The Journal of Real Estate Finance and Economics
https://doi.org/10.1007/s11146-024-09983-2

The Effects of Capital Controls on Housing Prices

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Accepted: 21 February 2024

Abstract
Policymakers increasingly use capital control policies (i.e., capital flow management) to manage capital flows. However, whether the implementation of such policies can effectively affect housing prices and to what extent is less discussed. In this paper, I study the effects of four types of granular capital control policies on housing prices using a large cross-country panel of 53 economies from 1995 to 2017. I find that the estimated effects of capital controls are distinct for different capital flow types and flow directions, but all capital control inflow indices appear to reduce housing prices in the long-run. Additionally, I find that capital controls have asymmetric effects on housing prices for advanced economies and emerging markets. The negative effects of capital controls on housing prices are mainly driven by pre-crisis subsample, which means capital controls have been in effect several times before the Global Financial Crisis. I also estimate the effects for boom and slump periods respectively and find that capital control policies are implemented in an acyclical way. Since there exists endogeneity for capital control on real estate transactions, I further use IPWRA method to rebalance capital control actions and find that IPWRA estimators can weaken the negative effects on housing prices, and the attenuation effects can be attributed to endogenous factors.

Keywords Capital control policy · Housing price · Local projections · Inverse probability weighted regression adjusted estimator

JEL Classification F21 · F32 · F38 · F41 · G28

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Published online: 04 April 2024

Introduction

The risk of sudden capital flow reversals, commonly referred to as "sudden stops", poses a significant challenge for emerging market economies (Batini, 2020; OECD, 2020; ElFayoumi & Hengge, 2021). These abrupt shifts in capital flow can expose these economies to heightened volatility and financial instability. One key arena strongly affected by the impact is the domestic housing market. As presented by Everaert (2020), while the local factors still explain most of the volatility of housing price, the global factors appear to influence the housing price after the 2008 Global financial crisis (GFC). Particularly, as I have depicted in the "Literature Review", most researchers reach consensus towards the causal relationship between capital inflows and housing price appreciation.[1] Although the capital inflows are welcomed in EMs for contributing to their economic development, they are also disputed by their amplification of economic cycles, increasing of financial system vulnerabilities, and deterioration of overall macroeconomic instability (Forbes & Warnock, 2012). In addition, housing price is also a critical factor in "financial accelerator" mechanism. Suffered by external shocks, the capital flight would tighten the financial conditions and then amplify the crisis by decreasing of collateral value and restricting further the borrowing capacity of households and firms (Kiyotaki & Moore, 1997; Bruno & Shin, 2014; Cesa-Bianchi et al., 2018). As Schularick & Taylor (2012) shows, abnormal credit and housing price growth are the two main early warning indicators of financial crises. Therefore, policies aimed to reduce credit and housing price growth are effective for maintaining financial stability and reducing the probability of a financial crisis.

There are several policy tools in mitigating the volatility of housing prices, such as the monetary and macroprudential policies. Particularly, macroprudential policies are proved to be effective in lowering bank credit growth and housing price appreciations (Akinci and Olmstead-Rumsey, 2018; Kuttner & Shim, 2016). Besides, there are also measures to stabilize housing prices by indirectly affecting the global factor (i.e., capital flows), namely the capital control policies. Capital control policies are opposed and undesirable by IMF for a long time before 2011, whereas policymakers have gradually realized that capital flow management can address the negative effects of volatile capital flows (Forbes et al., 2015). Indeed, Everaert (2020) documents that five advanced economies (Australia, Canada, Hong Kong SAR, New Zealand, and Singapore) have restricted foreigners to invest in domestic real estate after 2011. China, India, Indonesia, and Switzerland even outright prohibit portfolio investment in real estate. Compared with the literature of monetary and macroprudential policies, however, there is very little empirical evidence on the effects of capital control on housing prices.

[1] As for the theoretical literature on capital flows and housing prices, Kim & Yang (2011) raise three channels: direct channel, liquidity channel (capital flows result in money supply and then boost asset prices), indirect channel (capital flows result in economic boom and then lead to increasing of asset price). Bruno & Shin (2014) use an exchange rate channel to explain the bank capital flows and financial stability. In addition, combined with collateralized borrowing and international financial intermediation, Cesa-Bianchi et al. (2018) study the mechanism of international credit supply to the boom in asset prices.

In this paper, I explore the effects of capital control policies (restrictions on direct investment, hereafter "di"; financial credit, hereafter "fc"; commercial credit, hereafter "cc"; and real estate transaction, hereafter "re") on housing prices for a large cross-country panel of 53 economies from 1995 to 2017. Since the decision to implement capital control policy is taken contingent on countries' economic conditions, in order to measure the effects correctly and precisely, I follow Richter et al. (2019) and propose three rules that capital control policy actions should be satisfied: (i) the capital control policy actions should be exogenous with regarding to current and lagged financial variables, such that it would be sufficient to calculate the average treatment effect (ATE) for restricted one and that of unrestricted. (ii) the capital control policy actions should be uncorrelated with other shocks, such as the monetary policy, or macroprudential policy shocks. (iii) the capital control policy actions should not be anticipated. Similar to macroprudential policies, the unsystematic nature of capital controls means they are typically unexpected (Richter et al., 2019). The second rule can be addressed by including monetary policy and macroprudential policy variables in the regression equations. To verify if capital control policy actions are exogenous to housing prices, I check firstly the objectives of capital control by previous literature, and then use the balance condition test proposed by Jordà & Taylor (2016), and confirm that the capital control on real estate transactions are endogenous (except capital controls on real estate inflow), while other capital control policies (except capital controls on financial credit inflow) are exogenous to housing prices.

After identifying the endogeneity of capital control variables, I calculate the impact of capital control using local projections method developed by Jordà (2005). Results show that most capital control indices I have analyzed in this paper appear to reduce real housing prices. Specifically, capital control inflow measures can reduce housing prices and the results are statistically significant in the long-run. As for capital control outflow measures, although all of them show that they can reduce housing prices, most of them are insignificant except capital control on commercial credit outflow (hereafter, "cco"), real estate outflow (purchase abroad by resident, hereafter "re_pabr"), and real estate outflow (sale locally by nonresident, hereafter "re_slbn"). Besides, I find that capital controls have asymmetric effects on housing prices for Advanced Economies (AEs) and Emerging markets (EMs). As for EMs, capital controls on all types of capital control inflow measures can reduce housing prices, while for AEs, only capital control on commercial credit inflows (hereafter, "cci") and financial credit inflow (hereafter, "fci") can reduce housing prices.

I provide a series of robustness exercises and find that my specification are broadly robust when I substitute the control variables with alternative proxies, expand the prediction horizons to 10 years, and consider the correlation and sample issues of capital control indices. I also offer the estimation using the up-to-date Generalized Synthetic Control Method, and reach a consensus on the baseline results. In addition, I estimate the effects in pre- and post-crisis subsamples. Results suggest that the negative effects of housing prices are mainly driven by pre-crisis subsample from capital control on financial credit inflow ("fci") and commercial credit inflow ("cci"). I also estimate the effects for boom and slump periods respectively and find that although capital controls appear to be acyclical, the inflow controls on direct investment ("dii") and financial credit ("fci") are implemented in a countercyclical manner to some extent.

Since there exists endogenous problem for capital controls on real estate transaction ("re"), I address this issue using inverse probability weighted regression adjusted (IPWRA) estimator. As depicted by Jordà & Taylor (2016) , I first rebalance the sample of "implementing capital controls" (treatment group) and "not implementing capital controls" (control group) by putting more weight to the capital controls that are implemented as surprises and allocating lower weight on capital controls that are implemented endogenously. Then, I apply local projections to the rebalanced sample and obtain the IPWRA estimators. Results show that the negative effects for all capital controls on real estate transaction are weakened after using IPWRA estimators.The attenuation effects of the IPWRA estimators are stronger in the long-term and thus I can attribute much of the long-term real housing price variation to endogenous factors. In addition, as the housing demand proxy variables, I also estimate the response of two credit variables ("bank credit" and "credit to households") to capital control on real estate transactions. The effects of capital control on real estate transaction inflow ("rei") and outflow ("re_slbn") can reduce the volume for both credits, but the effects of real estate transaction inflow ("rei") are relatively insignificant. However, the response to real estate transaction outflow ("re_pabr") is positive for all horizons. This may relate to the fact that preventing the domestic investors from investing in foreign housing market makes them have no choice but invest in the domestic housing market.

This paper is structured as follows: "Literature Review" reviews the empirical literature related to capital flows, capital controls and housing prices. "Data and Identification Strategy" describes the data and identification strategies used in the estimation. "The Effects of Implementing Capital Control Policies" presents the methodology and empirical results towards the response of real housing prices to the implementation of capital controls. "Robustness and Sensitivity Analysis" presents a series of robustness exercises. In "Endogeneity Problem Revisiting", I further consider the endogeneity problem and estimate the response of financial variables to capital controls on real estate transaction by IPWRA method. "Conclusion" summarizes the main conclusions.

Literature Review

This paper relates to two strands of literature, the first relates to the impact of capital flow on housing prices, while the second associates with the effectiveness of capital control policies on capital flows, and financial cycles.

In the aftermath of GFC, much literature has focused on the impact of capital flows on asset prices, especially the housing prices. Most researchers reach a consensus on the relationship between the current account deficits (or capital inflows) and real housing price appreciation. Although these studies reach similar results, as pointed out by Cheung et al. (2017), different types of capital flows have different impact on assets pricing. For example, Aizenman & Jinjarak (2009), Gete-Sanchez (2015), Laibson & Mollerstrom (2010), Adam et al. (2012), and Sá et al. (2014) use current account (deficits) as the proxy variable of capital flow, while others prefer capital flows extracted from financial account or more specific indicator “global liquidity”.[2] Chow & Xie (2016) and Feng et al. (2017) analyze only the impact of FDI for Singapore and China respectively. Feng et al. (2017) argue that hot money net inflow shock and FDI net inflow shock significantly increase housing prices, while FDI net inflow shock has no effect on stock prices. Kim & Yang (2011) analyze five Asian countries using only portfolio inflow, and they find although capital inflows contribute to asset price appreciation, they explain a small part of price fluctuations. In addition, Hernandez-Vega (2022), Kim & Yang (2009) analyze the impact of both FDI and portfolio investment. Hernandez-Vega (2022) finds that both FDI and portfolio flows contribute to higher housing prices, yet only portfolio flows have more persistent effects. Besides, Tillmann (2013), Olaberria et al. (2012), and Baba & Sevil (2020) use three types of capital flow: direct investment, portfolio investment, and other investment. Olaberria et al. (2012) find that debt related inflows are more associated with booms in assets prices. Unlike the aforementioned studies, Pavlov & Somerville (2020) find that wealthy immigrants can raise neighborhood house prices. This also sheds light on the discussion of the effects of foreign capital inflows on local residential real estate markets, since the wealthy immigration is associated with capital inflows.

As for the indicator of “global liquidity”, Belke et al. (2010) define it as a broad monetary aggregate and they find that high money growth rates have coincided with a rise in asset prices, while stock prices do not present any positive response. Darius & Radde (2010) use the summation of U.S. monetary base and world international reverses as global liquidity and estimate a VAR model for G7, they find a similar result as Belke et al. (2010) which global liquidity had significant impacts on the buildup of housing prices, but its effects were limited on equity price. Using similar global liquidity indicator, however, Brana et al. (2012) estimate a Panel-VAR for 16 emerging countries, and they find that the relationship between global liquidity shocks and share prices or estate prices is weaker. Compared to the “official global liquidity” mentioned above, Cesa-Bianchi et al. (2015) and Romero et al. (2020) prefer the “private global liquidity” defined by Matsumoto (2011) which is related to the availability for risky assets such as real estate or equity. Both of them find that in emerging markets (or financially less developed countries), global liquidity shock has much stronger impact in explaining the historical dynamics of housing prices. As discussed by Romero et al. (2020), more developed countries have alternative investment opportunities such that housing prices are less sensitive to shocks from global liquidity. As a supplement for the discussion above, Sá et al. (2014) focus on the development of mortgage markets rather than overall financial development, and they find that the positive responses of capital inflow shocks on real housing prices are stronger in countries with developed mortgage markets, since households are highly indebted and they are more sensitive to changes of collateral value in advanced mortgage markets.

There still exist a few contributions reporting opposed conclusions. Kim & Yang (2009) find that capital inflow shocks lead to the stock price increasing in Korea, but

[2] As for the relationship between current account and capital flows, Borio & Disyatat (2015) point out that even if sometimes the terms "current account" and "capital flows" are used interchangeably, they believe that current account should be complemented by gross flows and gross positions in order to fully analyze financial stability risk, since in an environment of massive corss border flows, financial imbalance becomes more important source of macroeconomic dislocations.

the influence is limited in housing prices. Brana et al. (2012) find that the relationship between global liquidity and stock prices or housing prices is weaker than GDP and CPI for emerging countries. Favilukis et al. (2012) find that in both boom and bust periods, capital flows have little explanatory power for residential real estate fluctuations.

Many studies have examined the effectiveness of capital control measures on stemming capital flows, but findings are mixed. Ahmed & Zlate (2014) and Landi & Schiavone (2021) admit the generally effectiveness of capital controls to discourage capital inflows. However, it should be noted further that the effects of capital controls vary across the types of capital controls, both assets categories, flows directions, and countries' income levels (Binici et al., 2010). Dell'Erba & Reinhardt (2015) find an opposite effect that controls on short-term debt flows would decrease the possibility of surges in banking debt flows whereas increase the possibility of surges in financial sector FDI. Beirne & Friedrich (2017) show that higher regulatory quality and higher credit-to-deposit ratio increase the effectiveness of capital control policies. Binici et al. (2010) find that both debt and equity controls can reduce outflows significantly, but the effects of inflows are weak, and only advanced countries can effectively implement outflow controls, whereas Bruno et al. (2017) argue that banking sector and bond market capital flow management policies are effective in reducing the banking inflow growth before 2007 and bond inflow before 2009, respectively.

There are also a few contributions comparing the effectiveness between macroprudential and capital control policies. Ostry et al. (2012) develop new indices of *de jure* measures for 51 emerging economies over the period of 1995 to 2008, and they find that both capital controls and FX-based prudential measures are related with lower portion of FX lending in domestic bank credit, and also for portfolio debt in external liabilities. Similarly, Osina (2021) agrees with the facts that both capital controls and macroprudential policies are effective in reducing the volume of cross-border bank flows, while macroprudential policies should be used as first priority since they can optimally manage capital flows without discriminating foreign investors. Conversely, Forbes et al. (2015) show that macroprudential policies can reduce significantly several types of financial fragility, while most capital flow managements have limited influence on their objectives, such as reducing capital inflows. Frost et al. (2020) also support that macroprudential policies may be more effective in responding to volatile capital inflows than capital controls, and they show insignificant effects of capital controls on the quantity of capital inflows. Giraldo et al. (2023) evaluate the effect of a special type macroprudential policy that was firstly implemented in Colombia in May 2007, the PBA, which limits the ability of banks to issue loans denominated in foreign currency using synthetic control methods. They show that while this policy was costly in financial stability terms in the pre-GFC period, it was effective in reducing Colombia's financial stability risks during the crisis. Besides, Baba & Kokenyne (2011), and Forbes & Warnock (2012) both find the insignificance effect of capital controls on capital inflows. Baba & Kokenyne (2011) also find that outflow control liberalization could not dampen currency appreciation. Cerutti et al. (2014) find the dampening effects of capital control on cross-border bank claims, but to a lesser extent.

Although, as argued above, there exist many studies on the relationship between capital inflows and housing price appreciation, there are very few studies on how the implementing of capital control policies affect housing prices. Ohno & Shimizu

(2015) analyze the impact on Asian housing market and highlight the relationship between housing price and financial market openness for 7 economies over 1998 to 2010. They find that housing prices rise more rapidly with more open financial markets. Banti & Phylaktis (2019) also study the impact of global liquidity on house prices for 48 countries between 2000Q1 to 2014Q4 using a PVAR framework. They find that in emerging markets, housing prices are affected positively and significantly by global liquidity only when capital controls on real estate transaction are looser. Pavlov et al. (2023) investigate the extent to which the effects of foreign buyer tax in British Columbia, Canada on local house prices. They find that house prices decline by 6% in neighborhoods with above median concentrations of foreign buyers after the tax relative to prices in neighborhoods with below median concentrations of foreign buyers.

This study extends the literature in two ways. As for data, Ohno & Shimizu (2015) only use general indices on capital controls (e.g., "KAOPEN"), which are not specific to restrictions on real estate transactions. Banti & Phylaktis (2019) only use the aggregated index of capital control on real estate transactions. Unlike the two papers, I distinguish between capital inflow and outflow and also analyze various capital (market) account transactions (i.e., capital control on direct investment, financial credit, commercial credit, and real estate) that can affect housing prices. It matters because policymakers often regulate different capital accounts separately rather than the entire capital account as a whole, and relying solely on a general index would overlook the heterogeneous effects of these controls; furthermore, ignoring the different natures of inflows and outflows could introduce significant bias in estimating the impact of capital controls on housing prices. As for methodology, unlike Ohno & Shimizu (2015)'s one-way panel data regression and Banti & Phylaktis (2019)'s panel SVAR, I estimate the impulse response using the "misspecification robust" local projection (LP) method and the robustness of these results is further confirmed by the up-to-date Generalized Synthetic Control Method. Moreover, unlike the methods used in previous research placed more emphasis on correlation rather than on causal relationships, I explicitly point out and diagnose the presence of endogeneity (the "selection bias") of capital controls to financial indicators and employ the "inverse probability weighted regression-adjusted (IPWRA) estimator" to mitigate endogeneity by rebalancing the sample.

Data and Identification Strategy

Data Description

I estimate the model using unbalanced panel data with 53 economies (for 31 advanced economies and 22 emerging markets) and yearly basis from 1995 to 2017. Dependent variables contain housing price, and two types of bank credit: credit to private non-financial sector from banks (hereafter "bank credit"), and credit to households and NPISHs from all sectors (hereafter "credit to households"). Explanatory variables include the capital control measures on two types of credit (financial credit and commercial credit), direct investment, and real estate transactions. Control variables are monetary policy (central bank policy rate and short term interest rate), exchange rate (bilateral nominal exchange rate and real efficient exchange rate), macroeconomic fundamentals (GDP and CPI), macroprudential policy (i.e., LTV caps), capital flows (inflows and outflows of direct, portfolio, and other investment), cross border loans (cross border loans from BIS reporting banks and cross border loans from BIS banks to non-banks), and global liquidity (VIX and TED spread). Table 8 presents the detailed information of source and full description of the data sample, and Table 1 reports summary statistics for key variables used in this paper.

Dependent Variables

According to Akinci and Olmstead-Rumsey (2018) and Richter et al. (2019), I choose housing price appreciation in "The Effects of Implementing Capital Control Policies" and "Endogeneity Problem Revisiting", bank credit growth, and households credit growth in "Endogeneity Problem Revisiting" as dependent variables since these variables are usually used as the objective of macroprudential policy. Besides, these variables are closely related with boom-bust financial cycle.

Housing price data

The housing price data used in this paper rely on Bank for International Settlements (BIS) residential property prices. The data series are annual basis (2010 as the base year for price index equal to 100), and have been adjusted to real housing prices by CPI. I collect data over 1995 to 2017 subject to the data availability. Housing price data cover 51 countries, including 31 advanced economies and 20 emerging markets.[3] Since the BIS dataset used in the analysis is an unbalanced panel dataset, I have supplemented the range of data (for all dependent variables) for each country in Table 7.

Because of the short period and limited coverage of emerging markets, Cesa-Bianchi et al. (2015) extend existing indices by extrapolating with historical data, and increase the coverage for emerging markets. Banti & Phylaktis (2019) also supplement the BIS dataset with other sources for longer time series. However, as pointed out by Hernandez-Vega (2022), collecting data from different sources may generate comparability and compatibility problems since the data are usually compiled in different ways. The BIS's "Selected residential property price series – data documentation" on the BIS homepage argues that "*[t]o facilitate cross-country comparison, the BIS additionally publishes the selected series data set, ... As a result, the selected residential property price data set is as homogeneous as possible despite of the prevailing discrepancies in sources and compilation methods*" (page 1/8). Therefore, we employ the BIS selected residential property price dataset, which is suitable for cross-country comparison.

[3] The 31 advanced economies are Australia, Austria, Belgium, Canada, Cyprus, Czech Republic, Germany, Denmark, Spain, Finland, France, Greece, Hong Kong SAR, Ireland, Iceland, Israel, Italy, Japan, Korea, Latvia, Malta, Netherlands, Norway, New Zealand, Portugal, Singapore, Slovenia, Sweden, Switzerland, United Kingdom, and United States. The 20 emerging markets contain Bulgaria, Brazil, Chile, China, Colombia, Hungary, Indonesia, India, Morocco, Mexico, Malaysia, Peru, Philippines, Poland, Romania, Russia, South Africa, Thailand, Turkey, and United Arab Emirates.

