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The Effects of Capital Controls on Housing Prices*

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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, we study the effects of four types of granular capital control polices on housing prices using a large cross-country panel of 53 economies from 1995 to 2017. We find that the estimated effects of capital controls are distinct for different capital flow types and flow directions, but most capital control indices appear to reduce housing prices. Specifically, we find that capital controls have asymmetric effects on housing prices for advanced and emerging economies. The negative effects of capital controls on housing prices are mainly driven by pre-crisis subsample. This means that capital controls have been in effect several times before Global Financial Crisis. We 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, we further use inverse probability weights to rebalance capital control actions and find that this method can weaken the negative effects on housing prices, and the attenuation effects can be attributed to endogenous factors.

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1 Introduction

The outbreak of COVID-19 pandemic not only hits the worldwide health care system and threatens people's lives, but also gives both demand and supply shock to global economic and spoils the stability of financial system. Responding to the economic consequence and policy-makers' policy intentions, the cross border investors would change their global risk appetite and adjust the capital allocation and quickly rush to more safety markets. As measured by Batini (2020) and OECD (2020), about 103 billion USD were pulled from EMs between mid-January and mid-May 2020, and even 83 billion USD happened in March alone. This phenomenon calls "sudden stop" and has happened several times when country suffers from negative growth shocks. Batini (2020), OECD (2020) and ElFayoumi and Hengge (2021) show that the sudden stop in portfolio flows was of historically large magnitude during similar events in recent years, such as the 2008 Global Financial Crisis (GFC), the 2013 "Taper Tantrum", and 2015 Chinese stock market shock.

Documented by several empirical studies[1], we have known that capital flows can affect 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 Global Financial Crisis. Particularly, as we have depicted in the literature review in section 2, most researchers reach consensus towards the casual relationship between capital inflows and housing price appreciation. 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 and 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 and Moore, 1997; Bruno and Shin, 2014; Cesa-Bianchi et al., 2018).

There are several policy tools in mitigating the volatility of housing prices, such as the

[1]As for the theoretical literature on capital flows and housing prices, Kim and 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 and 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.

monetary and macro-prudential policies. Particularly, macro-prudential policies are proved to be effective in lowering bank credit growth and housing price appreciations (see Akinci and Olmstead-Rumsey, 2018; Kuttner and 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 (CFM) 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 literature of monetary and macro-prudential policies, however, there is very little empirical evidence on the effects of capital controls on housing prices.

In this paper, we explore the effects of capital control polices (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, we 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 macro-prudential 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 macro-prudential policy variables in our regression equations. To verify if capital control policy actions are exogenous to housing prices, we check firstly the objectives of capital control by previous literature, and the we use the balance condition test proposed by Jordà and 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, we calculate the impact of capital control using local projections method developed by Jordà (2005). Our results show that most capital control indices we analyzed in this paper appear to reduce real housing prices. The implementation of capital controls on direct investment inflow (hereafter, "dii") and outflow (hereafter, "dio") can reduce real housing price to some extent but the results are imprecisely estimated. Capital controls on financial credit inflow (hereafter, "fci") and outflow (hereafter, "fco"), and on commercial credit inflow (hereafter, "cci") and outflow (hereafter, "cco") can all reduce housing prices and the effects are significant in long-term for inflow restrictions, while the effects are immediately and statistically significant for restrictions on commercial credit outflow over all horizons.

We also estimate the impacts on real housing prices using capital control on real estate transactions with the loss of preciseness to some extent. Capital controls on real estate inflow (hereafter, "rei") can reduce real housing prices, but the effects are less precisely estimated. Capital controls on real estate outflow (purchase abroad by resident, hereafter "re_pabr") can reduce real housing prices during a short period, but it would increase real housing prices in long-term. The reversal of coefficients can be related to the wresling between the negative effects of capital flight and the positive effects of preventing residents investing in foreign housing market. Capital controls on real estate outflow (sale locally by nonresident, hereafter "re_slbn") can reduce real housing price, and the coefficients are significant after 3 years.

We provide a series of robustness exercises and find that our specification are broadly robust when we 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. In addition, we estimate the effects in pre-crisis and post-crisis subsamples, as well as in boom and slump periods respectively. Results suggest that the negative effects are mainly driven by pre-crisis subsample, and capital controls appear to be acyclical.

Since there exists endogenous problem for capital controls on real estate transaction ("re"), we address this issue using inverse probability weighted regression adjusted (IPWRA) estimator. As depicted by Jordà and Taylor (2016) , we 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, we apply local projections to the rebalanced sample and obtain the IPWRA estimators. We find 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 long-term and thus we can attribute much of the long-term real housing price variation to endogeneous factors. In addition, as the house demand proxy variables, we also estimate the response of two credit variables ("credit to households" and credit to households"") to capital control on real estate transactions. The effects of capital control on real estate transaction inflow ("rei") and real estate transaction 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: section 2 reviews the empirical literature related to capital flows, capital controls and housing prices. Section 3 describes the data and identification strategies used in our estimation. Section 4 presents the methodology and empirical results on the response of real housing prices to the implementation of capital controls on direct investment, financial credit, commercial credit, and real estate transaction. Section 5 presents a series of robustness exercises. In section 6, we further consider the endogeneity problem and estimate the response of financial variables to capital control on real estate transaction by IPWRA method. Section 7 summarizes the main conclusions.

2 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 Global Financial Crisis, 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 and Jinjarak (2009), Gete-Sanchez (2015), Laibson and 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 and 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 and 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, Vega (2019), Kim and Yang (2009) analyze the impact of both FDI and portfolio investment. Vega (2019) 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 and 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.

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 and Radde (2010) use the summation of U.S. monetary base and world international reverses as global liquidity and estimates a VAR model for G7, they find a similar result as Belke et al. (2010) where 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, 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

[2]As for the relationship between current account and capital flows, Borio and 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.

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, 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 and Yang (2009) find that capital inflow shocks lead to the stock price increasing in Korea, but 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 and Zlate (2014) and Landi and 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 and 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 and 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 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 macro-prudential

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 macro-prudential policies are effective in reducing the volume of cross-border bank flows, while macro-prudential 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 macro-prudential 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 macro-prudential 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. Besides, Baba and Kokenyne (2011), and Forbes and Warnock (2012) both find the insignificance effect of capital controls on capital inflows. Baba and Kokenyne (2011) 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.

There are very few studies on how the implementing of capital control policies affect housing prices. Ohno and 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 and 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.

Our study complements the above discussions in two ways. First, as in Ohno and Shimizu (2015), they use a more general and aggregated index "KAOPEN" developed by Chinn and Ito (2008) that is not specific to restrictions on real estate transaction. Banti and Phylaktis (2019) use more granular indices of capital control developed by Fernández et al. (2016) on real estate transactions. We also study the effects of capital control policies on housing prices using Fernández et al. (2016)'s dataset. However, we analyze not only the effects of capital

controls on inflows and outflows respectively, but also various types of capital control policies that may influence real housing prices. Besides, we estimate the impulse response using a more "mis-specification robust" Local projection (LP) method. Second, we check the endogeneity of Fernández et al. (2016)'s dataset using econometric method. For further causal identification, we employ the "inverse probability weighted regression-adjusted (IPWRA) estimator" to analyze the response of financial variables to an endogenous capital control policy on real estate transaction.

3 Data and Identification Strategy

3.1 Data description

We 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), macro-prudential 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 1 presents the detailed information of source and full description of the data sample, and Table 2 reports summary statistics for key variables used in this paper.

3.1.1 Dependent variables

According to Akinci and Olmstead-Rumsey (2018) and Richter et al. (2019), we choose housing price appreciation in section 4 and 6, bank credit growth, and households credit growth in section 6 as our dependent variables since these variables have often been the objective of

macro-prudential policy and this paper tests the effects of the implementing of capital control policies on the objectives of macro-prudential policies. 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. We collect data over 1995 to 2017 subjects to the data availability. Housing price data cover 51 countries, including 31 advanced economies and 20 emerging markets[3].

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 and Phylaktis (2019) also supplement the BIS dataset with other sources for longer time series. However, as pointed out by Vega (2019), collecting data from different sources may generate comparability and compatibility problems since data from different sources are usually compiled in different ways. Thus, we only employ the BIS dataset, which is more suitable for cross-country comparison.

Credit data

We also choose credit as the other dependent variables for measuring financial cycles. Besides, as noted by Vega (2019), since the not availability of housing demand variable such as residential investment for most of the emerging markets, we use credit data as the proxy variables for housing demand. In this paper, we use two types of credit data: "bank credit" and "credit to households". We 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. We also choose the time period from 1995 to 2017 for 42 economies [4].

[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.

[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

3.1.2 Explanatory variables

The capital control measures used in this paper 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 and Ito (2008)[6] 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. Fernández et al. (2016)'s dataset is an 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. We choose 53 countries according to the availability of dependent variables.

To measure the effects of capital controls on housing prices, we choose four types of categories closely associated with them:

1. Capital controls on direct investment: these category contain direct investment control on inflows ("dii"), and direct investment control on outflows ("dio"). We 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 (see Vega, 2019; Feng et al., 2017; Chow and Xie, 2016; Kim and 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: these indices contain 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 related with international trade transactions or with the rendering of international service.

   (b) Capital controls on financial credit: these indices also include capital control on inflows ("fci"), and on outflows ("fco"). Different from capital controls on commer-

(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 and Ito (2008) contains data for 182 countries from 1970 to 2018, but this dataset does not contain any granular data on specific assets.

cial 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, we 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: this series of indices are the most direct indicators related with real estate transactions. 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 we 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 we 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 and 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, it is inevitable to use an aggregated indicator which may result in

misleading results.

3.1.3 Control variables

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

Policy measures

We consider three types of policy measures: monetary policy rate, macro-prudential 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, we control for monetary policy, macro-prudential policy, and exchange rate shocks in all our specifications.

To measure monetary policy, we 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, we obtain from CEIC database in monthly basis over 1995M1 to 2007M12 and we average the monthly data to annual basis.[8]

The macro-prudential policy used here focuses on "domestically oriented macro-prudential measures" categorized by Bruno et al. (2017), and we 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 macro-prudential literature (see Akinci and Olmstead-Rumsey, 2018; Bruno et al., 2017; Kuttner and Shim, 2016; Richter et al., 2019; Banti and Phylaktis, 2019). This indicator restricts the amount of the loan to a certain portion of collateral value. We obtain this data from the dataset of prudential instruments developed by Cerutti et al. (2017).[9]

With respect to exchange rate policies, we 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

[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, we collect the data by ourselves. For example, we 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 we 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.

available for all 53 countries from the BIS effective exchange rate (EER) indices. As studied by Cesa-Bianchi et al. (2015), Bruno and 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, we 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 other investment from IMF Balance of Payment and International Investment Position Statistics (BOP/IIP). Vega (2019) 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 and 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 and Phylaktis (2019) choose a price measures – the amount outstanding of repos in the US, UK, and Europe. In this paper, we follow the measures of Cesa-Bianchi et al. (2015). These data are easily accessed from Joint External Debt Hub (JEDH) database. We 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, we choose VIX and TED spread as in Cesa-Bianchi et al. (2015) and Banti and Phylaktis (2019), and the bank leverage as in Bruno and Shin (2014). 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. We 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, and also be processed as VIX index. Bank

leverage is defined as the ratio of bank credit to bank deposits, and can be obtained from Global Financial Development Database from 1995 to 2017.

