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GLOBAL CAPITAL AND LOCAL ASSETS:
HOUSE PRICES, QUANTITIES, AND ELASTICITIES

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ABSTRACT

Interconnected capital markets allow mobile global capital to flow into immobile local assets. This paper examines how foreign demand affects U.S. housing markets, and uses this demand shock to estimate local price elasticities of supply. Other countries introduced foreign-buyer taxes meant to deter Chinese housing investment beginning in 2011. We first show house prices grew 8 percentage points more in U.S. zipcodes with high foreign-born Chinese populations after 2011, subsequently reversing with the onset of the U.S.–China trade war. Second, we use international tax policy changes as a U.S. housing demand shock and estimate local house price and quantity elasticities with respect to international capital. We find that a 1% increase in instrumented foreign capital raises house prices at the zip code level by 0.27%, and housing supply by 0.004%. Finally, we use the two elasticities to construct new local house price elasticities of supply for the largest 100 CBSAs. These supply elasticities average 0.1 and vary between 0.02 and 0.7, suggesting that local housing markets are inelastic in the short run and exhibit substantial spatial heterogeneity.

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1 Introduction

The emergence of a new international supply of savings, the so-called "global savings glut," has been suggested as a possible source of asset market instability and contributor to the Global Financial Crisis (e.g. Bernanke (2005); Justiniano, Primiceri and Tambalotti (2014)). Investors in search of yield, vacation homes, or protection from corruption crack-downs, frequently invest in housing abroad (Favilukis and Van Nieuwerburgh, 2017; Badarinza and Ramadorai, 2018). In the United States, this phenomenon has been especially dramatic: Foreign purchases of residential real estate increased from \$66 billion in 2010 to \$153 billion in 2017 (NAR, 2017). These foreign residential investments are often contentious because they are perceived to drive up housing costs for local residents, exacerbating existing concerns with affordability. In response, many countries have enacted foreign real estate buyer taxes, designed to deter foreign capital inflows and stabilize housing prices.

In this paper, we use geographic and temporal variation in exposure to international capital flows to study their impact on an untaxed substitute market, the U.S. housing market. First, we examine the effects of these extreme capital flows and international foreign buyer taxes on housing prices and quantities. Second, we estimate elasticities of house prices and quantities with respect to foreign capital. Third, we use this natural experiment to isolate a housing demand shock and estimate new local house price elasticities of supply for the 100 largest U.S. cities.

In order to measure the direct impact of increased foreign capital on domestic housing markets, we exploit both time-series variation in international tax policy and cross-sectional variation in the likely destinations for these investments. Foreign buyer taxes were first imposed in Singapore in December 2011, and have followed a predictable path thereafter based on the proximity to China and the associated influx of Chinese foreign capital through Hong Kong, Australia, and Canada. We therefore define our policy intervention date based on Singapore's adoption of their foreign buyer tax.

Our cross-sectional variation in the predicted destinations for foreign investment builds on the immigration literature that finds differential likelihoods of immigrant arrival based on the pre-existing mix of foreign-born residents in a local market (Altonji and Card, 1991; Card and DiNardo, 2000; Card, 2001); namely, foreign capital is expected to flow to foreign-born enclaves. We compare house price growth in zipcodes with larger shares of foreign-born residents to those less likely to attract foreign capital. To establish the credibility of the demand shock, we initially focus on foreign-born Chinese purchasers. While applicable to all foreign purchasers, the substantial capital outflows from China over this period and large Chinese resident population in the U.S. enable a difference-in-differences analysis to document meaningful domestic price effects of foreign macroprudential policy.[1]

Estimates from our difference-in-differences design suggest that house prices rose differentially in high foreign-born Chinese neighborhoods relative to the rest of the U.S. by 8 percentage points (pp) from 2012 to 2018. In contrast, quantities (as measured by permitting activity) rose differentially by 1.85pp. We explore a range of alternative counterfactual house price paths to address concerns related to the parallel trends assumption inherent in this comparison: house prices are assumed to have evolved similarly across zipcodes before and after 2012. Beyond comparing pre-trends in an event study design, we also generate control groups through propensity score matching and synthetic control exercises. Our preferred specification includes MSA-by-quarter fixed effects to control for heterogeneity in the housing market recovery across cities and instead focus exclusively on within-city variation in pre-shock immigrant concentration. Using only this within-city variation for identification also mitigates concerns of sorting by investors or immigrants based on inter-city labor market prospects. Finally, we show that in light of recent capital control tightening in China, as well as escalation of the U.S.–China trade war, the differential price gain in immigrant enclaves has fallen sharply since 2018.

In order to measure the elasticity of house prices and quantities to foreign capital more generally, we move away from focusing only on foreign-born Chinese neighborhoods and develop a continuous measure of expected foreign capital inflows for each zipcode. This approach introduces other foreign buyers besides the Chinese and relaxes the binary "high foreign born" treatment approach taken in our first empirical exercise, thereby capturing the broader variation in the foreign-born population across zipcodes. Our continuous expected

[1]Using other immigrant groups with less capital variation in such a blunt research design is noisy. In our later analysis we unpack this reduced form method, study continuous capital flows directly, and incorporate all foreign purchaser groups.

capital flow measure distributes foreign capital from any country of origin throughout the U.S. to zipcodes based on their pre-period foreign-born composition (in the style of a Bartik instrument). We then instrument for expected capital flows into these foreign-born enclaves with the difference-in-differences in the first stage, using fraction foreign-born instead of binary zipcode status. The benefit of using this method is twofold. First, it removes the simultaneity bias coming from domestic housing supply responses, since this shock is driven by foreign tax policy changes. Second, it uses only relevant housing demand-side variation since individual purchasers experience the tax shock, while foreign firm and government capital is not affected.

This two-step research design yields an estimated elasticity of house prices to expected capital flows of 0.27. Expected capital flows increased by 32% for the median zipcode. This increase in foreign investment would thus translate into an 8.6% increase in house prices. The median zipcode experienced 42% house price growth over our sample period, so increased foreign capital can explain roughly one-quarter of observed house price growth. For quantities, we estimate a supply elasticity with respect to capital flows of 0.004. The median county in our sample increased expected capital flows by 80%. With the median county's quarterly growth rate of 0.003 to its housing stock, the influx of foreign capital more than doubles the low baseline permitting rate for new construction. We conclude that foreign capital flows significantly affected both prices and quantities of exposed housing markets.

In the final empirical exercise, we use the demand shock from foreign capital inflows to construct a new local house price elasticity of supply for a given city. We construct this elasticity by taking the ratio of the estimated house price and quantity elasticities with respect to expected capital flows. We estimate short-run supply elasticities for the 100 largest U.S. cities (as measured by population in 2010). The average city's housing market is highly inelastic in the short run, with an elasticity of 0.10. Our new measure produces a metro ranking consistent with others in the literature, with Providence, San Francisco, and Boston as the least elastic metros, and places like Ocala, FL and Salisbury, MD as the most elastic over this ten-year period (2009–2018). These estimates are robust to a variety of instrument specifications and controls for differential tech sector growth, and provide new local measures of how responsive housing construction is to demand shocks over the most recent time horizon.