Table 1 Descriptive statistics

	Obs	Mean	Std. Dev	Min	Max
Real housing price growth	871	2.030	7.480	-50.456	29.954
Real housing price detrended	922	0.000	0.078	-0.324	0.369
Real bank credit growth	924	4.908	9.123	-95.900	34.188
Real bank credit detrended	966	0.000	0.090	-0.525	0.502
Real credit to households growth	846	6.552	11.298	-76.139	71.305
Real credit to households detrended	888	0.000	0.104	-0.744	0.487
Real GDP growth	1166	3.032	3.276	-15.550	22.923
CPI growth	1165	4.248	9.968	-4.581	244.960
Real cross border loan growth	1166	3.657	18.323	-85.492	85.603
Real cross border loan to nonbank growth	1166	4.559	18.146	-77.112	106.508
Direct investment (inflow) to GDP ratio	1159	6.121	24.709	-45.659	387.288
Portfolio investment (inflow) to GDP ratio	1156	3.103	10.760	-50.404	148.623
Other investment (inflow) to GDP ratio	1157	3.410	17.312	-80.408	256.809
Direct investment (outflow) to GDP ratio	1156	4.827	22.979	-83.988	331.705
Portfolio investment (outflow) to GDP ratio	1153	4.289	15.668	-74.253	195.876
Other investment (outflow) to GDP ratio	1158	2.924	14.123	-87.912	214.233
Policy rates	1076	5.150	8.727	-0.750	160.000
Short-term interest rates	1059	5.309	8.085	-0.819	98.395
Exchange rate growth	1166	2.696	13.725	-33.175	224.651
REER growth	1166	0.104	6.887	-79.095	35.569
VIX	1219	19.950	6.099	11.090	32.693
TED spread	1219	48.759	31.042	19.217	154.802

Notes: This table summarizes key variables with respect to their mean, standard deviation, minimum, and maximum. Real housing price growth, real bank credit growth, real credit to households growth, real GDP growth, CPI growth, real cross border loan growth, real cross border loan to nonbank growth, direct investment (inflow/outflow) to GDP ratio, portfolio investment (inflow/outflow) to GDP ratio, other investment (inflow/outflow) to GDP ratio, exchange rate growth, REER growth, and TED spread are expressed in growth rates in percentage terms. Policy rate and short term interest rate are expressed in percentage terms

Credit data

I also choose credit as the other dependent variables for measuring financial cycles. Besides, as noted by Hernandez-Vega (2022), since the not availability of housing demand variable such as residential investment for most of the emerging markets, I use credit data as the proxy variables for housing demand. In this paper, I use two types of credit data: "bank credit" and "credit to households". I employ these indicators from BIS Statistics for bank credit to the non-financial sector, and total credit to households. These data are in domestic currency, and adjusted to real term using CPI. The time period is from 1995 to 2017 for 42 economies.[4]

Explanatory Variables

The capital control measures used in this chapter are compiled by Fernández et al. (2016) which are based on *de jure* measures from Schindler (2009).[5] Although there are datasets developed by Quinn (1997) or Chinn & Ito (2008) for broad coverage or longer time period, these datasets are broad indices of "capital account openness" instead of granular data which are not only divided up into inflows and outflows, but also disaggregated by different categories of assets.[6] Fernández et al. (2016)'s dataset is a desirable one which provides more granularity by distinguishing the direction and category of capital flows and it also covers 100 economies over 1995 to 2017. I choose 53 countries according to the availability of dependent variables.

To measure the effects of capital controls on housing price, I choose four types of categories closely associated with it:

1. Capital controls on direct investment: this category contains direct investment control on inflows ("dii"), and direct investment control on outflows ("dio"). I choose this category since it relates with long lasting economic relation, and the empirical literature shows that FDI shocks have positive effects on housing prices or growth rate (Hernandez-Vega, 2022; Feng et al., 2017; Chow & Xie, 2016; Kim & Yang, 2011). Thus, it is necessary to study the effects of direct investment capital controls on housing prices.
2. Capital controls on credit:

   (a) Capital controls on commercial credit: this contains capital control on commercial credit inflows ("cci"), and outflows ("cco"). Following the definition of Schindler (2009) and Fernández et al. (2016), commercial credits are directly

[4] The 42 economies contain 26 AEs (Australia, Austria, Belgium, Canada, Czech Republic, Germany, Denmark, Spain, Finland, France, Greece, Hong Kong SAR, Ireland, Israel, Italy, Japan, Korea, Netherlands, Norway, New Zealand, Portugal, Singapore, Sweden, Switzerland, United Kingdom, United States) and 16 EMs (Argentina, Brazil, Chile, China, Colombia, Hungary, Indonesia, India, Mexico, Malaysia, Poland, Russia, Saudi Arabia, South Africa, Thailand, Turkey).

[5] Schindler (2009)'s dataset has 91 economies from 1995 to 2005, and he also divides the capital control indices up into inflows and outflows for six different categories.

[6] The dataset of Quinn (1997) uses five point scale at the granular level to assess the intensity on capital flows, but his dataset does not distinguish capital controls on inflow or outflow. The latest dataset of Chinn & Ito (2008) contains data for 182 countries from 1970 to 2018, but this dataset does not contain any granular data on specific assets.

related with international trade transactions or with the rendering of international service.

(b) Capital controls on financial credit: this also includes capital control on inflows ("fci"), and on outflows ("fco"). Different from capital controls on commercial credit, this indicator contains credit other than commercial credit granted by residents (including banks) to non-residents or vice versa.

Cesa-Bianchi et al. (2015) focus on a particular component of capital flows, namely the cross border bank lending (to the domestic bank sector) to study the impact of capital flows on housing prices. Inspired by them, I choose these two types of capital control policies on credit as proxy variables of capital controls on cross border bank lending.

3. Capital controls on real estate transactions: these series of indices are the most direct indicators related with housing prices. They contain three categories which can be written as

$$\text{re:}\begin{cases}\text{inflow:} & \text{rei} = \text{re\_plbn: real estate purchase locally by non-residents}\\ \text{outflow:} & \text{reo:}\begin{cases}\text{re\_pabr:} & \text{real estate purchase abroad by residents}\\ \text{re\_slbn:} & \text{real estate sale locally by non-residents}\end{cases}\end{cases}$$

As defined by Fernández et al. (2016), these indices only restrict the acquisition of real estate not associated with direct investment, namely, the investment of purely financial objectives in real estate or acquisition of real estate for personal use.

These four indicators are all 0-1 binary variables with 1 representing the implementing of such capital control restriction, and 0 for no such restriction. Fernández et al. (2016) also provide aggregated data which are calculated by the average of the inflow and outflow indicators. These aggregated indicators can be used as intensity measure to some extent, while I do not use them for estimation in this paper. As argued by Binici et al. (2010), the aggregated measures may generate misleading and biased estimation of the effectiveness of capital control policy such that the policymakers are puzzled by which specific indicator is effective. Besides, the capital control categories I used for estimation are abstracted from portfolio flow categories, one of the reason is that the portfolio inflows usually target at short-term investment (i.e., hot money). As shown by Kim & Yang (2011), portfolio inflows can directly affect the demand for assets such as the stock transactions. The other reason is that there are a wide variety of capital control indicators on portfolio flows, such as capital control on money market, bonds, equities, derivatives. Thus, using portfolio controls are inevitable to use an aggregated indicator which may result in misleading results.

Control Variables

I include several control variables – policy measures, capital flows, and global liquidity – as possible determinants of housing prices and credit growth.

Policy measures

I consider three types of policy measures: monetary policy rate, macroprudential policy, and exchange rate. As discussed in Richter et al. (2019), to address the correlation problem of capital control policy with other policies acting at the same time, I control for monetary policy, macroprudential policy, and exchange rate shocks in my specifications.

To measure monetary policy, I use two indicators: the central bank policy rate and short term interest rate. The central bank policy rate data are obtained from BIS statistics in annual basis, from 1995 to 2017.[7] As for the short term interest rate, I obtain from CEIC database in monthly basis over 1995M1 to 2007M12 and average the monthly data to annual basis.[8]

The macroprudential policy used here focuses on "domestically oriented macroprudential measures" categorized by Bruno et al. (2017), and I choose the typical instrument targeting at the housing market – the Loan to Value (LTV) caps for mortgage loans which is usually used to measure the demand side of housing credit in the macroprudential literature (Akinci and Olmstead-Rumsey, 2018; Bruno et al., 2017; Kuttner & Shim, 2016; Richter et al., 2019; Banti & Phylaktis, 2019). This indicator restricts the amount of the loan to a certain portion of collateral value. I obtain this index from the dataset of prudential instruments developed by Cerutti et al. (2017).[9]

With respect to exchange rate policies, I consider the bilateral nominal exchange rate and the real effective exchange rate (REER). The data of bilateral nominal exchange rate are obtained from Penn World Table (PWT) by Feenstra et al. (2015), and the REER data are available for all 53 countries from the BIS effective exchange rate (EER) indices. As studied by Cesa-Bianchi et al. (2015), Bruno & Shin (2014), and Cesa-Bianchi et al. (2018), the exchange rates are included as control variables since the local currency appreciation would contribute to intensifying the boom by increasing the value of collateral, thus this mechanism provides a channel between exchange rate and financial stability.

Global liquidity and its driving force

As regards the capital flow data, I average the quarterly net acquisition of financial assets ("gross-net" outflows of domestic capital) and net incurrence of liabilities ("gross-net" inflows of foreign capital) for direct investment, portfolio investment, and

[7] For euro zone countries, they share the same policy rate started from 1999. However, for countries which are not covered in BIS policy rate dataset, I collect the data from other source. For example, I collect the policy rate for Bulgaria and Morocco from their central bank respectively, and then use exchange rate as the policy rate for Singapore, EBIOR rate for United Arab Emirates.

[8] The data of Peru, Brazil, and Chile are not included in this database, thus I use the average interbank rate (from Central Reserve Bank of Peru), short term interest rate (from FRED for Brazil), and 90 days interbank rate (from FRED for Chile), respectively.

[9] This dataset has been updated at 2018, thus it covers 53 countries used in this paper and the time horizons are extended from 2000 to 2017.

other investment from IMF Balance of Payment and International Investment Position Statistics (BOP/IIP). Hernandez-Vega (2022) uses these broad indicators study the impact of capital flows on housing prices for emerging market.

Cesa-Bianchi et al. (2015), however, use a narrower indicator to gauge capital flows – the cross border bank loan, and they also refer to it as "global liquidity". Although Cesa-Bianchi et al. (2015) and Banti & Phylaktis (2019) reach a consensus that the definition of global liquidity means "the supply of global financing", they choose different measurement methods. Different from Cesa-Bianchi et al. (2015)'s quantity side measures, Banti & Phylaktis (2019) choose a price measures – the amount outstanding of repos in the US, UK, and Europe. In this paper, I follow the measures of Cesa-Bianchi et al. (2015). These data are easily accessed from Joint External Debt Hub (JEDH) database. I choose two types of cross border loans: the cross border loans from BIS reporting banks and the cross border loans from BIS banks to non-banks.

As for the global driving force of global liquidity, I choose VIX and TED spread as in Cesa-Bianchi et al. (2015) and Banti & Phylaktis (2019). VIX index is the volatility of S&P 500 stock price which measures the willingness of banks to risk themselves at the global credit market. I obtain VIX index from Chicago Board Option Exchange (CBOE) and average it from 1995 to 2017. TED spread is the interest rate difference between 90 days interbank interest rate and government bond yields. This index is available at Federal Reserve Bank of St. Louis.

Macroeconomic fundamentals

I choose GDP and CPI as the fundamentals variables. GDP are available from PWT database, specifically, the "rgdpna" series are real GDP series that can be used in cross country regressions (Feenstra et al., 2015). As for the CPI data, I employ the BIS consumer price index dataset and supplement it with the FRED, and the data in 2010 are adjusted to 100.

Identification of Capital Control Policy Shocks

As discussed in Kuvshinov & Zimmermann (2019), if I want to measure the causal effects of capital control on housing prices and other credit variables, I need to compare two counterfactual scenarios: one where the representative economies in our sample restricted and the other where it did not. Besides, following Richter et al. (2019), I also propose three criteria that should be fulfilled:

1. The capital control policy actions should be exogenous with regarding to current and lagged financial variables, such that it would be sufficient to calculate the average treatment effect (ATE) for restricted one and that of unrestricted.
2. The capital control policy actions should be uncorrelated with other shocks, such as the monetary policy, or macroprudential shocks. To solve this problem, I can add monetary policy, macroprudential policy as control variables in the estimation process.

3. The capital control policy actions should not be anticipated.[10]

Before proceeding to the estimation part, I need to verify if capital control policy actions are exogenous to financial variables.

As the first step, I need to clarify the purposes or objectives for policymakers when they implement capital control policies. Because if the objectives of capital control contain financial variables, policymakers may implement policies according to the financial cycle. Magud et al. (2018) find that there are two prominent objectives for governments to impose capital controls: (i) reducing the volume of capital flow, (ii) reducing the exchange rate pressures. Thus, stabilizing housing prices and households credits may not become the primary objective for policymakers. In addition, Fernández et al. (2015) find that the booms and busts in aggregate activity are not relevant to the movements in capital controls. These results show that policymakers do not change capital control over the business cycle.

Although related literature has clarified the objective of capital controls, as argued by Richter et al. (2019), policymakers may target financial objectives without stating them explicitly when they implement capital control policies. Thus, I formally examined the relationship between capital control policies and financial variables using the balance condition test proposed by Jordà & Taylor (2016). It should be noted that in the ideal randomized controlled trial, with treatment and control units allocated randomly, the probability density function of each of the financial variables would be the same for each subpopulation and there would be perfect overlap between the two subpopulation densities. A simple way to check for this balance condition is to do a test of the equality of the means across subpopulations. If the null hypothesis of balance is rejected, it suggests that the treated group are not truly exogenous events.

Table 2 reports the results of balance condition test. All capital control policy variables are considered and broken down by restriction on inflows and outflows. Following the measures chosen by Richter et al. (2019), I compare real housing price, real bank credit, real credit to households in treatment and control group based on two types of measures. The first measure is the smoothed growth rates of these variables over the previous year, and they are also demeaned at country level. The second measure is the detrended level of such variables. The results indicate that capital controls on real estate transactions are indeed endogenous to financial variables. Particularly the restrictions on outflows (namely, “re_pabr” and “re_slbn”) show significant different for overall financial variables. This is also true for capital control on financial credit inflow (“fci”). However, for other policy variables (e.g., “fco”, “cci”, “cco”, “dii”, and “dio”), they only show significant difference in credit variables, especially for capital control on commercial credit outflow (“cco”), strongly suggesting its endogeneity to credit variables.

[10] Policymaking is a forward-looking activity. Similar to monetary policy, policymakers may not only implement capital controls with response to lagged changes in economic variables, but also respond to anticipated future evolution of these (Pasricha, 2022). Monokroussos (2011) estimates a forward-looking, dynamic, discrete-choice monetary policy reaction function for U.S. using the “Greenbook forecasts” by the staff of Fed before each FOMC meeting. However, such forecasts are not available for emerging countries. Therefore, in this paper, I have no choice but assume capital control actions are not be anticipated. In addition, similar to macroprudential policies, the unsystematic nature of capital controls means they are typically unexpected (Richter et al., 2019).

Table 2 Balance condition test: for all capital control policy variables

	rei	re_pabr	re_slbn	fci	fco	cci	cco	dii	dio
Real housing price detrended	0.00	-0.01	-0.01	-0.00	0.00	0.00	-0.01	-0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Real bank credit detrended	0.01	0.00	-0.01	-0.00	-0.00	0.00	-0.01**	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Real credit to households detrended	0.00	0.00	-0.02	-0.00	0.00	-0.00	-0.02**	0.00	-0.01*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Real housing price smoothed	-0.00	-0.01**	-0.01***	0.01***	0.00	0.01	0.00	-0.00	0.00
growth, demeaned	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Real bank credit smoothed	0.01***	0.02***	0.03***	0.04***	0.02***	0.03***	0.03***	0.02***	0.03***
growth, demeaned	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Real credit to households smoothed	0.02***	0.02***	0.05***	0.05***	0.03***	0.04***	0.04***	0.03***	0.05***
growth, demeaned	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Observations	1115	1114	1092	1115	1106	1115	1109	1115	1111

Notes: Each cell is the difference between treatment (implemented capital control on real estate inflow "rei", purchase abroad by residents "re_pabr", sale locally by non-residents "re_slbn", financial credit inflow "fci" and outflow "fco", commercial credit inflow "cci" and outflow "cco", direct investment inflow "dii" and outflow "dio", respectively) and control group (no such restriction) for interested financial variables (housing price, bank credit, and credit to households). The null hypothesis is the equality of means for each subpopulation. Standard errors in parentheses. *, **, and ***, indicate the significant at 10%, 5%, 1% levels respectively

In the next section, I will study the effect of capital control policies considering endogeneity problem. As the results presented above, capital control on real estate transactions ("re" related variables) are endogenous to real housing prices. Since the relationship between capital control on real estate transactions and housing prices are the main interest in this paper, in "Endogeneity Problem Revisiting", I employ *inverse probability weighted regression adjusted (IPWRA) estimators* to address the endogeneity problem between them.[11]

The Effects of Implementing Capital Control Policies

In order to calculate the marginal effects of capital control policies, I use local projection (LP) estimator developed by Jordà (2005). As a preferable method than VARs, the impulse response can be calculated by a sequence of projections of the endogenous variables which are moved forward in time on its lags. Compared with VARs' extrapolating, these projections are local to forecast horizons and become more robust to misspecifications (Jordà, 2005).[12] The inference is based on Driscoll & Kraay (1998) robust standard errors that allow arbitrary correlations of the error term across countries and time.

An impulse response can be defined as the difference between two forecasts. One is the forecast when a capital control policy is implemented, and the other is the forecast when it is not implemented. I characterize the impulse response of housing prices to capital control policies as

$$\tau(h) = E\left(HP_{t+h} - HP_t | CaCP_t = 1; \Omega_t\right) - E\left(HP_{t+h} - HP_t | CaCP_t = 0; \Omega_t\right) \tag{1}$$

where $HP_{t+h} - HP_t$ denotes the change in housing prices from the year when a capital control policy is implemented to a future time h years later. $CaCP_t$ is the capital control index that is 1 if a capital control policy is implemented at period t, and 0 otherwise. Ω_t is the available information set at period t.

Since I estimate $\tau(h)$ using local projection method, the regression equation can be written as

$$\Delta_h HP_{i,t+h} = \alpha_i^h + \gamma_t^h + \beta^h CaCP_{i,t} + \sum_{k=0}^{1} \phi_k^h \Delta X_{i,t-k} + \beta_c^h HP_{i,t}^c + \varepsilon_{i,t+h}; \quad h = 1, ..., .5 \tag{2}$$

where $\Delta_h HP_{i,t+h} = HP_{i,t+h} - HP_{i,t}$, here $HP_{i,t}$ denotes the real housing price for country i in period t. $\hat{\beta}^h = \hat{\tau}(h)$ is the marginal effects of a specific capital control policy on the expected housing prices at a future time h years later. α_i^h is the country dummies which are used to control for the country-specific growth. γ_t^h represents the

[11] Capital control on financial credit inflow ("fci") also appears endogenous to real housing prices, thus I also calculate the marginal effects using IPWRA method in "The Effects of Implementing Capital Control Policies" for "fci".

[12] Plagborg-Møller & Wolf (2021) find that LP and VAR estimators are simply two dimension reduction techniques with common estimand but different finite-sample properties. In addition, linear VARs are as robust to nonlinearities as linear LPs.

time-fixed effects which are used to control the global trend of housing prices. $X_{i,t}$ denotes the control variables included up to one lags, and it contains real housing price growth, real GDP growth, CPI growth, direct investment (inflow or outflow) to GDP ratio, other investment (inflow or outflow) to GDP ratio, central bank policy rate, nominal bilateral exchange rate, and VIX. $HP^{c}_{i,t}$ is the real housing price detrended, denoting the cyclical component of real housing price. It is calculated by deviation of log real housing price from an HP filtered trend estimated with the yearly smooth parameters $\lambda = 100$.

Before moving to the empirical results, I summarize the potential mechanism and the expected signs for the impulse response of the implementation of capital control indices on housing prices for inflow and outflow controls respectively in Fig. 1. As for inflow controls, I assume it restricts the investment fund flowing into domestic estate markets. Thus, the investment and housing demand decline, resulting the decrease of housing prices. Unlike inflow controls, I assume the outflow controls to influence housing prices through two channels. The directly channel shows that outflow controls can limit directly the investment flowing out to foreign estate markets, and thus the domestic estate demand is not decreasing and the housing prices can be maintained. The indirect channel has two opposite mechanisms: (i) for domestic investors, they have difficulty investing abroad and thus cannot but invest in domestic housing market, leading to the increase of housing prices; (ii) for foreign investors, they also have trouble in bringing back their funds, and they are not willing to futher invest in countries that restrict their exit (Acosta-Henao et al., 2020), leading to a drop in housing prices. In addition, I also consider the results if outflow controls are circumvented. The capital flight may happen such that the domestic housing demand declines, resulting the decrease of housing prices.