Macroeconomic fundamentals

We choose GDP and CPI as the fundamentals variables. GDP data 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, we employ the BIS consumer price index dataset and supplement it with the FRED, and the data in 2010 are adjusted to 100.

3.2 Identification of capital control policy shocks

As discussed in Kuvshinov and Zimmermann (2019), if we want to measure the causal effects of capital control on housing prices and other credit variables, we 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), we 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 macro-prudential shocks. To solve this problem, we can add monetary policy, macro-prudential policy as control variables in estimation process.

3. The capital control policy actions should not be anticipated.

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

As the first step, we 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) reduce the volume of capital flow, (ii) reduce 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) study whether governments systematically impose capital controls in a countercyclical fashion. 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 implement capital control policies. Thus, we will formally examine the relationship between capital control policies and financial variables using balance condition test proposed by Jordà and Taylor (2016). It should be noted that in the ideal randomized controlled trial, the probability density of treatment and control group should be the same. The simple way to test this condition is to compare the mean of those subpopulations and test their equality.

Table 3 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), we 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.

In the next section, we 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, other capital control policies will be considered as exogenous events[10].

[10]Capital control on financial credit inflow ("fci") also appears endogenous to real housing prices, thus we also calculate the marginal effects using IPWRA method in section 4 for "fci".

4 The effects of implementing capital control policies

In order to calculate the marginal effects of capital control policies, we 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).

In particular, we characterize the impulse response of housing prices to capital control policies as

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

where $HP_{t+h} - HP_t$ denotes the conditional forecast cumulative change of housing prices from the period t the capital control policies are implemented to a future h periods (years) later. $CaCP_t$ is the capital control index which is a binary variable that is 1 for capital control policy implemented at period t, and 0 otherwise. Ω_t is the available information set at period t.

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

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

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. α_i^h is the country dummies which are used to control for the country-specific growth. γ_t^h represents the 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, VIX. $HP_{i,t}^c$ 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 $\lambda = 100$.

4.1 The effects of capital control on direct investment

The results of estimating equation (1) using the capital control policies of direct investment ("dii" and "dio") are reported in Table 4 and Figure 1. The left and right panels depict the cumulative response of real housing price ($\times 100$) 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 Figure 1 shows that the response of changing to capital control on direct investment inflow is 2.852% lower of real housing price after 5 years. However, this response is rather imprecisely estimated and not significant over 5 years as depicted by zero axis getting across the light gray area. As for right panel of Figure 1, the data also support the negative relationship between capital control and real housing prices, whereas the impacts are still less precisely estimated for over 5 years. As we mentioned above, Tillmann (2013), Chow and Xie (2016), Feng et al. (2017), and Vega (2019) all find that the FDI inflows contribute to higher housing prices. The less evident results can be related to the fact that capital controls cannot completely block the direct investment inflow to each economies. This is also in line with the results of Frost et al. (2020), who also find no significant effects of capital control policies on FDI inflows.

As pointed out by Richter et al. (2019), the capital control policies are usually implemented with other policy rules, such as monetary policy and macro-prudential 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, we control the changing of central bank policy rate in equation (1) and then check the response of monetary policy rate to the implementing of capital control. We find that monetary policy rate is not significantly responding to capital controls on direct investment inflows and outflows. Besides monetary policy, not considering macro-prudential policies could also bias our estimation results. Thus we control for macro-prudential proxy variable – the borrower-oriented LTV caps.[11] The results are in line with our baseline findings that both inflow and outflow controls lower real housing prices, and the results are still insignificant for both of them.[12]

[11]The results of Figure 1 and Table 4 do not consider macro-prudential variable since the data are only available from 2000 to 2017. This is also the case for other estimation results.

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

4.2 The effects of capital control on financial credit

In this section, we change the explanatory variables to capital controls on financial credit inflow ("fci") and outflow ("fco") and then estimate the marginal effects using baseline equation (1). The results are presented in Table 5 and Figure 2. 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 Figure 1 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 and 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 section 3.2, we have found that capital control on financial credit "fci" also appears endogenous to real housing prices, thus we also provide the results of IPWRA estimator for comparison without describing its mechanism here. The results are presented in Figure 12 in appendix. We find that the negative effects are also significant in long term though they are weakened and even change to positive for period 2 to 4.

In right panel, the real housing price response after capital control on "fco" has a weak negative effects after 1 year, and in the second year, the response reaches the trough at -1.459%. Then the negative effects fade out gradually until period 5. These results are almost imprecisely estimated except period 2 after capital control "fco" is taken. Compared with the results of capital inflow restriction, the effects of outflow controls are immediate after implementing such policy even if the coefficients are marginally statistically significant.

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

4.3 The effects of capital control on commercial credit

In this section, we consider the effect of other type of capital control on credit – commercial credit – on real housing price. Thus, we estimate equation (1) using capital control on commercial credit inflow ("cci") and outflow ("cco"). The results are visualized in Figure 3 and Table 6. The left panel of Figure 3 shows that the response of changing to capital control "cci" lowers real housing price in the first year after restriction, and then gradually decreases to -3.147% in year 4 and -4.539% in year 5. Similar to the effects of capital control on financial credit ("fci"), the marginal effects of "cci" are also less pronounced in period 1 to 3, but statistically different from 0 after 4 years. As for the right panel of Figure 3, we find that the response is 1.121% lower real housing price after 1 year, then plummets suddenly to -6.900% after 4 years, and recovers to -6.515% in year 5. The coefficients are significant for all horizons, which means the strong negative effects of capital control on commercial credit outflow "cco". These results are similar to the findings in last section, where capital control on inflows take 4 years on evidencing significant impacts on housing prices, while the effects of capital control on outflows are rapidly materialized.

Besides, for the right panel of figure 3, we find a moderation (or "U" type) of the negative marginal effects of outflow constraint after period 4. These can be related to a trade-off effect in implementing capital control on outflows. This policy, on the one hand, restricts the capital outflow and thus prevents the depreciation of exchange rate and the plummeting of housing prices. On the 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 reverse directions. Besides, when new policy is implemented, there also exists the likelihood of capital flight which are detrimental to real housing prices. In short-term, the negative effects dominate such that the real housing prices decrease rapidly. In long-term, the positive effects become evident and prevent the slump of housing prices.

As for the possibility of monetary policy reacting to capital control actions, we have added central bank policy rate in our baseline model, but we 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 macro-prudential policy, we also control the LTV caps in our baseline model and the results are consistent with our 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.

4.4 The effects of capital control on real estate transactions

In this section, we estimate the impact on real housing prices using capital control on real estate transactions. As we have discussed in Section 3.2, even if indices of capital control on real estate transactions are endogenous to real housing prices, for comparison with other capital control policies in same pattern, we also consider capital control on "re" here with the loss of preciseness to some extent. Indeed, we will further discuss the endogeneity problem in section 6 and provide a method to deal with it.

Figure 4 and Table 7 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 in the first to third year, but at forth year, it decreases sharply and then turns to 4.949% lower of real housing prices at period 5. Although the effects of capital control on "rei" are negative, the coefficients cannot be distinguished from 0. The lower-left panel displays the result of capital control on purchase abroad by residents "re_pabr". We find that the response keeps decreasing from year 1 to 3 and then recovers from 4th years and turns to positive after 5 years. The effects also show a reversal from period 3 like we have found in other capital control outflow indices, and the reversal effects are more evident. 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 long-term. It may be related to the facts that investors who have no method 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 the finding by Banti and 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, we 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, we add central bank policy rate in our baseline model. As for macro-prudential policy, we also control the LTV caps and we find that the negative response is weaker for capital controls on "rei", "re_pabr", and "re_slbn".

4.5 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 and Friedrich (2017) suggest that higher regulatory quality and a higher credit-to-deposit rate increase the effectiveness of macro-prudential policies in managing cross-border bank flows. Banti and Phylaktis (2019) find that AEs can use macro-prudential policies to shield their housing markets from global shocks, but not effective for EMs. However, EMs can adopt foreign currency macro-prudential policies and capital controls on real estate transactions to limit the liquidity impact on housing prices. To explore if the impacts of capital control on real housing prices depend on the economic development degree, we deal with this issue using our baseline specification with additional AEs or EMs dummy variables. The specification takes the following form:

$$\Delta_h HP_{i,t+h} = \alpha_i^h + \gamma_t^h + \beta_1^h CaCP_{i,t} + \beta_2^h CaCP_{i,t} \times EMD_{i,t} + \sum_{k=0}^{1} \phi_k^h \Delta X_{i,t-k} + \beta_c^h HP_{i,t}^c + \varepsilon_{i,t+h}; \quad h = 1, ..., .5 \quad (2)$$

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 we divide our sample into 31 AEs and 22 EMs.

Figure 5 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) for AEs and EMs. 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 two 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 short-run and then prevents the slump of housing prices in long-term.

Figure 6 and 7 report the results for capital controls on financial credit ("fc") and commercial credit ("cc") respectively. In the left panel of Figure 6, we find that the negative effects are immediate for AEs and the impacts are persistent in long-term, while the effects for EMs are not statistically different from 0 over 3 years and only become negative and significant in long term. As for the right panel of Figure 6, however, the impacts of capital control on "fco" cannot be distinguished from 0 for AEs. Thus the negative response is nearly driven by EMs in short-term. The shape and trend are similar for capital control on "cc". In left panel of Figure 7, we find that the negative effects are significant and persistent for AEs, while the negative effects for EMs are weak and less precisely estimated. As for the right panel, the negative effects are significant for EMs unitl period 4. In period 5, the negative effects are significant for AEs. Thus, for capital control on credit indices, we find that AEs are the driving force of the negative effects on credit inflow restrictions, while EMs are the driving force of the negative effects on credit outflow restrictions. These results are in line with the facts that capital control policies are more effective in high-income countries documented by Binici et al. (2010), since the credit inflow control policy restricts the inflows to AEs and thus decreases the house prices, and the credit outflow control policy prevents the outflows from AEs and avoids the slump of housing prices.

As showed in Figure 8, for three capital control policies on real estate transaction "rei", "re_pabr", and "re_slbn", the negative response of EMs is stronger than AEs and the effects for

AEs are muted and almost insignificant for most horizons. Thus, we believe that "rei" restrict inflows and reduce house prices for EMs, "re_pabr" and "re_slbn" prevent outflows and the slump of housing prices for AEs.

Overall, capital controls have asymmetric effects for AEs and EMs on housing prices. As for EMs, capital controls on "dii" and "rei" can reduce housing prices, and "dio" also prevents the slump of housing prices. However, for AEs, capital control on "cci" and "fci" can reduce housing prices, and "cco", "fco", "re_pabr", and "re_slbn" can prevent the slump of housing prices.

5 Robustness and sensitivity analysis

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

5.1 Alternative proxy variables

We substitute for variables in the baseline model using alternative proxies. For example, we use Ted spread as an alternative variable for VIX which represents the exogenous global liquidity. Besides, we 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) we use, the effects of real housing prices are comparable for all capital control indices used in this paper.