Our work contributes to a growing literature on cross-country capital flows and their impact on asset markets such as housing. In related concurrent work, Li, Shen and Zhang (2019) find that a Chinese demand shock in three California cities between 2007 and 2013 significantly raised house prices in areas exposed to more Chinese immigrants, with the largest impacts after 2012, in line with our post–period results. Badarinza and Ramadorai (2018) examine inflows to the London housing market from countries experiencing political risk, Sá (2016) explores properties in the U.K. owned by foreign companies, while Cvijanovic and Spaenjers (2018) study the effect of international buyers on the Paris housing market. A now extensive literature has also emphasized the role of investors and out-of-town buyers during the U.S. housing boom (Bayer et al., 2011; Chinco and Mayer, 2015; Favilukis et al., 2012; Favilukis and Van Nieuwerburgh, 2017; DeFusco et al., 2018). We extend this literature by exploring a time period when international housing purchases in the U.S. skyrocketed and were uniquely driven by the growth in purchases from one country (China).

These results also inform the literature on the impact of immigration on housing markets. We similarly exploit cross-sectional variation in pre-existing population shares, as in Card (2001), expanding the applicability of this strategy beyond the flow of migrants to predicted exposure to capital flow shocks. Many papers have examined the impact of immigrants on house prices directly, such as Sá (2014), Saiz (2003, 2007), Saiz and Wachter (2011), Akbari and Aydede (2012), Pavlov and Somerville (2016). In our setting, as in some mentioned above, these housing purchases are likely to be used as secondary residences, rather than as primary residences or investment properties; In Appendix B.2, we show that while rents have risen slower than house prices, there is still evidence of rental displacement as rents rise by 5% more on average in highly exposed areas.[2]

Our work documents an important consequence of international macroprudential policies: Foreign buyer taxes in other countries affect the flow of capital into the United States. Claessens (2014) provides an overview of these macroprudential policy tools and their relationship with housing markets. As discussed in detail by Gergen (2017), taxes of this

[2] A related strand of literature has explored the role of institutional, but not necessarily international, investors in the U.S. housing market. See, e.g. Lambie-Hanson, Li and Slonkonsky (2019), and Agarwal, Sing and Wang (2018) on foreign institutional investors in commercial real estate.

nature are especially likely to lead to avoidance or evasion. We innovate by using these non-U.S. macroprudential policies as a shock to U.S. housing markets, thereby incorporating the effects of policy to influence the destination of international capital flows.

Finally, our work contributes to a growing literature estimating local house price elasticities. Gyourko and Summers (2008) show that the U.S. housing market has a large spatial distribution of regulatory policies, and construct a local measure of regulatory stringency. Saiz (2010) uses this local measure in combination with geographic and topographic characteristics to provide long–run estimates of supply elasticities, and Cosman and Williams (2018) update this model by incorporating dynamic changes to available land. Consistent with Gyourko, Hartley and Krimmel (2019) survey-based result that local housing markets have become increasingly regulated, Aastveit, Albuquerque and Anundsen (2019) instrument for house prices with crime rates and disposable income changes and find that housing markets have become more inelastic. In complementary work, Baum-Snow and Han (2019) use Bartik labor demand shocks, in conjunction with theory, to construct hyper-local house price elasticities. While a local labor market shock exploits intensive-margin variation in demand within a market coming from improvements in local labor outcomes, we instead exploit extensive-margin variation in the form of a housing demand shock originating from foreign countries to estimate the housing market's short-run response. These two new approaches use different sources of variation, and can be calculated over different geographies and time horizons, thus providing new directions for estimating more locally-relevant house price elasticities of supply.

In the next section, we provide background on the tax policies and Chinese capital flight. Sections 3 and 4 discuss our data and differences-in-differences research design, while Section 5 presents the results. We discuss our instrumental variables design and results in Section 6. House price elasticity results are presented in section 7. Section 8 concludes.

2 Background

Our identification strategy relies on a change in the probability of international investment in the U.S. housing market after foreign purchaser tax policies are implemented elsewhere.

While applicable to all international buyers, many of these policies targeted Chinese buyers during a period when capital flight from China sharply increased. In this section, we document the growth in Chinese capital investment in the U.S. housing market and describe the foreign buyer tax policies that steered capital toward the untaxed U.S. market starting in 2011.

2.1 Chinese Policies Impacting Capital Flight

As shown in Figure 1(a), beginning in 2012 and accelerating in 2014, massive amounts of capital fled China, peaking at over $200 billion per quarter in the second half of 2015.[3] While China is known for its tight capital controls, limiting official renminbi to USD conversions to $50,000 per person per year in 2015, huge volumes of capital left the country through pooled individual conversions and underground banking networks.[4] Relaxation of policies related to capital outflows, in conjunction with anticipatory behavior around an expanded anti-corruption campaign, led to sharp increases in capital flight beginning in 2013 and continuing for the next four years.[5]

In September 2013, the Chinese State Council approved the Shanghai Free Trade Zone, with the intention to increase investment in China and abroad.[6] Combined with free trading of renminbi in offshore financial markets like Hong Kong, capital began to flow out of China in earnest in 2014. In addition, the Chinese savings glut had trouble finding returns in domestic asset markets outside of housing, which became increasingly expensive.[7]

The drive to crackdown on corruption seemed, if anything, to initially spur even more capital flight. In contrast to financial institutions, real estate agents were generally not required to report suspicious activity associated with money laundering, such as large cash

[3] Our measure of capital outflows comes from China's State Administration of Foreign Exchange's (SAFE) time series of balance of payments. Capital outflows are defined as the quarterly sum of: 2.2.1.2. Portfolio Investment, 2.2.1.4. Other Investment, and 3. Net errors and omissions, in accordance with the definition used by Fitch Ratings.

[4] Hu, Fox, Alfred Liu, Paul Panckhurst and Sheridan Prasso. "China's Money Exodus: Here's how the Chinese send billions abroad to buy homes," *Bloomberg News*, Nov. 2, 2015.

[5] Sender, Henry. "China's anti-corruption push may drive wealthy overseas." *The Financial Times*, Nov. 11, 2014.

[6] Orlik, Tom. "China Signals Speedier Moves to Loosen Capital Controls; Bank Official Says Recent Volatility Shouldn't Hinder Reform," *The Wall Street Journal*, Sept. 5^{th}, 2013

[7] Bradsher, Keith and Dionne Searcey. "Chinese Cash Floods U.S. Real Estate Market." *The New York Times*, Nov. 28, 2015.

transactions or purchases involving LLCs Hundtofte and Rantala (2018). In November 2014, private bankers in Hong Kong stated that demand for foreign real estate had grown since the anti-corruption campaign began.[8] Not surprisingly, from 2013–2014, 76% of Chinese home purchases in the U.S. were all-cash, with an average transaction price of over $590,000. For comparison, during the same time period, 33% of domestic purchases were all cash, with a mean transaction price of $247,000.[9]

Two notable events have limited capital flight from China during the later years of our data period. First, at the end of 2016, the Chinese government began limiting the amount of capital leaving the country by requiring financial institutions to report overseas transfers above $10,000, and disallowing the $50,000 individual quota to be used to purchase overseas property. Capital outflows dropped in earnest during 2017 and 2018. Second, the rise of the U.S.–China trade war has disrupted investment in a variety of U.S.-based assets. In this paper, we largely focus our analysis on the period 2010–2018; however, we also provide evidence that these two capital limitations have negatively impacted domestic housing markets in Section 5.3.

2.2 Foreign Buyer Tax Policies

Observing foreign investment bidding up domestic house prices, many countries have imposed taxes on the purchase of housing by foreign buyers. For instance, Singapore, Hong Kong, Australia, and Canada have all introduced taxes in recent years.[10] These policies add a stamp tax or additional duty to purchases by foreign buyers, ranging from 3% (Victoria, Australia's first tax) to 20% (Singapore's third tax). Some of these foreign buyer taxes have been coupled with "empty home" taxes as in British Columbia and New South Wales, or limits on foreign ownership of new apartment and hotel construction projects, as in New South Wales and New Zealand.

