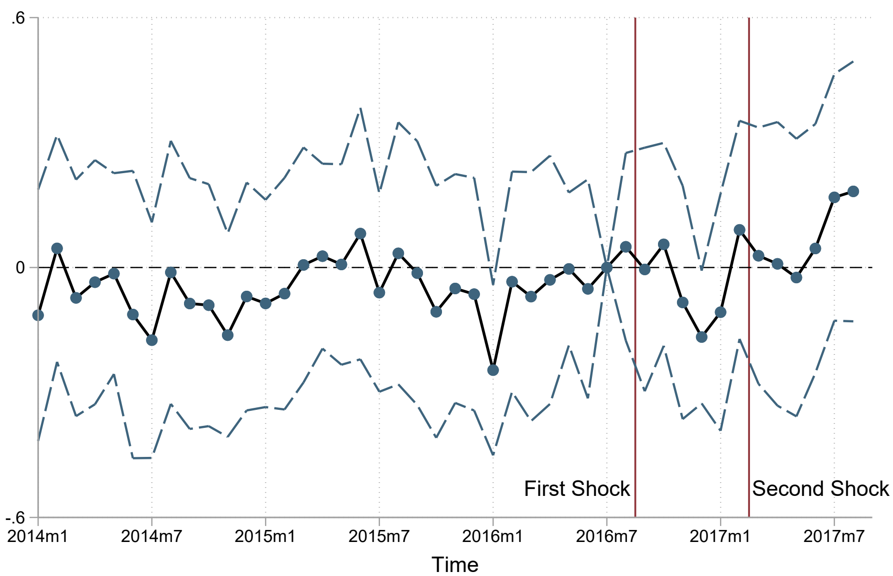
Notes: This figure plots the average responses in log automobile spending of survey-predicted renters and homeowners as estimated in Table 13, where we compare the difference-in-differences estimate of spending responses to house price booms following the house purchase restriction spillover shock for survey-predicted renters, and homeowners.

Figure 11: Parallel Trends in Automobile Spending: Local Buyers vs Immigrant Buyers