5.2 Longer prediction horizons

Inspired by Richter et al. (2019), we investigate whether the results are maintained for a long (10 years) time horizons. The results in Figure 13, 14, 15, and 16 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 Figure 13 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

for the left panel of Figure 14 show the negative response holds after 5 years, and also holds for the right panel, despite both effects are insignificant. As for Figure 15, 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 10 years even if the results cannot be distinguished from 0 after 6 years. As for the results in Figure 16, we find that the effects of capital control on "rei" and "re_slbn" are broadly consistent after 5 years. The results of capital control on "re_pabr" become positive after 5 years, and become significant after 7 years. These results mean that in long-term, the positive effects of preventing from investing abroad dominate the marginal effects. Above all, these results are broadly consistent with those obtained in Section 4.1 to 4.4 using short prediction horizons of capital control indices.

5.3 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 we use contains several economies always restricting or librelizing their capital account for the whole sample horizons. The most representative index is the capital control on "re_slbn", where there are 37 economies always restricting or librelizing this account, only 14 economies[13] usually change their policy actions. We estimate the baseline specification using these 14 economies only. The results (available upon request) are comparable with the results using full samples.

We also consider the correlation problem among the 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, we consider the following regression equations and estimate the

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

marginal effects for each capital control index:

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

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

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

Following Richter et al. (2019), we also study whether our results are not driven by a single country. We choose countries based on Klein (2012) where he categorizes country as three types: Open, Gate, and Wall country. He defines the "Gate country" as a country use capital controls episodically. Thus, we choose the representative "Gate countries" to test if one country can dominate the estimation results. We first eliminate the "Gate countries" one by one from our baseline model and then estimate the results for all capital controls indices.[14] The results (available upon request) show that even if we drop these countries one by one, the estimated results are still consistent to the full samples.

5.4 Pre-crisis v.s. Post-crisis

We know the macro-prudential policies are rarely used before GFC in 2009 (Forbes et al., 2015 and Richter et al., 2019). Thus, we have incentive to test if the effects of capital control policies on real housing prices are also the case. We address this issue by dividing the full sample into two subsamples: pre-2007 (blue solid lines) and post-2007 (red dash lines). The results of capital control on direct investment ("di") show in Figure 17. We find that both for inflow controls and outflow controls, and both for pre-2007 and post-2007 subsamples, the results show no visible change compared with the baseline model. Thus, there is no significant different between pre-2007 and post-2007 subsample after the capital controls on "di" are implemented.

As for capital controls on financial credit ("fc"), the different responses for pre and post crisis are displayed in Figure 18. In the left panel, the response to capital control on "fci" is broadly negative in the pre-2007 subsample. For the right panel, pre-2007 subsample is also the mainly

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

significant driving force for the negative effects in short-term. The impacts of capital control on commercial credit ("cc") before and after crisis are depicted in Figure 19. The left panel shows that the negative response is entirely driven by pre-2007 subsample, while the right panel shows that both pre-2007 and post-2007 subsamples are the robust drivers of the negative response of real housing prices, and the negative effects are larger for post-2007 subsample in long-term.

Figure 20 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 they are all insignificant. For capital control on "re_pabr" in bottom-left, we find the pre-2007 subsample is the mainly driver of the negative effects for short-term over year 1 to 3. As for capital control on "re_slbn" in bottom-right, both subsamples are statistically significant after 3 years and the pre-2007 subsample has larger impacts on the negative response.

Abovel all, the negative effects of capital 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.

5.5 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 paper, we also test if the capital controls on inflow and outflow are implemented in a countercyclical manner. We address this issue by dividing sample into boom (real GDP above its trend) and slump (real GDP below its trend) periods, and then regress using equation (1). The results of capital control on "di" are depicted in Figure 21, where blue solid lines denote the boom periods, and red dash lines are slump periods. Both left and right panel show that the impacts of capital controls are weak if anything in the boom periods, and also for slump periods. Thus, it is likely that no matter if business cycle is in boom or slump period, both inflow and outflow restrictions have weak and insignificant negative effects.

We also test the responses in boom and slump subsamples for capital controls on "fc". We show in the left and right panels of 22 that the negative response is driven by boom periods in long-run for "fci", and for "fco" is also driven by boom periods but not significant enough.

Thus, in boom periods, policymakers use 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, we report the results of capital control on "cc" in Figure 23. The results show that the negative response of real housing prices to capital control on "cci" is entirely driven by subsample in slump period. As for impacts of capital control on "cco", the negative response is significantly driven by both boom and slump periods and the coefficients are approximately equal between boom and slump periods. However, the width of confidence bands are different for these two subsamples where the estimates for boom periods present less uncertainty. Thus, there are not evident cyclical properties of capital controls on "fc".

As for the results for capital controls on "re" in Figure 24, we find that no matter what types of subsample we 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. Above all, these results are broadly in line with Fernández et al. (2015)'s conclusion where they find that capital controls are remarkably acyclical.

6 Endogeneity problem revisiting

As discussed by Richter et al. (2019), policymakers may target financial objectives without stating them explicitly when implement capital control policies.[15] 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.[16]

To address the endogeneity problem, we employ *inverse probability weighted regression ad-*

[15]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 (2017) 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.

[16]For example, Ostry et al. (2012), Beirne and Friedrich (2017), Landi and 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 and Zlate (2014) also show the endogeneity would bias coefficients being positive.

justed (IPWRA) estimator developed by Jordà and 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, we use local projections to the rebalanced sample and obtain the IPWRA estimators. There are several studies addressing the endogeneity problem using this method, and we further study the effects of capital control on real estate transactions to real housing prices and other credit variables.[17]

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

$$\hat{P}\left(CaCP_{i,t}=1\right)=\Phi\left(\alpha_i+\hat{\beta}Z^p_{i,t-1}+\hat{\gamma}_1\widetilde{Z}^p_{i,t-1}+\hat{\gamma}_2\widetilde{Z}^p_{i,t-2}\right), \tag{3}$$

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, $Z^p_{i,t}$ with as much one lag, and $\widetilde{Z}^p_{i,t}$ with two lags. $\Phi\left(\cdot\right)$ 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 control group $(CaCP_{i,t}=0)$. In the second step, we 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

$$\Delta_h FA_{i,t+h}=\alpha^h_i+\gamma^h_t+\beta^h CaCP_{i,t}+\sum_{k=0}^{1}\phi^h_k\Delta Z^c_{i,t-k}+\beta^h_c FA^c_{i,t}+\varepsilon_{i,t+h};\quad h=1,...,.5 \tag{4}$$

where $FA_{i,t}$ is the financial variables: real housing prices, real bank credit, and real credit to households. $FA^c_{i,t}$ corresponds to the detrended variable. $Z^c_{i,t}$ denotes the control variables. As discussed by Kuvshinov and Zimmermann (2019) and Jordà and Taylor (2016), we use a

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

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:

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

where $\widehat{\Delta FA}_{i,t+h}$ is the prediction obtained by estimating equation (4), and $n_{cacp=1} = \sum_t CaCP_{i,t}$ and $n_{cacp=1} = \sum_t (1 - CaCP_{i,t})$ are the numbers of observations in treatment and control group respectively.

6.1 Diagnostic test

Before calculating the IPWRA estimator, we 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 and Taylor (2016) report three diagnostic test (balance condition test, omitted variables test, and predicable test) to assure the existence of endogeneity.

We have done a balance condition test previously in Table 3 mainly for dependent variables. Here we will extend to control variables. The results are showed in Table 8 for several macroeconomic control variables used in equation (1). 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 we check if the dependent and control variables can predict the implementation of capital controls. To address this issue, we 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. We will estimate the response of real housing prices and credit variables in the next section, thus we do this test for different explanatory variables. Table 9 shows the pooled probit estimator for real housing prices, credit variables and other predicting variables. In the second row, We

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, we 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_pabr", 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". These results can be due to the fact that the credit variables used here are not perfect substitution for the housing demand. Nevertheless, we find that real GDP growth is also the significant predictor for all three types of capital controls.

Following Jordà and Taylor (2016), Richter et al. (2019), and Kuvshinov and Zimmermann (2019), we further confirm the predictive ability using AUC statistic. In other words, this statistic measures whether such probit model can correctly categorize observations into "restriction" and "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 9, all AUCs are larger than 0.71, even 0.87 for "re_slbn". Thus, the AUCs are all significantly larger than 0.5.

We also provide the overlap test and the results are depicted in Figure 25. The dependent variables for the probit model are the forward variable of capital controls on real estate transaction "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 and Taylor (2016), the ideal empirical distribution of propensity score should be uniform and identical for treatment and control groups. In addition, Jordà and Taylor (2016) admits 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 in Figure 25 are consistent with these features and show substantial region of overlaps. Thus we believe that the inverse propensity score method identifies successfully the ATE of capital control policies.

6.2 The effects on housing price

In this section, we will further study the response of real housing prices to capital control on real estate transactions using IPWRA estimators. Figure 9 presents the results of IPWRA estimators and we also report the OLS results for comparison. We 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, while for IPWRA estimators, the negative response changes to -4.090% of real housing prices, roughly four fifth the size of the conditional OLS. Besides, we find that the attenuation effects of the IPWRA estimators are stronger in long-term, since the gaps between conditional OLS and IPWRA estimators widen as period goes by. Consistent with Kuvshinov and Zimmermann (2019), we 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.

6.3 The effects on credit variables

In addition to the impact of capital controls on real housing prices, we 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 10 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 insignificant for "rei". Besides, the negative effects of IPWRA estimator are marginally stronger than OLS one for almost all horizons, and the IPWRA results also have narrower confidence bands.

The response to capital control on real estate outflow "re_pabr" is positive immediately after implemented such policy and keeps positive over all horizons. The coefficients of IPWRA are lower than OLS results and also statistically distinguished from 0. The response is different from the one for real housing prices (see Figure 9) where 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 real estate outflow on "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.

Similarly, the results for real credit to households are reported in Figure 11. Compared with the results for real bank credit, we 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 Figure 10, 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 we rebalance the sample by IPWRA method, the negative response is significant and larger than its OLS counterpart.

7 Conclusion

In this paper, we study the effects of capital control polices on real housing prices. Our analysis complements the existing literature by using a more granular index of capital control dataset compiled by Fernández et al. (2016) that allow us to study whether capital controls on specific asset types and flow directions are effective in increaseing or decreasing the real housing prices. For this purpose, we 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 "mis-specification robust" local projection method and we also check the endogeneity of Fernández et al. (2016)'s dataset using econometric method.

Our results show that most capital control indices we analyzed in this paper appear to reduce real housing prices even if some of them tend to be insignificant and marginal. Besides, we find that capital controls have asymmetric effects on housing prices for AEs and EMs. After employing a series of robustness test, we show that the negative effects of capital controls on housing prices are mainly driven by pre-crisis subsample that means the capital controls have

been used several times before Global Financial Crisis. We also estimate the effects for boom and slump periods respectively and we find that capital control policies are implemented in an acyclical way.

There exists endogenous problem in capital controls on "re", and we address this issue using IPWRA estimator because such method can achieve the random allocation of capital control treatment. We 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 long-term and thus we can attribute much of the long-term real housing prices variation to endogenous factors. Then, we also estimate the response of credit variables to capital control on real estate transactions to study the effects on housing demand. We 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.