The Effects of Capital Control on Direct Investment

The results of estimating Eq. 2 using the capital control policies of direct investment ("dii" and "dio") are reported in Table 3 and Fig. 2. The left and right panels depict the

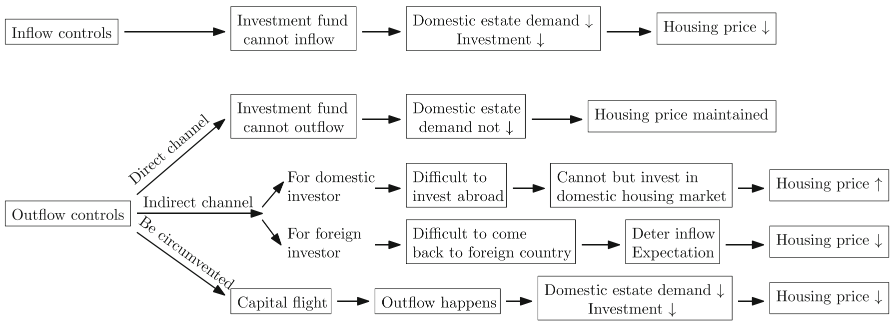

Fig. 1 The potential mechanism from capital controls to housing prices. Source: made by author

Table 3 Local projection: Impact of capital controls of direct investment on real housing prices

Dep. Var.: 100 × log (real housing price)					
	$h = 1$	$h = 2$	$h = 3$	$h = 4$	$h = 5$
CACP. direct investment (inflow)	-0.615	-0.987*	-1.056**	-1.272**	-2.852**
	(0.541)	(0.534)	(0.415)	(0.533)	(1.078)
Observations	672	623	574	525	476
CACP. direct investment (outflow)	-0.402	-0.712	-0.828	-0.133	0.524
	(0.455)	(0.689)	(0.902)	(1.222)	(2.024)
Observations	668	619	570	521	472

Notes: Driscoll & Kraay (1998) standard errors in parentheses. Regression equations contain country fixed effects and time fixed effects. Other control variables include real housing price detrended, the growth rate and one lag growth of real housing price, real GDP, CPI, direct investment (inflow or outflow) to GDP ratio, central bank policy rate, bilateral nominal exchange rate, and VIX. *, **, and ***, indicate the significant at 10%, 5%, 1% levels respectively

cumulative response of real housing price (×100) to the changing of capital control index (from 0 to 1, which means varying from no restriction to capital flows restricted) over the following 5 years respectively. The left panel of Fig. 2 shows that the response of changing capital control on direct investment inflow is significantly lower than zero after period 3 and it declines by 2.852% at period 5. As for the right panel of Fig. 2, the data show the decline of housing prices to a smaller extent, and the impact is less precisely estimated for over 5 years.

As pointed out by Richter et al. (2019), the capital control policies are usually implemented with other policy rules, such as monetary policy and macroprudential

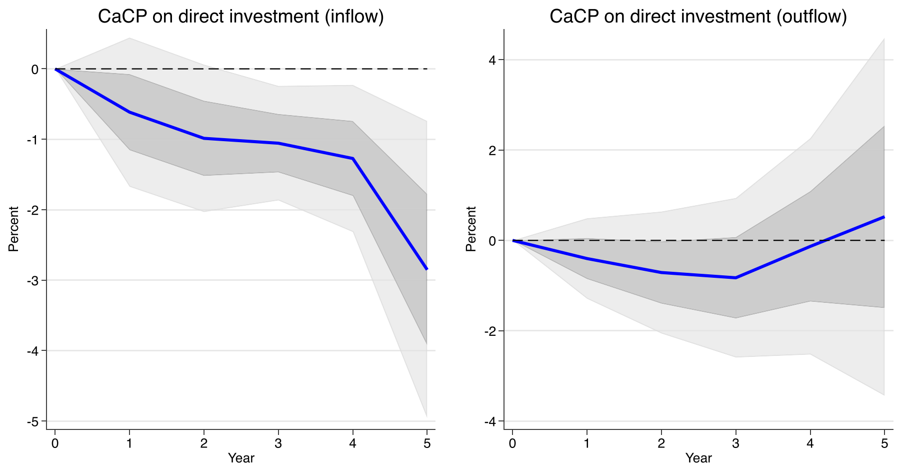

Fig. 2 Local Projection: Impact of capital controls of direct investment on real housing prices. Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing prices over 5 years following the changing in capital control of direct investment inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas are 1 standard error (dark) and 1.96 standard error (gray) Driscoll & Kraay (1998) bands around the response estimates

policies. To eliminate the potential estimation bias due to the fact that monetary policy may respond to the changing of capital controls on direct investment, I control the changing of central bank policy rate in Eq. 2 and then check the response of monetary policy rate to the implementing of capital control. I find that monetary policy rate is not significantly responding to capital controls on direct investment inflows and outflows. Besides, not considering macroprudential policies could also bias the estimation results. Thus I control for macroprudential proxy variable – the borrower-oriented LTV caps.[13] The results are in line with our baseline findings that both inflow and outflow controls lower real housing prices, but the results are only significant for inflow controls.[14]

The Effects of Capital Control on Financial Credit

In this section, I change the explanatory variables to capital controls on financial credit inflow ("fci") and outflow ("fco") and then estimate the marginal effects using baseline Eq. 2. The results are presented in Table 4 and Fig. 3. The left and right panels depict the cumulative response of real housing prices ($\times 100$) to the changing of capital control inflow and outflow index over the following 5 years respectively. The left panel of Fig. 2 shows that the response of real housing prices to the changing of capital control "fci" is higher than zero for the first 2 years, and then crosses the zero line with a negative influence on real housing prices after period 3 and at last is 6.158% lower real housing price at period 5. The marginal effects are significant after a long time adjustment for 4 years. These results are consistent with the findings by Dell'Erba & Reinhardt (2015) and Bruno et al. (2017) who find that banking sector CFMs are effective in decreasing the banking inflows. Thus, the financial credit are prevented from flowing into the domestic housing market, and the housing prices are also depreciated. In "Identification of Capital Control Policy Shocks", I have found that capital control on financial credit "fci" also appears endogenous to real housing prices, thus I also provide the results of IPWRA estimator for comparison without describing its mechanism here. The results are presented in Fig. 13 in appendix. I find that the negative effects are also significant in the long term though they are weakened and even change to positive for period 2 to 3. In the right panel, the response of real housing price after capital control on "fco" has a weak negative effects over 5 years. These results are almost imprecisely estimated.

As I have done in the last section, I have added central bank policy rate in the baseline model, and then I test that monetary policy is not significantly responding to both capital controls "fci" and "fco". As for macroprudential policy, I also control the LTV caps in the baseline specification and find that the effects of capital control on "fci" and "fco" are both marginally weaker.

[13] The results of Fig. 2 and Table 3 do not consider macroprudential variable since the data are only available from 2000 to 2017. This is also the case for other estimation results.

[14] The results that include LTV caps are available upon request for all these estimation results.

Table 4 Local projection: Impact of capital controls of financial credit on real housing prices

Dep. Var.: 100 × log (real housing price)					
	$h = 1$	$h = 2$	$h = 3$	$h = 4$	$h = 5$
CACP. financial credit (inflow)	1.140	0.414	-0.547	-3.005**	-6.158***
	(1.249)	(1.417)	(1.609)	(1.117)	(1.006)
Observations	543	504	465	426	387
CACP. financial credit (outflow)	-0.372	-1.459	-1.141	-1.102	-1.052
	(0.513)	(0.982)	(1.153)	(1.292)	(1.483)
Observations	540	501	462	423	384

Notes: Driscoll & Kraay (1998) standard errors in parentheses. Regression equations contain country fixed effects. Other control variables include real housing price detrended, the growth rate and one lag growth of real housing price, real bank credit, real credit to households, real GDP, CPI, other investment (inflow or outflow) to GDP ratio, central bank policy rate, bilateral nominal exchange rate, and VIX. *, **, and ***, indicate the significant at 10%, 5%, 1% levels respectively

The Effects of Capital Control on Commercial Credit

In this section, I consider the effect of other type of capital control on credit – commercial credit – on real housing price. Thus, I estimate Eq. 2 using capital control on commercial credit inflow ("cci") and outflow ("cco"). The results are visualized in Fig. 4 and Table 5. The left panel of Fig. 4 shows that the response of changing to capital control "cci" lowers real housing price in the first year, and then gradually decreases to -3.453% in year 4 and -4.896% in year 5. Similar to the effects of capital control on financial credit ("fci"), the marginal effects of "cci" are also less pronounced

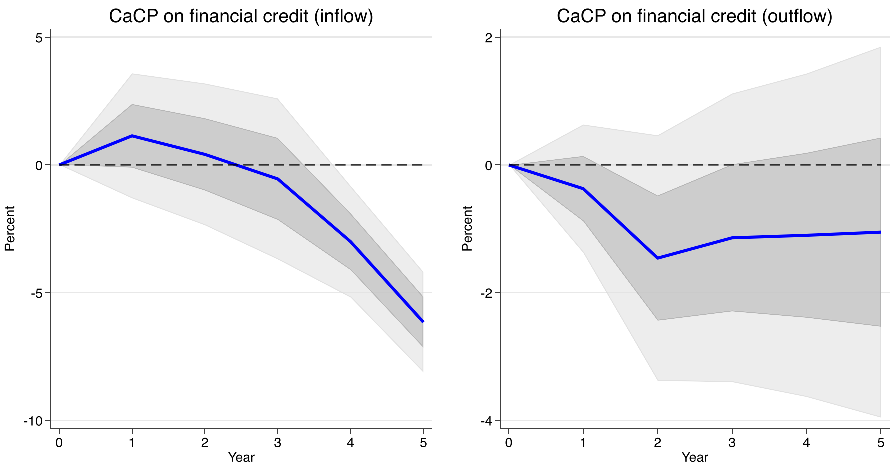

Fig. 3 Local Projection: Impact of capital controls of financial credit on real housing prices. Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing prices over 5 years following the changing in capital control of financial credit inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas are 1 standard error (dark) and 1.96 standard error (gray) Driscoll & Kraay (1998) bands around the response estimates

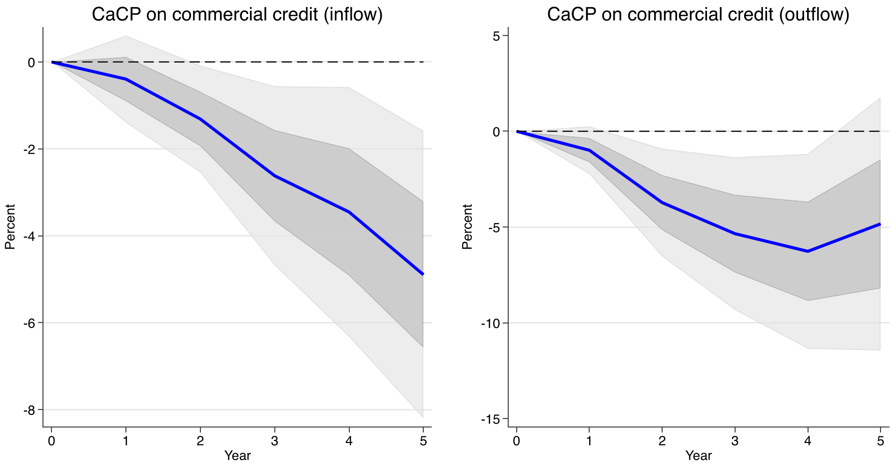

Fig. 4 Local Projection: Impact of capital controls of commercial credit on real housing prices. Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing price over 5 years following the changing in capital control of commercial credit inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas are 1 standard error (dark) and 1.96 standard error (gray) Driscoll & Kraay (1998) bands around the response estimates

in period 1 to 2, but statistically different from 0 after 3 years. As for the right panel of Fig. 4, I find that the response is 3.725% lower real housing price after 2 years, then plummets suddenly to -6.268% after 4 years.

The drop of housing prices for capital controls of commercial credit outflow "cco" could be reasonably explained using the following two reasons. On the one hand, when new policy is implemented, there also exists the likelihood of capital flight which are detrimental to real housing prices. As emphasized by Kitano & Zhou (2022), for capital controls on commercial credit, trade misinvoicing (i.e., under- and over-invoicing exports and imports) is a main channel of capital flight for emerging countries. On the

Table 5 Local projection: Impact of capital controls of commercial credit on real housing prices

Dep. Var.: 100 × log (real housing price)					
	$h = 1$	$h = 2$	$h = 3$	$h = 4$	$h = 5$
CACP. commercial credit (inflow)	-0.394	-1.313*	-2.620**	-3.453**	-4.896**
	(0.512)	(0.630)	(1.053)	(1.468)	(1.689)
Observations	516	477	438	399	360
CACP. commercial credit (outflow)	-0.991	-3.725**	-5.347**	-6.268**	-4.833
	(0.639)	(1.439)	(2.037)	(2.597)	(3.377)
Observations	513	474	435	396	357

Notes: Driscoll & Kraay (1998) standard errors in parentheses. Regression equations contain country fixed effects. Other control variables include real housing price detrended, the growth rate and one lag growth of real housing price, real bank credit, real credit to households, real GDP, CPI, other investment (inflow or outflow) to GDP ratio, central bank policy rate, bilateral nominal exchange rate, and VIX. *, **, and ***, indicate the significant at 10%, 5%, 1% levels respectively other hand, as pointed out by Acosta-Henao et al. (2020), restrictions on outflow may also deter inflows since investors are not willing to invest in countries that restrict their exit. These two effects are in the same directions and superimposed on each other, resulting the significant decline of housing prices.

As for the possibility of monetary policy reacting to capital control actions, I have added central bank policy rate in the baseline model, but I also test the response of central bank policy rate to the capital control actions and find no evidence on policy rates responding to capital control actions. As for macroprudential policy, I also control the LTV caps in the baseline model and the results are consistent with my finding above. The response to capital control on "cci" is weaker in year 1 to 3 but stronger in year 4 and 5, while the response to capital control on "cco" is weaker for all horizons.

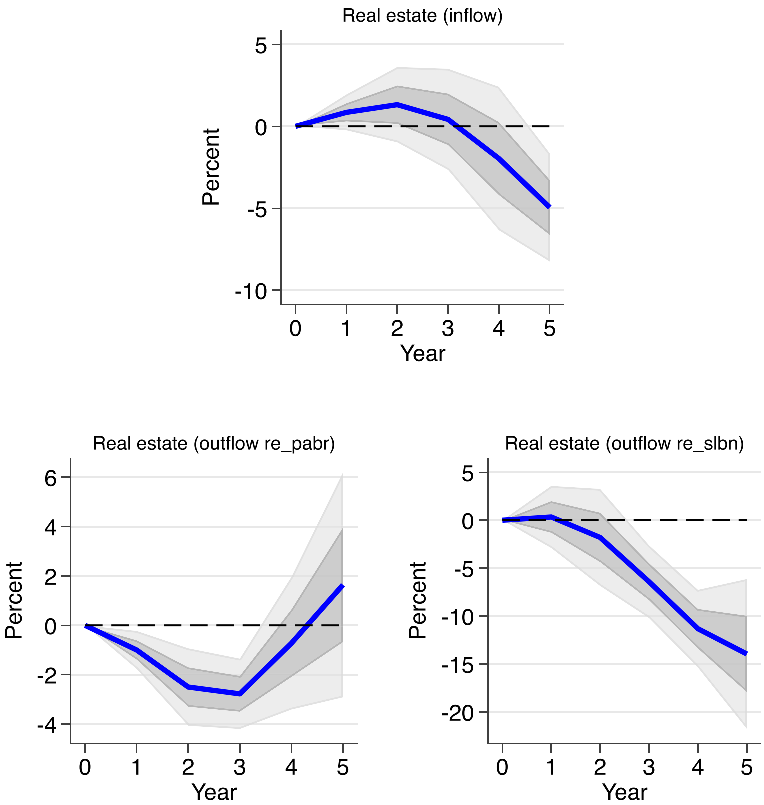

Fig. 5 Local Projection: Impact of capital control of real estate transactions on real housing prices. Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing price over 5 years following the changing in capital control of real estate transaction inflow (purchase locally by nonresident) and outflow (purchase abroad by resident, and sale locally by nonresident) from no restriction "0" to restriction "1" respectively. Shade areas are 1 standard error (dark) and 1.96 standard error (gray) Driscoll & Kraay (1998) bands around the response estimates.

The Effects of Capital Control on Real Estate Transactions

In this section, I estimate the impact on real housing prices using capital control on real estate transactions. As I have discussed in "Identification of Capital Control Policy Shocks", even if indices of capital control on real estate transactions are endogenous to real housing prices, for comparison with other capital control policies in the same pattern, I also consider capital control on "re" here with the loss of preciseness to some extent. Indeed, I will further discuss the endogeneity problem in "Endogeneity Problem Revisiting" and use a method to deal with it.

Figure 5 and Table 6 depict the response of real housing prices after changing capital control on real estate transactions inflow ("rei") and outflow ("re_pabr" and "re_slbn"). The upper panel shows that the negative response of changing to capital control "rei" is indistinct from the first to third year, but at fourth year, it decreases sharply and then turns to statistical significant 4.949% lower of real housing prices at period 5. The lower-left panel displays the results of capital control on purchase abroad by residents "re_pabr". I find that the response keeps decreasing from year 1 to 3 and then recovers from fourth years and turns to positive after 5 years. Capital control on "re_pabr" prevents residents from investing in abroad housing market. If this policy is effective, the housing prices wound not decrease. However, the negative effects suggest there may exists capital flight after implementing such policy immediately. Then the negative effects fade out in the long-term. It may relates to the facts that investors who have no way to transfer their money abroad illicitly cannot but invest in domestic housing market with lower return. This supports the domestic housing market instead. As for the lower-right panel, the negative response is 6.419% lower of real housing prices in the third year, and drops to -13.960% after 5 years. The coefficients are both statistically and economically significant after 3 years. These results are in line with

Table 6 Local projection: Impact of capital control of real estate transactions on real housing prices

Dep. Var.: 100 × log (real housing price)

	$h = 1$	$h = 2$	$h = 3$	$h = 4$	$h = 5$
CACP. real estate (inflow)	0.851	1.321	0.425	-1.965	-4.949**
	(0.559)	(1.175)	(1.576)	(2.235)	(1.688)
Observations	667	617	567	517	467
CACP. real estate (outflow re_pabr)	-0.998**	-2.499***	-2.774***	-0.726	1.640
	(0.394)	(0.801)	(0.727)	(1.371)	(2.326)
Observations	671	621	571	521	471
CACP. real estate (outflow re_slbn)	0.336	-1.799	-6.419***	-11.325***	-13.960***
	(1.656)	(2.586)	(1.941)	(2.073)	(3.994)
Observations	663	613	563	513	463

Notes: Driscoll & Kraay (1998) standard errors in parentheses. Regression equations contain country fixed effects. Other control variables include real housing price detrended, the growth rate and one lag growth of real housing price, real GDP, CPI, portfolio investment (inflow or outflow) to GDP ratio, other investment (inflow or outflow) to GDP ratio, central bank policy rate, bilateral nominal exchange rate, and VIX. *, **, ***, indicate the significant at 10%, 5%, 1% levels respectively the finding by Banti & Phylaktis (2019) who also show that restrictions on foreign investors significantly dampen the investment willings (expectations) and thus lower the capital flow into domestic housing market.

As for the possibility of monetary policy reacting to capital control actions, I test the response of central bank policy rate to the capital control actions and find that the coefficients of capital control on "rei" and "re_pabr" are both insignificant, whereas the coefficients of "re_slbn" are significant at 5% level. Thus, I add central bank policy rate in the baseline model. As for macroprudential policy, I also control the LTV caps and find that the negative response is weaker for capital controls on "rei", "re_pabr", and "re_slbn".