(a) Within Treated Cities

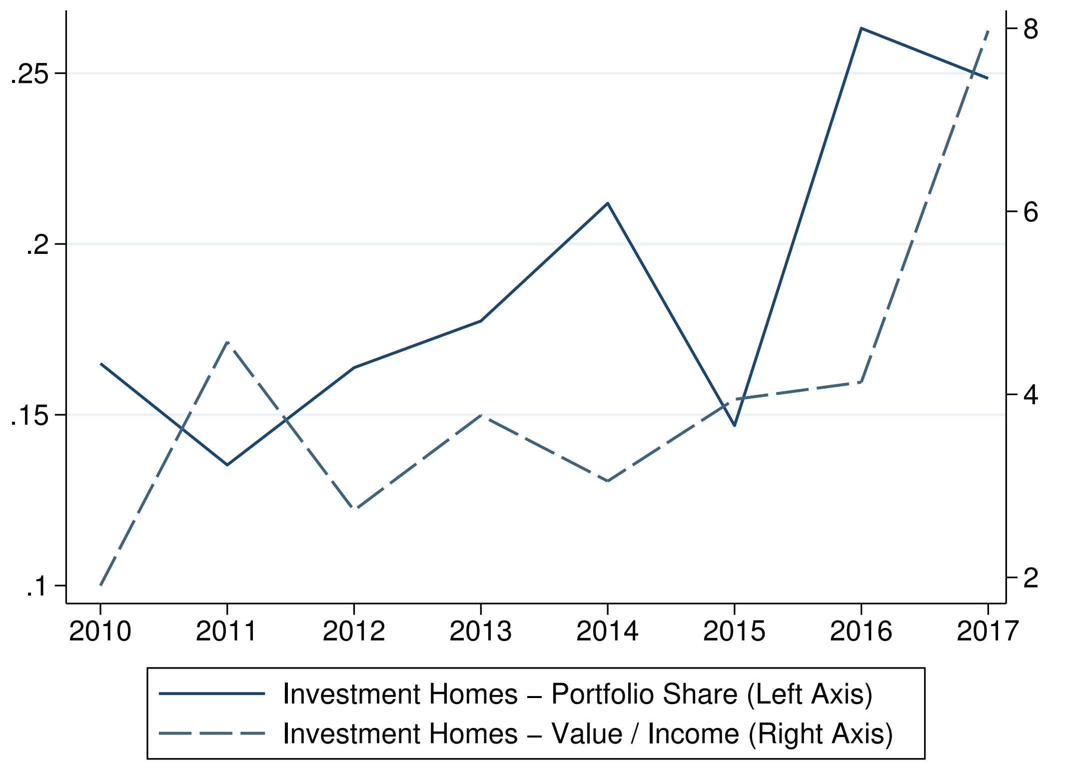


(b) Within Control Cities



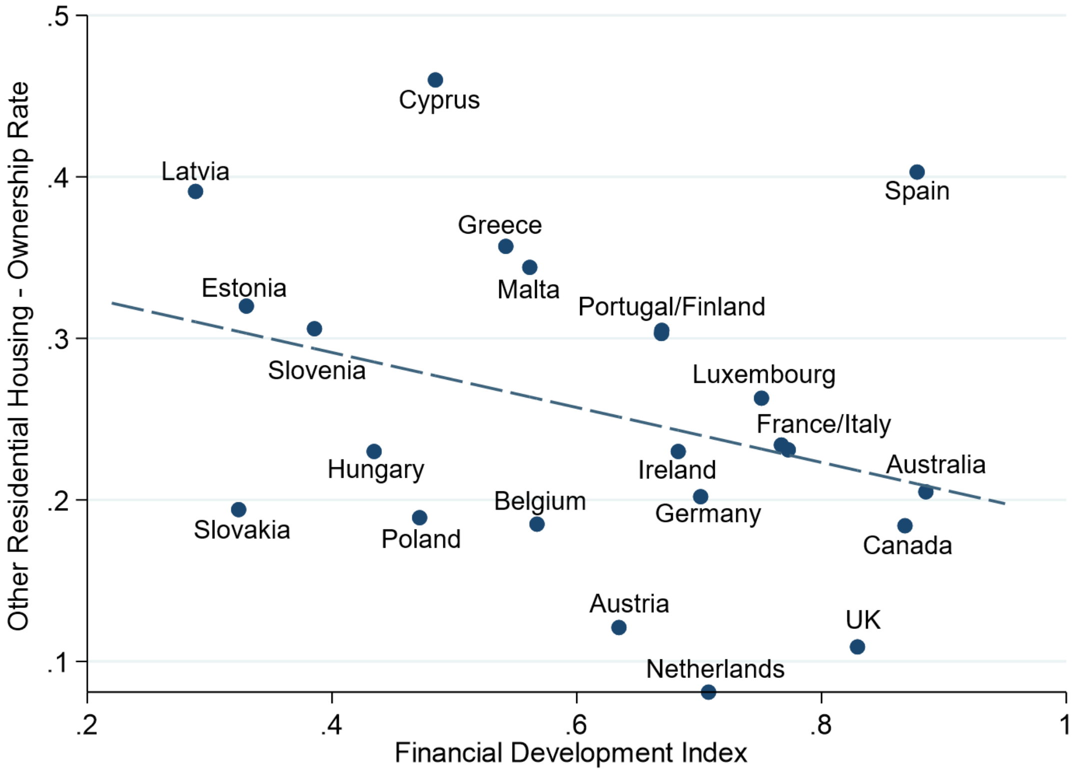
Notes: The figure plots the estimated responses of automobile spending by local buyers relative to immigrant buyers, both before and after the policy spillover shocks took effect. The responses are estimated using difference-in-differences regressions replacing post-treatment dummies with time dummies. All responses are relative to the level of responses in July 2016. Panel (a) plots the relative responses of local buyers estimated within the treated cities, that is, those within 250 km from the closest 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 trends are controlled for. Automobile spending is seasonally adjusted. The 95% confidence interval is drawn based on standard errors clustered at city level. Two rounds of policy spillover shocks, in September 2016 and March 2017, are labeled by vertical red lines.

Figure 12: Multi-Property Ownership in China



Notes: The solid line plots the value of average Chinese household's holding of investment homes as a share of the value of household total assets. The dashed line plots the ratio of the value of average Chinese household's holding of investment homes to the sum of labor income and pension income. Investment homes are defined as residential housing units (apartments, condos, as well as single or multi-family housing units) other than the main residence, not occupied by the owner. Based on authors' calculation using data from the 2010, 2012, 2014, 2016 waves of the China Family Panel Studies (CFPS), and the 2011, 2013, 2015, 2017 waves of the China Household Finance Survey (CHFS).

Figure 13: Cross Country Comparison of Multi-Property Ownership Rate



Notes: The figure plots the participation rate of households in holding investment real estate assets, defined as real estate assets other than the main residence, and the financial development index, of the 23 countries in **?**. The participation rate data is based on the Household Finance and Consumption Survey (HFCS) for Europe except UK, and **?** for Australia, Canada and UK. The financial development index is the IMF financial development index (Svirydzenka, 2016). Dashed line plots the fitted values from an OLS regression.

Table 1: First Round of House Purchase Restrictions

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

Notes: This table summarizes the policy changes in the first round of impositions of house purchase restrictions in the regulated cities. The policy information is collected from city government announcements and the China Index Academy(a company collecting information on China's real estate market). The resident and non-resident purchases mentioned above are at the household level instead of the individual level. The criteria for non-resident purchases usually include that the nonresidents must live in the city and pay taxes for a certain period of time.

Table 2: Second Round of House Purchase Restrictions

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

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

Table 3: Summary Statistics

	Count	Mean	Std. Dev.	10th	50th	90th
City-level data						
Fang et al. (2016) house price index	13641	2.05	1.02	0.99	1.82	3.46
CityRE house price index	31373	1.55	0.52	1.02	1.43	2.23
Combined house price index	19401	2.52	1.36	1.03	2.25	4.19
CityRE rent index	28975	1.39	0.41	0.98	1.32	1.90
Home sales index	4642	100.78	63.90	43.59	90.83	164.65
Baidu house search index	8316	384.62	319.14	27.64	326.41	777.71
Consumer new automobile spending (¥ mil.)	59130	281.30	534.20	14.85	108.22	662.97
Consumer new automobile purchases	59130	2124	3437	141	982	5162
Luxury automobile spending (¥ mil.)	59130	54.56	150.17	0.51	10.69	113.16
Luxury automobile purchases	59130	99	275	1	18	198
Baidu non-car spending index	91709	1542.66	1118.80	571.00	1294.00	2724.00
Per capita gross regional product (¥)	47040	32437	28541	7961	24543	65694
Residential population (1,000)	47040	4266	5163	1368	3531	7652
Square meters of road per capita	46320	9.95	10.70	3.82	8.67	16.59
Public buses per 1,000 residents	46344	0.67	0.63	0.21	0.58	1.17
GRP (¥ bil.)	2525	215.20	266.20	43.33	127.66	467.85
Real estate investment (¥ bil.)	2542	27.54	44.92	2.77	12.41	64.10
Bank deposits (¥ bil.)	2396	348.27	583.39	56.74	163.89	848.82
Employment growth	1186	0.02	0.06	0.01	0.01	0.03
Residential population (annual,mil.)	2290	4.23	3.07	1.24	3.52	7.86
GRP Growth	2188	0.09	0.08	0.02	0.09	0.18
Industrial output growth	2393	0.12	0.08	0.04	0.10	0.21
City-demographic group-level data						
Aggregated automobile spending of birthplace groups (¥ mil.):						
Born locally	53317	146.36	251.94	1.72	51.88	380.21
Migrants and out-of-towners	53317	158.77	363.75	6.49	44.85	376.80