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Table 1: Data definitions and sources

Type	Variables	Source	Description
Dependent Variables	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 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 Variables	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
	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 Variables (Monetary Policies)	Central bank policy rate	BIS	from 1995 to 2017. Other sources: Bulgaria and Morocco (Central bank) Singapore (using exchange rate), UAE (using EBIOR)
	Short-term interest rate	CEIC	from 1995M1 to 2017M12, averaged. Other sources: Peru (central bank) Brazil (FRED), Chile (FRED)
(MaPP)	LTV caps	Cerutti et al. (2017)	0-1 Binary variables, from 2000 to 2017
(Exchange Rates)	Real effective exchange rate	BIS	CPI based, broad indices, annual basis
	Biliteral nominal interest rate	Penn World Table v9.1	National currency per USD, from 1995 to 2017
(Capital flows)	Direct investment (inflow/outflow)	IFS (BOP/IIP)	Data in USD, annual basis, from 1995 to 2017
	Portfolio investment (inflow/outflow)	IFS (BOP/IIP)	Data in USD, annual basis, from 1995 to 2017
	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 liquidity driving)	VIX	CBOE	Daily basis, averaged, from 1995 to 2017
	Ted spread	Federal Reserve Bank of St. Louis	Daily basis, averaged, from 1995 to 2017
	Bank leverage	Global Financial Development Database	Annual basis, 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

Table 2: 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.

Table 3: 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 growth, demeaned	-0.00	-0.01**	-0.01***	0.01***	0.00	0.01	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Real bank credit smoothed growth, demeaned	0.01***	0.02***	0.03***	0.04***	0.02***	0.03***	0.03***	0.02***	0.03***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Real credit to households smoothed growth, demeaned	0.02***	0.02***	0.05***	0.05***	0.03***	0.04***	0.04***	0.03***	0.05***
	(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) 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. *, **, ***, indicate the significant at 10%, 5%, 1% levels respectively.

Table 4: 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
	(0.603)	(1.292)	(2.123)	(2.989)	(3.800)
Observations	672	623	574	525	476
CACP. direct investment (outflow)	-0.402	-0.712	-0.828	-0.133	0.524
	(0.669)	(1.368)	(2.351)	(3.297)	(5.360)
Observations	668	619	570	521	472

Notes: Clustered (by country) 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. *, **, ***, indicate the significant at 10%, 5%, 1% levels respectively.

Table 5: 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**
	(1.401)	(1.916)	(2.145)	(1.598)	(2.361)
Observations	543	504	465	426	387
CACP. financial credit (outflow)	-0.372	-1.459*	-1.141	-1.102	-1.052
	(0.444)	(0.861)	(1.286)	(1.618)	(2.061)
Observations	540	501	462	423	384

Notes: Clustered (by country) 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. *, **, ***, indicate the significant at 10%, 5%, 1% levels respectively.

Table 6: 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)	-0.457	-1.401	-2.699	-3.417*	-4.539*
	(0.898)	(1.901)	(2.496)	(1.957)	(2.357)
Observations	543	504	465	426	387
CACP. commercial credit (outflow)	-1.121**	-3.761**	-5.843**	-6.900**	-6.515**
	(0.509)	(1.697)	(2.525)	(2.589)	(2.975)
Observations	540	501	462	423	384

Notes: Clustered (by country) 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. *, **, ***, indicate the significant at 10%, 5%, 1% levels respectively.

Table 7: 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
	(0.897)	(1.578)	(2.506)	(3.156)	(3.107)
Observations	667	617	567	517	467
CACP. real estate (out. pur.)	-0.998	-2.499**	-2.774*	-0.726	1.640
	(0.661)	(1.227)	(1.497)	(1.865)	(2.571)
Observations	671	621	571	521	471
CACP. real estate (out. sale.)	0.336	-1.799	-6.419*	-11.325***	-13.960***
	(1.345)	(2.771)	(3.229)	(3.418)	(5.171)
Observations	663	613	563	513	463

Notes: Clustered (by country) 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.

Table 8: Balance condition test: for control variables

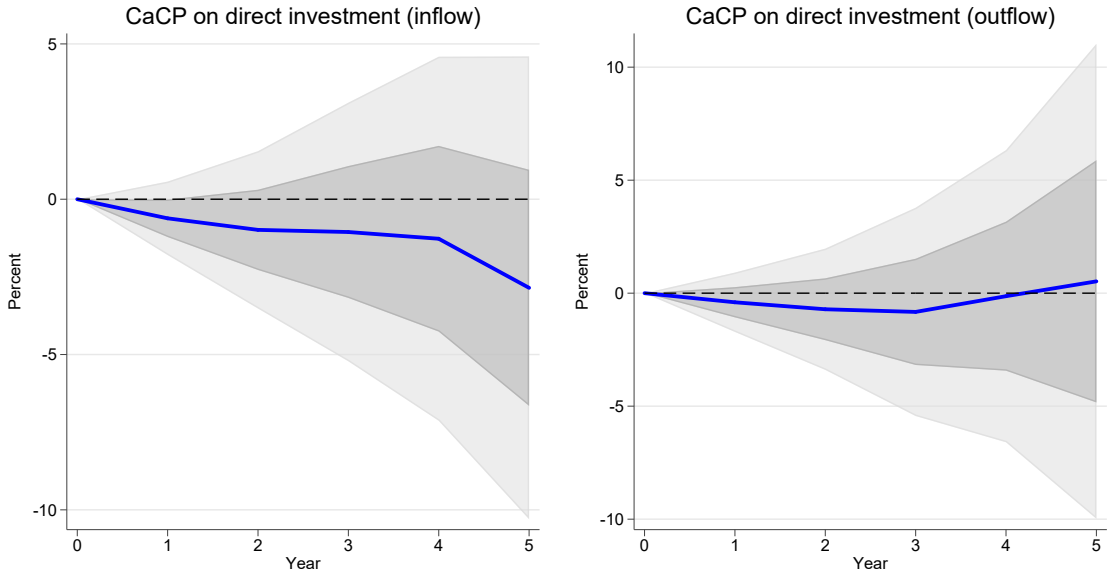
	rei	re_pabr	re_slbn
Real GDP detrended	0.00	-0.00	-0.00**
	(0.00)	(0.00)	(0.00)
Real GDP growth	0.01***	0.01***	0.02***
	(0.00)	(0.00)	(0.00)
CPI detrended	-0.00	0.01*	0.01**
	(0.00)	(0.00)	(0.01)
CPI growth	0.02***	0.02***	0.04***
	(0.01)	(0.01)	(0.01)
Real cross border loan growth	0.03**	-0.04***	-0.01
	(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 9: Pooled probit estimation of credit variables: prediction of capital control variables

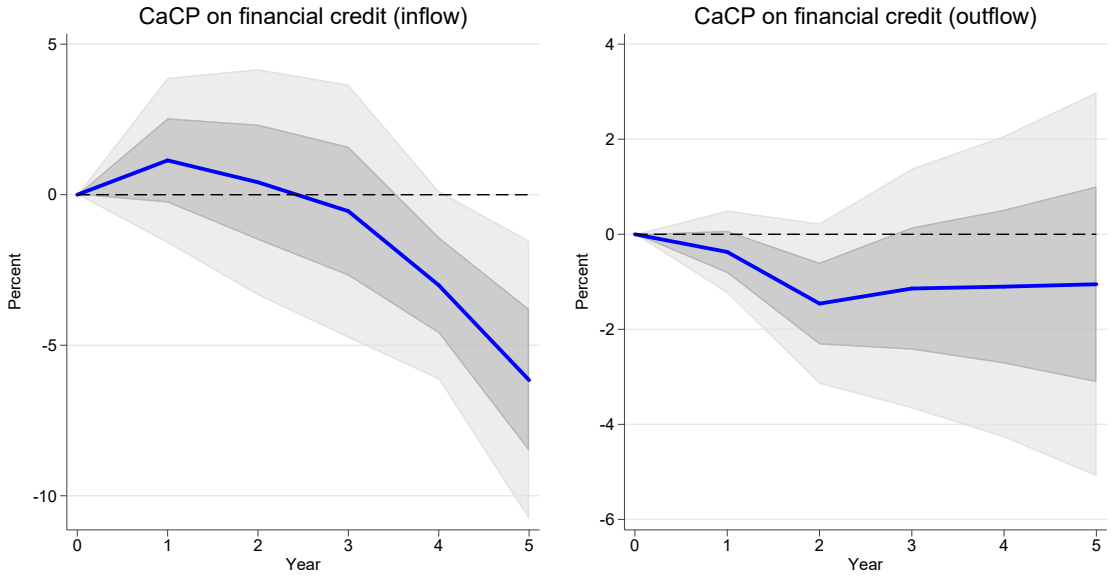
	rei	re_pabr	re_slbn
Real housing price	0.698***	0.455***	0.156***
	(0.088)	(0.088)	(0.054)
Real bank credit	-0.117**	0.178***	0.075***
	(0.053)	(0.049)	(0.028)
Real credit to households	0.071	-0.194***	-0.062**
	(0.054)	(0.049)	(0.028)
Real GDP detrended	-4.888***	-1.349	-0.916
	(1.029)	(0.969)	(0.582)
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.



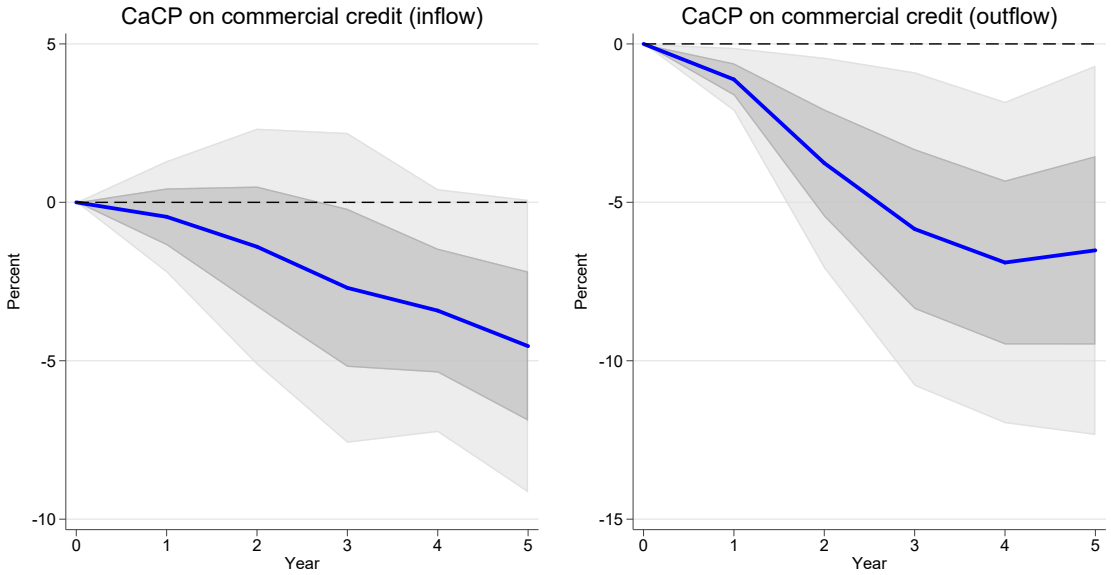
Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing prices over 5 years following the changing in capital control of direct investment inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas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