Emerging Markets and Advanced Economies

There is a stereotype that most of the EMs still manage their capital account but most of the AEs have welcomed capital account librelization. Nevertheless, threre are also 18 of 42 AEs implementing capital controls episodically (Fernández et al., 2016; Klein, 2012). Indeed, previous literature shows that the effects of capital controls are different for AEs and EMs. Binici et al. (2010) find that capital controls are more effective in AEs than in EMs, and they attribute it to the institutional ability to enforce controls. Beirne & Friedrich (2017) suggest that higher regulatory quality and a higher credit-to-deposit rate increase the effectiveness of macroprudential policies in managing cross-border bank flows. Banti & Phylaktis (2019) find that AEs can use macroprudential policies to shield their housing markets from global shocks, but not effective for EMs. However, EMs can adopt foreign currency macroprudential policies and capital controls on real estate transactions to limit the liquidity impact on housing prices. To explore if the impact of capital control on real housing prices depends on the economic development degree, I deal with this issue using the baseline specification with additional AEs or EMs dummy variables. The specification takes the following form:

$$\begin{aligned}\Delta_h HP_{i,t+h} &= \alpha_i^h + \gamma_t^h + \beta_1^h CaCP_{i,t} \\ &+ \beta_2^h CaCP_{i,t} \times EMD_{i,t} + \sum_{k=0}^{1} \phi_k^h \Delta X_{i,t-k} + \beta_c^h HP_{i,t}^c + \varepsilon_{i,t+h}; \quad h = 1, ..., .5\end{aligned} \tag{3}$$

where EMD_{it} is the dummy variables for EMs. Thus, the maginal effects

$$\partial \Delta_h HP_{i,t+h} / \partial CaCP_{i,t} = \beta_1^h + \beta_2^h EMD_{i,t}.$$

The category standard of AEs and EMs is based on "WEO Groups and Aggregates Information" whereby I divide the sample into 31 AEs and 22 EMs.

Figure 6 shows the response of real housing prices to capital controls on direct investment ("di"), where blue solid lines show the result for AEs and red dash lines for EMs. For capital controls on inflows ("dii", left panel), the response of AEs is insignificant at all horizons and thus the negative response to the capital controls on "dii" is exclusively driven by EMs. However, there are fully opposite results for the response on direct investment outflows ("dio", right panel). The results for EMs are positive and significant for most horizons, while for AEs, the effects are negative and only statistically different from zero in first three periods. There are entirely divergence and highly heterogeneous responses for AEs and EMs. These results suggest that capital control on "dio" effectively prevents the slump of housing prices (even raise the housing prices) for EMs, while for AEs, it first decreases real housing prices in the short-run and then prevents the slump of housing prices in the long-term.

Figure 7 and 8 report the results for capital controls on financial credit ("fc") and commercial credit ("cc") respectively. The left panel of Fig. 7 and 8 show that both the inflow control measures for AEs and EMs decrease the housing prices. Specifically, for AEs, the effects are more negative but only statistically significant at the third year. The negative effects for EMs materialize in the long-term and statistically significant after 3 years. As for the right panel of Fig. 7 and 8, the impacts of capital control on "fco" and "cco" cannot be distinguished from 0 for AEs. For EMs, the negative impacts of capital control on "cco" are significant unitl period 4.

As showed in Fig. 9, for three capital control policies on real estate transaction "rei", "re_pabr", and "re_slbn", the negative response of EMs is stronger than AEs. Specifically, capital control "rei" restricts inflows and reduces housing prices for EMs, capital control "re_pabr" and "re_slbn" cannot prevent outflows and the slump of housing prices for EMs.

Overall, capital controls have heterogeneous effects for AEs and EMs on housing prices, respectively. As for EMs, capital controls on all types of capital control inflow measures ("dii", "fci", "cci", and "rei") can reduce housing prices, and capital controls

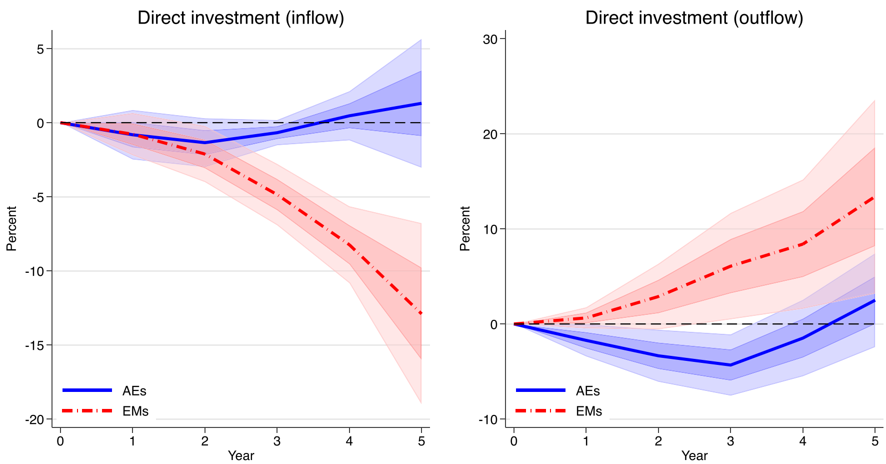

Fig. 6 Local Projection: Impact of capital controls of direct investment on real housing price – comparison of Advanced economies (AEs) and Emerging market (EMs). Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing prices for Advanced economies and Emerging markets respectively over 5 years following capital control policies implemented on direct investment

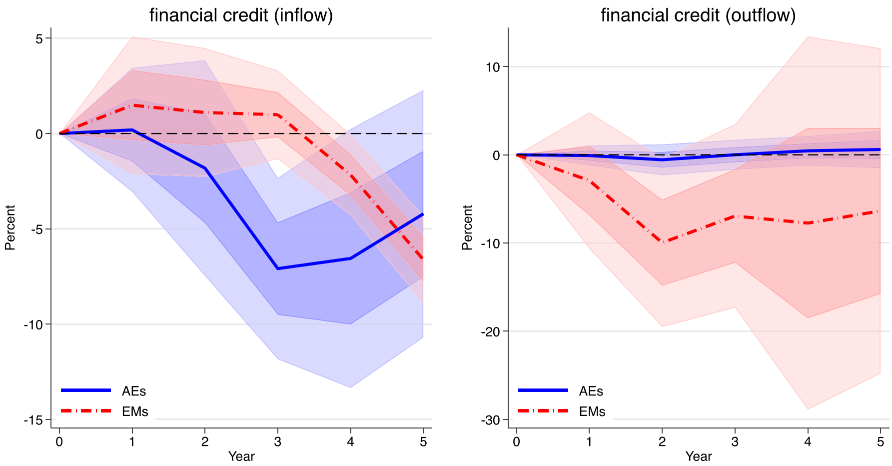

Fig. 7 Local Projection: Impact of capital controls of financial credit on real housing prices – comparison of Advanced economies (AEs) and Emerging market (EMs). Note: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing prices for Advanced economies and Emerging markets respectively over 5 years following capital control policies implemented on financial credit

on "dio" also prevent the slump of housing prices. However, for AEs, only capital control on credit inflows ("cci" and "fci") can reduce housing prices.

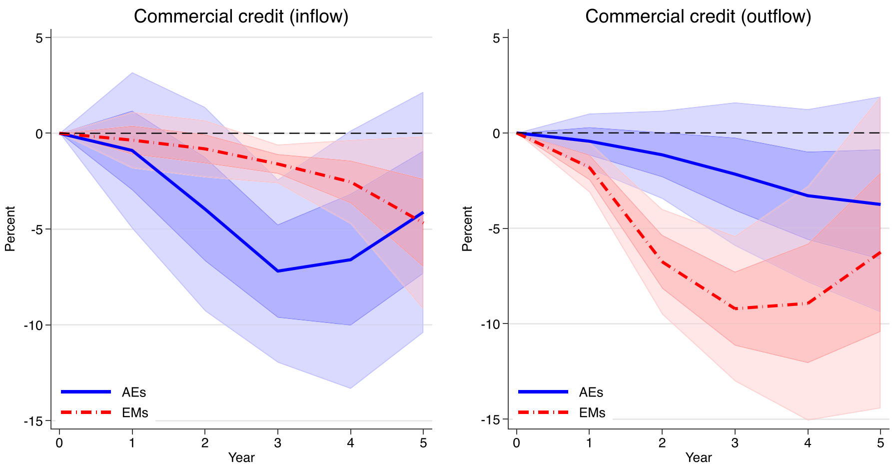

Fig. 8 Local Projection: Impact of capital controls of commercial credit on real housing prices – comparison of Advanced economies (AEs) and Emerging market (EMs). Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing prices for Advanced economies and Emerging markets respectively over 5 years following a capital control policies implemented on commercial credit

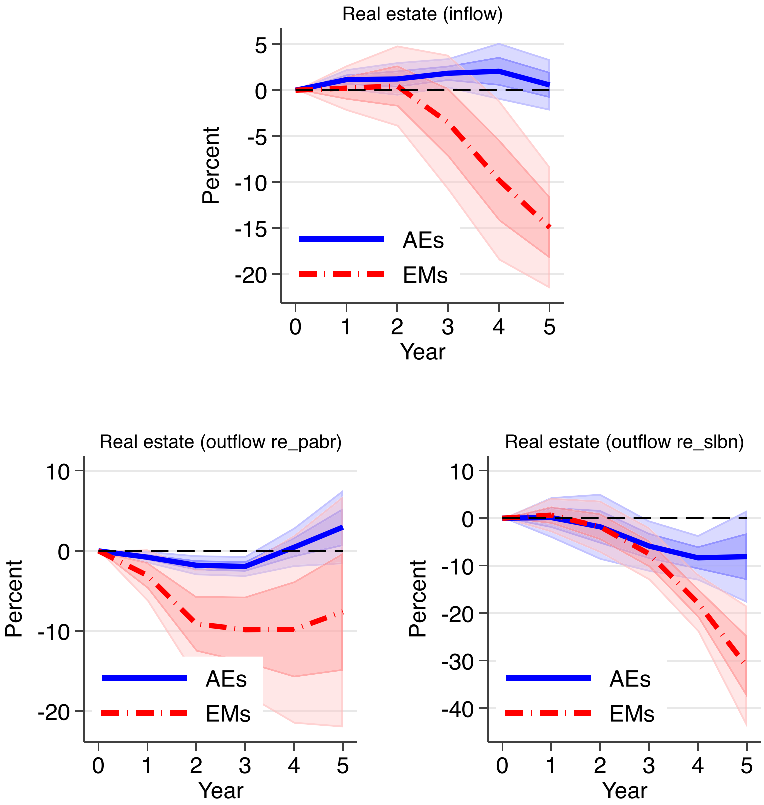

Fig. 9 Local Projection: Impact of capital control of real estate transaction on real housing price – comparison of Advanced economies (AEs) and Emerging market (EMs). Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for Advanced economies and Emerging markets respectively over 5 years following a capital control policy implemented on real estate transaction

Robustness and Sensitivity Analysis

In addition to the baseline model, I also perform several robustness checks in order to test the validity of the local projection method.

Using Generalized Synthetic Control Method (GSCM)

I check the robustness of the results using the generalized synthetic control method (GSCM), which is an imputation-based causal inference method for panel data with binary treatments. The capital control data used in this paper are such binary treatment with general treatment pattern (switch on-and-off). The method proposed by Xu (2017) and Liu et al. (2022) not only relaxes the often-violated “parallel trends” assumption but also unifies the synthetic control method (Abadie et al., 2010, 2015) with linear fixed effects models under a simple framework including difference in differences (DID). It computes counterfactuals for each treated unit using control group information based on a linear interactive fixed effects (IFE) model that incorporates unit-specific intercepts interacted with time-varying coefficients.

The estimation model is written as

$$HP_{i,t} = \delta_{it} CaCP_{it} + x'_{it}\beta + \lambda'_i f_t + \varepsilon_{it}, \tag{4}$$

where the capital control index (treatment indicator) $CaCP_{it}$ equals 1 if unit i has been exposed to the treatment prior to time t and equals 0 otherwise. δ_{it} is the heterogeneous treatment effect on unit i at time t, and x_{it} is a vector of observed covariates. The latter includes important characteristics of countries that are closely related to housing prices. Following the setting I have used in the baseline model (OLS) and IPWRA section, I choose CPI, real effective exchange rate (REER), cross border loan, real GDP growth, nominal exchange rate, population, international debt-to-GDP ratio, and bank leverage. The choice of covariates ensures that the synthetic controls can reproduce the features of the countries with capital control policies. The data are also for 53 economies from 1995 to 2017, which are consistent with the baseline model.

I choose two types of capital controls on real estate: capital control on real estate purchased abroad by resides (“re_pabr”), which is related to outflow, and capital control on real estate purchased locally by non-resident (“re_plbn”), which is related to inflow. Figure 14 shows the impact of capital control of real estate purchased abroad by resident (“re_pabr”) on housing prices estimated by the generalized sythetic control method (GSCM). The black solid line (“Treated Average”) indicates the evolution of average housing prices in the treated countries with capital control policy “re_pabr”, and the blue dashed line (“Estimated Y(0) Average”) indicates their synthetic controls. The vertical axis is the housing prices, and the horizontal axis is the time period where 0 means the capital controls are implemented. The results show that the synthetic controls (“Estimated Y(0) Average”) closely reproduced the trajectories of housing price in the treated countries (“Treated Average”) prior to the implementation of capital controls. This close fit between the black solid and blue dashed lines prior to 0 implies that the synthetic controls are reasonable comparison group to investigate the effects of capital controls on housing prices in the treated countries. The estimate effects of capital controls are the difference between the housing prices in the treated countries (black solid line) and their synthetic controls (blue dashed line). After countries implement capital control policies (in period 0), the housing price of the treated countries (“Treated Average”) and their synthetic counterpart (“Estimated Y(0) Average”) begin to diverge substantially. Although both trajectories display upward trends, the housing price of the treated countries are lower than synthetic controls, which means that the implementation of capital control on “re_pabr” decreases housing prices.

Figure 15 shows the impact of capital control of real estate purchased locally by non-resident (“re_plbn”) estimated by the generalized sythetic control method (GSCM). The synthetic controls (“Estimated Y(0) Average”) are also able to replicate the path of housing prices (“Treated Average”) in the pretreatment periods. The difference between the housing prices in the treated countries (black solid line) and their synthetic controls (blue dashed line) are evident after the implementation of capital control. The result of capital control on “re_plbn” in Fig. 15 shows that it decreases housing prices substantially as well as in the case of “re_pabr” in Fig. 14.

The combined evidence of Figs. 14 and 15 suggests that the capital control on real estate decreases housing prices, which is consistent with the results in the baseline and IPWRA model.

Placebo Test

I conduct falsification tests by simulating all the LP regression specifications with placebo policies in accordance with the practices of Borjas (2017), de Haan & Wiese (2022), and Wiese et al. (2023). I investigate whether the main results are merely artefacts of the estimator applied to the data (or not). In other words, whether the significant average treatment effects (ATE) in the main analysis are the result of type I errors (or not). To maintain comparability with the main results, I randomly draw the placebo policies from a binomial distribution with a probability of treatment equal to the proportion of each capital control implementation in the sample. In Figs. 16, 17, 18, 19, 20, 21, 22, 23, and 24, I show the results of the falsification test for the conditional capital control indices (“dii, dio, fci, fco, cci, cco, re_plbn, re_plbn, and re_slbn”), respectively. The simulated average treatment placebo effects are normally distributed around zero for all forecast horizons from 1st year to 5th year in each subfigure. The dotted vertical lines in the five sub-figures marks the ATEs from the baseline (OLS) model. It should be noted that the dotted vertical lines are clearly placed in the tails of the distribution of the simulated average treatment placebo effects in Figs. 16, 17, 18, 19, 20, 21, 22, 23, and 24. Therefore, It means that the significant average treatment effects (ATE) that I find in the baseline analysis are not the result of type I errors in all cases.

Alternative Proxy Variables

I substitute for variables in the baseline model using alternative proxies. Specifically, I use Ted spread as an alternative variable for VIX which represents the exogenous global liquidity. Besides, I use short term interest rate as the alternative for central bank policy rate, and real effective exchange rate instead of bilateral nominal exchange rate. The results (available upon request) show that no matter what variables (original variables or alternative variables) I use, the effects of real housing prices are comparable for all capital control indices used in this paper.

Longer Prediction Horizons

Inspired by Richter et al. (2019), I investigate whether the results are maintained for a long (10 years) time horizons. The results in Figs. 25, 26, 27, and 28 are response of real housing prices to capital control on direct investment (“dii” and “dio”), financial credit (“fci” and “fco”), commercial credit (“cci” and “cco”), and real estate transaction (“rei”, “re_pabr”, and “re_slbn”) extending to 10 years horizon respectively. The results in Fig. 25 show that the response of real housing prices keeps insignificant after 5 years for the left panel. For the right panel, even if the response becomes negative from period 6, the impact is also limited. The results in the left panel of Fig. 26 show the negative response holds after 5 years. The results in the right panel are consistent with the baseline model. As for Fig. 27, the results show a consistent negative response after year 5 to year 9 for the left panel. For the right panel the negative response lasts for 8 years even if the results cannot be distinguished from 0 after 4 years. As for the results in Fig. 28, the effects of capital control on “rei” (top) and “re_slbn” (bottom right) are broadly consistent after 5 years. The results of capital control on “re_pabr” (bottom left) become positive after 5 years, and become significant after 7 years. Overall, these results are broadly consistent with those obtained in “The Effects of Capital Control on Direct Investment” to “The Effects of Capital Control on Real Estate Transactions” using short prediction horizons of capital control indices.

The Sample of Capital Control Indices

Acosta-Henao et al. (2020) identify that capital controls are “sticky” since changes do not occur frequently and even if they are changed, they will keep this new policy for a long time. Thus, the dataset I use contains several economies always restricting or librelizing their capital account for the whole sample horizons. The most representative index is capital control on “re_slbn”, for 37 economies always restricting or librelizing this account, only 14 economies usually changing their policy actions.[15] I estimate the baseline specification using these 14 economies only. The results (available upon request) are comparable with the results using full samples.

I also consider the correlation problem among capital control indices. Fernández et al. (2016) find that policymakers usually pair controls across different asset categories or between inflows and outflows. They show that “di” (37%) and “re” (30%) have the lowest correlation between inflow and outflow controls. Besides, there are lowest correlations between “re” and other categories. Nevertheless, I consider the following regression Eqs. 5 to 7 and estimate the marginal effects for each capital control index:

$$\Delta_h HP_{i,t+h} = \alpha_i^h + \gamma_t^h + \sum_{j=\{dii,cci,fci,rei\}} \beta_j^h CaCP_{i,t}^j + \\ + \sum_{k=0}^{1} \phi_k^h \Delta X_{i,t-k} + \beta_c^h HP_{i,t}^c + \varepsilon_{i,t+h}; \tag{5}$$

[15] The 14 economies are Australia, Austria, Cyprus, Iceland, Korea, Malta, Bulgaria, Chile, Colombia, Morocco, Poland, Romania, Russia, and Thailand.

$$\Delta_h HP_{i,t+h} = \alpha_i^h + \gamma_t^h + \sum_{j=\{dio,cco,fco,re\_pabr,re\_slbn\}} \beta_j^h CaCP_{i,t}^j + \sum_{k=0}^{1} \phi_k^h \Delta X_{i,t-k} + \beta_c^h HP_{i,t}^c + \varepsilon_{i,t+h}; \quad (6)$$

$$\Delta_h HP_{i,t+h} = \alpha_i^h + \gamma_t^h + \beta_{in}^h CaCP_{i,t}^{in} + \beta_{out}^h CaCP_{i,t}^{out} + \sum_{k=0}^{1} \phi_k^h \Delta X_{i,t-k} + \beta_c^h HP_{i,t}^c + \varepsilon_{i,t+h}; \quad (7)$$

The estimation results (available upon request) show that my findings are robust even I consider inflow and outflow, as well as other capital indices simultaneously.

Following Richter et al. (2019), I also study whether these results are not driven by a single country. I choose countries based on Klein (2012) which categorizes country as three types: “Open”, “Gate”, and “Wall” country. He defines the “Gate country” as a country use capital controls episodically. Thus, I choose the representative “Gate countries” to test if one country can dominate the estimation results. I first eliminate the “Gate countries” one by one from the baseline model and then estimate the results for all capital controls indices.[16] Results (available upon request) show that even if I drop these countries one by one, the estimated results are still consistent to the full samples.

Pre-crisis v.s. Post-crisis

We have known that the macroprudential policies are rarely used before GFC in 2009 (Forbes et al., 2015, Richter et al., 2019). Thus, I have incentive to test if the effects of capital control policies on real housing prices are also the case. I address this issue by using pre-crisis (blue solid lines) and post-crisis (red dash lines) dummy variables. The results of capital control on direct investment (“di”) show in Fig. 29. Both for inflow and outflow controls, and both for pre- and post-crisis subsamples, the results show no visible change compared with the baseline model, whereas the pre-crisis subsmple for inflow controls is more statistically significant in the period 5.

As for capital controls on financial credit (“fc”), the different responses for pre- and post-crisis are displayed in Fig. 30. In the left panel, the response to capital control on “fci” is broadly negative for the pre-crisis subsample. For the right panel, both pre- and post-crisis subsamples are very similar to the full sample results. The impacts of capital control on commercial credit (“cc”) before and after crisis are depicted in Fig. 31. The left panel shows that the negative response is entirely driven by pre-crisis subsample. The right panel shows that both pre- and post-crisis subsamples fail to effectively curb the decline in housing prices, and the negative effects are robust to baseline results.