[8] Henry Sender, "China's anti-corruption push may drive wealthy overseas," *Financial Times*, November 11th, 2014.

[9] Yun, Lawrence, Jed Smith and Gay Cororaton, "2014 Profile of International Home Buying Activity: Purchases of U.S. Real Estate by International Clients for the Twelve Month Period Ending March 2014." *National Association of Realtors*, June 2014.

[10] See Appendix A for details of these tax policies. In addition, New Zealand has recently banned non-resident foreigners from buying homes, while the United Kingdom's Conservative Party and the New York Democratic governor have new proposed pied-a-tierre taxes.

The reported political motivations for these taxes have focused on the macroprudential stability of housing markets and affordability for domestic residents. However, the implementation of these taxes have predictably responded to an influx of Chinese foreign capital sharply driving up the cost of housing. Figure 2 shows the time series of price indices of select international housing markets, with vertical lines denoting periods between Singapore's first tax in December 2011 and the relevant location's foreign buyer tax adoptions. Most geographically proximate to China, Singapore and Hong Kong experienced rising prices from 2010 to 2012, as shown in panels (a) and (b). Chinese investment moved east to Australia, shown in panels (b) and (c), then further east to Canada, shown in panels (d) and (e). Figure 3 summarizes the timing and location of the enactment of these taxes, displaying how they originated in countries closest to China beginning in 2011 and expanded outward thereafter.

Figure 1(b) presents time series of foreign home sales from the National Association of Realtors from 2009–2019. The gap between Chinese and all foreign home sales volumes began to widen over 2013–2014, as the anti-corruption campaign escalated and the tax polices came into effect in more countries. In what follows, we estimate the effect of this influx of international capital on U.S. housing markets.

3 Data

3.1 Treatment Definition

In order to differentiate exposure to Chinese capital flowing into the U.S. housing market, we draw on the methods from Altonji and Card (1991) and Card (2001), in which immigrants tend to move to enclaves in which other immigrants of their same origin country previously settled. In our context for capital, possibly attached to immigrants but in many cases unattached, we anticipate that Chinese capital is most likely to flow to locations with *ex-ante* high shares of Chinese immigrants. This is the hypothesis we test in our difference-in-differences specification.

Chinese purchasers may seek to invest their capital in cities with initially high Chinese populations, and purchase real estate by employing an agent who has worked with Chinese buyers in the past. Recent work by Badarinza, Ramadorai and Shimizu (2019) suggests purchasers of commercial real estate prefer to transact with sellers of the same origin country, regardless in which country the transaction takes place, while Li, Shen and Zhang (2019) show a direct increase in Chinese names among home buyers in areas with prior exposure to many Chinese immigrants. News sources report that Chinese capital has flowed disproportionately to cities known to have higher than average shares of residents of Asian descent, such as Melbourne, Sydney, Vancouver, San Francisco, and Seattle.[11] These areas are likely attractive to foreign buyers as they already have familiar language, other cultural infrastructure, and communities for the foreign buyers. Note, of course, that residential real estate purchases need not be tied to historical immigration networks, as these properties may not be regularly visited, or visited at all, but instead owned solely for investment purposes.

To define our treatment group, we use data from the 2011 American Community Survey (ACS) to construct the share of the zipcode's population originating from any foreign country.[12] For our difference-in-differences analysis, we define as "treated" those zipcodes whose Chinese immigrant share in 2011 is above the 99^{th} percentile, denoted as "foreign-born Chinese" zipcodes, FBC_i.

$$FBC_i = 1\left\{\frac{FBCpop_i}{pop_i} \geq 99^{th} percentile\right\} \tag{1}$$

The treatment indicator equals 1 for those zipcodes with at least 5.7% foreign-born Chinese residents, the 99^{th} percentile cutoff. Nationally, the average zipcode in our sample is 0.4% foreign-born Chinese, with the median zipcode having no Chinese immigrants. For our instrumental variables approach to estimating the price elasticity of supply, we define a continuous treatment measure, expanded to all immigrant groups, discussed in Section 6.

Figure 4 shows the geographic distribution of our treatment variable, $FBC = 1$. Panel (a) shows that treated zipcodes are clustered in many coastal cities such as New York City,

[11] Fong, Dominique, Rachel Pannet and Paul Vieira. "Western Cities Want to Slow Flood of Chinese Home Buying. Nothing Works." *The Wall Street Journal*, June 6, 2018. Bradsher, Keith and Dionne Searcey. "Chinese Cash Floods U.S. Real Estate Market," *The New York Times*, Nov. 28, 2015.

[12] ACS 5-year estimates, see table DP05 for total population and table B05006 for foreign-born population by country of origin. The ACS tables are available at the zipcode level from 2011 onwards. The Decennial Census (2000) provides "Place of Birth for the Foreign-Born Population" (Table PCT019). There is no comparable data available between 2001 and 2010.

Seattle, San Fransisco, Los Angeles, Washington, D.C., and Boston. Note however that our treatment definition is not restricted to the coasts; large Chinese immigrant communities are also present in Houston, Atlanta, and even Nebraska. Panel (b) shows the fraction of county population that is foreign-born Chinese (used in our housing supply analysis). Counties shaded in red are treated (by being in the top 1% of counties), and are distributed across almost every state. Furthermore, 25% of the counties in our sample have at least 0.2% of their total population born in China.

3.2 Policy Intervention Definition

We define our policy intervention date based on Singapore's first foreign-buyer tax adoption in 2011q4:

$$Post_t = 1\{t \geq 2011q4\} \tag{2}$$

Amid all of the tax policy changes listed in Section 2.2, we choose the timing of Singapore's adoption of the foreign buyer tax as it was the first of its kind and prompted a wave of similar policies.

3.3 House Prices

We use CoreLogic's transactions database to construct zipcode-level hedonic house price indices from 2000 to 2018. We limit the sample to the 48 contiguous states as well as Washington, D.C., and only include zipcodes with at least 20 transactions between 2000 and 2018 to cut down on especially noisy index construction. Our data contains numerous characteristics of the transacted home and lot.

We rely on hedonic price indices because it is not feasible to use repeat-sales to quality control for a housing stock at such small geographies. Since the main aim of the repeat-sales index is to control for housing characteristics, we include covariates in the hedonic index that capture the variation in housing quality and characteristics over the time period. As shown in Equation 3, for each transaction j we control for lot size in acres, living square footage, year built, number of bedrooms, number of bathrooms, and whether the house has a garage.

We construct the index separately for each zipcode i. This yields a zipcode-by-quarter panel of house price indices, $hpi_{it} = \beta^i_t$. To validate the robustness of our hedonic methodology, to examine the most recent time periods after 2018, and to study rental markets, we also use Zillow's Zillow Home Value Index and the Zillow Rent Index in our analysis.[13]

$$ln(Price^i_{jt}) = \beta^i_t qtr_t + \delta Acres^i_{jt} + \gamma Sqft^i_{jt} + Built^i_{jt} + Bed^i_{jt} + Bath^i_{jt} + Garage^i_{jt} + \eta^i_{jt} \quad (3)$$

After constructing these indices for each zipcode, we limit our sample to 2009–2018 to avoid the house price collapse in 2007–2008. This yields valid house price indices for 26,040 zipcodes across 2,659 counties, covering 42.5 million transactions. Appendix Table D1 shows the housing characteristics for the zipcode-quarters in our sample.

3.4 Housing Supply and Additional Economic Data

To study the supply of new housing, we use data from the Census' Building Permits Survey, 2005–2018. We collect monthly county-level building permits for single– and multi–family units, aggregating totals to the quarterly level of analysis to be consistent with the house price indices.