Figure 1: Local Projection: Impact of capital controls of direct investment on real housing prices



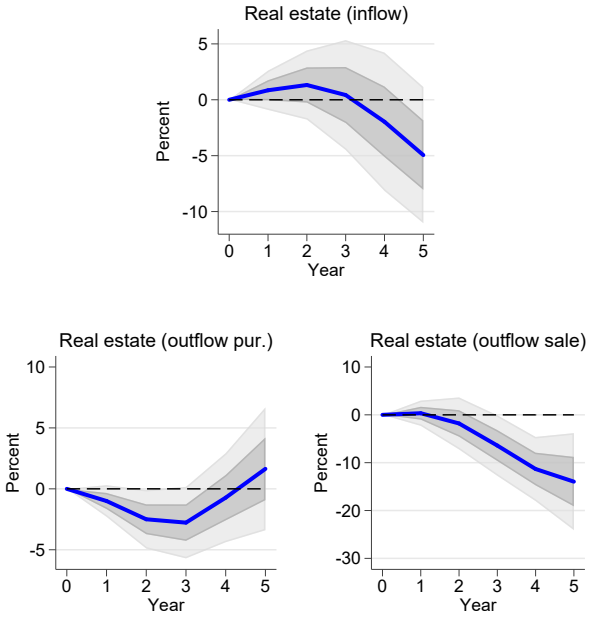
Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing prices over 5 years following the changing in capital control of financial credit inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas are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

Figure 2: Local Projection: Impact of capital controls of financial credit on real housing prices



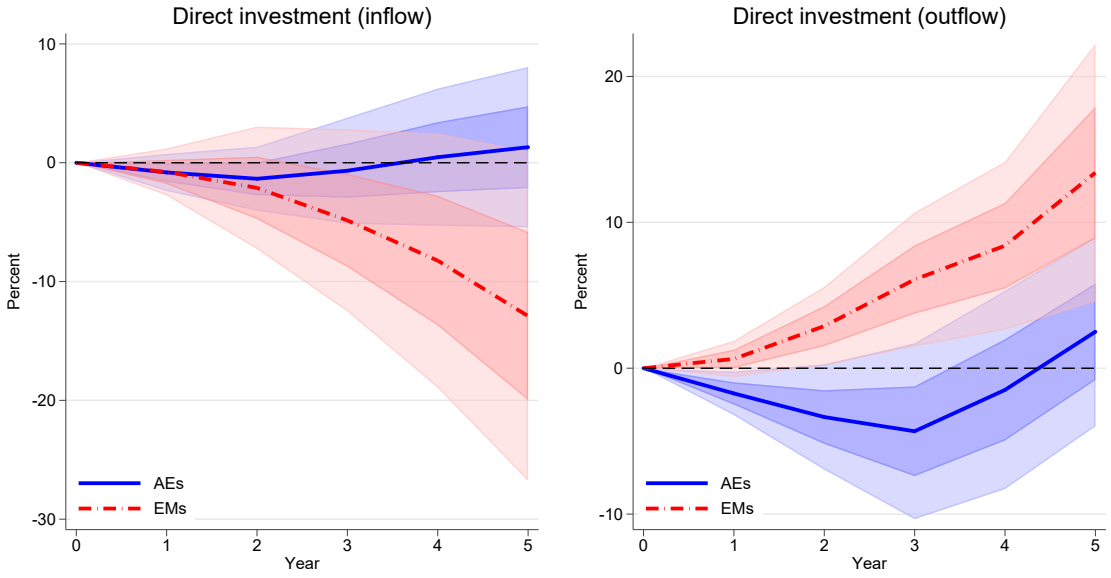
Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing price over 5 years following the changing in capital control of commercial credit inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas are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

Figure 3: Local Projection: Impact of capital controls of commercial credit on real housing prices



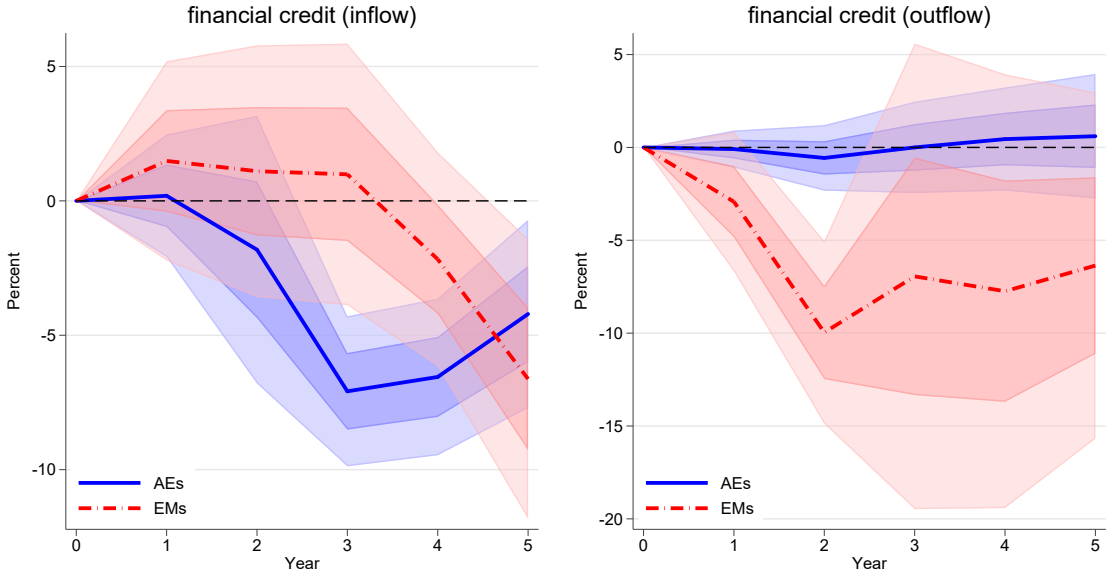
Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing price over 5 years following the changing in capital control of real estate transaction inflow (purchase locally by nonresident) and outflow (purchase abroad by resident, 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) bands around the response estimates.

Figure 4: Local Projection: Impact of capital control of real estate transactions on real housing prices



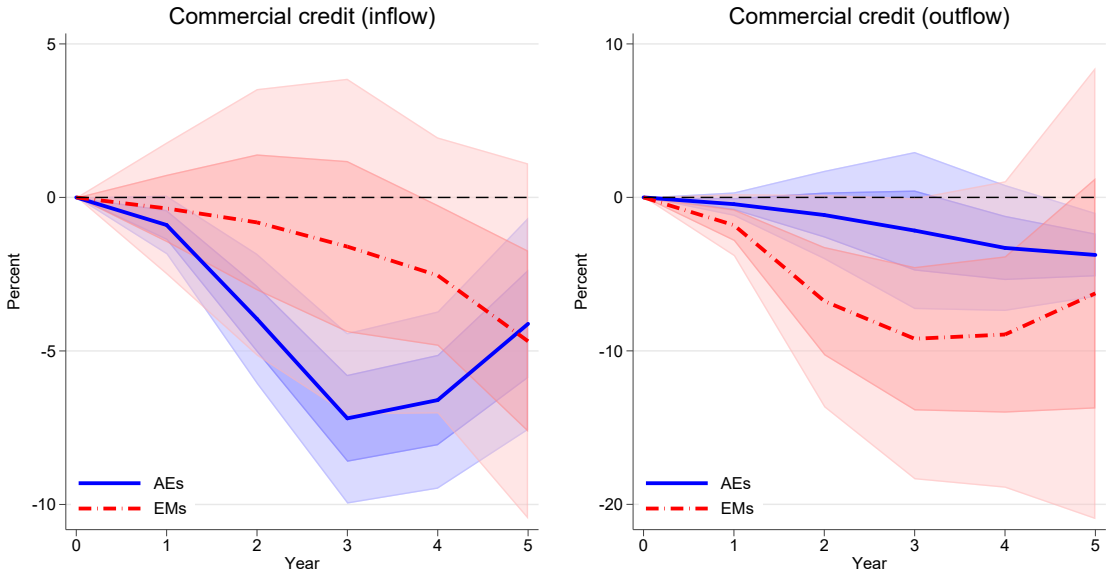
Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing prices for Advanced economies and Emerging markets respectively over 5 years following capital control policies implemented on direct investment.

Figure 5: Local Projection: Impact of capital controls of direct investment on real housing price – comparison of Advanced economies (AEs) and Emerging market (EMs)



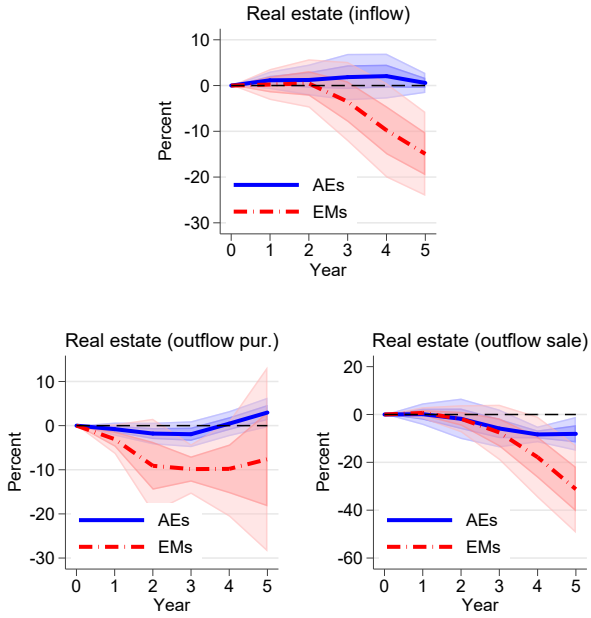
Note: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing prices for Advanced economies and Emerging markets respectively over 5 years following capital control policies implemented on financial credit.

Figure 6: Local Projection: Impact of capital controls of financial credit on real housing prices – comparison of Advanced economies (AEs) and Emerging market (EMs)



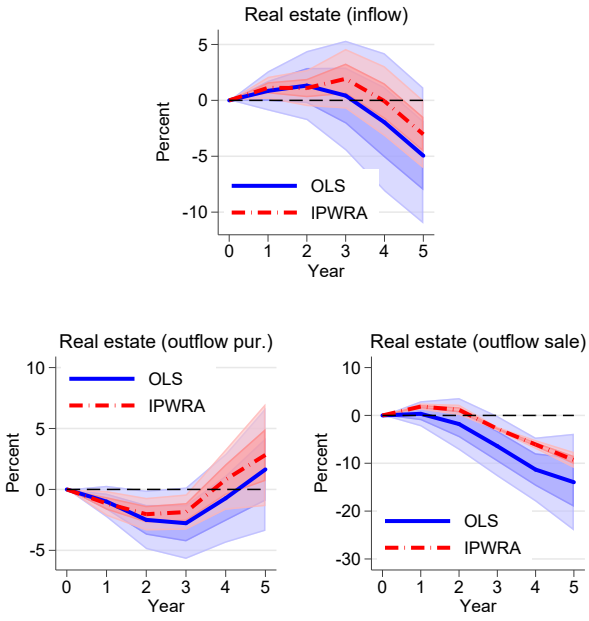
Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing prices for Advanced economies and Emerging markets respectively over 5 years following a capital control policies implemented on commercial credit.