[16] The representative “Gate countries” are Argentina, Chile, Cyprus, Czech Republic, Hungary, Iceland, Korea, Romania, and Russia.

Figure 32 reports the impact of capital control on real estate transactions ("re") before and after crisis. As for the inflow controls in the top panel, both subsamples are the driving force for the negative result though only the pre-crisis sample is significant in the period 5. For capital control on "re_pabr" in the bottom-left, I find that both the pre- and post-crisis samples show negative effects for housing prices which are consistent with baseline results. As for capital control on "re_slbn" in the bottom-right, both subsamples are statistically significant after 3 years which are in line with the baseline estimates.

Abovel all, no matter before or after crisis, the results of capital controls on direct investment ("di") and real estate transaction ("re") are consistent with the baseline model. As for capital controls on financial credit ("fc") and commercial credit ("cc"), the negative effects of inflow controls on real housing prices are mainly driven by pre-2007 subsample. These results are in line with the description of Blanchard et al. (2013) and Ostry et al. (2012) that capital controls have been used several times before crisis.

Boom v.s. Slump

The theoretical literature shows that a countercyclical capital control policy is desirable since it can enhance financial stability (Bianchi, 2011; Korinek, 2018). Thus, in this section, I also test if the capital controls on inflow and outflow are implemented in a countercyclical manner. I address this issue by using boom (real GDP above its trend) and slump (real GDP below its trend) dummy variables. The results of capital control on "di" are depicted in Fig. 33, where blue solid lines denote the boom periods, and red dash lines are slump periods. The left panel shows that the negative impacts of capital controls on inflow are mainly driven by the boom period. The right panel shows the insignificant negative effects for outflow restriction as in the baseline model.

I also test the responses in boom and slump subsamples for capital controls on "fc". Results in the left panels of Fig. 34 show that the negative response is driven by boom periods in the long-run for capital controls on "fci". Thus, in the boom periods, policymakers have used capital controls prevent the inflow and decrease the real housing prices. The inflow controls are implemented in a countercyclical manner to some extent. In addition, I report the results of capital control on "cc" in Fig. 35. The results show that the negative response of real housing prices to capital control on "cci" is entirely driven by both boom and slump periods. Capital control on "cco" cannot deter housing prices from decreasing and the negative response is also significantly driven by both boom and slump periods. Thus, there are no evident cyclical properties of capital controls on "cc".

As for the results for capital controls on "re" in Fig. 36, I find that no matter what type of subsample I use, the responses are consistent with the results for full sample. In other words, policymakers use capital controls on "re" in the same pattern for both boom and slump periods. Overall, although these results are broadly in line with Fernández et al. (2015)'s conclusion that capital controls are acyclical, the inflow controls on "dii" and "fci" are implemented in a countercyclical manner to some extent.

Endogeneity Problem Revisiting

As discussed by Richter et al. (2019), policymakers may target financial objectives without stating them explicitly when implement capital control policies.[17] Besides, the decision to implement capital control policy is taken contingent on such countries' economic conditions. In other words, Countries which implement capital control policies are often responding to changes in variables that policies are intended to affects (Forbes et al., 2015). This result would lead to "selection bias" problem which means the randomization can not be achieved. Thus, if capital control on real estate transactions are endogenous to housing prices or other credit variables, the estimation result may be biased.[18]

To address the endogeneity problem, I employ *inverse probability weighted regression adjusted (IPWRA) estimator* developed by Jordà & Taylor (2016). With regard to the selection bias problem, this method can rebalance the sample of "implementing capital control" (treatment group) and "not implementing capital control" (control group) by putting more weight to the capital controls that are implemented as surprise and allocating lower weight on capital controls that are implemented endogenously. Then, I use local projections to the rebalanced sample and obtain the IPWRA estimators. There are several studies addressing the endogeneity problem using this method, and I study the effects of capital control on real estate transactions to real housing prices and other credit variables.[19]

The IPWRA estimators are calculated in two steps. In the first step, I model the implementing of capital control by estimating a propensity score (or probability) for each observation using a probit model:

$$\hat{P}\left(CaCP_{i,t}=1\right)=\Phi\left(\alpha_i+\hat{\beta}Z^p_{i,t-1}+\hat{\gamma}_1\widetilde{Z}^p_{i,t-1}+\hat{\gamma}_2\widetilde{Z}^p_{i,t-2}\right), \tag{8}$$

where $\hat{P}\left(CaCP_{i,t}=1\right)=\hat{p}_{i,t}$ is the predicted capital control probability for countries i at period t. $Z^p_{i,t}$ and $\widetilde{Z}^p_{i,t}$ are both predictor variables which $Z^p_{i,t}$ with as much one lag, and $\widetilde{Z}^p_{i,t}$ with two lags. $\Phi(\cdot)$ is the cumulative standard normal distribution function. The first step rebalances the sample by giving the weights, namely the inverse propensity scores $1/\hat{p}_{i,t}$ for the treatment group ($CaCP_{i,t}=1$) and $1/\left(1-\hat{p}_{i,t}\right)$ for

[17] In fact, Fratzscher (2012) shows that capital controls are used to dampen the overheating of domestic economy, in the form of high credit growth. Forbes et al. (2015) emphasize that the purpose of CFM includes reducing specific measure of financial fragility. Pasricha (2022) finds that capital control may be used to underpin financial stability. Thus, even if policymakers do not break down the "financial stability" to dampen housing prices appreciation, the housing prices become an index that may affect the decision of policymakers.

[18] For example, Ostry et al. (2012), Beirne & Friedrich (2017), Landi & Schiavone (2021) show that if countries tend to tighten controls when the volume of capital flows is high, the OLS estimates should be upward biased. Ahmed & Zlate (2014) also show the endogeneity would bias coefficients being positive.

[19] Jordà & Taylor (2016) analyze the response of macroeconomic aggregates to the fiscal austerity (endogeneity: the trigger of fiscal austerity depends on the macroeconomic condition), Kuvshinov & Zimmermann (2019) document the impact of sovereign default to GDP (the measure of default cost). The occurrence of sovereign default is also endogenous to the macroeconomic condition. Richter et al. (2019) study the effect of macroprudential policy to financial variables. The using of macroprudential policies also depends on the financial cycle.

control group ($CaCP_{i,t} = 0$). In the second step, I estimate the response of real housing prices and other credit variables using weighted least squares (WLS) given by the inverse propensity scores. The IPWRA baseline regression equation can be written as

$$\Delta_h FA_{i,t+h} = \alpha_i^h + \gamma_t^h + \beta^h CaCP_{i,t} + \sum_{k=0}^{1} \phi_k^h \Delta Z_{i,t-k}^c + \beta_c^h FA_{i,t}^c + \varepsilon_{i,t+h}; \quad h = 1, ..., .5 \tag{9}$$

where $FA_{i,t}$ is the financial variables: real housing prices, real bank credit, and real credit to households. $FA_{i,t}^c$ corresponds to the detrended variable. $Z_{i,t}^c$ denotes the control variables. As discussed by Kuvshinov & Zimmermann (2019) and Jordà & Taylor (2016), I use a richer set of predictor Z^p in step 1 than control variables Z^c in step 2. The predictors in step 1 should contain all variables that help forecast the implementing of capital control policies, and the control variables in the second step should both consider the predictability and the explanatory ability of financial variables.

Then, the average treatment effect (ATE), namely the average difference in potential results of "implementing capital control" and "not implementing capital control" across the sample, can be calculated by:

$$ATE_h\left(CaCP_{i,t}\right) = \frac{1}{n_{cacp=1}} \sum_i \sum_t \frac{\Delta\widehat{FA}_{i,t+h} \cdot CaCP_{i,t}}{\hat{p}_{i,t}} - \frac{1}{n_{cacp=0}} \sum_i \sum_t \frac{\Delta\widehat{FA}_{i,t+h} \cdot \left(1 - CaCP_{i,t}\right)}{1 - \hat{p}_{i,t}}, \tag{10}$$

where $\Delta\widehat{FA}_{i,t+h}$ is the prediction obtained by estimating Eq. 9, and $n_{cacp=1} = \sum_t CaCP_{i,t}$ and $n_{cacp=0} = \sum_t \left(1 - CaCP_{i,t}\right)$ are the numbers of observations in treatment and control group respectively.

Diagnostic Test

Before calculating the IPWRA estimator, I first check the validity of this method. Forbes et al. (2015) show that two tests should be satisfied: the overlap test and balance condition test. Jordà & Taylor (2016) report three diagnostic test (balance condition test, omitted variables test, and predicable test) to assure the existence of endogeneity.

I have done a balance condition test previously in Table 2 mainly for dependent variables. Here I will extend to control variables. The results are showed in Table 9 for several macroeconomic control variables used in Eq. 2. The results show that for most of control variables, the null hypotheses are rejected, which means the capital control on real estate transaction "rei", "re_pabr", and "re_plbn" are endogenous to some extent. Then I check if the dependent and control variables can predict the implementation of capital controls. To address this issue, I test if the capital control policies that will be implemented at year $t + 1$, can be predicted with dependent and control variables at year t using a pooled probit estimator. I will estimate the response of real housing prices and credit variables in the next section, thus I do this test for different explanatory variables. Table 10 shows the pooled probit estimator for real housing prices, credit variables and other predicting variables. In the second row, I find that the coefficients of real housing prices are positive and statistically different from zero for all three types of capital control indices. These results mean that policymakers appear to implement capital control on real estate transactions when real housing prices increases.

Besides, I find that when real bank credit is high, there is also an increase in the probability of implementing capital control on outflow “re_pabr” and “re_slbn”, while the likelihood of implementing capital control on inflow decreases. These counter-intuitive results also happen to real credit to households, where the increase in real bank credit to households can reduce the likelihood of implementing capital control on outflow “re_pabr” and “re_slbn”. Compared with housing prices, the prediction effects of credit are economically weaker. These may attribute to the fact that credit variables are not perfect substitution for the housing demand. Nevertheless, I find that real GDP growth is also the significant predictor for all three types of capital controls.

Following Jordà & Taylor (2016), Richter et al. (2019), and Kuvshinov & Zimmermann (2019), I further confirm the predictive ability using AUC statistic. In other words, this statistic measures whether such probit model can correctly categorize observations into “restriction” or “no restriction”. When AUC is equal to 0.5, it means this model has no classification ability. If AUC is equal to 1, it means a perfect classification. In Table 10, all AUCs are larger than 0.71, even 0.87 for “re_slbn”. Thus, these AUCs are all significantly larger than 0.5.

I also provide the overlap test and the results are depicted in Fig. 37. The dependent variables for the probit model are the forward variable of capital controls on “rei”, “re_pabr”, and “re_slbn”. This test provides the empirical kernel density functions of predicted probabilities calculated by probit model of propensity score and then compares the propensity score. The red dash lines show the estimated probability of implementing capital control and the blue solid lines denote no such restriction. As explained by Jordà & Taylor (2016), the ideal empirical distribution of propensity score should be uniform and identical for treatment and control groups. In addition, they admit that the distribution of treatment should peak at 1 and be zero elsewhere, while for control group, the distribution should peak at 0 and be zero elsewhere. The results in sub-figures of Fig. 37 are consistent with these features and show substantial region of overlaps. Thus I believe that the inverse propensity score method identifies successfully the ATE of capital control policies.

The Effects on Housing Price

In this section, I will further study the response of real housing prices to capital control on real estate transactions using IPWRA estimators. Figure 10 presents the results of IPWRA estimators and I also report the OLS results for comparison. I find that the negative effects for all capital control policy variables are weakened after using IPWRA estimators and the significance is still maintained. For example, the negative response of capital control on “rei” is -4.949% after five years for conditional OLS,

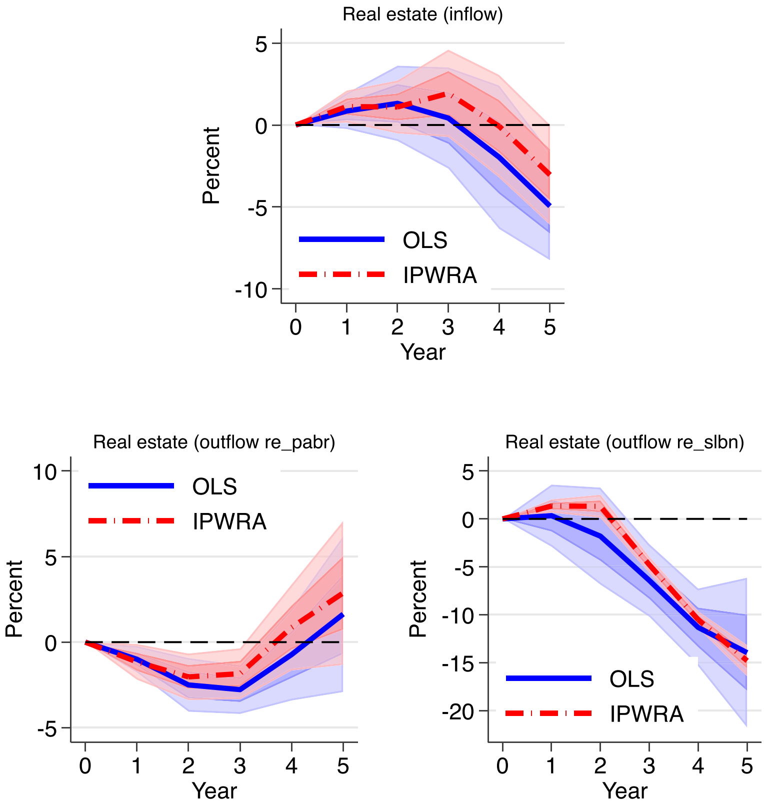

Fig. 10 Local Projection: Impact of capital control of real estate transactions on real housing prices by IPWRA estimation. Notes: Y-axes denotes 100×log (real housing price). The explanatory variables for sub-figure are capital control on real estate inflow "rei", purchase abroad by residents "re_pabr", sale locally by non-residents "re_slbn", respectively. The blue and red lines denote the coefficients of cumulative response of real housing price estimated by OLS and IPWRA local projection respectively over 5 years. Shade areas are 1 standard error (dark) and 1.96 standard error (light) Driscoll & Kraay (1998) bands around the response estimates

while for IPWRA estimators, the negative response is -4.090% of real housing prices, roughly four fifth the size of the conditional OLS. Besides, I find that the attenuation effects of the IPWRA estimators are stronger in the long-term, since the gaps between conditional OLS and IPWRA estimators are widening as period goes by. Consistent with Kuvshinov & Zimmermann (2019), I can attribute much of the long-term real housing prices variation to endogenous factors. In addition, the confidence bands of IPWRA estimates are narrower than OLS results, especially for capital control on "re_slbn" which presents less uncertainty.

The Effects on Credit variables

In addition to the impact of capital controls on real housing prices, I also analyze the impact of these policies on credit variables (bank credit and credit to households) since the credit variables can be used to measure the housing demand. Figure 11 presents the results for real bank credit. The response to capital control on real estate transaction "rei" seems to be indistinct after implemented for 2 years, but after 3 years, the negative response starts to be obvious, though the effects are both insignificant for IPWRA and OLS estimators.

The response to capital control on real estate outflow "re_pabr" is positive immediately after implemented and keeps positive over all horizons. The coefficients of

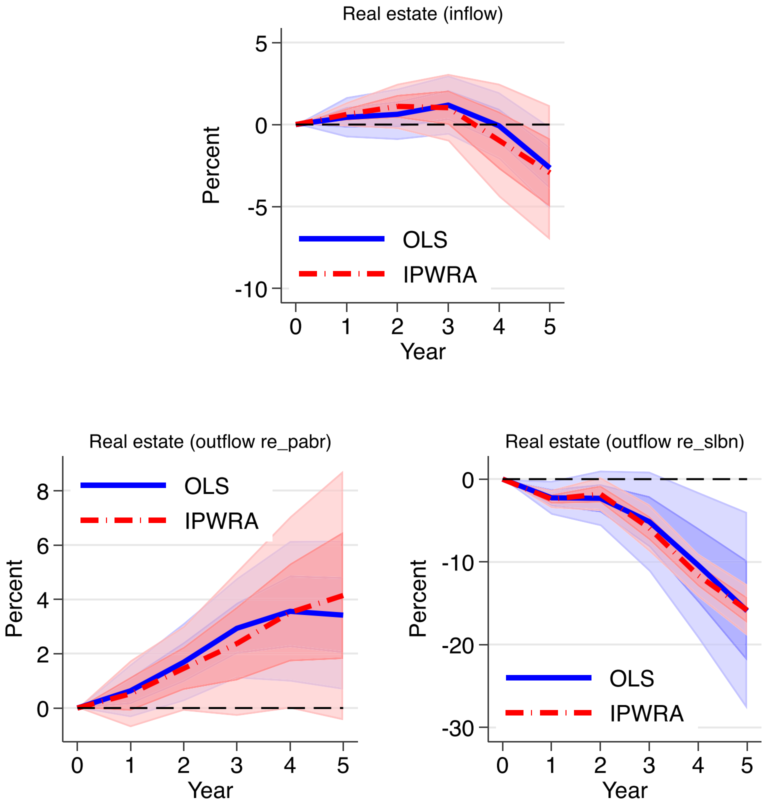

Fig. 11 Local Projection: Impact of capital control of real estate transactions on real bank credit to non-financial sector by IPWRA estimation. Notes: Y-axes denotes 100×log (real bank credit). The explanatory variables for sub-figure are capital control on real estate inflow "rei", purchase abroad by residents "re_pabr", sale locally by non-residents "re_slbn", respectively. The blue and red lines denote the coefficients of cumulative response of real bank credit to non-financial sector estimated by OLS and IPWRA local projection respectively over 5 years. Shade areas are 1 standard error (dark) and 1.96 standard error (light) Driscoll & Kraay (1998) bands around the response estimates

OLS are statistically distinguished from 0 after 2 years. The response is different from the one for real housing prices (see Fig. 10) that the response of real housing prices decreases initially and then changes to positive after 4 years. This may relate to the fact that preventing the domestic investors from investing in foreign housing market makes them have no choice but invest in the local housing market. The rising of credit predicts the future increasing of real housing prices. As for capital control on real estate outflow "re_slbn", the negative effects of real bank credit are strong both for OLS and IPWRA estimators, and IPWRA results have stronger effects than OLS. In addition, the confidence bands are narrower for IPWRA, resulting the significant coefficients for all periods.

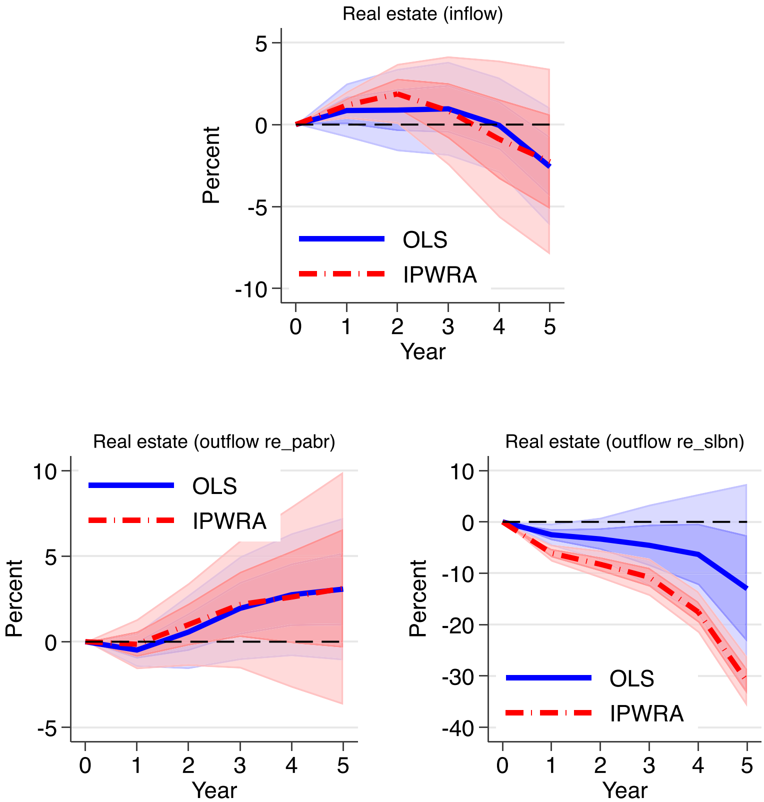

Fig. 12 Local Projection: Impact of capital control of real estate transactions on real credit to households and NPISHs by IPWRA estimation. Note: Y-axes denotes 100×log (real credit to households). The explanatory variables for sub-figure are capital control on real estate inflow "rei", purchase abroad by residents "re_pabr", sale locally by non-residents "re_slbn", respectively. The blue and red lines denote the coefficients of cumulative response of real credit to households and NPISHs estimated by OLS and IPWRA local projection respectively over 5 years. Shade areas are 1 standard error (dark) and 1.96 standard error (light) Driscoll & Kraay (1998) bands around the response estimates

Similarly, the results for real credit to households are reported in Fig. 12. Compared with the results for real bank credit, I find that the response to capital control on real estate inflow “rei” is indistinct and insignificant for all periods and for both estimators. The response to capital control on real estate outflow “re_pabr” is similar to the response of real bank credit (see Fig. 11, lower left), while the response is indistinct over all periods and both estimators are insignificant. The response to capital control on real estate outflow “re_slbn” shows negative effects for OLS estimators but the coefficients are not significant. After I rebalance the sample by IPWRA method, the negative response is significant and larger than its OLS counterpart.