In our matched sample and synthetic control robustness checks, we include a number of real economic variables to control for local economic characteristics. We use zipcode level annual employment, establishment counts, and payroll data from the County Business Patterns, 2005–2011. We also include zipcode population data from the 2010 Decennial Census and 2011 zipcode population data and median income from the ACS.

3.5 Expected Capital Flows

As our measure of capital flows, we collect aggregate data on foreign sales volume from the National Association of Realtors' (NAR) Annual Profiles of International Home Buyers from 2011 to 2019. This measure is derived from an annual survey of randomly selected realtors. The 2019 survey was sent to 150,000 randomly selected realtors, of which about 12,000

[13] See Appendix B.2 for details on Zillow data construction.

replied, with 12% reporting experience helping an international client in the last 12 months. The NAR observes substantial specialization among realtors, with 4% of all realtors in 2011 reporting that over 75% of their transactions came from international clients (NAR, 2011). This pattern is likely due to language and cultural familiarity among a subset of realtors, supporting the network effects assumption we make in order to define the treatment group of zipcodes. In contrast to other methods that attempt to identify foreign-born residents by name, such as Li, Shen and Zhang (2019), we use aggregated data on identified international clients. This approach assuages concerns of identifying American citizens and residents as international when they share similar ethnic names, a particular concern given that foreign investors tend to purchase in cultural enclaves.

The 2011 NAR report includes data as far back as 2009. Each report provides a national estimate for the sales volume purchased by international clients originating from Canada, China, Mexico, India and the United Kingdom, as well as the total sales volume purchased by all international clients. The NAR defines an international client in two ways: 1) Clients with a permanent residence outside of the United States, purchasing in the United States for the purpose of investment, vacation, or stays shorter than 6 months; or 2) Clients who have immigrated to the United States in the past two years, or who have temporary visas and plan to reside in the United States for more than 6 months. The NAR profiles do not distinguish between sales volume going to the two types of international clients, nor do they provide detailed geography by source country of these foreign sales.[14]

4 Difference-in-Differences Empirical Design

While the foreign-buyer taxes impact any buyer from a foreign country, no immigrant group invested as much in the U.S. housing market as the Chinese after the implementation of these taxes. Chinese home purchase volume more than tripled from $6.8 billion in 2009 to $31.6 billion in 2017, before falling to $13.4 billion in 2019.[15] The difference-in-differences

[14] Appendix B.3.2 attempts to disentangle whether these international clients contribute to immigration or population changes.

[15] Over the same time frame, purchases from Canada grew from $11B to $19B (73%), those from India grew from $5B to $7.8B (56%), and from the United Kingdom fell from $11.4B to $9.5B (-20%). In addition, local immigrant population shares are much smaller from these countries; the 99^{th} percentile treatment

framework is too blunt a design to estimate local house price responses to foreign-buyer taxes without large changes in purchase volumes and meaningful population shares. However, after showing that the foreign-buyer taxes do in fact affect local prices and quantities in the Chinese context, we expand the analysis to both include home purchase capital flows other foreign countries, and relax the treatment condition to use the continuous fraction of the foreign-born population.

The difference-in-differences analysis compares treated zipcodes, those with high shares of foreign-born Chinese residents, to control zipcodes, those with lower shares. For this design to be valid, treated and control zipcodes must trend similarly in house prices and quantities absent the tax policy changes that redirected capital to the U.S. housing market. While panel (a) in Figure 5 and Table 1 support parallel trends in the pre-period for housing market characteristics, not all demographic or labor market characteristics are balanced between treated and control zipcodes. To mitigate concerns regarding the comparability of treatment and control groups and the sorting of foreign-born purchasers into markets with positive amenities correlated with house prices, we modify our approach by including geographic time trends. Going further, we perform additional analyses with a propensity score matched sample and a synthetic control sample based on pre-period data, using the modified samples in the differences-in-differences specification.

Our baseline specification in Equation 4 uses a generalized difference-in-difference design for zipcode i in quarter t:

$$HPI_{it} = \alpha + \beta FBC_i \times post_t + \zeta_i + \theta_t + \eta_g \times \theta_t + \varepsilon_{it} \tag{4}$$

The parameter of interest is β, which measures the differential house price in treated versus control zipcodes after Singapore introduced its foreign buyer tax. We also include zipcode, ζ_i, and quarter, θ_t, fixed effects. In order to address concerns that our design is capturing broader local trends instead of level differences in means, we additionally control for state-by-quarter, commuting zone-by-quarter, or MSA-by-quarter trends, with trend geography denoted by g.

zipcode cutoffs would be 1.7% for the U.K., 2.5% for Canada, and 4% for India.

These regressions compare zipcodes with relatively larger shares of foreign-born Chinese population to the rest of the United States. We directly address labor market or investment sorting concerns by controlling for geography-by-time fixed effects to make comparisons *exclusively* within the same MSA in the same quarter. Below, we also construct matched samples and apply a synthetic control method, but we first present the straightforward difference-in-difference estimates using the full sample of control zipcodes.

5 Establishing the Local Housing Demand Shock

5.1 Difference-in-Differences Results

Figure 5a presents the comparison between the house prices of high fraction foreign-born Chinese (FBC) zipcodes, that is, zipcodes in the top 1% of all zipcodes ("treated" zips), and all other zipcodes ("control" zips). The figure first shows smooth and parallel house price trends prior to the start of 2012, after which Chinese capital flows (Chinese home purchases in the U.S.) increased. After the last quarter of 2011 (indicated by the vertical line), the two house price series sharply diverge, with treated zipcodes experiencing much greater house price appreciation between 2012 and 2018.

Table 2 formalizes this comparison in our difference-in-differences regression framework, with associated quarterly event study difference-in-differences coefficients from column (4) presented in Figure 5b. Column (1) of the table includes both quarter and zip fixed effects, and each column adds progressively more restrictive geography-by-time fixed effects to flexibly account for different patterns in house prices in different geographies. The estimated differences in house prices between treated and control zipcodes are consistently large and statistically significant, ranging from 8 to 17 percentage points (pp) higher in FBC zipcodes, depending on the specification.[16] Our preferred estimate in is column (4), where even after flexibly conditioning on commuting zone-specific time trends, we estimate that after 2012, house prices in high foreign-born Chinese zipcodes were 9.5pp higher than in control zipcodes in the same MSA.

[16] Standard errors are clustered by quarter in column (1), and in the other columns are clustered at the level of geography associated with the geography-specific time fixed effects.

Decomposing the treatment group into a more continuous treatment measure, Table 3 shows the house price changes for zipcodes with foreign-born Chinese population shares in the $50^{th} - 90^{th}$ percentiles, $90^{th} - 95^{th}$ percentiles, $95^{th} - 99^{th}$ percentiles, and above 99^{th} percentile relative to the lower half of the distribution of zipcodes (which contain no foreign-born Chinese residents). The results are consistent with the more blunt treatment measure: House prices rose the most in zipcodes with higher shares of foreign-born Chinese residents. Specifically, in our preferred specification in column (4), we find that zipcodes in the 99^{th} percentile of foreign-born Chinese share see house prices 13pp higher than those in the bottom half of the distribution. However, the zipcodes need not be that concentrated; zipcodes in the $95^{th} - 99^{th}$ percentiles see a 6pp house price increase, the $90^{th} - 95^{th}$ percentiles see a house price increase of 2.5pp, and the $50^{th} - 90^{th}$ percentiles a 2pp increase.

