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

Yinglu Deng[1] Li Liao[1] Jiaheng Yu[2] Yu Zhang[3]

[1]PBC School of Finance, Tsinghua University

[2]Sloan School of Management, MIT

[3]Guanghua School of Management, Peking University

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Motivation

- **Policy spillovers**:

  When regulating housing speculation locally, can the policy have effects non-locally?

- **Non-local demand in housing markets**:

  How it affect house prices, and in turn, local residents?

- **Housing wealth effect**:

  How to estimate when house prices are correlated to levels of future productivity?

- **Key empirical challenge: Lack of suitable setting and data.**
  Theory: Favilukis & Van Nieuwerburgh (JF 2021).

What We Do

In this paper, we study the unintended consequences of regulating housing speculation at the local level:

- Specifically, we exploit a quasi-natural experiment in China where local authorities imposed house purchase restrictions in 2016 and 2017
- While these restrictions were effective in containing the local house price surge, they triggered capital flight into nearby, unregulated housing markets
- House prices in these unregulated cities rose sharply following the out-of-town home purchases despite no obvious improvement in local housing fundamentals
- Consumption spending on automobiles increased following the housing wealth increase overall in these unregulated cities, but the responses are different for demographic groups that are more likely to be owners (renters)

Literature

- Non-local demand and connectedness in asset markets:
  - **savings glut hypothesis**: Bernanke (2005).
  - **foreign buyers**: Badarinza and Ramadorai (2018), Cvijanovic and Spaenjers (2018), Favilukis and Van Nieuwerburgh (forthcoming).
  - **housing market connectedness**:

    Bailey et al. (2016), DeFusco et al. (2018), Yang, Yu and Deng (2018).
- Marginal propensity to consume out of housing wealth:
  - **credit card data**: Gan (2010), Agarwal and Qian (2017), Waxman et al. (2020).
  - **regional data without demographic info.**: Mian, Rao and Sufi (2013), Mian and Sufi (2014), Benmelech et al. (2017), Kaplan et al. (2020), Guren et al. (2021)
  - **geographically-linked survey data**: Aladangady (2017).
- Evaluation of house purchase restriction policies:
  - **outcomes in policy cities**: Sun et al. (2017), Qian et al. (2019).
- Housing market speculation:
  - Defusco, Nathanson and Zwick (2017), Gao, Sockin and Xiong (2018).
- House prices in the China context:
  - Wu, Gyourko, Deng (2015, 2016), Fang, Gu, Xiong, Zhou (2016).

Roadmap

- **Empirical Strategy**
  - **Background:** Spillover of house purchase restrictions (HPRs)
  - **Regression specification and data**
- **Empirical Results**
  - **Main results:** Spillover shocks $\rightarrow$ house prices (volume, search) $\rightarrow$ spending
  - **Redistribution:** Groups that are more likely to be owners (renters)
  - **Additional Discussions:** OLS bias; MPC

Background: House Purchase Restrictions

Empirical Strategy: We study the impact of plausibly exogenous spillovers, from the imposition of restrictions on housing asset purchases in certain large Chinese cities, on nearby unregulated cities.

Origin of the House Purchase Restrictions:

- During 2012-2016, house prices grow at a high speed of 14.9% annually in Tier 1 cities, but slower than 3% in Tier 3 cities.
- In September 2016 and March 2017, two rounds of policy changes named House Purchase Restrictions were implemented in all Tier 1 and many Tier 2 cities to contain surging house prices. list of regulated cities
- The policy changes targeted curbing housing market speculators and include:
  - raising down payment requirement to even higher levels for 2nd houses
  - outright forbidding the purchase of 2nd, 3rd houses by one family

The Treatment: the HPR spillover shocks

Simultaneous with the imposition of HPRs in the regulated cities, the **nearby non-regulated cities** appeared to experience a sharp increase in home sales and house prices.

- Immediately after 2016m9, house prices and transactions surge in the nearby non-regulated cities cities. ▸ example of nearby non-regulated cities ▸ volume patterns
- Out-of-town web searches from regulated cities for real estate in nearby non-regulated cities also increase. ▸ search patterns
- Nearby city governments start to cite spillover investor demand as a strong concern.