Figure 7: Local Projection: Impact of capital controls of commercial credit on real housing prices – comparison of Advanced economies (AEs) and Emerging market (EMs)



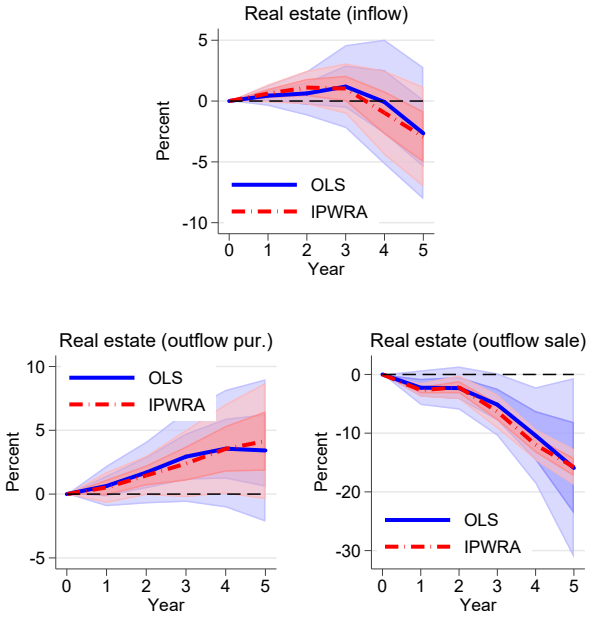
Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for Advanced economies and Emerging markets respectively over 5 years following a capital control policy implemented on real estate transaction.

Figure 8: Local Projection: Impact of capital control of real estate transaction on real housing price – comparison of Advanced economies (AEs) and Emerging market (EMs)



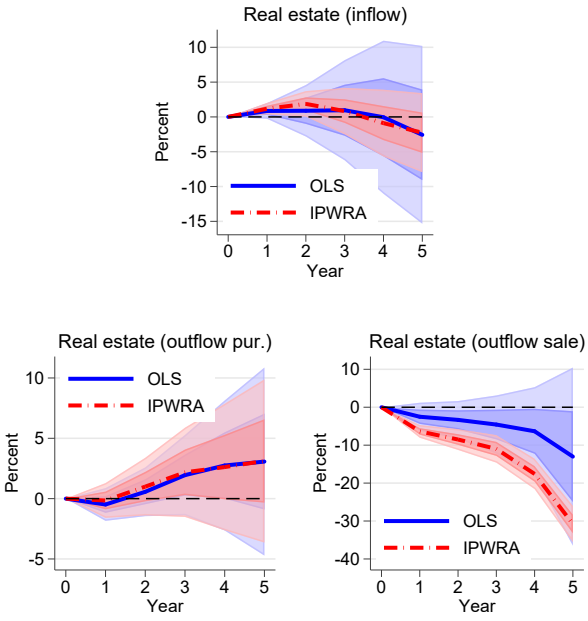
Notes: Y-axes denotes 100×log (real housing price). The explatory variables for sub-figure are "rei", "re_pabr", "re_slbn", respectively. The blue and red lines denote the coefficients of cumulative response of real housing price estimated by OLS and IPWRA local projection respectively over 5 years.

Figure 9: Local Projection: Impact of capital control of real estate transactions on real housing prices by IPWRA estimation



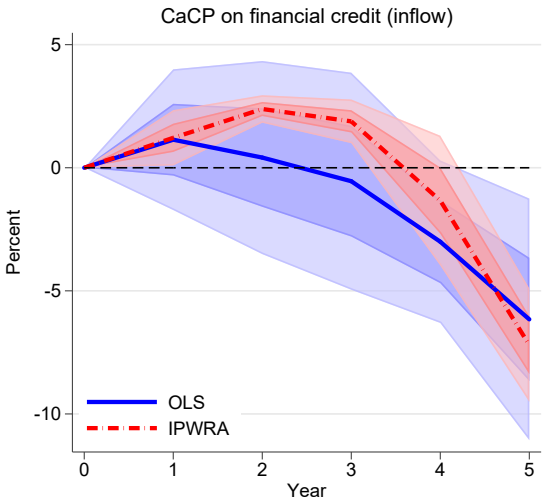
Notes: Y-axes denotes 100×log (real bank credit). The explatory variables for sub-figure are "rei", "re_pabr", "re_slbn", respectively. The blue and red lines denote the coefficients of cumulative response of real bank credit to non-financial sector estimated by OLS and IPWRA local projection respectively over 5 years.

Figure 10: Local Projection: Impact of capital control of real estate transactions on real bank credit to non-financial sector by IPWRA estimation



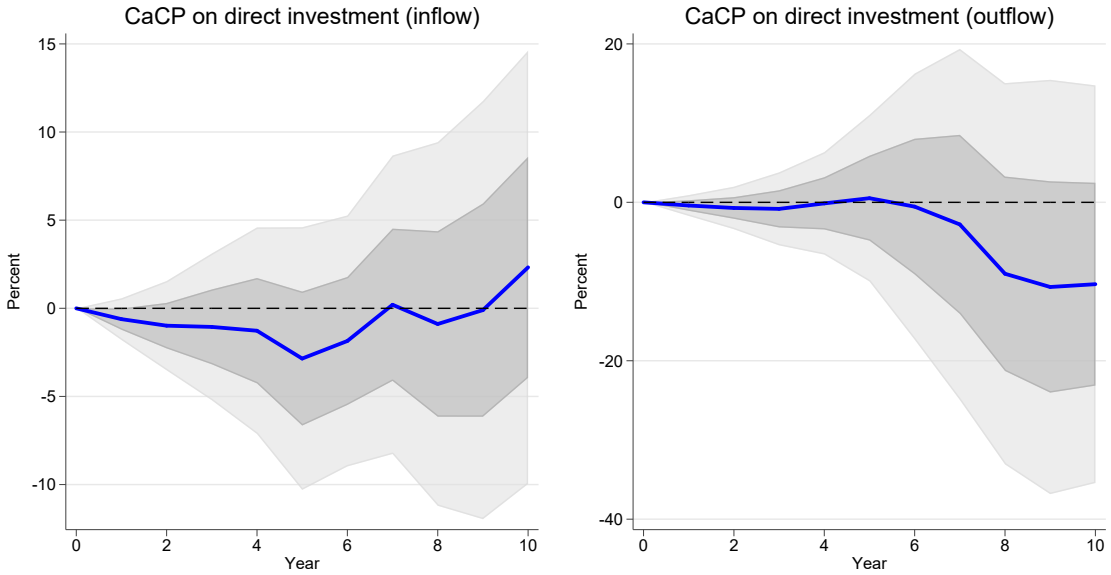
Note: Y-axes denotes 100×log (real credit to households). The explanatory variables for sub-figure are "rei", "re_pabr", "re_slbn", respectively. The blue and red lines denote the coefficients of cumulative response of real credit to households and NPISHs estimated by OLS and IPWRA local projection respectively over 5 years.

Figure 11: Local Projection: Impact of capital control of real estate transactions on real credit to households and NPISHs by IPWRA estimation



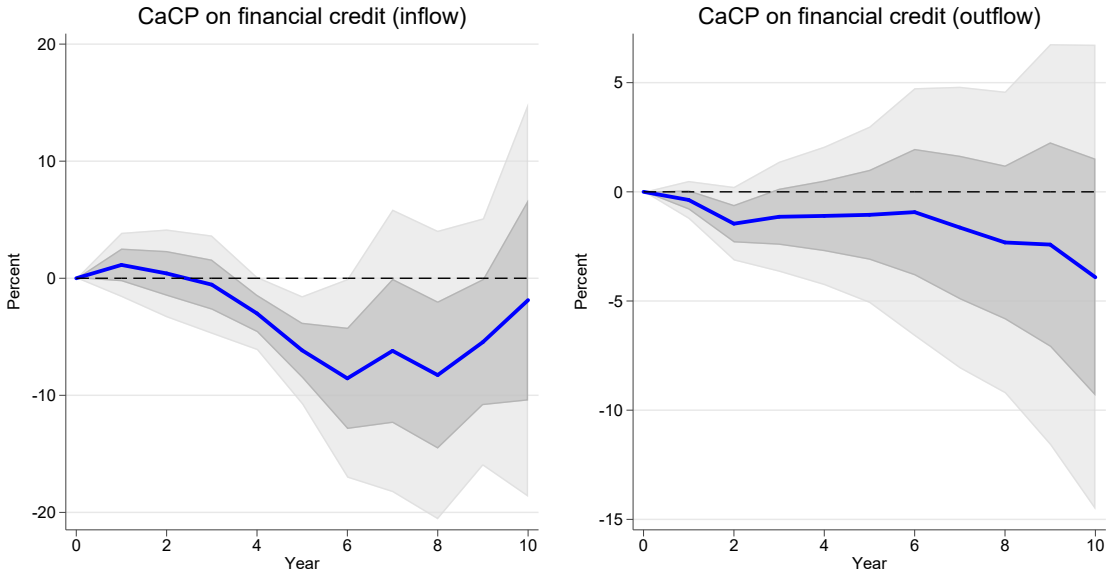
Notes: Y-axes denotes 100×log (real housing price). The explanatory variables is capital control on financial credit inflow "fci". The blue and red lines denote the coefficients of cumulative response of real housing price estimated by OLS and IPWRA local projection respectively over 5 years.

Figure 12: Local Projection: Impact of capital control of financial credit on real housing prices by IPWRA estimation



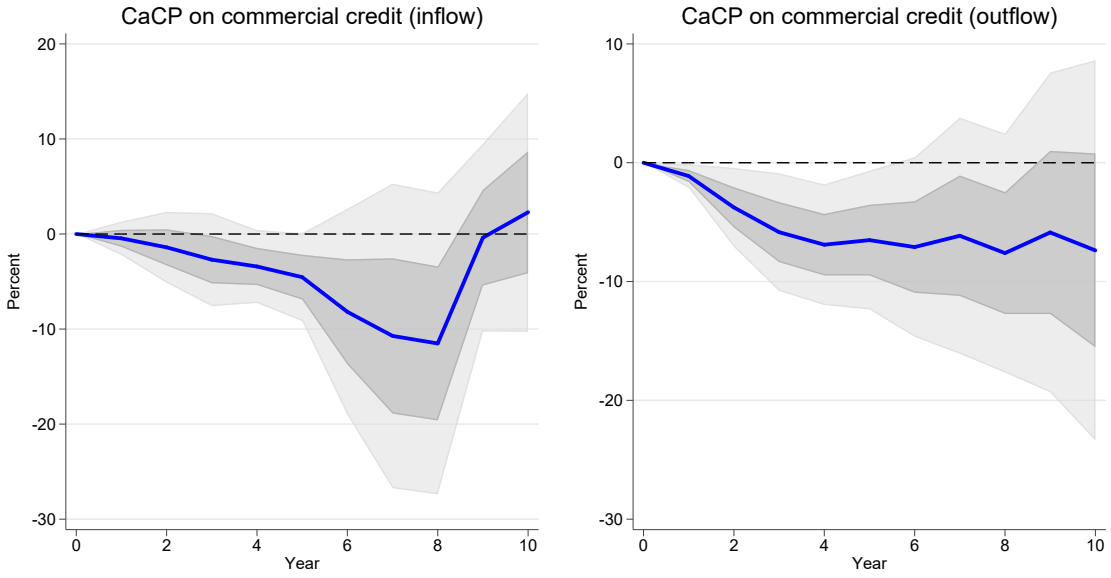
Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing prices over 10 years following the changing in capital control of direct investment inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas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