A notable feature of Chinese buyers of U.S. housing is that they tend to purchase relatively more expensive properties than either other foreign buyers or domestic U.S. buyers. As mentioned above, the average house price for Chinese buyers at their peak purchasing volume in 2017 was over $780,000, while other foreign buyers spent $537,000 and domestic buyers spent $278,000 on average (National Association of Realtors 2017). This difference may reflect the fact that Chinese and other foreign nationals buy more expensive houses in low-price communities, or, more likely, that they tend to locate in high-price communities. We thus investigate whether the responsiveness of house prices to Chinese capital flows is concentrated in the most expensive tier of housing. If house prices responded in all tiers, we might be concerned that other drivers of price appreciation, such as local labor markets or improved amenities, were instead contributing to the house price appreciation we document.

In Figure 6a, we separately plot the path of house prices for those high foreign-born Chinese zipcodes in the top quintile of the U.S. house price distribution (in red), and for those in the other four quintiles (in blue). The time series of house prices for all other zipcodes is plotted in black. Prior to 2012, the three house price series have nearly identical trends. Starting in the first quarter of 2012, the house price series for the foreign-born Chinese zipcodes in the top quintile of prices diverges sharply from the other two series and stays persistently higher through the end of 2018. Table 4 reproduces the difference-in-differences specifications of Table 2, but interacts the treatment indicator with the quintile of the house price distribution of U.S. zipcodes, grouping the bottom four quintiles into one indicator. The table shows minimal differences for zipcodes in the bottom four quintiles, with most of the effect of Chinese capital flows after 2012 concentrated in the high foreign-born Chinese zipcodes in the top 20 percent of the initial house price distribution. Not only does foreign capital flow to immigrant heavy communities, column (4) in Table 4 shows it targets expensive zipcodes within a given local market.

Has this increase in house prices, induced by an influx of foreign capital, translated into real economic effects? In Table 5 we explore this question, using data on the construction of new residential buildings from the U.S. Census' Building Permits Survey, as discussed in Section 3.4. The data is less granular than the house price indices we constructed from transaction data, and is instead available at the county-by-quarter level. The table presents estimates from difference-in-difference specifications similar to those in Table 2. The dependent variable is defined as the cumulative new stock (sum of all permits between 2011 and time t $\forall t$) relative to 2011, in percentage points:

$$NewBuild_{it} = 100 \times \sum_{\tau=2011}^{t} \frac{Permits_{i,\tau}}{Stock_{i,2011}} \tag{5}$$

In column (4), our preferred specification that includes commuting zone-specific time controls, we estimate that high foreign-born Chinese zipcodes experienced 1.85 percentage points more cumulative additional stock after 2012, a significant portion of the 2.63 percentage point average cumulative stock created over the same time period. This estimate provides new evidence that Chinese capital flows have had a direct and local effect on real construction activity in the United States.

In sum, in this section we have documented differential house price and housing supply growth in zipcodes that were ex-ante more likely to be destinations for Chinese capital, namely those with a larger pre-existing foreign-born Chinese population. In contrast, we find no differential house price effects in other zipcodes that may be affected by international capital flows, but that are not destinations with pre-existing relationships with large shares of Chinese residents.

5.2 Robustness to Counterfactual Construction: Matching and Synthetic Control Approaches

While the prior section provides straightforward evidence of similar patterns prior to 2012 and divergence thereafter between high foreign-born Chinese zipcodes and all other zipcodes, a natural concern might be that the drivers of house price growth may not necessarily have evolved in parallel across these two groups of neighborhoods. In addition, given that our approach to defining "treatment" is to focus on the top 1% of zipcodes in terms of foreign-born Chinese residents, there may be concerns with including other high-percentile zipcodes in the "control" group, when the treatment of increased capital flows may instead affect the top 5%, 10% or 25% of zipcodes (as suggested by Table 3).

In this section, we address this concern by creating two new counterfactual house price trends based on propensity score matching and synthetic control techniques. As both approaches yield results that are very similar to those of the baseline difference-in-differences design, we describe the matching exercise here and report on the synthetic control approach in Appendix Section B.1.

To construct a matched control group, we first restrict the sample to those with the fraction of foreign-born Chinese below the 75th percentile. This restriction, which limits the foreign-born Chinese population to at most 0.3 percent, addresses the issue related to the arbitrary cutoff of the top 1%. Next, we keep only those MSAs with at least one zipcode in the top 1% in order to match on MSA. Third, we generate a propensity score for each zipcode by regressing (probit) FBC_i on zipcode-level characteristics from 2010 and 2011. We include demographic, economic, and housing characteristics, listed in Table 1. Fourth, we match one nearest-neighbor control zipcode within the same MSA to each treated zipcode with replacement.[17]

Figure 7 presents the time series of house prices for the matched control group, in blue, and the treatment group (same series as in Figure 5a) in red. The two house price series are on the same trends in the pre-period, and then diverge starting in 2012 (the vertical line). Table 6 presents results from difference-in-difference specifications similar to those above, this

[17] Appendix Table D2 presents the covariate balance between FBC and non-FBC zipcodes after matching.

time using the matched control group (hence the much smaller number of observations). The results are quite stable regardless of the specificity of geography-by-time fixed effects, with estimated differences in prices between 8 and 17pp for more foreign-born Chinese locations relative to their matched counterparts.

The findings from this matching exercise suggest that our estimates are not particularly sensitive to the choice of control group when conducting the difference-in-differences estimation. In Appendix Section B.1, we conduct an additional counterfactual construction exercise using synthetic control techniques, and find relatively precisely estimated house price differences of around 11pp between high foreign-born Chinese zipcodes and their "synthetic" counterparts. In Appendix Section B.2, we substitute the zipcode-level Zillow Home Value Index (ZHVI) for our constructed house price index using Corelogic transactions data, and the results remain robust. Finally, Appendix Section B.3.1 discusses the impacts on real business outcomes such as employment and establishments, as well as immigration and population flows.

We conclude that these additional approaches to constructing plausible counterfactual house price paths support the broader difference-in-differences assumptions related to parallel trends: Namely, that this influx of Chinese capital represented an unexpected shock to local housing markets, and that the neighborhoods affected by this shock were predominantly those with high ex-ante exposure in the form of a larger share of foreign-born Chinese residents.

5.3 What Happens when Foreign Capital Dries Up?

The escalation of the U.S.–China trade war and concurrent crackdown on Chinese capital flight provides a stark reversal in treatment for the FBC zipcodes.[18] In order to expand the time series beyond the onset of the trade war, and the peak of the national housing market which occurred in late 2018, we use data from Zillow's Home Value Index (ZHVI) through the end of 2019.

[18] For a discussion of Chinese capital restrictions, see Shen, Samuel and Galbraith Andrew, "China constrict capital outflows with eye on yuan stability," October 11, 2018. For a brief summary of real estate investment declines in response to Chinese capital controls, see Olsen, Kelly "Beijing's capital controls are weighing on Chinese investors looking to buy property abroad," CNBC, February 26, 2019. For a timeline of the U.S.–China trade war, see "Timeline: Key dates in the U.S.–China trade war," Reuters, January 15, 2020.

To illustrate how one city's housing market can respond to swings in available foreign capital, we look to Seattle. Proximate to Vancouver, a city with increasingly tight foreign ownership regulation, and with a large foreign-born Chinese population, Seattle has experienced considerable house price volatility in recent years. Importantly, this volatility differs by a zipcode's foreign-born composition. We categorize each zipcode in the CBSA as either FBC, an FBC-border zipcode, or neither. FBC-border zipcodes are those that are contiguous with FBC zipcodes; they share a geographic border. This yields 9 FBC zipcodes, 22 border zipcodes, and 116 other zipcodes within the Seattle CBSA.