- In 2017m9, 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.

- We define the **treatment period of the HPR spillover shocks** as 2016m9 - 2017m8.
- We define the **treatment group** as the **nearby non-regulated cities**.
- We define the **control group** as the **far away non-regulated cities**.

The Treatment: the HPR spillover shocks

Treatment designation:

- We define the **treatment period of the HPR spillover shocks** as 2016m9 - 2017m8.
- We define the **treatment group** as the **nearby non-regulated cities**.
- We define the **control group** as the **far away non-regulated cities**.

More specifically:

- 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 km of a regulated city, then it belongs to the treatment group.
  - Otherwise, it belongs to the control group.
- Quintessentially, we adopt a spatially heterogeneous treatment effect strategy.
- One concern is the arbitrariness of the 250 km cutoff. Robustness checks:
  1. alternative discrete cutoffs: 300 km, 200 km, 150 km.
  2. consider railway travel time.
  3. model the treatment effect to decay continuously with distance.

Regression Model

- 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). Thus, we adopt a difference-in-differences specification that explicitly takes this into account (Wolfers, 2006):

$$\log \mathrm{Y}_{i,t} = \sum_{0 \le k \le 5} \beta_{1k} \cdot \mathit{Treat}_i \times \mathbb{I}_{t=2016m9+k} + \sum_{0 \le k \le 5} \beta_{2k} \cdot \mathit{Treat}_i \times \mathbb{I}_{t=2017m3+k}$$
$$+ \Gamma X_{i,t-1} + \sum_i \mathit{CityFE}_i + \sum_t \mathit{TimeFE}_t + \sum_i \mathit{City}_i \times \mathit{Time}_t + \epsilon_{i,t}$$

  The coefficients of interest are the averages of β_{1k}'s and β_{2k}'s, which measure the average treatment effect after the first (second) round of the HPR spillover shock. The specification imposes little structure on the response dynamics while allowing the estimated city-specific time trends to identify preexisting trends.

- The outcome variable $\mathrm{Y}_{i,t}$ can be $\mathrm{HPI}_{i,t}$, the monthly house price index in city i at time t, or Car $\mathrm{Spending}_{i,t}$, consumer spending on new automobiles in city i at time t. We also estimate effects on volumes, out-of-town searches, and rents.

Data

Our primary source of house price data is CityRE:

- CityRE HPI covers 307 cities from 2008 to 2017.
- We supplement it with Fang, Gu, Xiong and Zhou (2016) HPI, 120 cities 2003–2013.
- CityRE rent index covers 307 cities from 2008 to 2017.
- China Index Academy provides transaction volume data.
- We construct the Baidu house search index based on the Baidu search index data.

Registry-based data on automobile purchases is from CIITC:

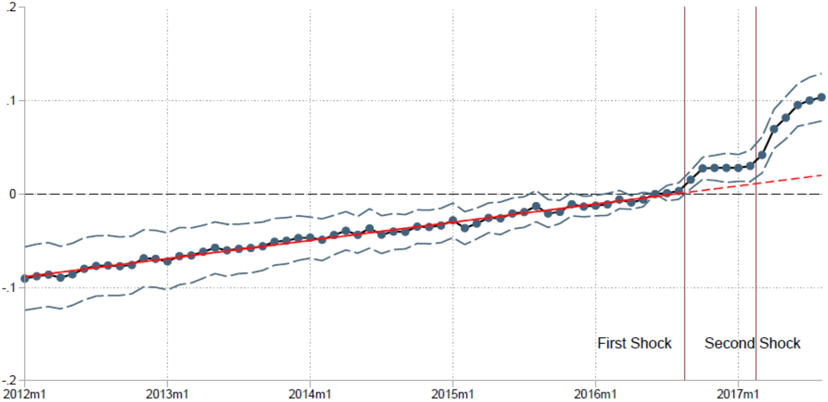
- Automobile insurance is mandatory. Each insurance registration is reported.
- We observe VIN of automobile, model and trim, license plate and birthplace of car buyers.
- We restrict baseline sample to household purchases of new passenger vehicles.
- We aggregate car spending to city-month or city-month-demographics level. aggregate trend
- We also examine a Baidu non-automobile consumption search index that covers big-ticket items such as iPhones, Nike, Estée Lauder, and Moutai, etc.