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

Table 4: DID Estimated Effects of House Purchase Restriction Spillover Shocks on House Prices and Automobile Spending: Main Results

	(1) log(House Price)	(2) log(Rent Index)	(3) Home Sales	(4) Baidu 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	20331	19483	3637	8052	21012	20749	21012	20944	20263
R^2	0.983	0.944	0.566	0.941	0.979	0.944	0.987	0.986	
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

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

Notes: This table reports the difference-in-differences regressions of variables related to housing market and automobile spending with respect to spillover shocks from the imposition of house purchase restrictions in nearby regulated cities. The sample consists of all non-regulated 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 search index of keywords related to house price and house market in column (4), log household automobile spending in column (5), and log household spending on on luxury automobiles in column (6). Column (7) log household automobile spending which is seasonally adjusted. Column (8) reports regression 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 Column (8) and (9). Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest regulated city. Post1 is a dummy that takes the value 1 if the time is after the first round of the House Purchase Restriction shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to regulated city GRP. City Trend is city-specific linear trend. Standard errors are clustered at the city level.

Table 5: Post Treatment Responses of Macroeconomic Variables

	(1) log(GRP)	(2) Industrial Output Growth	(3) GRP Growth	(4) Employment Growth	(5) log(Population)	(6) log(Real Estate 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	1551	1501	1484	1031	1463	1549	1449
R^2	0.998	0.735	0.373	0.856	0.976	0.934	0.998
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

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

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

Table 6: DID Estimated Effects of House Purchase Restriction Spillover Shocks on House Prices and Automobile Spending: Continuous Distance Specification

	(1) log(House Price)	(2) log(Rent Index)	(3) Home Sales	(4) Baidu 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)
log(Distance)	-0.022***	0.003	-12.754	-43.947***	-0.043***	-0.075***	-0.034***	-0.022**	
× Post1	(-5.470)	(0.693)	(-1.445)	(-4.777)	(-4.245)	(-5.223)	(-3.727)	(-2.434)	
log(Distance)	-0.050***	0.008	-23.706	-58.781***	-0.071***	-0.105***	-0.086***	-0.069***	
× Post2	(-8.161)	(1.130)	(-1.479)	(-4.108)	(-5.162)	(-4.802)	(-6.592)	(-5.093)	
log(House									1.391***
Price)									(4.213)
R^2	0.984	0.944	0.568	0.940	0.979	0.944	0.987	0.986	
Observations	20331	19483	3637	8052	21012	20749	21012	20944	20263
First Stage F									129.028
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

t statistics in parentheses

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

Notes: This table reports the robustness of the main difference-in-differences estimates of the house price and automobile spending responses, by showing how the responses in non-regulated cities relate to distance from the nearest regulated cities as a continuous variable. The sample consists of all non-regulated 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 search index of keywords related to house price and house market in column (4), log household automobile spending in column (5), and log household spending on on luxury automobiles in column (6). Column (7) log household automobile spending which is seasonally adjusted. Column (8) reports regression 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 Column (8) and (9). Distance is the distance of each city from the nearest city regulated by house purchase restrictions. Post1 is a dummy that takes the value 1 if the time is after the first round of the house purchase restriction spillover shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to regulated city GRP. City Trend is city-specific linear trend. Standard errors are clustered at the city level.

Table 7: DID Estimated Effects of House Purchase Restriction Spillover Shocks on House Prices and Automobile Spending: Matching Specification

	(1) log(House Price)	(2) log(Rent Index)	(3) Home Sales	(4) Baidu 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	20193	19739	3200	9240	20604	20493	20604	20536	20125
R^2	0.983	0.954	0.542	0.943	0.980	0.950	0.987	0.987	
First Stage F									15.14314
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

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

Notes: This table reports the robustness of the main difference-in-differences estimates to using matched treated and control cities. We perform matching on city GRP, economic exposure to nearest regulated city, and house prices. The sample consists of all non-regulated 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 search index of keywords related to house price and house market in column (4), log household automobile spending in column (5), and log household spending on on luxury automobiles in column (6). Column (7) log household automobile spending which is seasonally adjusted. Column (8) reports regression 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 Column (8) and (9). Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest regulated city. Post1 is a dummy that takes the value 1 if the time is after the first round of the House Purchase Restriction shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to regulated city GRP. Standard errors are clustered at the city level.

Table 8: Distribution of variables and balance of Matching

	Treated			Control			Comparison	
	Pre-Matching							
	Mean	Variance	No. of Cities	Mean	Variance	No. of Cities	Std-diff	Var-ratio
City GRP	10.646	0.182	152	10.525	0.328	151	0.238	0.555
Exposure Beta	0.317	0.359	152	0.258	0.467	151	0.092	0.770
House Price	8.501	0.092	152	8.327	0.088	151	0.583	1.038
	Post-Matching (Mahalanobis distance with replacement)							
	Mean	Variance	No. of Cities	Mean	Variance	No. of Cities	Std-diff	Var-ratio
City GRP	10.646	0.182	152	10.626	0.178	152	0.047	1.020
Exposure Beta	0.317	0.359	152	0.319	0.357	152	-0.004	1.006
House Price	8.501	0.092	152	8.466	0.076	152	0.121	1.206

Notes: This table reports distributional test statistics for three variables (pre-treatment values) we use in matching treated cities to control cities: city GRP, economic exposure to closest regulated city, and house prices. The table also assesses balance between treatment groups in the means (using the standardized difference) and in the variances (using the variance ratio).