Conclusion

In this paper, I study the effects of capital control polices on real housing prices. My analysis complements the existing literature by using a more granular index of capital control dataset compiled by Fernández et al. (2016) that allow me to study whether capital controls on specific asset types and flow directions are effective in decreasing or dampening the decrease of real housing prices. For this purpose, I estimate the marginal effects of four types of inflow and outflow capital control indices (including restrictions for direct investment “di”, financial credit “fc”, commercial credit “cc”, and real estate transaction “re”) on real housing prices respectively using a large cross-country panel of 53 economies from 1995 to 2017. The model is estimated using a more “misspecification robust” local projection method and I also offer a series of robustness checks including the estimation using the up-to-date Generalized Synthetic Control Method.

Results show that most capital control indices I analyzed in this paper appear to reduce real housing prices even if some of them tend to be insignificant and marginal. Specifically, inflow control measures reduce housing prices and the results are statistically significant in the long-run. As for outflow control measures, although all of them show that they reduce housing prices, most of them are insignificant except capital control on commercial credit outflow “cco” and real estate outflow “re_pabr” and “re_slbn”. Besides, I find that capital controls have asymmetric effects on housing prices for AEs and EMs. As for EMs, capital controls on all types of capital control inflow measures can reduce housing prices, while for AEs, only capital control on commercial credit inflows “cci” and financial credit inflow “fci” can reduce housing prices.

After employing a series of robustness test, results show that the negative effects of housing prices are mainly driven by pre-crisis subsample from capital control on financial credit inflow “fci” and commercial credit inflow “cci”, which are consistent with the previous literature that capital controls have been used several times before GFC. I also estimate the effects for boom and slump periods respectively and find that although capital control policies are implemented in an acyclical way, the inflow controls on direct investment “dii” and financial credit “fci” are implemented in a countercyclical manner to some extent. Last, the estimation of capital control on real estate "re" using Generalized Synthetic Control Method reaches a consensus on the baseline OLS results.

There exists endogenous problem in capital controls on real estate transaction "re", and I address this issue using IPWRA estimator because such method can achieve the random allocation of capital control treatment. I find that the negative response for all capital controls on "re" are weakened after using IPWRA estimators. The attenuation effects of the IPWRA estimators are stronger in the long-term and thus can be attributed much of the long-term real housing prices variation to endogenous factors. Then, I also estimate the response of credit variables to capital control on real estate transactions "re" and study the effects on housing demand. I find that the results of credit to households are similar to bank credit. The effects of capital control on real estate inflow "rei" and outflow "re_slbn" can reduce the volume of both credits, but the effects of "rei" are relatively insignificant. However, the response to "re_pabr" is positive for all horizons. This may relate to the fact that preventing the domestic investors from investing in foreign housing market makes them have no choice but invest in the local housing market.

Based on these empirical findings, policymakers can mitigate the rise in housing prices by implementing inflow control in different capital markets, but the timing of implementation and the targeted capital markets need to be accurately chosen, as the effects of these policies take time to materialize and vary in magnitude. Caution is needed when using capital outflow controls to manage housing prices, because on one hand, the effects of some policies are not significant enough, and on the other hand, different outflow control policies might lead to opposite outcomes.[20]

A key limitation of this paper is the difficulty to account for the effects of outflow controls on housing prices. Even if I have explained that outflow controls can prevent capital flight and then avoid the housing price plummet, there also have illegal methods to circumvent these restrictions, and restrcition on outflow may also deter inflows and further decrease housing demand. Therefore, there are several opposing effects and I cannot measure them accurately, and the mechanism between outflow control and housing prices are ambiguous. Indeed, most of previous studies only focus on the effects of inflow measures (Ahmed & Zlate, 2014; Beirne & Friedrich, 2017; Bruno et al., 2017; Frost et al., 2020). These should be solved in the future research.

[20] In fact, The AREAER indicates a trend towards loosening restrictions on capital flows associated with real estate transactions between 2012 and 2018, with a slightly greater emphasis on reducing restrictions for resident outflows compared to non-resident inflows (Everaert, 2020).

Appendix A Figures and Tables

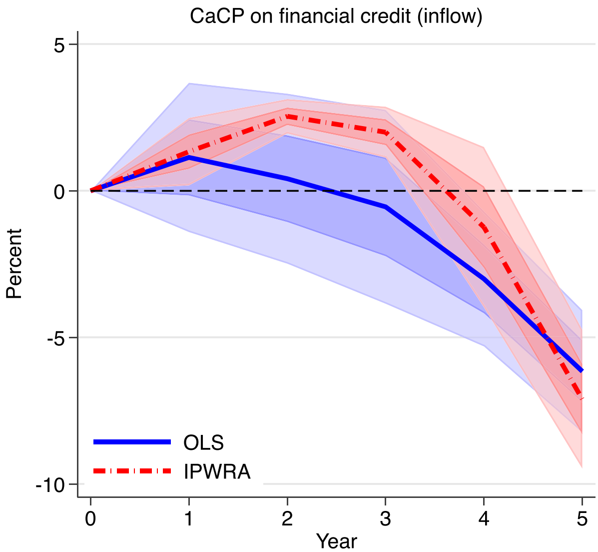

Fig. 13 Local Projection: Impact of capital control of financial credit on real housing prices by IPWRA estimation. Notes: Y-axes denotes 100×log (real housing price). The explanatory variables is capital control on financial credit inflow "fci". The blue and red lines denote the coefficients of cumulative response of real housing price estimated by OLS and IPWRA local projection respectively over 5 years. Shade areas are 1 standard error (dark) and 1.96 standard error (light) Driscoll & Kraay (1998) bands around the response estimates

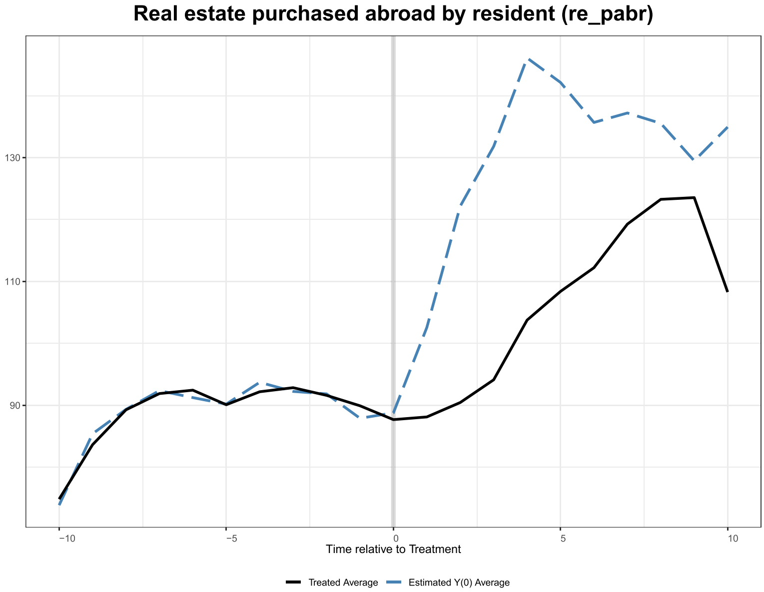

Fig. 14 Generalized Synthetic Control Method: Impact of capital control of real estate purchased abroad by resident ("re_pabr") on housing prices. Notes: The figure shows the average treatment effect of capital control of real estate purchased abroad by resident ("re_pabr") on housing prices for the treated countries (ATE). The outcome values refer to 10 years ($T_0 + 10$) after the treatment year (T_0). The estimated values based on the synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include CPI, real effective exchange rate (REER), cross border loan, real GDP growth, nominal exchange rate, population, international debt-to-GDP ratio, and bank leverage. The optimal number of factors is selected using cross validation to minimize the MSPE

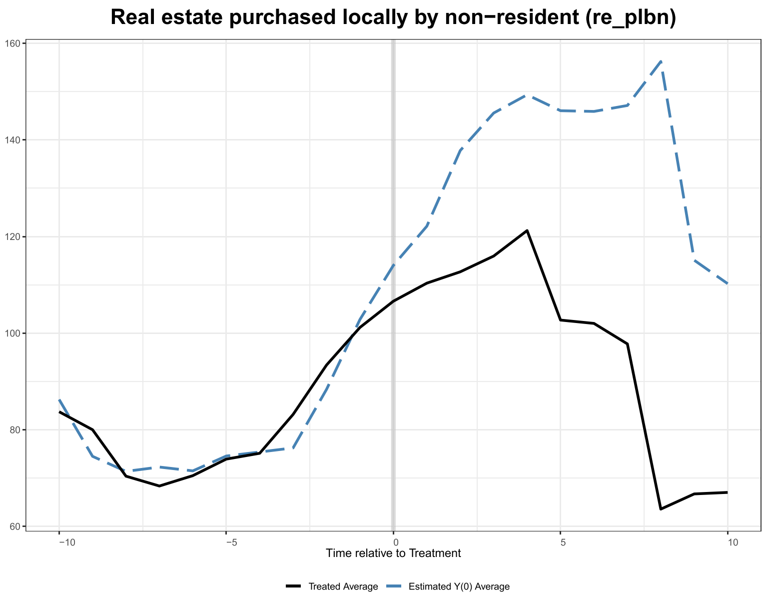

Fig. 15 Generalized Synthetic Control Method: Impact of capital control of real estate purchased locally by non-resident ("re_plbn") on housing prices. Notes: The figure shows the average treatment effect of capital control of real estate purchased locally by non-resident ("re_plbn") on housing prices for the treated countries (ATE). The outcome values refer to 10 years ($T_0 + 10$) after the treatment year (T_0). The estimated values based on the synthetic controls are computed using a two-way fixed effects model that accounts for unobserved country-specific and time-specific confounders. Standard errors are based on 1,000 parametric bootstraps at the country level. The covariates include CPI, real effective exchange rate (REER), cross border loan, real GDP growth, nominal exchange rate, population, international debt-to-GDP ratio, and bank leverage. The optimal number of factors is selected using cross validation to minimize the MSPE

Table 7 Data Coverage by Country

	Housing prices		Bank credit		Credit to households	
	Start year	End year	Start year	End year	Start year	End year
Argentina	.	.	1995	2017	1995	2017
Australia	1995	2017	1995	2017	1995	2017
Austria	2000	2017	1995	2017	1995	2017
Belgium	1995	2017	1995	2017	1995	2017
Brazil	2001	2017	1995	2017	1995	2017
Bulgaria	2005	2017	.	.	.	.
Canada	1995	2017	1995	2017	1995	2017
Chile	2002	2017	1995	2017	2002	2017
China	2005	2017	1995	2017	2006	2017
Colombia	1995	2017	1995	2017	1996	2017
Cyprus	2002	2017	.	.	.	.
Czech Republic	2008	2017	1995	2017	1995	2017
Denmark	1995	2017	1995	2017	1995	2017
Finland	1995	2017	1995	2017	1995	2017
France	1995	2017	1995	2017	1995	2017
Germany	1995	2017	1995	2017	1995	2017
Greece	2006	2017	1995	2017	1995	2017
Hong Kong	1995	2017	1995	2017	1995	2017
Hungary	2007	2017	1995	2017	1995	2017
Iceland	2000	2017	.	.	.	.
India	2009	2017	1995	2017	2007	2017
Indonesia	2002	2017	1995	2017	2001	2017
Ireland	1995	2017	1995	2017	2002	2017
Israel	1995	2017	1995	2017	1995	2017
Italy	1995	2017	1995	2017	1995	2017
Japan	1995	2017	1995	2017	1995	2017
Korea	1995	2017	1995	2017	1995	2017
Latvia	2006	2017	.	.	.	.
Malaysia	1995	2017	1995	2017	2006	2017
Malta	2005	2017	.	.	.	.
Mexico	2005	2017	1995	2017	1995	2017
Morocco	2006	2017	.	.	.	.
Netherlands	1995	2017	1995	2017	1995	2017
New Zealand	1995	2017	1995	2017	1995	2017
Norway	1995	2017	1995	2017	1995	2017
Peru	1998	2017	.	.	.	.
Philippines	2008	2017	.	.	.	.

Table 7 continued

	Housing prices		Bank credit		Credit to households	
	Start year	End year	Start year	End year	Start year	End year
Poland	2010	2017	1995	2017	1995	2017
Portugal	2008	2017	1995	2017	1995	2017
Romania	2009	2017	.	.	.	.
Russia	2001	2017	1995	2017	1998	2017
Saudi Arabia	.	.	1995	2017	1998	2017
Singapore	1998	2017	1995	2017	1995	2017
Slovenia	2007	2017	.	.	.	.
South Africa	1995	2017	1995	2017	2008	2017
Spain	1995	2017	1995	2017	1995	2017
Sweden	1995	2017	1995	2017	1995	2017
Switzerland	1995	2017	1995	2017	1999	2017
Thailand	1995	2017	1995	2017	1995	2017
Turkey	2010	2017	1995	2017	1995	2017
United Arab Emirates	2003	2017	.	.	.	.
United Kingdom	1995	2017	1995	2017	1995	2017
United States	1995	2017	1995	2017	1995	2017
N=53						

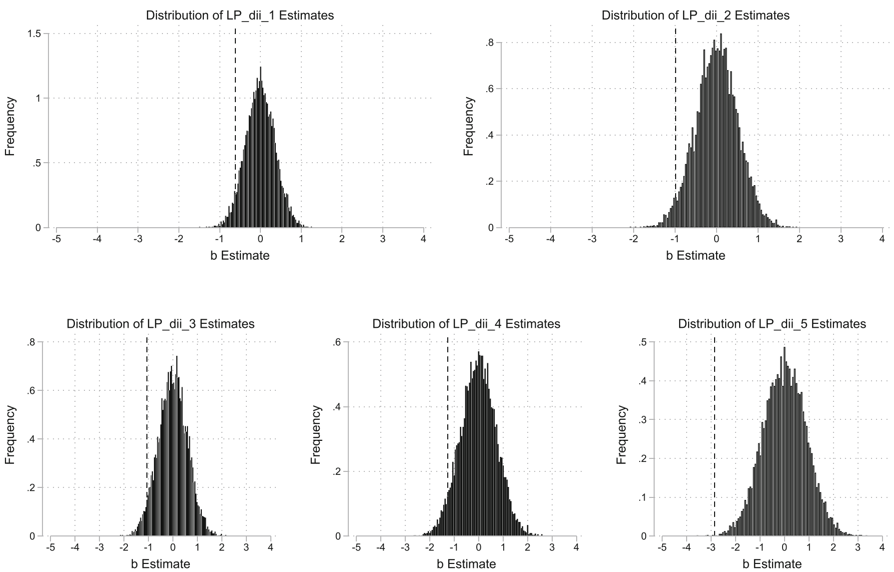

Fig. 16 Distribution of ATEs of placebo test: Impact of capital control of direct investment inflow ("dii") on housing prices. Notes: This figure displays the distribution of the conditional ATEs of randomly generated capital control of direct investment inflow ("dii"). The simulations are based on the Local projection OLS used to estimate the results in Table 3 and Fig. 2. The dotted vertical line marks the ATE from the baseline model. All simulations are based on 10,000 replications

Table 8 Data definitions and sources

Type	Variables	Source	Description
Dependent	Housing price	BIS & CEIC	Residential property price indices, adjusted by CPI
			from 1995 to 2017, annual basis
	Bank credit	BIS	Credit to private non-financial sector from bank, adjusted by CPI
Variables			from 1995 to 2017, annual basis
	Credit to households	BIS	Credit to households and NPISHs from all sectors, adjusted by CPI
			from 1995 to 2017, annual basis
Explanatory	Capital control on "di"	Fernández et al. (2016)	Contains "dii" and "dio" for restriction on inflows and outflows
			0-1 Binary variables, from 1995 to 2017
	Capital control on "fc"	Fernández et al. (2016)	Contains "fci" and "fco" for restriction on inflows and outflows
			0-1 Binary variables, from 1995 to 2017
Variables	Capital control on "cc"	Fernández et al. (2016)	Contains "cci" and "cco" for restriction on inflows and outflows
			0-1 Binary variables, from 1995 to 2017
	Capital control on "re"	Fernández et al. (2016)	Contains "rei" and "re_pabr", "re_slbn" for restriction on inflows
			and outflows 0-1 Binary variables, from 1995 to 2017
Control	Central bank policy rate	BIS	from 1995 to 2017. Other sources: Bulgaria and Morocco (Central bank)
Variables			Singapore (using exchange rate), UAE (using EBIOR)
(Monetary	Short-term interest rate	CEIC	from 1995M1 to 2017M12, averaged. Other sources: Peru (central bank)
Policies)			Brazil (FRED), Chile (FRED)
(MaPP)	LTV caps	Cerutti et al. (2017)	0-1 Binary variables, from 2000 to 2017

Table 8 continued

Type	Variables	Source	Description
(Exchange	Real effective exchange rate	BIS	CPI based, broad indices, annual basis
Rates)	Biliteral nominal interest rate	Penn World Table v9.1	National currency per USD, from 1995 to 2017
	Direct investment (inflow/outflow)	IFS (BOP/IIP)	Data in USD, annual basis, from 1995 to 2017
(Capital	Portfolio investment (inflow/outflow)	IFS (BOP/IIP)	Data in USD, annual basis, from 1995 to 2017
flows)	Other investment (inflow/outflow)	IFS (BOP/IIP)	Data in USD, annual basis, from 1995 to 2017
	Cross border loan from BIS reporting bank	JEDH	Data in USD, annual basis, from 1995 to 2017
	Cross border loan from BIS reporting bank to nonbanks	JEDH	Data in USD, annual basis, from 1995 to 2017
(Global	VIX	CBOE	Daily basis, averaged, from 1995 to 2017
liquidity)	Ted spread	Federal Reserve Bank of St. Louis	Daily basis, averaged, from 1995 to 2017
(Fundamentals)	GDP	Penn World Table v9.1	"rgdpna", adjusted by CPI, annual basis, from 1995 to 2017
	CPI	Penn World Table v9.1	annual basis, from 1995 to 2017, 2010 = 100

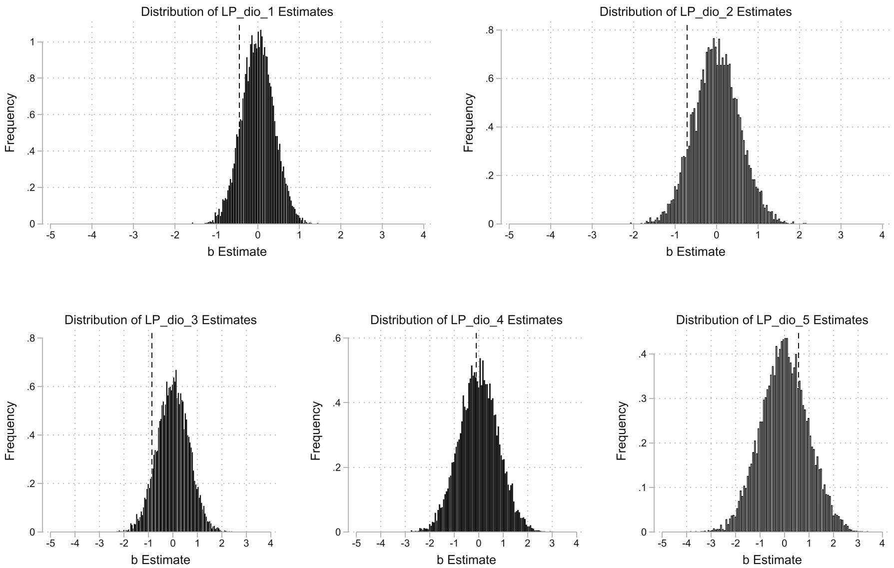

Fig. 17 Distribution of ATEs of placebo test: Impact of capital control of direct investment outflow ("dio") on housing prices. Notes: Notes: This figure displays the distribution of the conditional ATEs of randomly generated capital control of direct investment outflow ("dio"). The simulations are based on the Local projection OLS used to estimate the results in Table 3 and Fig. 2. The dotted vertical line marks the ATE from the baseline model. All simulations are based on 10,000 replications