Data on city-level controls is from City Statistics Yearbook and manual collection from city statistics reports. summary statistics

We supplement the house market and the administrative spending data with survey data (CFPS, CGSS, CHFS) on birthplace, homeownership, usage of refinancing, etc.

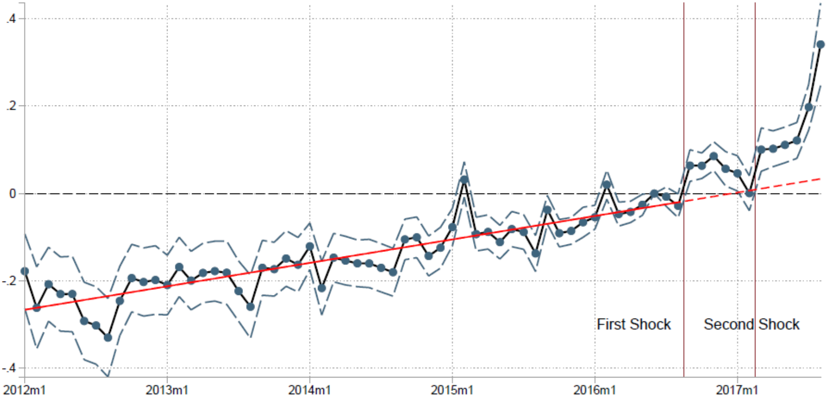
Pre-existing Trends and Dynamic Responses: House Price

We find significant quasi-experimental effect of HPR spillover shocks on house prices:



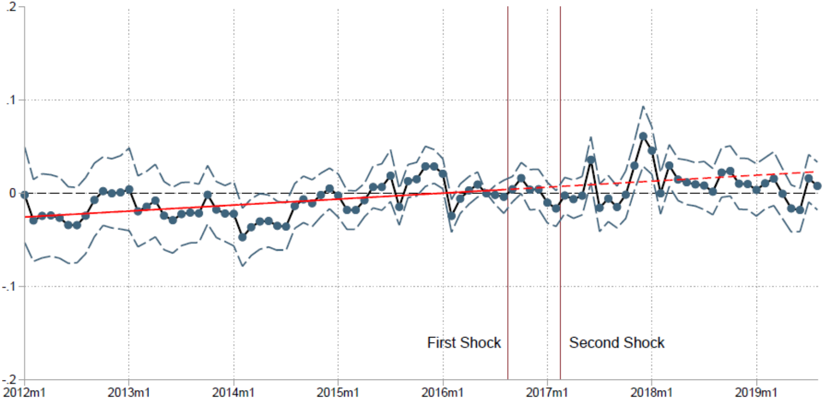
Pre-existing Trends and Dynamic Responses: Spending

We find significant quasi-experimental effect of HPR spillover shocks on car spending:



Pre-existing Trends and Dynamic Responses: Rents

We find that rents do not respond consistently differently in the treated cities than in the control group, even if we look at a longer post-event window:



Main Results: DID Effects of HPR Spillovers on House Prices and Automobile Spending

We find significant effects on: (a) house prices, volume, out-of-town search, (b) car spending (various margins and measures). We also find a sizable quasi-experimental estimate of the elasticity of spending on house prices.

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

Robustness: Model specification of pre-existing trends

Bilinski and Hatfield (2019) recommend using a "one step up" approach of perturbation, i.e. specifying a base model that includes a linear trend difference, as we did with the city-specific trends, and then check robustness to more complex trend differences, using restricted cubic splines. Results are reassuringly robust.