Table 9: DID Estimated Effects of House Purchase Restriction Spillover Shocks on House Prices and Automobile Spending: City-Specific Trend using Restricted Cubic Spline

	(1) log(House Price)	(2) log(Rent Index)	(3) Home Sales	(4) Baidu 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***	-0.012*	18.031**	25.808**	0.071***	0.144***	0.045***	0.032**	
	(4.447)	(-1.885)	(2.150)	(2.092)	(4.144)	(5.294)	(3.155)	(2.356)	
Treat × Post2	0.067***	-0.018*	33.024**	44.796**	0.107***	0.181***	0.120***	0.089***	
	(6.678)	(-1.773)	(2.326)	(2.035)	(4.237)	(4.724)	(4.841)	(3.347)	
log(House Price)									1.582***
									(3.117)
Observations	20331	19483	2505	8052	21012	20749	21012	20944	20263
R^2	0.990	0.957	0.572	0.950	0.981	0.947	0.990	0.989	
First Stage F									65.62792
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

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

Notes: This table reports the difference-in-differences regressions of variables related to housing market and automobile spending with respect to spillover shocks from the imposition of house purchase restrictions in nearby regulated cities. The sample consists of all non-regulated 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 search index of keywords related to house price and house market in column (4), log household automobile spending in column (5), and log household spending on on luxury automobiles in column (6). Column (7) log household automobile spending which is seasonally adjusted. Column (8) reports regression 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 Column (8) and (9). Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest regulated city. Post1 is a dummy that takes the value 1 if the time is after the first round of the House Purchase Restriction shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to regulated city GRP. City Trend is city-specific restricted cubic spline trend. Standard errors are clustered at the city level.

Table 10: House Price Growth and Home Transaction Volume in Regulated Cities

	Tier 1 Cities	Non Tier 1 Cities	All Regulated Cities
HPI Growth (15m8-16m8)	0.20	0.14	0.16
HPI Growth (16m8-17m8)	0.23	0.34	0.32
Δ HPI Growth	0.03	0.20	0.17
Home Transaction Volume (15m8-16m8)	41.06	11.27	16.94
Home Transaction Volume (15m8-16m8)	30.98	9.78	13.81
Δ Home Transaction Volume	-10.08	-1.49	-3.13

Notes: This table summarizes the change in house price growth and home transaction volume for regulated cities before and after the policy shocks. Tier 1 cities are 4 largest cities, including Beijing, Shanghai, Guangzhou, and Shenzhen. Non-Tier-1 cities are the remaining 18 regulated cities. HPI growth are calculated using the CityRE house price index.

Table 11: Heterogeneous Treatment Effect: Neighbors of Tier 1 Cities vs Neighbors of Non-Tier 1 Cities

	(1) log(House Price)	(2) log(Auto Spending)
Treat × Post1	0.014^{**}	0.039^{**}
	(2.053)	(2.381)
Treat × Post1 × Tier 1 City Neighbors	0.097^{***} (2.782)	0.022 (0.700)
Treat × Post2	0.053^{***}	0.105^{***}
	(4.348)	(3.963)
Treat × Post2 × Tier 1 City Neighbors	0.109^{**} (2.471)	0.104^{**} (2.158)
Observations	20331	20331
R^2	0.984	0.987
Controls	YES	YES
City FE	YES	YES
City Trend	YES	YES
Time FE	YES	YES

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

Notes: This table reports the heterogeneity treatment effect results comparing cities neighboring Tier 1 cities and cities neighboring non-Tier 1 cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m8. Regressions are at the city and month level. The dependent variables are log house price in column (1) and log seasonally adjusted automobile spending in column (2). Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest cities regulated by house purchase restrictions. Post1 is a dummy that takes the value 1 if the time is after the first round of the house purchase restriction spillover shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. Tier 1 City Neighbors is a dummy variable that takes the value 1 if the city's closest regulated city is a Tier 1 city. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita, exposure to regulated city's GRP. City Trend is city specific linear trend. Standard errors are clustered at the city level.

Table 12: Heterogeneous Treatment Effect by Policy Effectiveness on Closest Regulated City

	(1) log(House Price)	(2) log(Auto Spending)	(3) Home Sales Volume	(4) Baidu Search
Treat × Post1	0.018***	0.037**	8.769	66.984***
	(2.661)	(2.379)	(1.035)	(6.347)
Treat × Post1 × Closest Regulated City HPG Decline	0.024***	0.017	0.710	24.382**
	(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 ×Closest Regulated City HPG Decline	0.042***	0.038**	-7.287	-10.028
	(4.206)	(2.580)	(-0.899)	(-0.729)
Observations	20331	20331	2505	8052
R^2	0.984	0.987	0.554	0.941
Treat × Post1	0.021***	0.037**	5.411	66.335***
	(2.820)	(2.291)	(0.705)	(5.866)
Treat × Post1 × Closest Regulated City Volume Decline	0.010**	0.029***	4.286	20.728**
	(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 × Closest Regulated City Volume Decline	0.020**	0.037***	5.451	-12.426
	(2.281)	(2.663)	(0.902)	(-0.965)
Observations	19583	19583	2469	7744
R^2	0.983	0.987	0.555	0.941
Controls	YES	YES	YES	YES
City FE	YES	YES	YES	YES
City Trend	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

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

Notes: This table reports the heterogeneity treatment effect results comparing cities neighboring regulated cities differently impacted by House Purchase Restrictions. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m8. Regressions are at the city and month level. 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 search index of keywords related to house price and house market in Column (4). Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest cities regulated by house purchase restrictions. Post1 is a dummy that takes the value 1 if the time is after the first round of the house purchase restriction spillover shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. Closest Regulated City HPG Decline is the decline in house price growth rate from 2015m8-2016m8 to 2016m8-2017m8, of the closest regulated city. Closest Regulated City Volume Decline is the decline in home sales volumes from 2015m8-2016m8 to 2016m8-2017m8 , of the closest regulated city. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita, exposure to regulated city's GRP. City Trend is city specific linear trend. Standard errors are clustered at the city level.