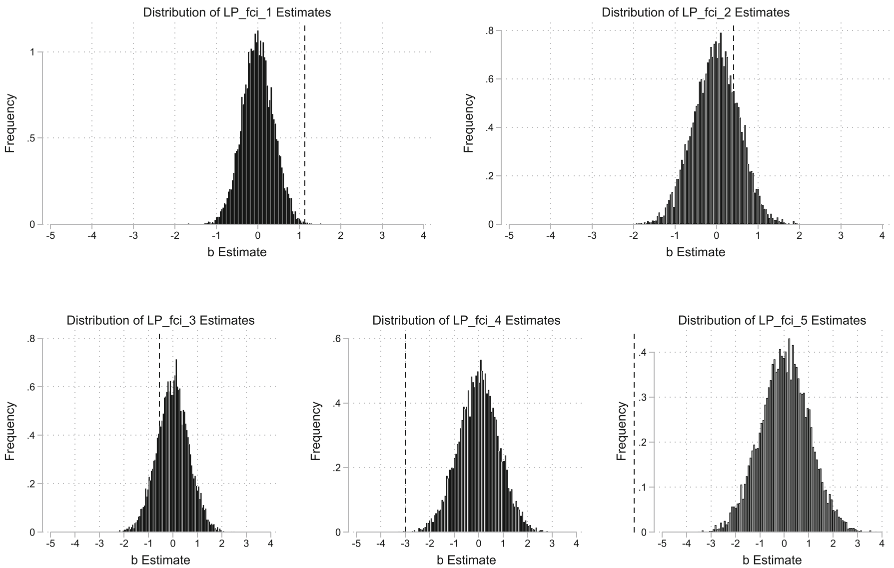

Fig. 18 Distribution of ATEs of placebo test: Impact of capital control of financial credit inflow ("fci") on housing prices. Notes: Notes: This figure displays the distribution of the conditional ATEs of randomly generated capital control of financial credit inflow ("fci"). The simulations are based on the Local projection OLS used to estimate the results in Table 4 and Fig. 3. The dotted vertical line marks the ATE from the baseline model. All simulations are based on 10,000 replications

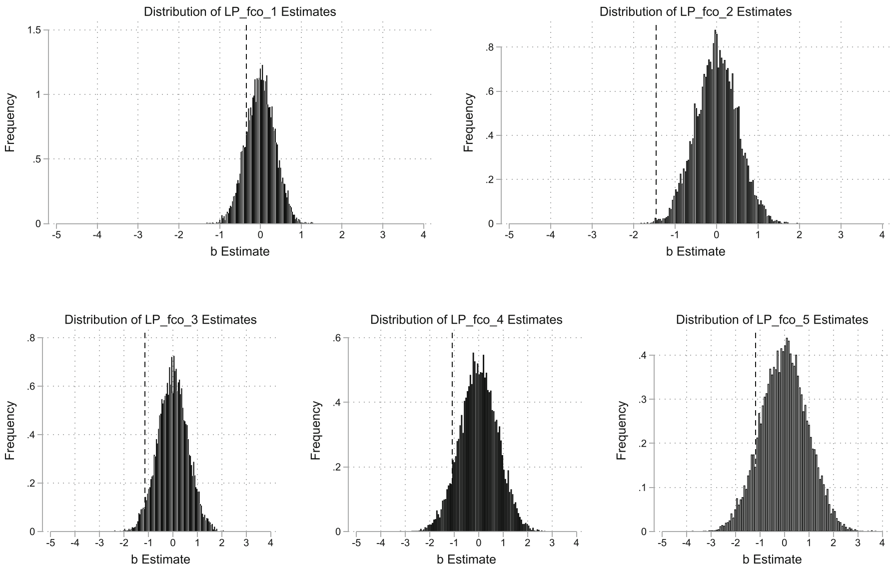

Fig. 19 Distribution of ATEs of placebo test: Impact of capital control of financial credit outflow ("fco") on housing prices. Notes: This figure displays the distribution of the conditional ATEs of randomly generated capital control of financial credit outflow ("fco"). The simulations are based on the Local projection OLS used to estimate the results in Table 4 and Fig. 3. The dotted vertical line marks the ATE from the baseline model. All simulations are based on 10,000 replications

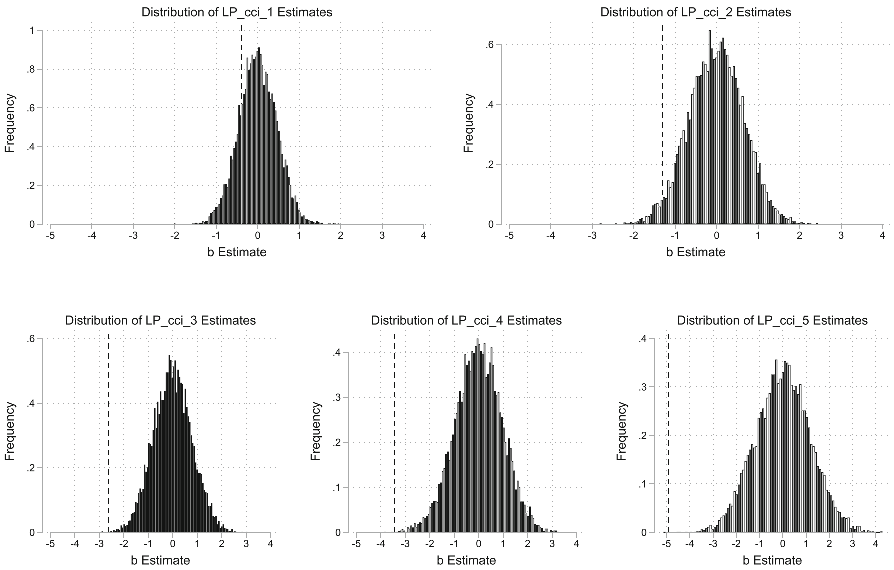

Fig. 20 Distribution of ATEs of placebo test: Impact of capital control of commercial credit inflow ("cci") on housing prices. Notes: This figure displays the distribution of the conditional ATEs of randomly generated capital control of commercial credit inflow ("cci"). The simulations are based on the Local projection OLS used to estimate the results in Table 5 and Fig. 4. The dotted vertical line marks the ATE from the baseline model. All simulations are based on 10,000 replications

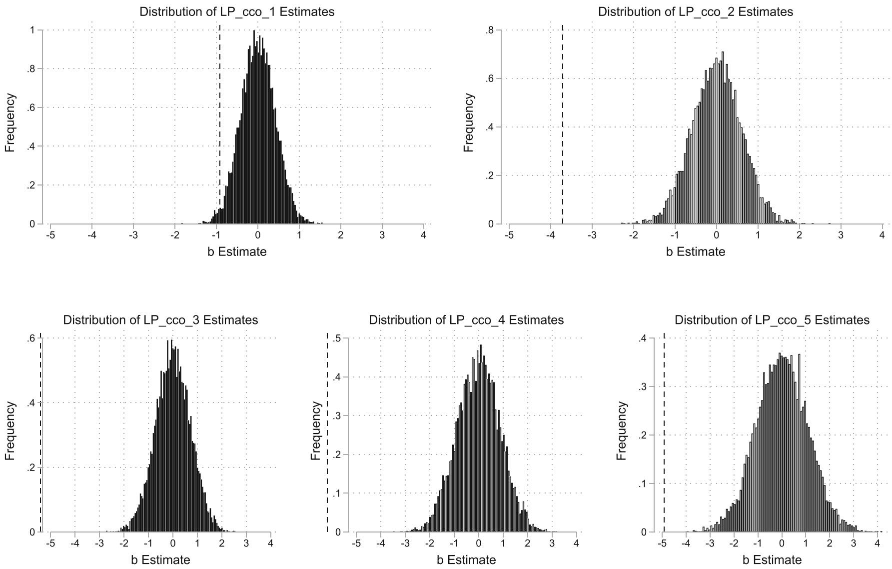

Fig. 21 Distribution of ATEs of placebo test: Impact of capital control of commercial credit outflow ("cco") on housing prices. Notes: This figure displays the distribution of the conditional ATEs of randomly generated capital control of commercial credit outflow ("cco"). The simulations are based on the Local projection OLS used to estimate the results in Table 5 and Fig. 4. The dotted vertical line marks the ATE from the baseline model. All simulations are based on 10,000 replications

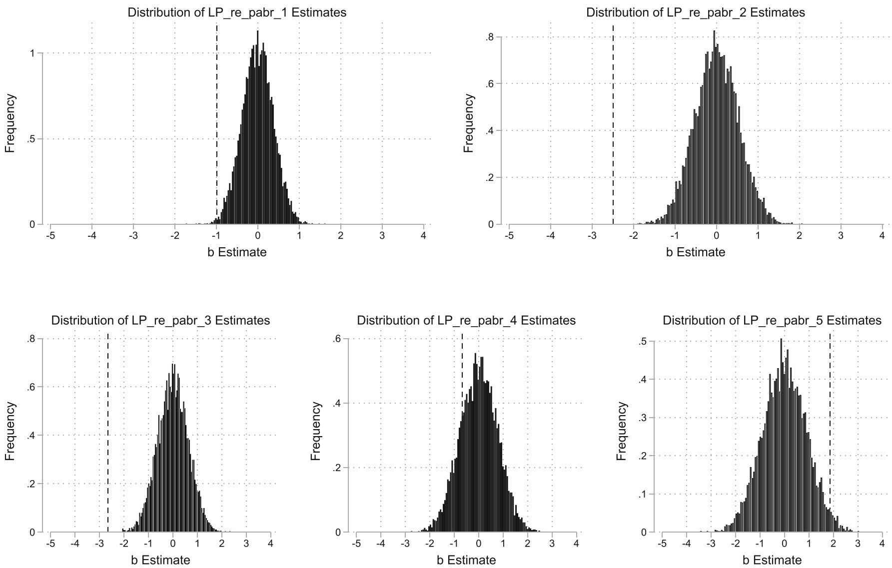

Fig. 22 Distribution of ATEs of placebo test: Impact of capital control of real estate purchase abroad by resident ("re_pabr") on housing prices. Notes: This figure displays the distribution of the conditional ATEs of randomly generated capital control of real estate purchase abroad by resident ("re_pabr"). The simulations are based on the Local projection OLS used to estimate the results in Table 6 and Fig. 5. The dotted vertical line marks the ATE from the baseline model. All simulations are based on 10,000 replications

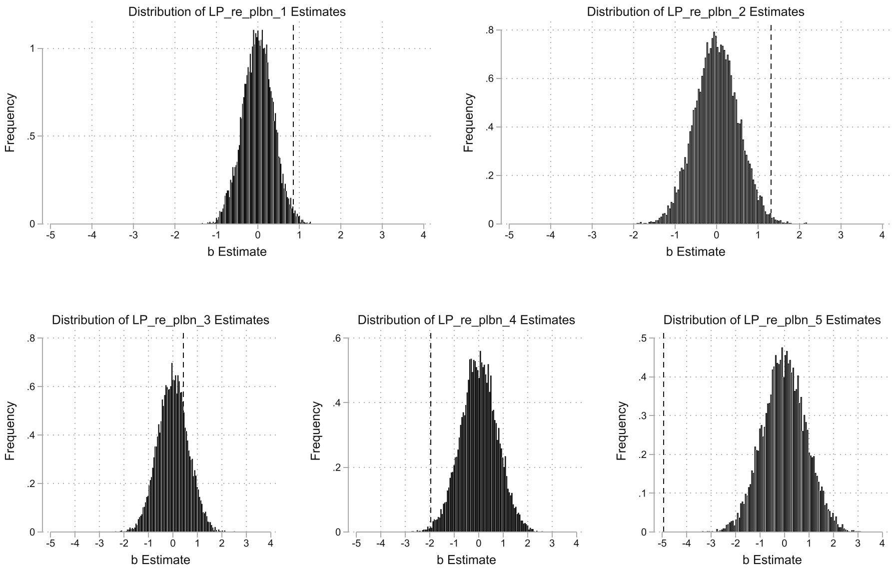

Fig. 23 Distribution of ATEs of placebo test: Impact of capital control of real estate purchase locally by non-resident ("re_plbn") on housing prices. Notes: This figure displays the distribution of the conditional ATEs of randomly generated capital control of real estate purchased locally by non-resident ("re_plbn"). The simulations are based on the Local projection OLS used to estimate the results in Table 6 and Fig. 5. The dotted vertical line marks the ATE from the baseline model. All simulations are based on 10,000 replications

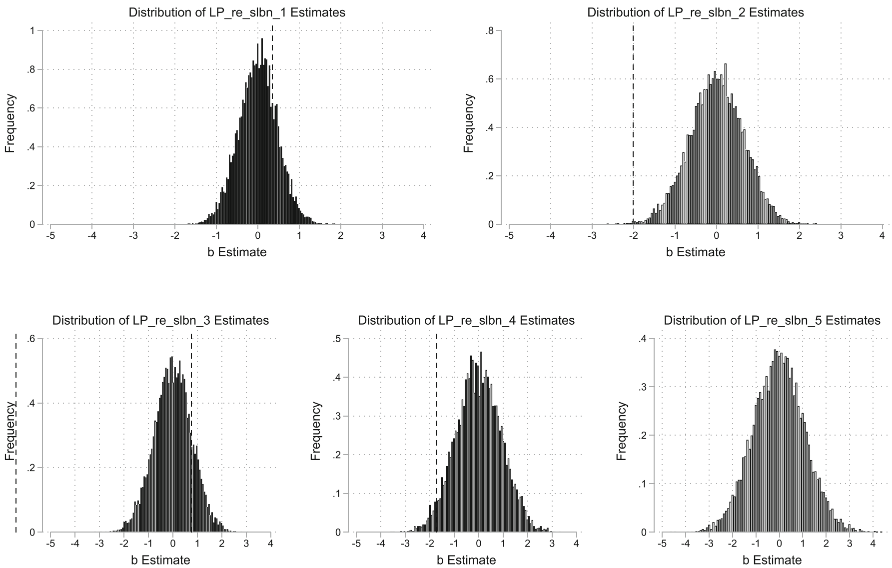

Fig. 24 Distribution of ATEs of placebo test: Impact of capital control of real estate sale locally by non-resident ("re_slbn") on housing prices. Notes: This figure displays the distribution of the conditional ATEs of randomly generated capital control of real estate sale locally by non-resident ("re_slbn"). The simulations are based on the Local projection OLS used to estimate the results in Table 6 and Fig. 5. The dotted vertical line marks the ATE from the baseline model. All simulations are based on 10,000 replications

Table 9 Balance condition test: for control variables

	rei	re_pabr	re_slbn
Real GDP detrended	0.00	-0.00	-0.00**
	(0.00)	(0.00)	(0.00)
Real GDP growth	0.01***	0.01***	0.02***
	(0.00)	(0.00)	(0.00)
CPI detrended	-0.00	0.01*	0.01**
	(0.00)	(0.00)	(0.01)
CPI growth	0.02***	0.02***	0.04***
	(0.01)	(0.01)	(0.01)
Real cross border loan growth	0.03**	-0.04***	-0.01
	(0.01)	(0.01)	(0.01)
Real cross border loan to nonbank growth	0.03***	-0.03***	-0.00
	(0.01)	(0.01)	(0.01)
REER growth	0.01**	-0.00	0.00
	(0.00)	(0.00)	(0.01)
Policy rates	1.17**	1.92***	4.34***
	(0.55)	(0.46)	(0.57)
Short-term interest rates	0.31	1.54***	2.85***
	(0.51)	(0.53)	(0.67)
Direct investment (inflow) to GDP ratio	0.03**		
	(0.01)		
Portfolio investment (inflow) to GDP ratio	0.00		
	(0.01)		
Other investment (inflow) to GDP ratio	0.02		
	(0.01)		
VIX	-0.30	0.22	0.29
	(0.36)	(0.37)	(0.46)
Direct investment (outflow) to GDP ratio		-0.02	0.00
		(0.01)	(0.02)
Portfolio investment (outflow) to GDP ratio		-0.05***	0.03***
		(0.01)	(0.01)
Other investment (outflow) to GDP ratio		-0.03***	0.00
		(0.01)	(0.01)
Observations	1218	1216	1193

Notes: Each column describes the mean difference between treatment and control group. Standard errors are in parentheses. *, **, *** indicate significant at 10%, 5%, 1% levels respectively

Table 10 Pooled probit estimation of credit variables: prediction of capital control variables

	rei	re_pabr	re_slbn
Real housing price	0.698***	0.455***	0.156***
	(0.088)	(0.088)	(0.054)
Real bank credit	-0.117**	0.178***	0.075***
	(0.053)	(0.049)	(0.028)
Real credit to households	0.071	-0.194***	-0.062**
	(0.054)	(0.049)	(0.028)
Real GDP detrended	-4.888***	-1.349	-0.916
	(1.029)	(0.969)	(0.582)
Real GDP growth	5.033***	2.873***	2.288***
	(0.809)	(0.723)	(0.434)
CPI detrended	-1.454	1.001	0.090
	(1.190)	(0.988)	(0.470)
CPI growth	1.946	-0.403	0.703
	(1.218)	(1.091)	(0.598)
Real cross border loan growth	0.026	0.063	-0.240**
	(0.190)	(0.172)	(0.108)
Real cross border loan to nonbank growth	0.134	-0.113	0.263***
	(0.170)	(0.153)	(0.102)
REER growth	0.075	-0.616*	-0.000
	(0.372)	(0.332)	(0.184)
Policy rates	0.012	0.006	0.001
	(0.009)	(0.008)	(0.004)
Direct investment (inflow) to GDP ratio	-0.481**		
	(0.226)		
Portfolio investment (inflow) to GDP ratio	0.001		
	(0.217)		
Other investment (inflow) to GDP ratio	-0.030		
	(0.160)		
VIX	0.004	0.002	0.004**
	(0.003)	(0.003)	(0.002)
Direct investment (outflow) to GDP ratio		-0.223	-0.420*
		(0.242)	(0.225)
Portfolio investment (outflow) to GDP ratio		-0.952***	-0.065
		(0.349)	(0.148)
Other investment (outflow) to GDP ratio		-0.262	0.225*
		(0.194)	(0.135)
Observations	619	619	610
Model AUC	0.728	0.713	0.874
s.e.	0.0194	0.0212	0.0254

Notes: The first row denotes the probit model of capital control treatment variables “rei”, “re_pabr”, and “re_slbn” at $t + 1$ period respectively. The first column is the predictive variables used in these regressions. Standard errors are in parentheses. *, **, *** indicate significant at 10%, 5%, 1% levels respectively

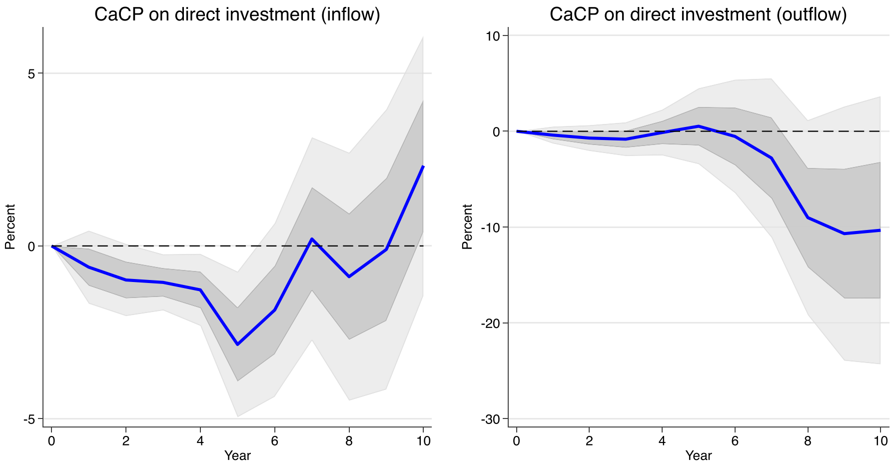

Fig. 25 Local Projection: Impact of capital controls of direct investment on real housing prices, 10 years horizons. Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing prices over 10 years following the changing in capital control of direct investment inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas are 1 standard error (dark) and 1.96 standard error (gray) Driscoll & Kraay (1998) bands around the response estimates

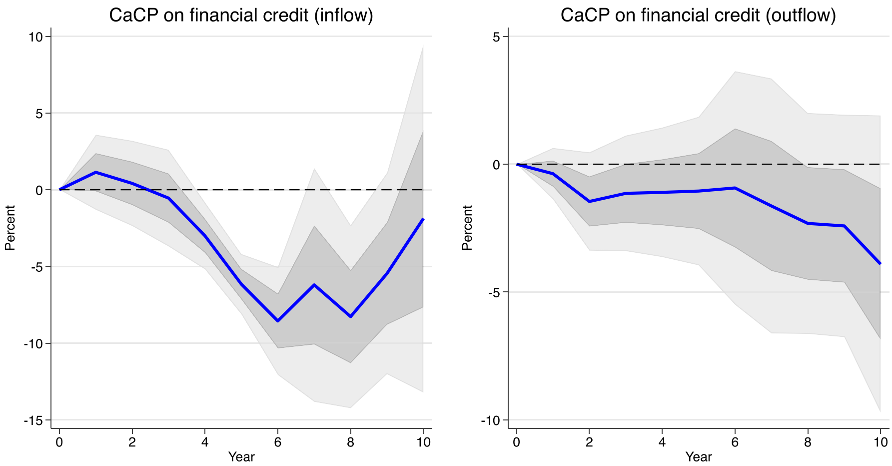

Fig. 26 Local Projection: Impact of capital controls of financial credit on real housing prices, 10 years horizons. Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing prices over 10 years following the changing in capital control of financial credit inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas are 1 standard error (dark) and 1.96 standard error (gray) Driscoll & Kraay (1998) bands around the response estimates