	(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

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

Robustness: Continuous Distance Specification

Our results are robust to allowing the effects of policy spillovers to continuously decay with distance, i.e. modeling the spatially heterogeneous treatment effects differently:

	(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) × Post1	-0.022***	0.003	-12.754	-43.947***	-0.043***	-0.075***	-0.034***	-0.022**	
	(-5.470)	(0.693)	(-1.445)	(-4.777)	(-4.245)	(-5.223)	(-3.727)	(-2.434)	
log(Distance) × Post2	-0.050***	0.008	-23.706	-58.781***	-0.071***	-0.105***	-0.086***	-0.069***	
	(-8.161)	(1.130)	(-1.479)	(-4.108)	(-5.162)	(-4.802)	(-6.592)	(-5.093)	
log(House Price)									1.391***
									(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

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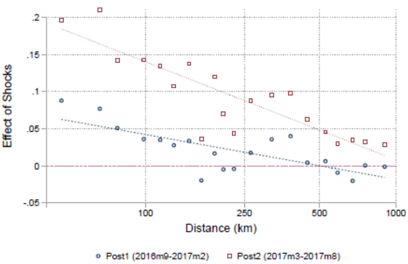
▸ Railroad travel time spec.
▸ Alternative cutoff distances

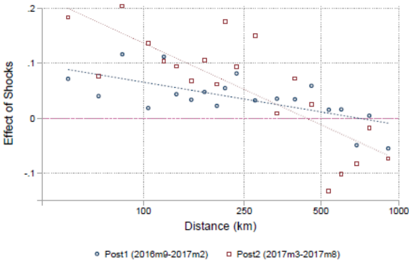
Robustness: Continuous Distance Specification

Our results are robust to allowing the effects of policy spillovers to continuously decay with distance, i.e. modeling the spatially heterogeneous treatment effects differently:

(a) log(House Price)



(b) log(Auto Consumption)



More on Spatially Heterogeneous Treatment Effects: Treatment cities nearby Tier-1 regulated cities

HPRs dampened house price growth and volume growth more for Tier-1 regulated cities. We find stronger responses to policy spillovers in treatment cities nearby Tier-1 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

	(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***	0.022
	(2.782)	(0.700)
Treat × Post2	0.053***	0.105***
	(4.348)	(3.963)
Treat × Post2 × Tier 1 City Neighbors	0.109**	0.104**
	(2.471)	(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

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

More on Spatially Heterogeneous Treatment Effects

More systematically, we find stronger responses to policy spillovers in treatment cities when the nearest regulated city, post-HPR, had stronger house price growth or volume declines.

	(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	0.024***	0.017	0.710	24.382**
× Closest Regulated City HPG Decline	(3.532)	(1.559)	(0.134)	(2.581)
Treat × Post2	0.054***	0.106***	21.615	136.113***
	(4.650)	(4.075)	(1.650)	(7.131)
Treat × Post2	0.042***	0.038**	-7.287	-10.028
×Closest Regulated City HPG Decline	(4.206)	(2.580)	(-0.899)	(-0.729)
Observations	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	0.010**	0.029***	4.286	20.728**
× Closest Regulated City Volume Decline	(2.023)	(2.977)	(1.286)	(2.435)
Treat × Post2	0.058***	0.112***	12.703	134.494***
	(4.697)	(4.204)	(1.060)	(6.819)
Treat × Post2	0.020**	0.037***	5.451	-12.426
× Closest Regulated City Volume Decline	(2.281)	(2.663)	(0.902)	(-0.965)
Observations	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

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

Robustness: Matching Specification

Our results are robust to matching treatment cities to control cities with similar level of economic development, but only differ in distance:

	(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
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	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

No Evidence of Improvement in Local Housing Fundamentals

We see no change in total output, industrial output growth, output growth, employment growth, population after the policy spillover shocks. An increase in bank deposits in the treated cities would be consistent with out-of-town investors inject funds into the treated cities when purchasing homes from the locals:

	(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

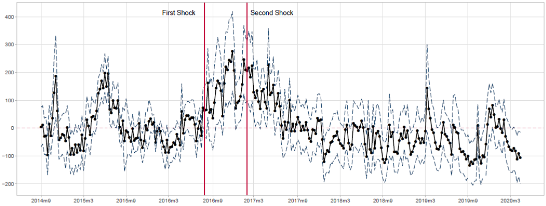
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Baidu non-car spending index

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).