Table 13: DID Heterogeneity in Spending Response: Local vs Migrants, Survey-Predicted Renters vs Homeowners

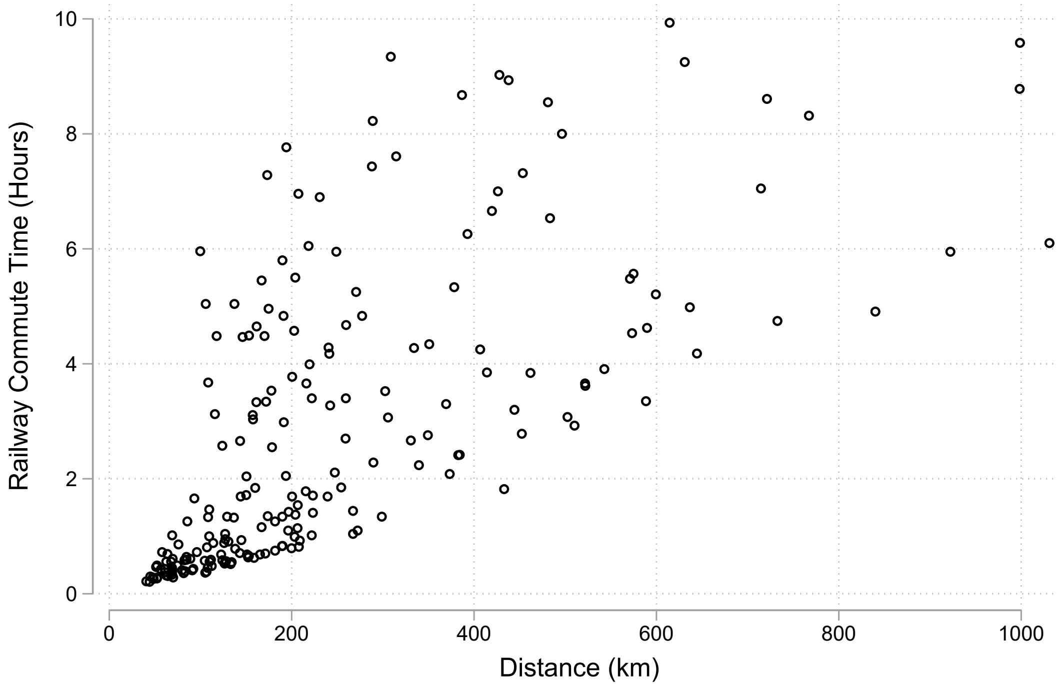
	(1) Home Ownership	(2) log(Auto Spending)	(3) log(Auto Purchases)	(4) log(Auto Spending)	(5) log(Auto Purchases)
Constant	0.812 ***				
	(58.675)				
Born in Current City	0.148***				
	(4.219)				
Treat × Post1		-0.032	-0.040*		
		(-1.385)	(-1.768)		
Treat × Post1		0.145***	0.130***		
× Born in Current City		(5.688)	(5.110)		
Treat × Post2		-0.018	-0.027		
		(-0.568)	(-0.898)		
Treat × Post2		0.180***	0.175***		
× Born in Current City		(7.519)	(7.048)		
Treat × Post1				-0.723***	-0.660***
× Renter				(-2.722)	(-2.625)
Treat × Post1				0.252***	0.216***
× Owner				(3.854)	(3.428)
Treat × Post2				-0.874***	-0.861***
× Renter				(-2.784)	(-2.642)
Treat × Post2				0.335***	0.317***
× Owner				(4.091)	(3.696)
Observations	62554	33185	33185	33185	33185
R^2	0.818	0.986	0.989	0.986	0.989
Controls		YES	YES	YES	YES
City× Migrants FE		YES	YES	YES	YES
City Trend		YES	YES	YES	YES
Time FE		YES	YES	YES	YES

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

Notes: Column (1) shows the first stage relationship between home ownership and local/non-local residency status based on survey data. The relationship is used to impute rentership and total homeownership. Standard errors are clustered at the survey-year-group level in Column (1). Column (2) and (3) show the difference-in-differences estimates of the responses of automobile spending and number of automobile purchases, of local buyers relative to migrants. Column (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 non-regulated cities, and the data span from 2012m1 to 2017m8. Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest regulated city. Post1 is a dummy that takes the value 1 if the time is after the first round of the House Purchase Restriction shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. Born in Current City is a dummy variable that takes the value 1 if the automobile purchase is made by individuals born in the city they reside in. Renter and Owner are the imputed rentership and total homeownership rates for each city by birth place group in each month. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to regulated city GRP. City Trend is city-specific linear trend. Standard errors are clustered at the city level in Column (2)-(3), and are bootstrapped standard errors in Column (4) and (5) taking into account of both first-stage and second-stage errors..