Figure 13: Local Projection: Impact of capital controls of direct investment on real housing prices, 10 years horizons



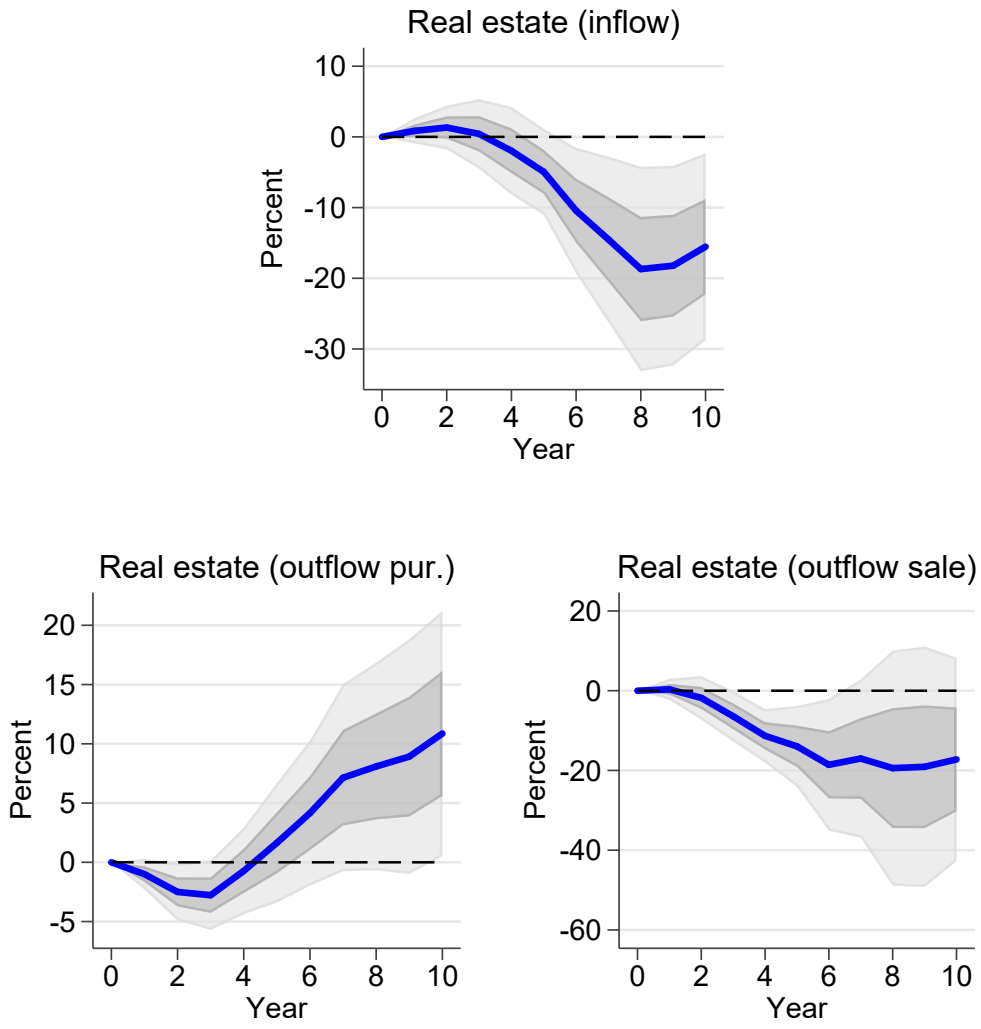
Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing prices over 10 years following the changing in capital control of financial credit inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas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

Figure 14: Local Projection: Impact of capital controls of financial credit on real housing prices, 10 years horizons



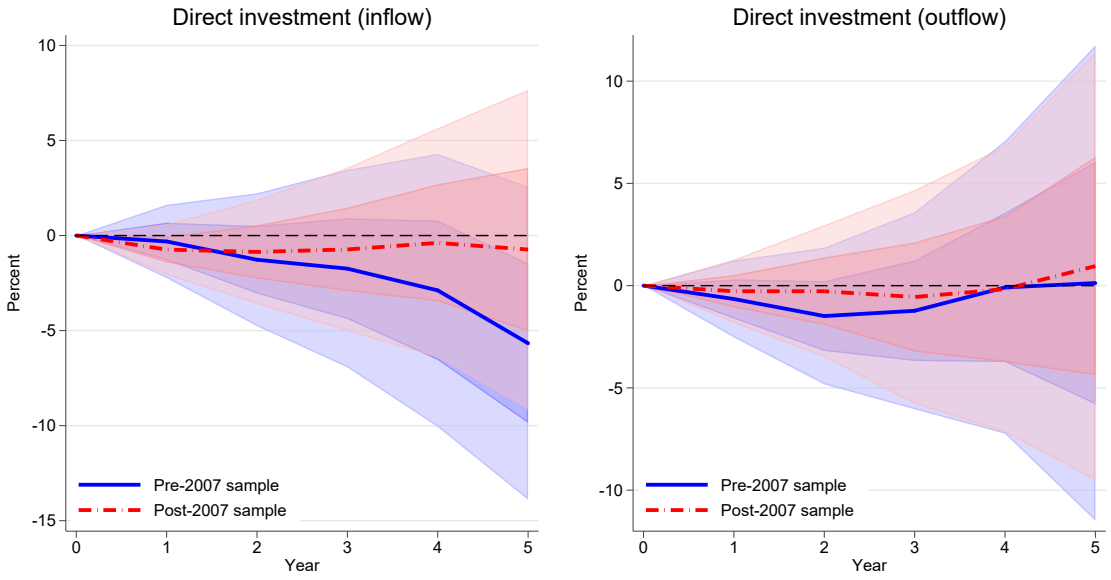
Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing price over 10 years following the changing in capital control of commercial credit inflow and outflow from no restriction "0" to restriction "1" respectively. Shade areas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

Figure 15: Local Projection: Impact of capital controls of commercial credit on real housing prices, 10 years horizons



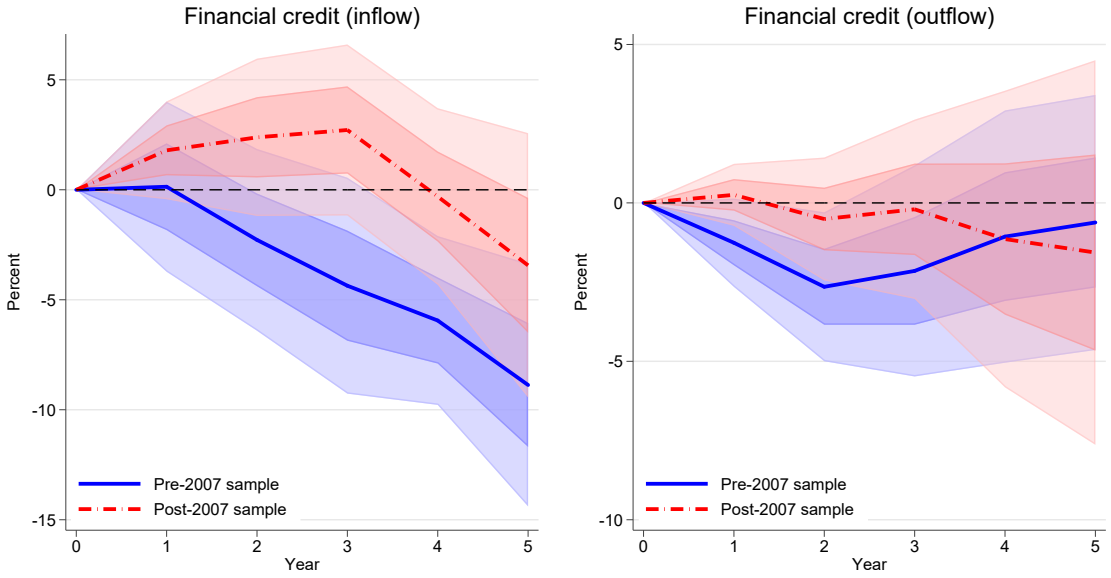
Notes: Y-axes denotes 100×log (real housing price). The blue lines denote the coefficients of cumulative response of real housing price over 10 years following the changing in capital control of real estate transaction inflow (purchase locally by nonresident) and outflow (purchase abroad by resident, 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) bands around the response estimates.

Figure 16: Local Projection: Impact of capital control of real estate transactions on real housing prices, 10 years horizons



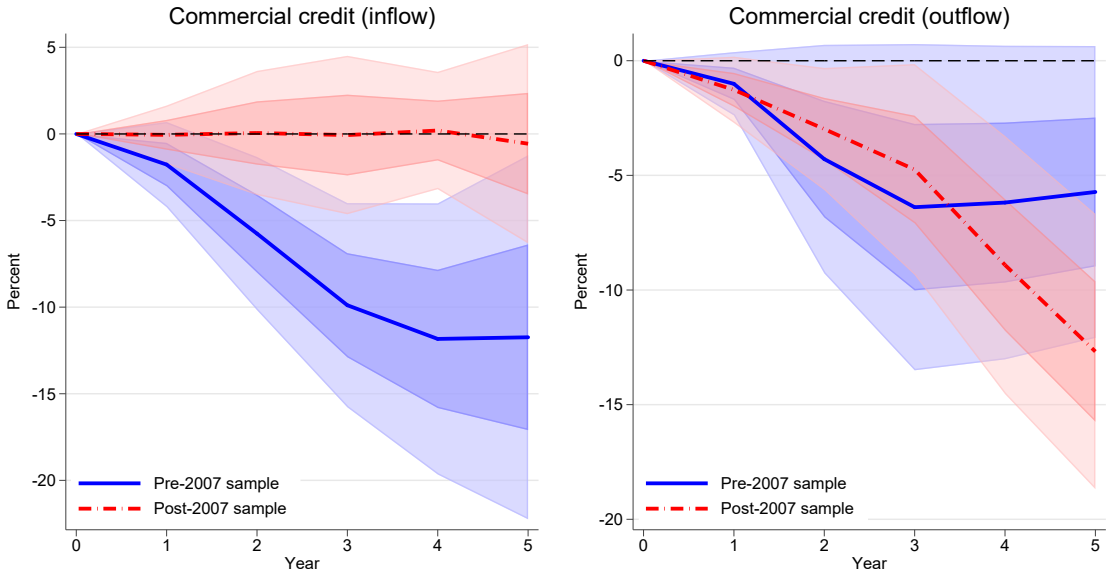
Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for pre-crisis period and post-crisis respectively over 5 years following a capital control policy implemented on direct investment.