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



Spending Response "Only" for Locals

After the HPR policy spillover shocks, we observe much larger increase in spending on automobiles **for the local-born consumers**, who are more likely homeowners and less likely renters:

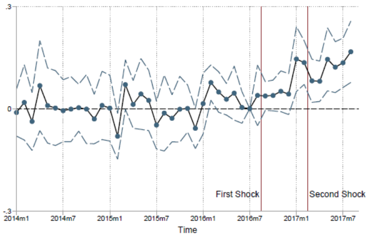
	(1) Home Ownership	(2) log(Auto Spending)	(3) log(Auto Purchases)	(4) log(Auto Spending)	(5) log(Auto Purchases)
Constant	0.812 ***				
	(58.675)				
Born in Current City	0.148***				
	(4.219)				
Treat × Post1		-0.032	-0.040*		
		(-1.385)	(-1.768)		
Treat × Post1		0.145***	0.130***		
× Born in Current 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
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

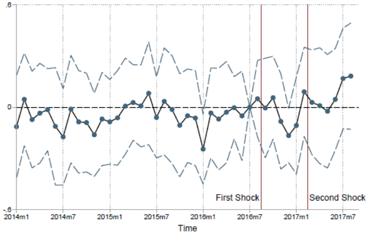
Spending Response "Only" for Locals

We observe no pre-existing trend differences across local-born and non-local-born groups within cities:

(a) Within Treated Cities



(b) Within Control Cities



Spending Response "Only" for Locals

Computing predicted share of renters and homeowners using survey data based on birthplace status, results suggest **significantly negative spending responses for renters to house prices, positive for 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)
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Time FE		YES	YES	YES	YES

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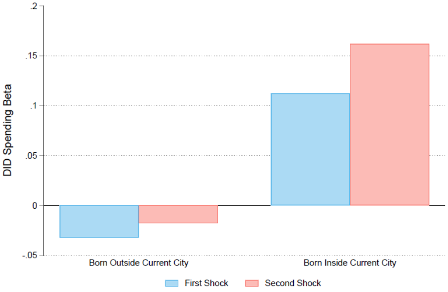


The "Pure" Wealth Effect

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, including *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, homes are important investment vehicles in China (Cao, Chen, and Zhang, 2018). We estimate that the survey multi-home ownership rate is around 18.0%, with only minor regional variations, which would increase average spending response for homeowners as a whole.
- 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.
- Aggregate-wise, a non-fundamental increase in house prices in the "pure" housing wealth effect generates a positive aggregate spending response.

Alternative explanations

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 (CHFS 2015 and 2017), we also observe a low fraction of households in our economic setting that have had refinanced mortgage debt or had HELOCs (2.2% of all homeowners), and an especially low fraction of households that use home-equity based borrowing for consumption spending (0.01% of all homeowners).
- 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%).

Additional Discussion of Results

Negative Bias in the OLS under Investment Demand

- Converting our elasticity estimates, the **marginal propensity to consume (MPC)** is 0.048:

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

- According to an OLS estimation of the elasticity, the MPC is 0.016.
- The finding of a negative bias in the OLS estimation of the MPC may be counterintuitive:
  - Conventional wisdom suggests productivity drives positive comovement.
- **Investment demand in the housing market may create a negative bias in OLS.**
  - House prices departs from permanent income $\Rightarrow$ canonical permanent income channel weakens.
  - The propensity to save using houses as investment vehicles forms an omitted variable (OV).
  - This OV positively correlates with house prices (X) and negatively correlates with spending (Y).
  - This OV produces negative comovement between Y and X.
- This bias is likely more important when the role of investment demand in house prices is more important.