A Appendix

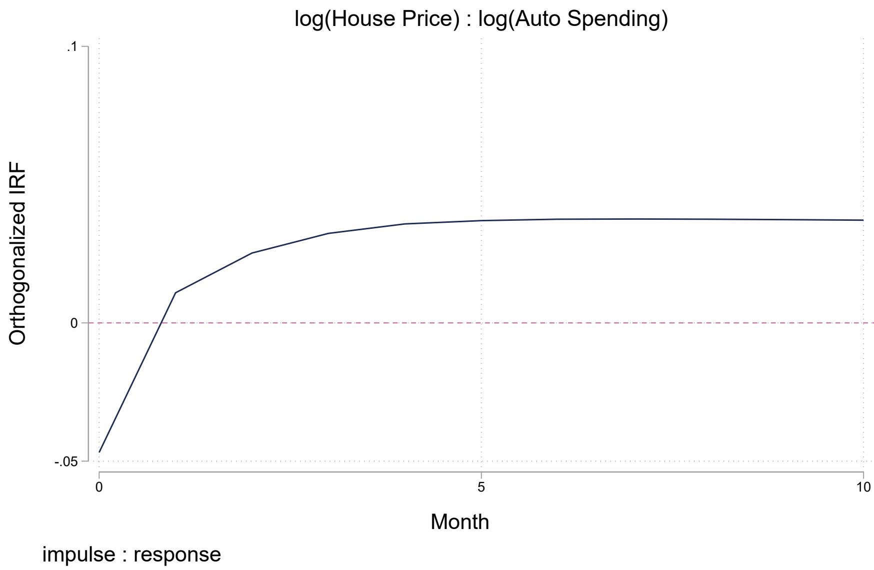
Figure A.1: Railway Travel Time vs Distance



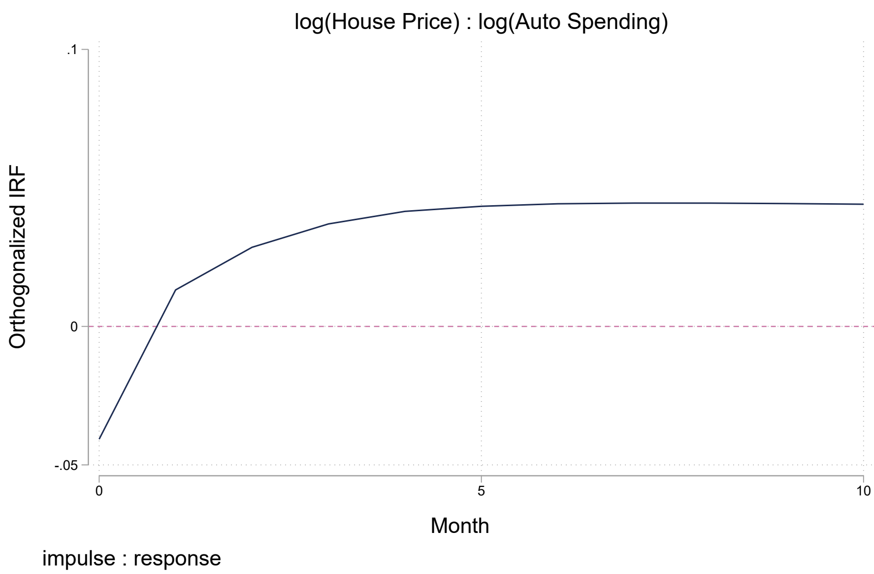
Notes: This figure plots the shortest railway travel time against the physical distance between non-regulated cities and their closest regulated cities. Railway travel time is calculated as the minimum travel time between cities, based on the schedules of all the trains operating in 2017 in China.

Figure A.2: Panel VAR Impulse Response Function for House Price Shocks

(a) Automobile Spending (All Cities)



(b) Automobile Spending (Treated and Control Cities)



Notes: The figure plots the orthogonized impulse response function describing the effect of a house price shock on automobile spending. The impulse response function is based on a panel vector autoregression of log automobile spending and log house price, with city level controls added, and using Helmert transformation to remove panel-specific fixed effects. Panel (a) is based on estimation using all cities. Panel (b) is based on estimation using only treated and control cities. We use two lags in the panel vector autoregressions. The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita, exposure to regulated city's GRP.

Table A.1: DID Estimated Effects of House Purchase Restriction Spillover Shocks on House Prices and Automobile Spending: Railroad Travel Time Specification

	(1) log(House Price)	(2) log(Rent Index)	(3) Home Sales	(4) Baidu 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	0.033^{***}	-0.004	3.509	66.785^{***}	0.047^{**}	0.092^{***}	0.041^{**}	0.004	
	(3.972)	(-0.580)	(0.363)	(4.702)	(2.476)	(3.369)	(2.224)	(0.244)	
Treat × Post2	0.080^{***}	-0.013	13.383	115.614^{***}	0.098^{***}	0.154^{***}	0.116^{***}	0.063^{**}	
	(5.962)	(-1.184)	(0.806)	(5.242)	(3.438)	(3.821)	(4.193)	(2.227)	
log(House Price)									1.046^{**}
									(2.296)
Observations	20331	19483	3637	8052	21012	20749	21012	20944	20263
R^2	0.983	0.944	0.563	0.940	0.979	0.944	0.987	0.986	
First Stage F									78.041
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

t statistics in parentheses

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

Notes: This table reports the difference-in-differences regressions of variables related to housing market and automobile spending with respect to spillover shocks from the imposition of house purchase restrictions in nearby regulated cities. The sample consists of all non-regulated 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 search index of keywords related to house price and house market in column (4), log household automobile spending in column (5), and log household spending on on luxury automobiles in column (6). Column (7) log household automobile spending which is seasonally adjusted. Column (8) reports regression 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 Column (8) and (9). Treat is a dummy that takes the value 1 if the minimum railway travel time in 2017 to the nearest regulated city is less than 2 hours. Post1 is a dummy that takes the value 1 if the time is after the first round of the House Purchase Restriction shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to regulated city GRP. City Trend is city-specific linear trend. Standard errors are clustered at the city level.