Figure 8 shows the evolution of Seattle's monthly Home Value Index from 2009 to the end of 2019, by zipcode type. We normalize each zipcode's index to 1 in 2011m12, the month Singapore imposed its first tax. The figure shows that by mid-2018, while FBC and border zipcodes more than double their index values, other zipcodes did not gain nearly as much, likely as they attracted less foreign capital.

The gray vertical bars show capital outflows from China through 2019q4. Capital controls and the beginning of the trade war led to falling capital outflows beginning in 2017, bottoming out in in mid-2018. This timing coincides with Seattle's housing market peak. In one year, between the peak in 2018m6 to the trough in 2019m6, FBC zipcodes lost 4% of their value, while other zipcodes saw constant prices. In the rebound since the local trough, other zipcodes again saw faster house price growth, adding 3%, while FBC zipcodes saw only a 1.5% rebound.

We next examine the effect of a sharp reduction in foreign investment nationwide in Figure 9. We index each zipcode's house price to the quarter of Singapore's tax introduction, 2011q4, and extend the previous analysis in Table 2 and Figure 5 through 2019. As shown in panel (a), according to Zillow's HVI, house prices in FBC zipcodes rose approximately 70pp between 2011q1 and 2018q4, while control zipcodes only saw 40pp price growth.

Implementing the differences-in-differences design, and including commuting zone time trends, panel (b) plots the difference-in-differences estimate for differential price growth in FBC areas. Panel (b) shows that on average, FBC zipcodes saw 6pp additional price growth that control zipcodes in the same commuting zone did not; however, this differential gain falls sharply almost to zero by the end of the sample period, due to the escalation of the trade war and capital crackdown which began in 2018. Our analysis thus uses variation in foreign capital increases and decreases to provide new evidence that liquid foreign capital can induce large price swings in domestic housing markets.

6 Price and Quantity Elasticities

We now address the more general question of how liquid foreign capital impacts local asset prices and quantities, with the goal of constructing new local house price elasticities of supply. Disentangling causality between global capital flows and local asset prices is challenging, as local communities may draw global capital to their own local markets in the United States through immigration, educational spending, or reverse remittances. Americans also demand foreign currencies in order to purchase goods produced abroad. Adding measurement error, foreigners supply capital in many ways, by investing in U.S. firms, buying U.S. government debt, and purchasing commercial real estate, in addition to their investments in residential real estate.

In our setting, the series of foreign buyer tax policies adopted by other countries serve as an exogenous demand shifter into the United States housing market. We use this expansion of capital supply interacted with the fraction of the zipcode that is foreign born in 2011 ($fracFB_i$) to instrument for capital flows into the United States. In addition, we use the home purchase capital flows measure discussed in Section 3.5, instead of a more general gross capital flow measure, to reduce measurement error introduced by different types of foreign investors, such as firms or governments. By using home purchase capital flows in conjunction with variation targeting home purchasing, we can estimate the more fundamental elasticities of interest: the elasticity of price with respect to foreign capital, and the elasticity of supply with respect to foreign capital. Taking those two elasticities together, we can construct a new measure of the price elasticity of supply for local U.S. housing markets.

The instrumental variable design requires satisfaction of both the relevance condition and the exclusion restriction. The relevance condition in this context requires that more capital flows into the U.S. housing market after other countries impose foreign buyer taxes. Formally, $E[ln(\widehat{EC}F_{it})(fracFB_i \times Post_t)] \neq 0$. The difference-in-differences results presented above show that the instrument has a positive correlation on the second stage outcome variable. In addition, panel (a) in Figure 1 shows a clear positive correlation. Finally, we perform first stage tests to show positive correlation with a high F-statistic.

The exclusion restriction requires that foreign buyer tax policy changes only impact U.S. house prices by diverting capital into the housing market, $E[\epsilon_{it}(fracFB_i \times Post_t)] = 0$. A violation of this restriction would have foreign buyer taxes imposed on real estate purchases impacting the U.S. housing market through another mechanism besides direct investment in homes. Investment in the local economy more broadly, such as in local businesses, would be one example. In the difference-in-differences section, we control for precisely this concern using geographic trends, matching, and synthetic control estimators. Additionally, in Appendix C.1, we test whether investments related to growth in the tech industry violate the exclusion restriction, and find no support for this alternative explanation.

6.1 Expected Capital Flows IV

Because we are no longer estimating a simple comparison in means in response to a blunt tax policy, we introduce capital flows and populations from other foreign countries into our analysis to better estimate housing market responses to foreign demand shocks. We now include home purchase volume as our measure of capital flows from China, Canada, India, Mexico, the U.K. and "other" foreign countries, provided by the NAR, denoted by $capflow_{ct}$. Figure 10 shows the contribution of these top 5 international client groups to the overall international sales volume from 2010–2019. The darkest bar, at the bottom of the graph, is the Chinese contribution to the total. Next is Canada, followed by India, Mexico and the U.K. in that order. Finally, the bar is capped by the "all other foreign" contributions. The figure shows the rapid expansion of Chinese investment in U.S. residential real estate relative to other foreign buyers over this period, but also that Chinese investment alone makes up a relatively small fraction of total foreign investment.

We construct a measure of local expected capital flows (ECF_{it}) that "distributes" national home purchase capital flows ($capflow_{ct}$, in billions) to zipcodes based on pre-existing immigrant composition:

$$ECF_{it} = 1000 \times \sum_{c \in C} capflow_{ct} \times \frac{FBpop_{ic}^{2011}}{FBpop_{c}^{2011}} \tag{6}$$

where

$$1 = \sum_{i} \frac{FBpop_{ic}^{2011}}{FBpop_{c}^{2011}} \tag{7}$$

and C = {Canada, China, India, Mexico, U.K., Other}, i denotes zipcode, and t denotes quarter. Intuitively, ECF_{it} distributes capital coming from country c at time t, $capflow_{ct}$, to zipcode i based on how many people from country c ex-ante live in that zipcode relative to their national presence; in other words, ECF_{it} is the expected capital flowing to a zipcode, should the national flows be distributed uniformly by population.[19] We can also scale the per-capita term by the zipcode share of the relevant foreign-born population, $fracFB_{ic}$, to define an exposure measure. The exposure measure methods and results are discussed in Appendix C.2. We choose to focus on the per-capita ECF_{it} measure due to its ease of interpretation.[20]

Appendix Figure E3 shows the ECF_{it} distributions for 2009q1 and 2015q1, based on the pre–existing composition of foreign–born residents, with panel (a) showing the raw distribution, and panel (b) showing the logged distribution, which drops all zipcodes with no foreign-born residents.[21] In 2009q1, the median zip code in a CBSA with at least one treated zipcode received \$176,000 in ECF_{it}, which translates to about one home purchased by a foreigner assuming a price at the national average at the time of about \$173,000.[22] This

[19]This intuition is similar to a Bartik instrument, in which the local industry shares are the population shares, and the national industry growth rate is national foreign capital flows. Identification uses differential exposure to a common shock, in our case the the foreign–buyer tax policy change. Identification relies on the initial population shares being exogenous to house price growth or quantity growth. Goldsmith-Pinkham, Sorkin and Swift (2019) suggest testing this by examining how much the initial shares are correlated with confounders in the pre–period. We test at length in the difference-in-differences empirics, for example by constructing a matched sample and synthetic controls.

[20]While the CoreLogic transactions data contains the buyers' names listed on deeds, these are frequently omitted or provide the name of a legal entity (e.g. LLC), which precludes us from directly estimating the fraction of foreign buyers based, say, on the last name of the buyer.

[21]Appendix Table D13 provides a numerical example of ECF_{it} construction.