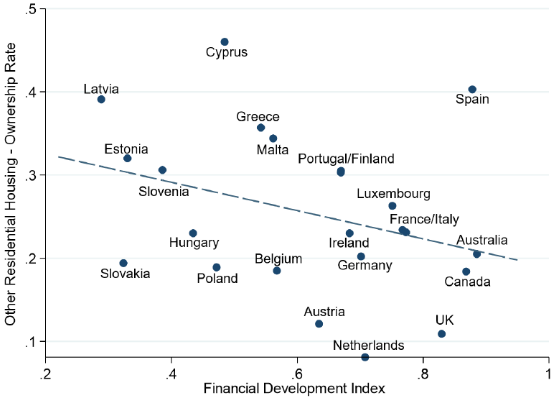
MPC Comparison and External Validity

- The point estimates of the automobile MPC out of house prices is larger than in Mian, Rao, and Sufi (2013) and Aladangady (2017):
  - A one-sided statistical test on that the baseline automobile MPC in the current analysis being larger than the baseline automobile MPC (0.018, s.e. of 0.001) 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%.
  - Across various robustness specifications, we did not obtain a point estimate that is lower than the Mian, Rao, and Sufi (2013) estimate for the United States (our lowest estimate being 0.023).
- Investment demand could be the culprit. It is prevalent in the Chinese housing market (Cao, Chen, and Zhang, 2018). According to theory it drives up the housing wealth effect (Buiter, 2008).

MPC Comparison and External Validity

- While what we find may not apply to all places, many countries have high ownership rates of investment real estate assets, including many European countries.

Figure 11: Cross Country Comparison of Multi-Property Ownership Rate



- Badarinza, Balasubramaniam, and Ramadorai (2016) additionally show that housing and land are the most important investment vehicles in India, even more so than in China.

Economic Significance of the Overall Spending Response

We estimate HPR spillovers to have a strong macroeconomic effect:

- **HPR spillover explains 12% to 25% of the average annual increase in private passenger automobile sales in 2016 and 2017.**
  - Across the baseline and the various robustness specifications, we find a baseline casual increase of 60.9 billion RMB and a lowest causal increase of 29.6 billion RMB (approximately 4.5 to 9.3 billion USD) in consumer spending on new automobiles.
  - According to the China Association of Automobile Manufacturers, the average annual increase in automobile sales during 2016 and 2017, the two years covering our event window, is 245.6 billion RMB (approximately 37.8 billion USD).
  - A back-of-envelope calculation gives the 12% to 25% range.
- As the local-born v.s. non-local-born analysis indicates, we also find spending redistributions that are the same order of magnitude as the overall increase.

Conclusions

- In this paper, we:
  - study the effects of capital flight to non-regulated cities, caused by spillovers from the local imposition of HPR in regulated cities.
  - find capital flight from regulated cities to drive **large and plausibly exogenous house price surges** in affected non-regulated cities.
  - ... that leads to **substantial causal increase in household spending on autos**.
  - spending responses are **highly heterogeneous** across household types.
  - **investment demand drives a negative OVB** in the OLS.
  - estimated magnitude of the spending effect is large in the aggregate.
- Future Directions:
  - Further investigation of household consumption and spending behavior in response to other important shocks: Stimulus, etc.

Thank you!

Background: House Purchase Restrictions

Table 2: First Round of House Purchase Restrictions

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

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Background: House Purchase Restrictions

Table 3: Second Round of House Purchase Restrictions

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

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Example: HPR Spillovers

Three pairs of cities illustrates the effect of policy spillover shocks on regulated (first) and neighboring (second) cities: Beijing–Tangshan, Hefei–Bengbu, and Wuhan–Xiangyang.

Figure 2: Locations of Three Pairs of Chinese Cities

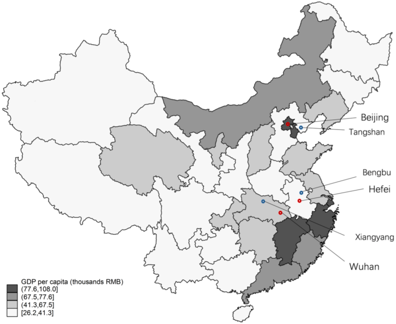
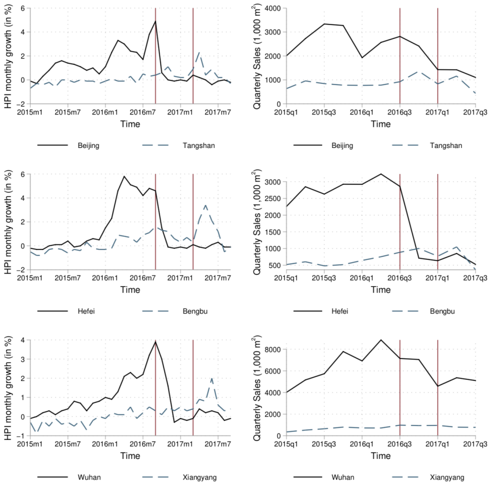