Table A.2: DID Estimated Effects of House Purchase Restriction Spillover Shocks on House Prices and Automobile Spending: Alternative Distance Cutoffs

	(1) log(House Price)	(2) log(Rent Index)	(3) Home Sales	(4) Baidu 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)
				Cutoff Distance=300 km					
Treat × Post1	0.026***	-0.007	17.822**	81.378***	0.079***	0.134***	0.061***	0.036**	
	(3.525)	(-0.896)	(2.395)	(7.360)	(3.996)	(4.852)	(3.381)	(2.176)	
Treat × Post2	0.070***	-0.016	38.412***	125.612***	0.131***	0.184***	0.159***	0.116***	
	(5.747)	(-1.258)	(3.116)	(6.628)	(4.546)	(4.458)	(5.672)	(4.317)	
log(House Price)									2.065***
									(3.218)
				Cutoff Distance=200 km					
Treat × Post1	0.032***	0.001	13.036	69.726***	0.060***	0.107***	0.047***	0.026	
	(3.986)	(0.097)	(1.373)	(5.178)	(3.187)	(3.944)	(2.629)	(1.523)	
Treat × Post2	0.078***	-0.003	15.822	88.552***	0.083***	0.134***	0.106***	0.064**	
	(5.893)	(-0.253)	(0.982)	(3.972)	(2.939)	(3.340)	(3.839)	(2.319)	
log(House Price)									0.909**
									(2.144)
				Cutoff Distance=150 km					
Treat×Post1	0.041***	-0.006	16.165	47.008***	0.059***	0.126***	0.053***	0.027	
	(4.164)	(-0.750)	(1.409)	(2.847)	(2.927)	(4.290)	(2.613)	(1.315)	
Treat×Post2	0.086***	-0.007	26.780	59.187**	0.101***	0.161***	0.118***	0.090***	
	(5.759)	(-0.564)	(1.309)	(2.252)	(3.286)	(3.714)	(3.982)	(3.039)	
log(House Price)									1.014**
									(2.535)
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

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

Notes: This table reports the robustness of the main difference-in-differences estimates of the house price and household spending responses to using alternative distance cutoffs of 150 km, 200 km and 300 km for assigning the treatment status of cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 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 search index of keywords related to house price and house market in column (4), log household automobile spending in column (5), and log household spending on on luxury automobiles in column (6). Column (7) log household automobile spending which is seasonally adjusted. Column (8) reports regression 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 Column (8) and (9). In different panels, Treat is a dummy that takes the value 1 if the city is within 300 km, 200 km, or 150 km from the nearest regulated city. Post1 is a dummy that takes the value 1 if the time is after the first round of the house purchase restriction spillover shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to regulated city GRP. City Trend is city-specific linear trend. Standard errors are clustered at the city level.

Table A.3: Baidu Consumption DID

	(1) Main	(2) Cutoff =300km	(3) Cutoff =200km	(4) Cutoff =150km	(5) Commute Time	(6) Cubic Trend	(7) Matching	(8) Continuous Distance
Treat× Post1	106.671***	105.649***	103.617***	92.719**	80.861*	143.479***	112.944***	
	(3.092)	(3.205)	(2.608)	(1.982)	(1.918)	(4.153)	(3.327)	
Treat× Post2	136.476***	156.746***	158.330***	151.636**	149.858***	199.134***	70.326	
	(2.778)	(3.253)	(3.015)	(2.534)	(2.700)	(3.100)	(1.341)	
log(Distance)× Post1								-55.680***
								(-4.814)
log(Distance)× Post2								-101.655***
								(-3.147)
Observations	44370	44370	44370	44370	44370	44370	43500	44370
R^2	0.933	0.932	0.933	0.933	0.932	0.939	0.948	0.933
Controls	YES	YES	YES	YES	YES	YES	YES	YES
City FE	YES	YES	YES	YES	YES	YES	YES	YES
City Trend	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES

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

Notes: This table reports the difference-in-differences regressions of variables related to housing market and automobile spending with respect to spillover shocks from the imposition of house purchase restrictions in nearby regulated cities. The sample consists of all non-regulated 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 search index of keywords related to house price and house market in column (4), log household automobile spending in column (5), and log household spending on on luxury automobiles in column (6). Column (7) log household automobile spending which is seasonally adjusted. Column (8) reports regression 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 Column (8) and (9). Treat is a dummy that takes the value 1 if the city is closer than 250 km to the nearest regulated city. Post1 is a dummy that takes the value 1 if the time is after the first round of the House Purchase Restriction shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to regulated city GRP. City Trend is city-specific linear trend. Standard errors are clustered at the city level.

Table A.4: DID Heterogeneity in Spending Responses: Individuals Born Locally Versus Migrants, Alternative Distance Cutoffs

	(1) log(Automobile Spending)	(2) log(Automobile Purchases)
	Cutoff Distance=300 km	
Treat × Post1 × Born in Current City	0.154***	0.140***
	(6.435)	(5.824)
Treat × Post2 × Born in Current City	0.180***	0.174***
	(7.838)	(7.348)
	Cutoff Distance=200 km	
Treat × Post1 × Born in Current City	0.129***	0.110***
	(4.526)	(3.871)
Treat × Post2 × Born in Current City	0.167***	0.161***
	(6.404)	(5.848)
	Cutoff Distance=150 km	
Treat × Post1 × Born in Current City	0.102***	0.080**
	(2.785)	(2.182)
Treat × Post2 × Born in Current City	0.133***	0.126***
	(4.021)	(3.573)
Controls	YES	YES
City× Migrants FE	YES	YES
City Trend	YES	YES
Time FE	YES	YES
Treat × Post1	YES	YES
Treat × Post2	YES	YES