[22]According to Zillow's National All Homes Index, ZHVI.

increased to \$322,000 in 2015q1 and to \$418,000 by 2018q1. The 99^{th} percentile zip code in 2009q1 received \$4.2m, \$17.1m in 2015q1, and \$20m in 2018q1. In short, our measure of expected capital flows ECF_{it} shows there is wide variance in the amount of money flowing to individual locations, and that over time, every zipcode has seen an increase in local capital flows.

Our multinational IV design proceeds as follows:

$$ln(ECF_{it}) = \alpha + \beta fracFB_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it} \tag{8}$$

$$ln(HPI_{it}) = \delta + \gamma^P ln(\widehat{EC}F_{it}) + \zeta_i + \theta_t + \epsilon_{it} \tag{2P}$$

$$\frac{dQ_{it}}{Q_i} = \delta + \gamma^Q ln(\widehat{EC}F_{it}) + \zeta_i + \theta_t + \epsilon_{it} \tag{2Q}$$

In the first stage, β measures the percent change in capital (in millions of dollars) in treated zipcodes in the post period. In the second stage, γ measures the elasticity of house prices or quantities with respect to an increase in expected local foreign capital.[23] $\frac{dQ}{Q}$ measures the number of building permits in a county i at time t, relative to the same county's stock in 2011.[24]

6.2 Expected Capital Flows IV Results

Tables 7 and 8 present the results from the expected capital flows estimation strategy. We present the price results using both the panel of zipcodes and the panel of counties, while the quantity results use only the panel of counties due to data availability. All coefficients are estimated using commuting-zone time trends, as in our preferred specification from the differences-in-differences analysis. We show in Table 7, column (1), that ECF_{it}, the expected foreign capital flowing to a zipcode, is strongly associated with the interaction of the foreign-

[23] γ is likely attenuated towards 0 since ECF is an imperfect measure of local FDI in the housing market.

[24] The elasticity of prices or quantities with respect to ECF_{it} are as follows: $\frac{dHPI_{it}}{HPI_{it}}\frac{ECF_{it}}{dECF_{it}}$ and $\frac{dQ_{it}}{Q_{it}}\frac{ECF_{it}}{dECF_{it}}$. $ln(X)$ approximates $\frac{dX}{X}$ well, so we use the log–log specification for HPI as it is well–estimated in the quarterly data, at a local level. By contrast, the data quality for annual housing stock from the ACS is very noisy. As such, we use quarterly permits data, dQ_{it}, normalized to pre–period stock, Q_i.

born share of the population and an indicator for post-2012 time periods. This instrument yields an F-statistic of 248, even after the inclusion of zipcode and quarter fixed effects. The median zipcode has a fraction foreign-born of 2.2%, and the 99^{th} percentile is 43% foreign-born. The estimated semi-elasticity of 1.01 implies that moving between these two zipcodes would increase expected capital flows by 41%. Column (2) shows the first-stage results with the panel of counties for the price outcomes, with the point estimate decreasing from 1.01 to 0.98. Finally, the third column shows the first stage for the panel of counties for which we have building permit data, and yields a first stage semi-elasticity of 1.1 with an F-statistic of 80, again demonstrating a strong first stage, as supported by section 5.1.

Table 8 reports our estimates of the elasticity of zipcode house prices and quantities on the zipcode's ECF_{it}, instrumented with the interaction of fraction foreign-born and the post-2012 indicator. Column (1) shows that a 1% increase in ECF_{it} raises house prices by 0.27% when using a panel of zipcodes. This price increase represents the response to capital without a concurrent change in quantity supplied, showing prices are quite sensitive to foreign capital. Between 2011q4 and 2018q4, expected capital flows increased by 32% for the median zipcode. This increase in foreign investment would thus translate into an 8.6% increase in house prices. The median zipcode experienced 42% house price growth over our sample period, so increased foreign capital can explain roughly one-quarter of observed house price growth.

Column (3) in Table 8 reports the comparable quantity elasticity; a 1% increase in expected capital flows to a county (holding prices fixed) increase quantities by 0.004%, showing that quantities are significantly less responsive than prices in the short run. The median county in our sample increased expected capital flows by 80%. With the median county permitting 0.3% of its housing stock each quarter in prior to 2011q4, the influx of foreign capital adds $(80 \times 0.004) = 0.32\%$ to the baseline permitting rate, doubling it as capital flows in. Taken together, these results imply that the U.S. housing market is highly inelastic over the span of one decade.

In order to mitigate concerns that house prices might rise faster in growing areas, and those same areas would attract foreign investment, we control directly for income and population in the IV regressions in Appendix Tables D14 and D15. Controlling for population and income does not seem to impact the baseline results in Tables 7 and 8, whose results are recorded in the first columns of the appendix tables. Limiting the sample to those zipcode-quarters available in the ACS and Census data that we use for population and income does not change the point estimate, and the elasticity only moderately increases when controlling for income and population.[25]

As we show in the Appendix, this local instrumental variables approach is robust to excluding China from both numerator and denominator of ECF_{it}. Appendix Table D17 shows that the second stage results remain similar, at 0.27 to 0.55 for prices and 0.005 for quantities, showing that while Chinese capital flows might be the most newsworthy, the rest of the world's capital also flows into the U.S. market, increasing prices. The results are also robust to alternative approaches of constructing ECF_{it}. For instance, in Appendix C.2, we weight the ECF_{it} by the fraction foreign-born in the zipcode, analogous to the exposure treatment measure in ongoing work by Abramitsky et al. (2019) on the impact of immigration quotas on local economies.[26] This alternative weighting scheme considers the overall number of people in a zip code, as a zip code with 100 foreign-born residents out of 200 may attract capital differently than one with 100 out of 1,000. By scaling the ECF_{it}, we find a price elasticity of 0.37 (see Appendix Table D19), and a quantity elasticity of 0.009, higher yet in line with our main results.

In sum, in this section we constructed a generalized instrument for international capital flows based on ex–ante foreign population shares, and used the timing foreign-buyer taxes in non-U.S. countries to show that U.S. house prices and quantities respond to international capital flows. In the short run, house prices are much more responsive than the supply of new housing units.

[25] Population and median household income data from the 2011–2018 ACS at the zipcode level. 2010 population at the zipcode level from the Decennial Census. 2009 population, and 2009–2010 median household income from the county level ACS as zipcode level data is not available prior to 2011.

[26] Abramitsky et al. (2019) interact the foreign-born population share from a given origin country with their measure of immigration quota bindingness, or intensity of treatment. In their setting, the intensity of the quota stringency will matter more in areas with high shares of foreign born populations. In our setting, the intensity of treatment is the expected capital flows to a zipcode.

7 Local House Price Elasticities of Supply

To estimate house price elasticities of supply across local housing markets, we use variation in the demand for housing induced by international tax policy changes, which is plausibly exogenous to local housing supply decision-makers, interacted with the tax policy change. Intuitively, local housing markets will be differentially shocked by foreign demand, depending on their pre–period foreign–born composition. This provides a unique demand shift for each market, holding the initial supply of housing fixed.

The ratio of the elasticities of price, $\frac{\partial ln(P)}{\partial ln(f)}$, and supply, $\frac{\partial ln(Q)}{\partial ln(f)}$, with respect to capital flows from the previous section's second stage results can be used to construct the house price elasticity of supply, η:

$$\frac{\frac{\partial ln(Q)}{\partial ln(f)}}{\frac{\partial ln(P)}{\partial ln(f)}} = \frac{\partial ln(Q)}{\partial ln(P)} = \eta \tag{9}$$

While a national house price elasticity is informative, we care more about how localities differ in their supply responses to price changes in the short run. In contrast to previous work describing local house price elasticities through the lens of housing supply restrictions either due to regulation or topography (Gyourko and Summers (2008); Saiz (2010)), we exploit exogenous variation in demand for housing to estimate the slope of the supply curve. While Baum-Snow and Han (2019) trace out the housing supply curve using Bartik local labor market shocks, capturing an intensive-margin response as residents get wealthier, our approach captures an extensive-margin response as foreign investment increases in the local market. Additionally, we can construct an elasticity for any geography that has exposure to the tax policy shock, i.e. any location with a nonzero share of foreign-born residents, $fracFB_i$.