Figure 17: Local Projection: Impact of capital controls of direct investment on real housing prices – comparison of pre-crisis and post-crisis



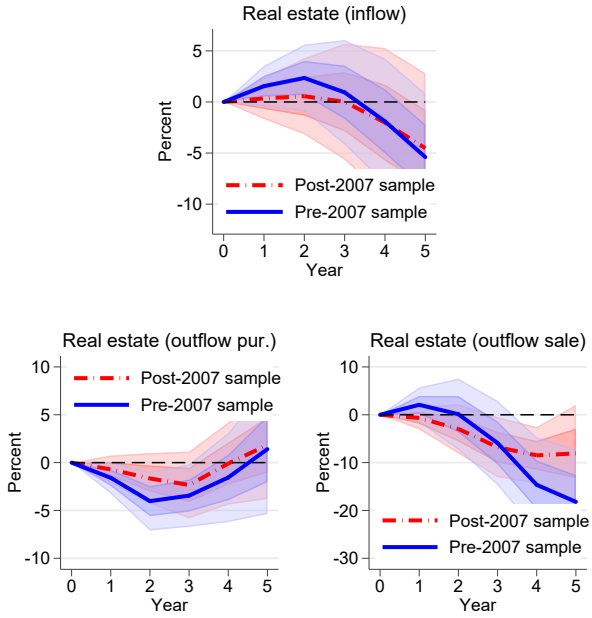
Note: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for pre-crisis period and post-crisis respectively over 5 years following a capital control policy implemented on financial credit.

Figure 18: Local Projection: Impact of capital controls of financial credit on real housing prices – comparison of pre-crisis and post-crisis



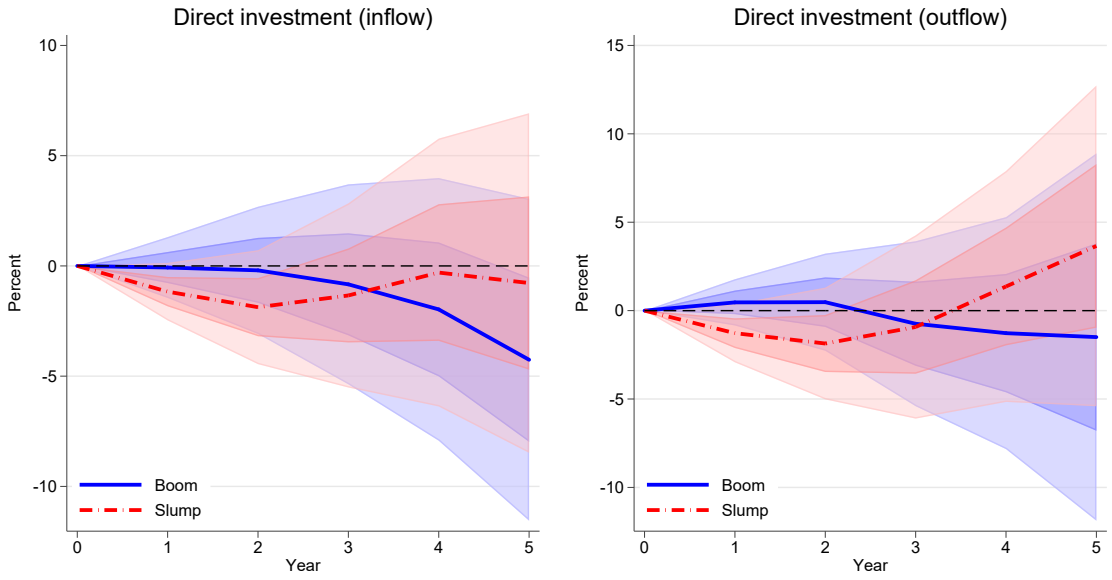
Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for pre-crisis period and post-crisis respectively over 5 years following a capital control policy implemented on commercial credit.

Figure 19: Local Projection: Impact of capital controls of commercial credit on real housing prices – comparison of pre-crisis and post-crisis



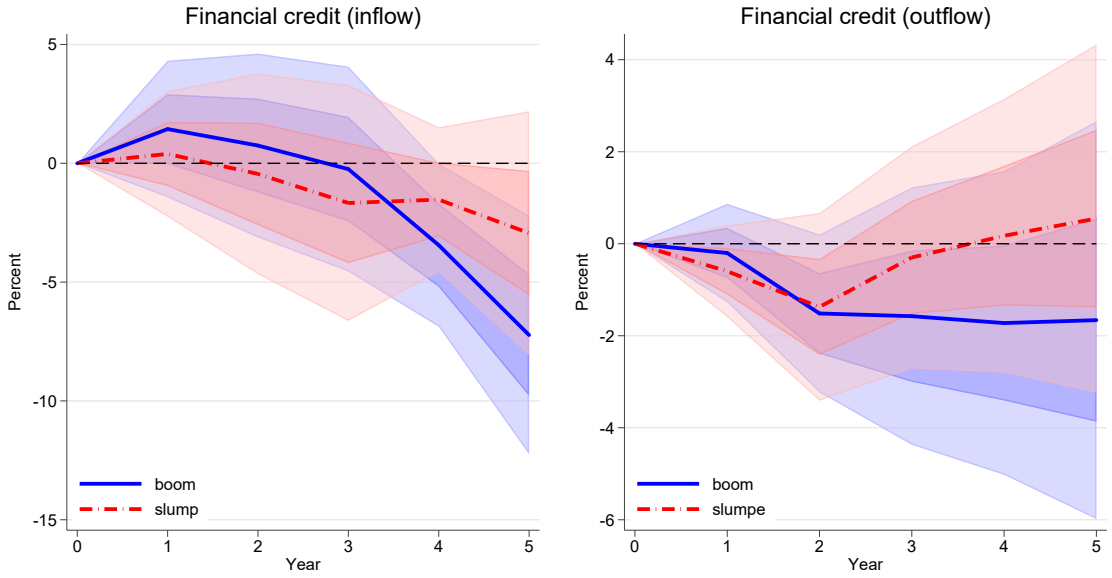
Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for pre-crisis period and post-crisis respectively over 5 years following a capital control policy implemented on real estate transaction.

Figure 20: Local Projection: Impact of capital control of real estate transactions on real housing prices – comparison of pre-crisis and post-crisis



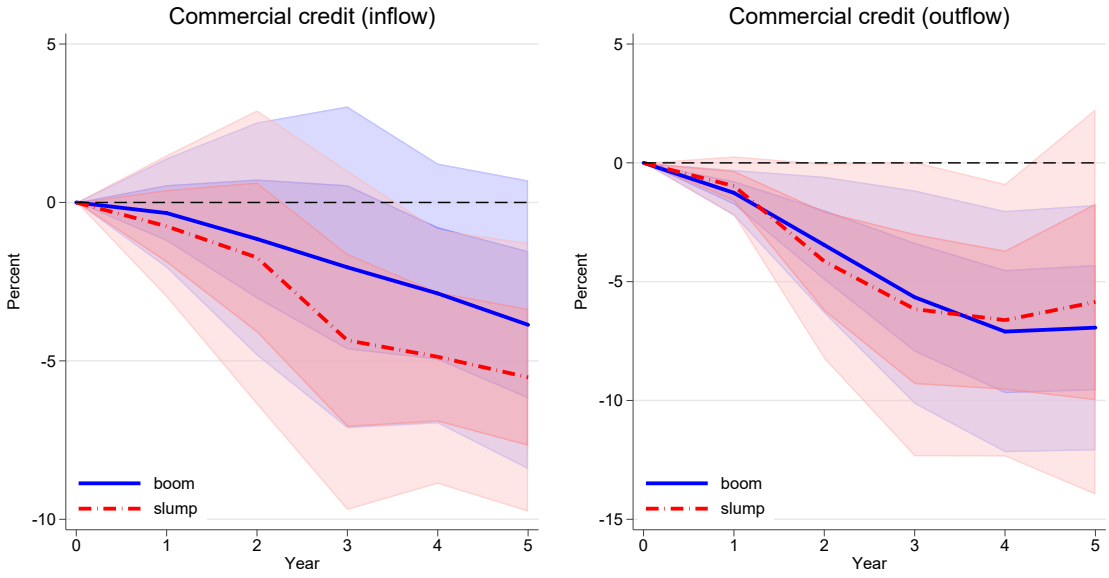
Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for boom and slump periods respectively over 5 years following a capital control policy implemented on direct investment.

Figure 21: Local Projection: Impact of capital controls of direct investment on real housing prices – comparison of boom and slump subsamples



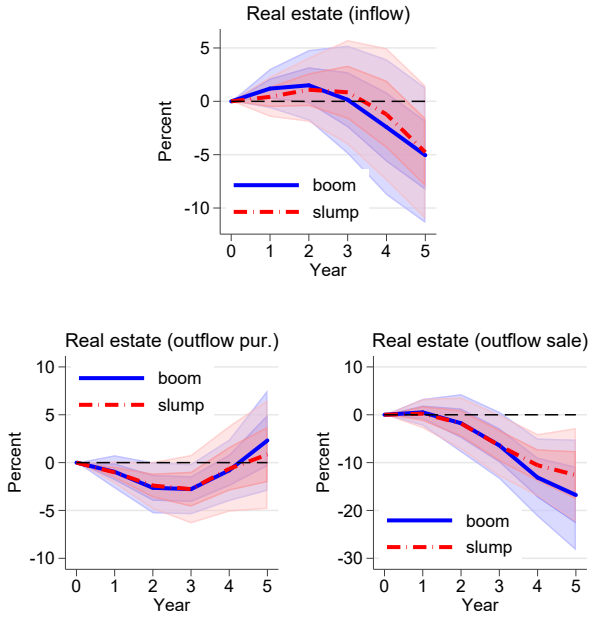
Note: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for boom and slump periods respectively over 5 years following a capital control policy implemented on financial credit.

Figure 22: Local Projection: Impact of capital controls of financial credit on real housing prices – comparison of boom and slump subsamples



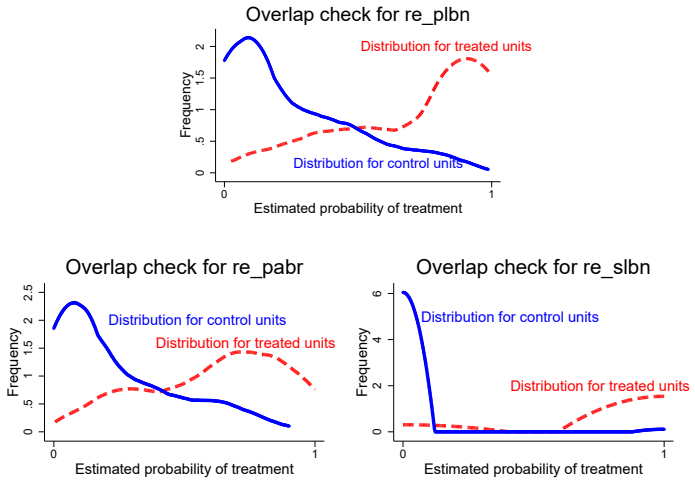
Notes: Y-axes denotes 100×log (real housing price). The blue and red lines denote the coefficients of cumulative response of real housing price for boom and slump periods respectively over 5 years following a capital control policy implemented on commercial credit.

Figure 23: 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 credit to households). 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.

Figure 24: Local Projection: Impact of capital controls of real estate transaction on real housing prices – comparison of boom and slump subsamples



Notes: The red dashed lines denote the empirical density of the predicted probabilities of implementing each capital control "rei", "re_pabr", "re_slbn", the blue solid lines display the control observations. The propensity score is estimated using the specification in Table 9 with including country fixed effects.

Figure 25: Overlap test: probit results for capital control on "re"