Figure 3: Reactions of House Prices and House Sales to the Restrictions - Some Examples



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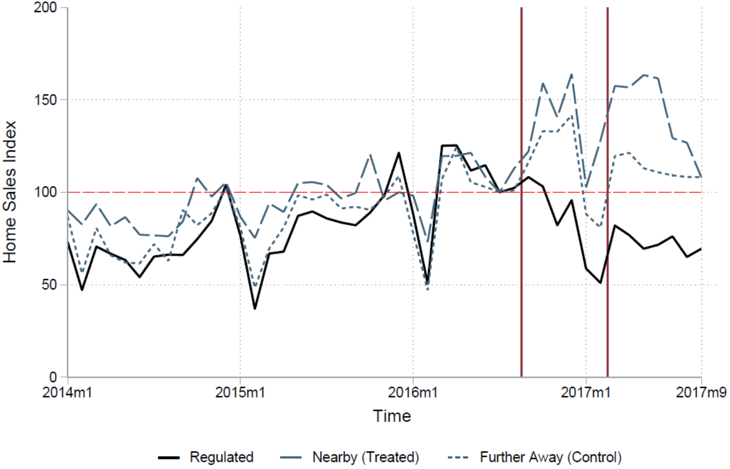
Example: Transaction Volumes Wanes and Waxes

We also find motivating evidence that reductions in volumes in the regulated cities are consistent with the increase in volumes in the nearby non-regulated cities.



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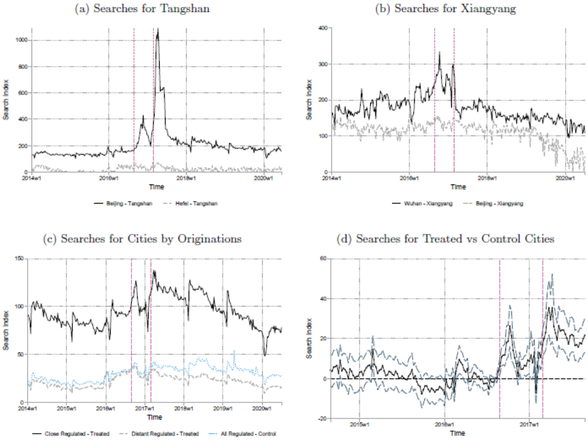
Example: Additional Patterns in Out-of-Town Searches

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.

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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

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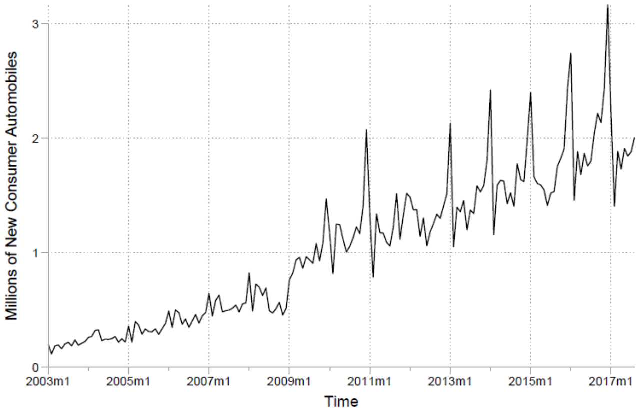
Aggregate Trend of CIITC Data

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.

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Robustness: Railroad Travel Time

Our results are robust to defining the treatment group as cities with 2 hours or less railroad travel time to any of the regulated cities:

	(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

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

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Robustness: Alternative Distance Cutoffs

Our results are robust to using alternative distance cutoffs instead of 250 km in the baseline estimation:

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

back