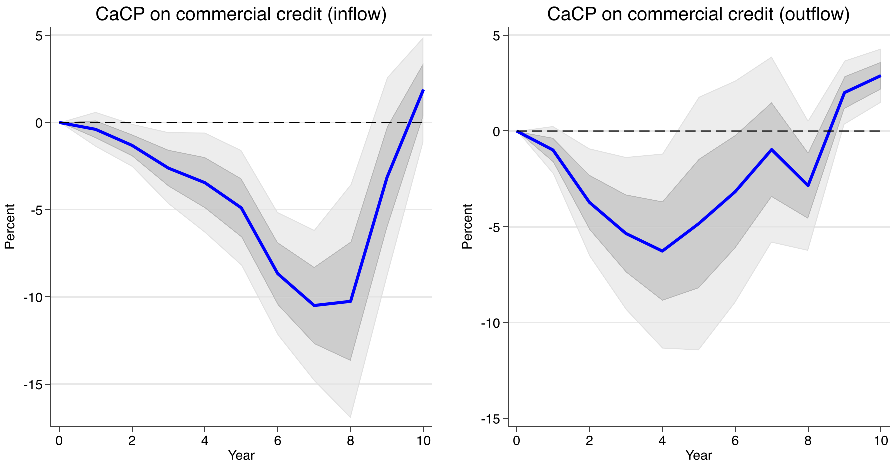

Fig. 27 Local Projection: Impact of capital controls of commercial credit on real housing prices, 10 years horizons. Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing price over 10 years following the changing in capital control of commercial credit inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas are 1 standard error (dark) and 1.96 standard error (gray) Driscoll & Kraay (1998) bands around the response estimates

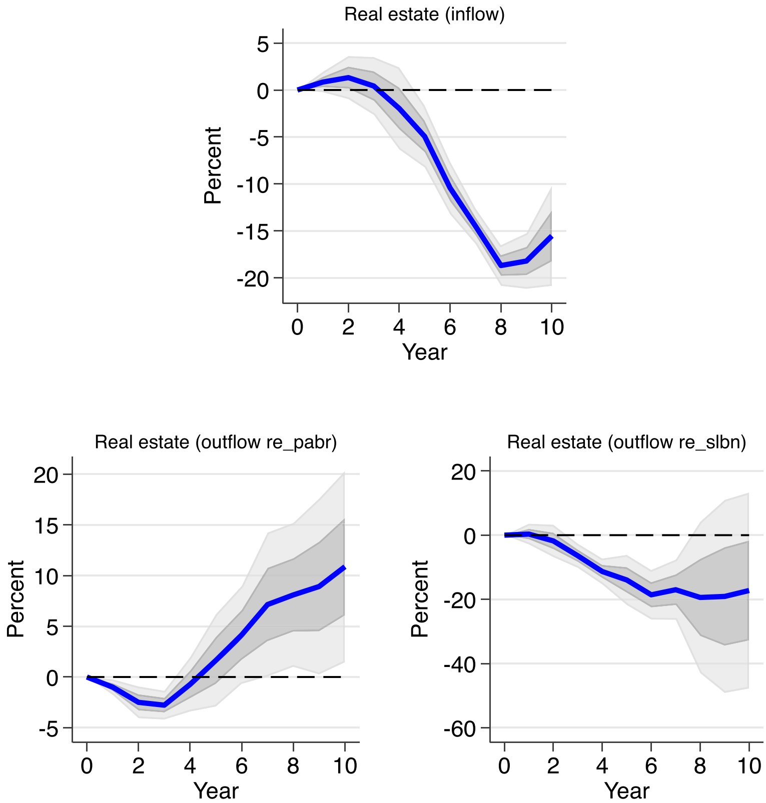

Fig. 28 Local Projection: Impact of capital control of real estate transactions on real housing prices, 10 years horizons. Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing price over 10 years following the changing in capital control of real estate transaction inflow (purchase locally by nonresident) and outflow (purchase abroad by resident, and sale locally by nonresident) from no restriction "0" to restriction "1" respectively. Shade areas are 1 standard error (dark) and 1.96 standard error (gray) Driscoll & Kraay (1998) bands around the response estimates

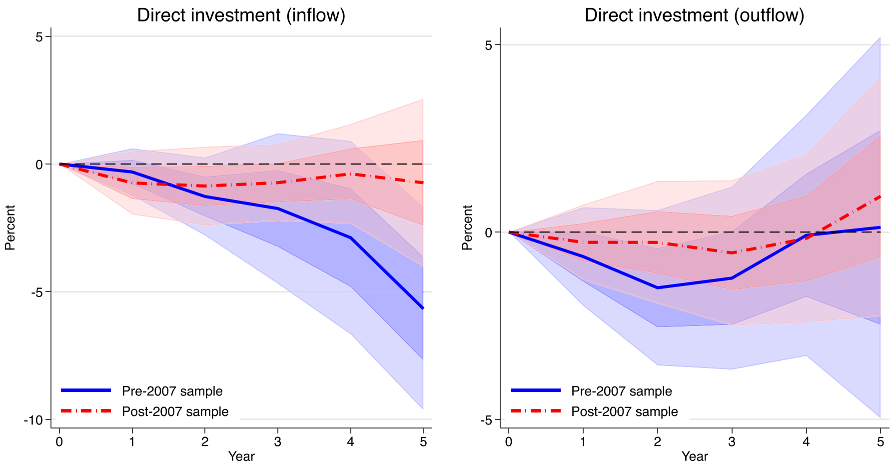

Fig. 29 Local Projection: Impact of capital controls of direct investment on real housing prices – comparison of pre-crisis and post-crisis. Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for pre-crisis period and post-crisis respectively over 5 years following a capital control policy implemented on direct investment

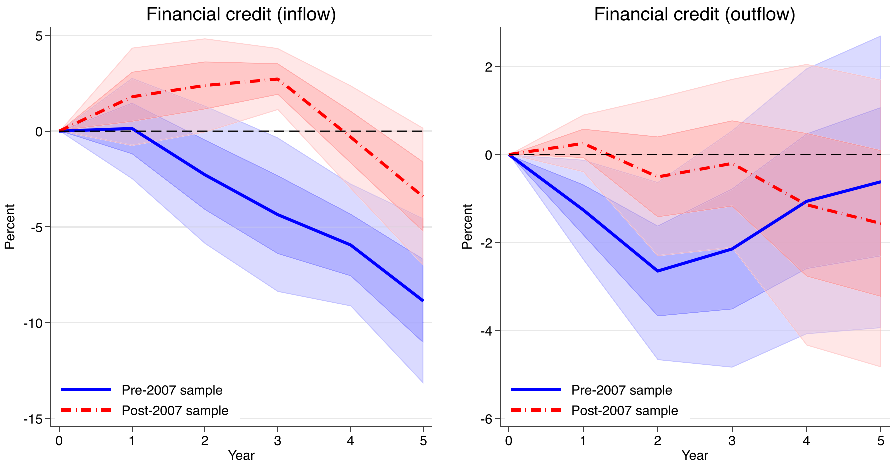

Fig. 30 Local Projection: Impact of capital controls of financial credit on real housing prices – comparison of pre-crisis and post-crisis. Note: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for pre-crisis period and post-crisis respectively over 5 years following a capital control policy implemented on financial credit

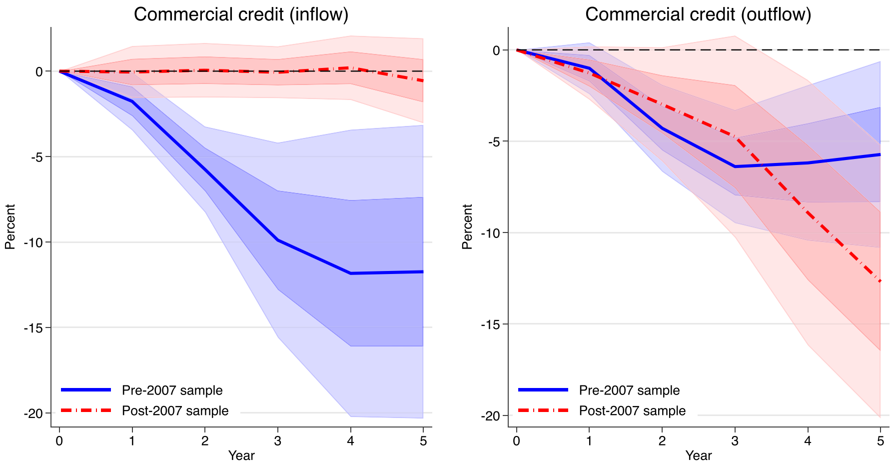

Fig. 31 Local Projection: Impact of capital controls of commercial credit on real housing prices – comparison of pre-crisis and post-crisis. Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for pre-crisis period and post-crisis respectively over 5 years following a capital control policy implemented on commercial credit

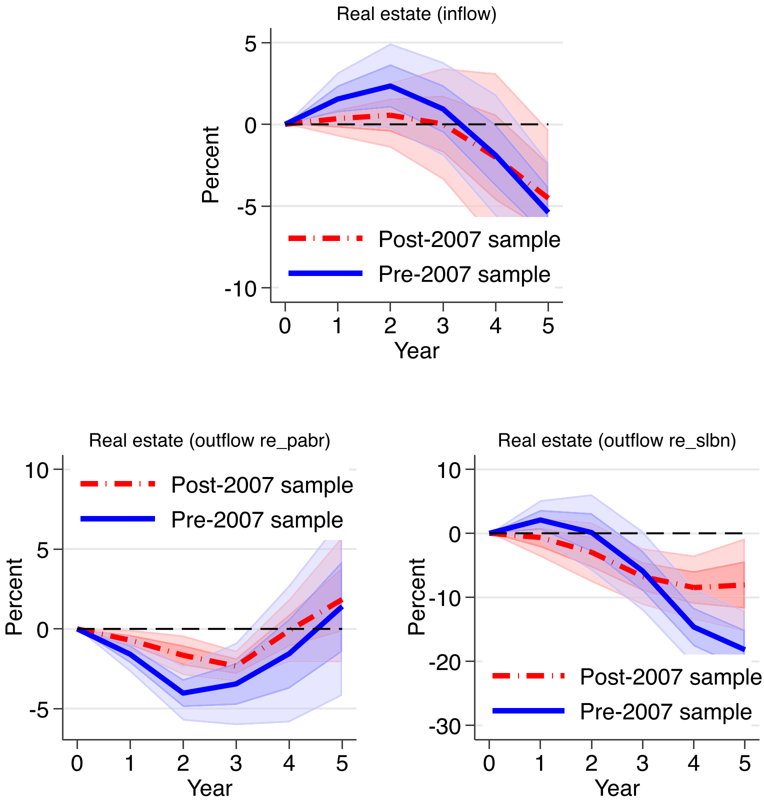

Fig. 32 Local Projection: Impact of capital control of real estate transactions on real housing prices – comparison of pre-crisis and post-crisis. Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for pre-crisis period and post-crisis respectively over 5 years following a capital control policy implemented on real estate transaction

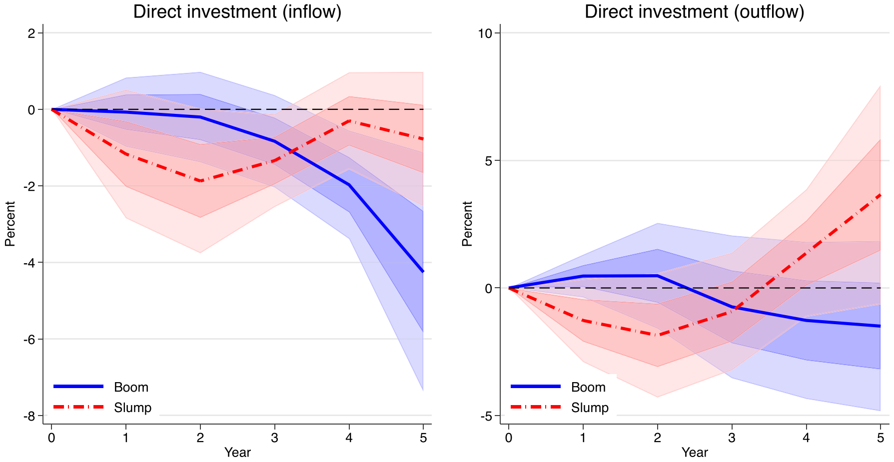

Fig. 33 Local Projection: Impact of capital controls of direct investment on real housing prices – comparison of boom and slump subsamples. Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for boom and slump periods respectively over 5 years following a capital control policy implemented on direct investment

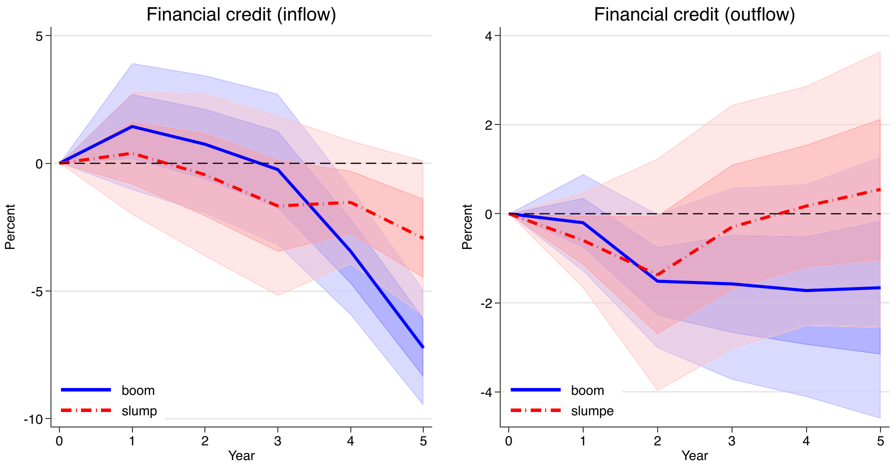

Fig. 34 Local Projection: Impact of capital controls of financial credit on real housing prices – comparison of boom and slump subsamples. Note: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for boom and slump periods respectively over 5 years following a capital control policy implemented on financial credit

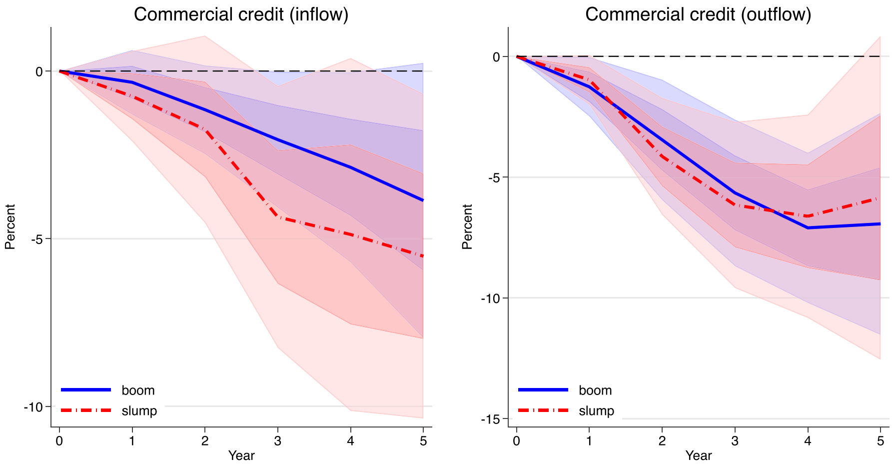

Fig. 35 Local Projection: Impact of capital controls of commercial credit on real housing prices – comparison of boom and slump subsamples. Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for boom and slump periods respectively over 5 years following a capital control policy implemented on commercial credit

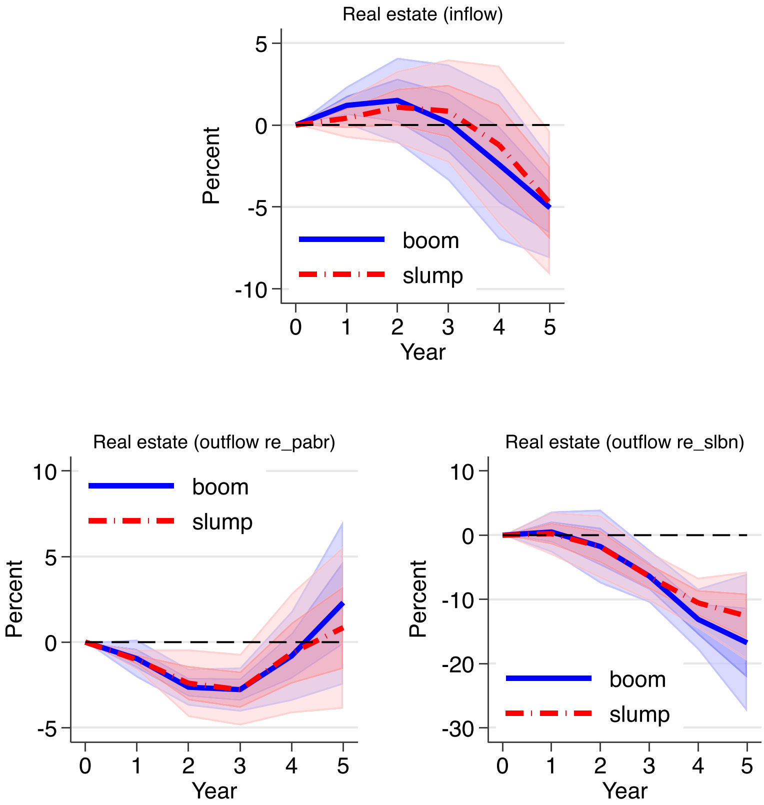

Fig. 36 Local Projection: Impact of capital controls of real estate transaction on real housing prices – comparison of boom and slump subsamples. Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for boom and slump periods respectively over 5 years following a capital control policy implemented on real estate transaction.

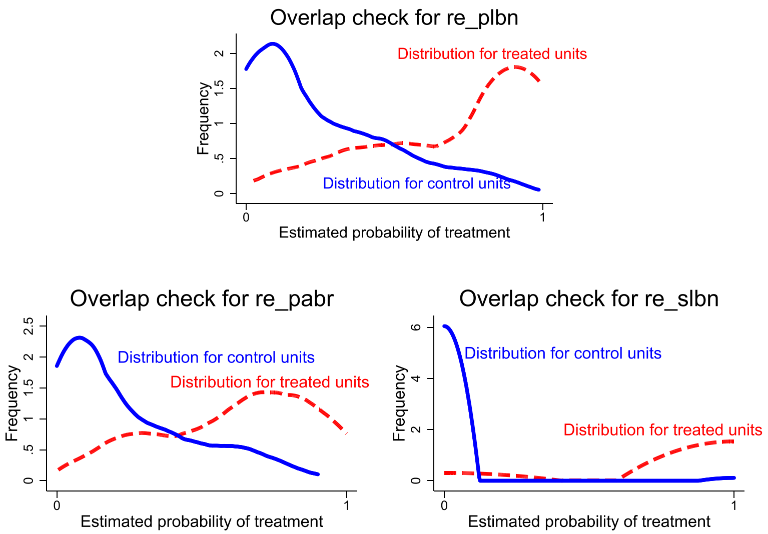

Fig. 37 Overlap test: probit results for capital control on "re". Notes: The red dashed lines denote the empirical density of the predicted probabilities of implementing each capital control on real estate inflow "re_plbn" (namely, "rei"), purchase abroad by residents "re_pabr", and sale locally by non-residents "re_slbn", the blue solid lines display the control observations. The propensity score is estimated using the specification in Table 10 with including country fixed effects

Acknowledgements I would like to thank Kitano, SHIGETO, Masahiko SHIBAMOTO, Yushi YOSHIDA, Taro ESAKA, Kazumichi IWASA, Hugo Vaca Pereira, and two anonymous referees for their useful comments and suggestions. I acknowledge the comments of the participants of the 2021 Annual Meeting (Fall) of Japan Society of Monetary Economics (JSME) and the 80th Annual Meeting of the Japan Society of International Economics (JSIE), 2022th Kanematsu Seminar, and WEAI 97th Annual Conference (Portland Downtown). All errors remain my own. This paper was awarded the 2021 Kanematsu Prize by the RIEB, Kobe University. This paper was supported by JSPS KAKENHI grant Numbers JP23K18796.

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