t statistics in parentheses

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

Notes: This table reports the robustness of the difference-in-differences estimates of the heterogeneity in consumer spending responses between individuals born locally and migrants to using alternative distance cutoffs of 150 km, 200 km and 300 km for assigning the treatment status of cities. The sample consists of all non-regulated cities, and the data span from 2012m1 to 2017m8. Regressions at at the city, birthplace group, and month level. The dependent variables are log seasonally adjusted automobile spending in column (1) and log seasonally adjusted number of automobiles purchased by consumers in column (2), measured for each city, each birthplace group in each month. Treat is a dummy that takes the value 1 if the city is within 300 km, 200 km, or 150 km from the nearest cities regulated by house purchase restrictions. Post1 is a dummy that takes the value 1 if the time is after the first round of the house purchase restriction spillover shock and before the second round. Post2 is a dummy that takes the value 1 if the time is after the second round of shock. Born in Current City is a dummy variable that takes the value 1 if the automobile purchase is made by individuals born in the city they reside in. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita, and exposure to regulated city GRP. Standard errors are clustered at the city level.

Table A.5: Consumer Spending on New Automobiles vs House Price (OLS)

Dependent variable: log(Automobile Spending)						
	(1)	(2)	(3)	(4)	(5)	(6)
	All Cities		Treatment & Control Cities		Treatment & Control Cities (Baseline Period)	
log(CityRE house price index, 08-17)	0.334***		0.604***		0.636***	
	(2.804)		(9.380)		(6.504)	
log(Combined house price index, 03-17)		0.412***		0.688***		0.679***
		(2.910)		(5.865)		(5.365)
Observations	30358	16769	27928	13672	20263	5780
R^2	0.965	0.965	0.959	0.962	0.963	0.959
Controls	YES	YES	YES	YES	YES	YES
City FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES

t statistics in parentheses

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

Notes: This table reports the panel fixed effect regressions of consumer spending on new automobiles on two house price indices covering different time periods in China. The data spans 2003m1 to 2017m8. The dependent variable is log household spending on new automobiles in each city in each month. In columns (1)-(2), the sample includes all cities. In columns (3)-(4), the sample includes treatment and control cities in the quasi-experimental strategy. In columns (5)-(6), the sample includes observations for the treatment and control cities in the sample period (covering 2012m1 to 2017m8) of the quasi-experimental strategy. The main explanatory variables are the CityRE constant-quality house price index, covering a broader set of cities for 2008m1 to 2017m12 in columns (1), (3) and (5), and the combined house price index covering a longer period (2003m1 to 2017m12) but a more restricted set of cities in columns (2), (4) and (6), taking data from Fang et al. (2016) and CityRE for the set of cities in Fang et al. (2016), using the Fang et al. (2016) semi-repeated-sale house price index data whenever available. The control variables are per capita GRP, resident population, square meters of road per capita, number of public buses per capita and exposure to regulated city GRP. Standard errors are clustered at the city level.

Table A.6: Panel VAR Results: Relationship Between House Price and Auto Spending

	log(Auto Spending)	log(House Price)
	(a). All Cities	
L.log(House Price)	0.580	0.712***
	(1.538)	(32.490)
L2.log(House Price)	0.380***	0.266***
	(3.322)	(18.347)
L.log(Auto Spending	0.214***	0.004
	(4.757)	(1.615)
L2.log(Auto Spending)	0.119***	0.006***
	(3.399)	(2.947)
Observations	29468	29468
	(b). Treatment and Control Cities	
L.log(House Price)	0.647**	0.696***
	(2.536)	(34.802)
L2.log(House Price)	0.416***	0.281***
	(3.865)	(19.289)
L.log(Auto Spending)	0.248***	0.002
	(5.871)	(0.922)
L2.log(Auto Spending)	0.143***	0.006***
	(4.389)	(2.737)
Observations	27100	27100

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

Notes: This table reports the estimation results from a panel vector autoregression of log automobile spending and log house price, with city-level controls added, and using Helmert transformation to remove panel-specific fixed effects. Panel (a) uses the sample of all cities (including regulated cities). Panel (b) uses the sample of treated and control cities (only non-regulated cities). The control variables are per capita GRP, resident population, square meters of road per capita and number of public buses per capita, exposure to regulated city's GRP. Standard errors are two-way clustered at the city and time level.

Table A.7: Home Equity Borrowing, Automobile Purchase Financing, and Homeownership in China

Homeownership	Year	Mean	Std. Dev
(across provinces)	2015	88.0%	4.0%
	2017	88.0%	4.4%
Multi-home ownership		Mean	Std. Dev
(across provinces)	2015	18.1%	4.8%
	2017	18.0%	3.7%
Share of spending on car		Share	
purchases to total	2014	10.5%	
non-housing spending	2016	10.6%	
Share of homeowners with		Share	
existing refinanced	2015	2.2%	
mortgages or HELOCs	2017	1.4%	
Purpose of refinanced		Share	Purpose
or HELOC funds	2015	87.30%	Home Purchase
		5.62%	Personal Business
		2.74%	Interpersonal Lending
		2.56%	Misc. (others)
		1.37%	Financial Investment
		0.41%	Purchase of cars
Attitude toward		Share	Attitude
"Borrowing to consume"	2018	1.7%	Agree
		6.3%	Somewhat Agree
		1.8%	Neutral
		23.6%	Somewhat Disagree
		66.7%	Disagree
Fraction of auto purchases		Share	
on installment loans	2013	21.2%	
	2014	17.4%	
	2015	17.9%	
	2016	23.4%	
	2017	23.9%	

Notes: This table summarizes the usage of home equity borrowing and automobile purchase financing, the fraction of automobile consumption as of total household consumption, and homeownership rates in China. This is based on data from household surveys CHFS and CFPS.