To obtain local house price elasticities for each CBSA m, η^M, we modify the instrumental variables strategy discussed in Section 6. First, we use the county as the unit of observation, as this is the granularity available for building permits, our measure of dQ_{ct}, normalized to the 2011 stock, Q_c. As above, we instrument for capital flows, ECF_{ct}, with fraction foreign–born interacted with the "post" indicator, $fracFB_c \times Post_t$, and regress prices and quantities on instrumented capital flows:

$$ln(HPI_{ct}) = \gamma^P ln(\widehat{ECF}_t^c) + \gamma_M^P ln(\widehat{ECF}_t^c) \times CBSA_c + \eta^{ct} \quad \text{(2PM)}$$

$$\frac{dQ_{ct}}{Q_c} = \gamma^Q ln(\widehat{ECF}_t^c) + \gamma_M^Q ln(\widehat{ECF}_t^c) \times CBSA_c + \nu^{ct} \quad \text{(2QM)}$$

This design allows us to estimate both a short–run national and local impact of capital flows on house prices and quantities: γ^k for the average national elasticity, γ_M^k for the CBSA–specific additional elasticity. To recover the distribution of price elasticities of supply, for each CBSA we then calculate

$$\eta^M = \frac{\gamma^Q + \gamma_M^Q}{\gamma^P + \gamma^P} \quad (10)$$

with η^M providing the CBSA–specific house price elasticity of supply. We construct η^M for the largest 100 CBSA's by population in 2010, mapped in Figure 11, a sample of which is displayed in Table 9.[27] The most inelastic cities in our sample have price elasticities of supply of about 0.02, while the most elastic have an elasticity closer to 0.7.[28]

The map in Figure 11 shows the geographic distribution of the elasticities, dividing the 91 positive values into 4 quartiles. The most inelastic markets tend to be on the coasts, though Minneapolis–St. Paul, MN turns out to be one of our empirically most inelastic markets.[29] The middle of the country remains relatively more elastic, though large areas of the Mid–Atlantic also seem elastically supplied over this period.

[27] The full table of elasticities by CBSA is provided in Appendix table D20.

[28] Based on our methodology, nine CBSAs have negative elasticity estimates: Greenville, NC, Columbus, GA, Detroit, MI, Deltona, FL, Wilmington, NC, Allentown, PA, Tallahassee, FL, Vineland, NJ, and Atlantic City, NJ. These CBSA's represent cities in decline and cities that overbuilt in the last housing cycle, for which either the estimated price elasticity with respect to foreign capital, or the estimated quantity elasticity is negative.

[29] Using an entirely different methodology, Aastveit, Albuquerque and Anundsen (2019) also unexpectedly find that Minneapolis is highly inelastic.

Figure 12 provides the distribution of local house price elasticities, with the bulk of short-run elasticities falling below 0.1. This figure shows that over a ten-year period, the U.S. housing market appears to be highly inelastically supplied. That we observe such inelastic markets is perhaps unsurprising given the historic sustained growth in house prices over the duration of our sample, with the Case–Shiller national house price index rising over 40 percent from 2010q1 to 2018q4. This rise in prices has not been driven by an expansion of credit or a large construction response that characterized the housing bubble of 2003–2007, the last time we saw such sharp price increases.

Taking the example of the Boston CBSA, which has an estimated elasticity of 0.03, we can ground these elasticities in observed changes in prices and quantities. In 2010, the average home transacted for $350,000, and there were 1.96 million units in the CBSA. This implies that a 1% change in price of $3,500 would result in $0.03 \times 0.01 \times 1.96$ million ≈ 590 more housing units in response to this demand shock. At the CBSA level, house prices rose by 35%, implying an additional 20.6k units would be built between 2010 and 2018, holding the supply schedule fixed. Over that same period, equilibrium supply increased by 64.5k units, suggesting a supply response.

To better understand the mechanics of our instrument, we plot observed and predicted price and quantity changes for our sample of the 100 largest cities. Panels (c) and (d) in Figure 13 plot the predicted and raw price and quantity changes from the data, respectively. We observe that the slope for the predicted values (c) is much steeper than the slope for the raw change (d). The intuition for this disparity is shown in panels (a) and (b). If we use only a demand shock to the local housing market, a large change in P is associated with a large change in Q; however, if we do not hold the supply of housing fixed, and fail to isolate a pure demand shock, a large change in Q is associated with a small change in P. Panels (c) and (d) then show that our IV design solves this simultaneity problem; large changes in Q are now associated with large changes in P for the predicted panel, while large changes in Q are associated with small changes in P for the raw equilibria. The average elasticity for the predicted panel (weighted by city size) is lower than that for the raw equilibria, $\overline{\eta}^M_{predicted} = 0.16 < 0.59 = \overline{\eta}^M_{raw}$, highlighting the need for an instrument to isolate the demand shock, and the apparent success of our natural experiment in doing so.

8 Conclusion

International capital flows have the potential to rapidly inflate the value of assets. While some asset prices may not necessarily have meaningful implications for the real economy, inflating the value of physical assets such as real estate can distort economic activity towards home construction at the expense of other industries. In this paper, we first document the effect of large capital outflows from China on the U.S. housing market, emphasizing that a series of foreign-buyer taxes in other target housing markets may have made American cities more attractive investments. Using a difference-in-differences design, we estimate that house prices rose 8 to 15 percent more in zipcodes with a larger share of foreign-born Chinese residents prior to the capital shock, and subsequently reversed following the onset of the U.S.–China trade war.

Estimating the housing market's sensitivity to global (and not just Chinese) capital more generally, we find that a 1% increase in instrumented foreign capital raises house prices at the zip code level by 0.27%, and housing supply at the county level by 0.004%. We then exploit this demand shock to provide new estimates of the price elasticity of housing supply with respect to global capital inflows into the U.S. housing market. We find that U.S. housing markets seem relatively inelastic in the short run, with the average city having a house price elasticity of supply near 0.11 over a ten year period.

Our findings have three primary implications. First, we establish that foreign-buyer taxes, generally imposed in response to Chinese investment, have spillovers in other non-targeted markets; for instance, imposing foreign buyer taxes in Vancouver has affected Seattle's housing market. Next, we show that neighborhoods with a large share of foreign residents are more susceptible to house price swings in response to foreign capital flows. From an affordability standpoint, these neighborhoods are less accessible to existing U.S. residents as prices rise due to foreign investment. However, we also show that the real economy responds to these signals, with new construction adding additional housing stock in the same neighborhoods. Finally, we document that the U.S. housing market is highly inelastic in the short run, but heterogeneous across cities. Our results are consistent with the recent rise in house prices nationally creating an affordability crisis, as cities are not rapidly adding stock in response.

Whether this expansion of the housing stock in high-exposure neighborhoods is sustainable or not depends on how these homes are used and whether capital continues to flow to the same destination zipcodes. The current elasticities are estimated under the assumption that new units are occupied. On the other hand, if these homes are used only as largely-unoccupied pied-à-terres, this usage will increase the housing costs of other residents competing to live in the same neighborhood; in effect, this biases our estimates downwards. This concern begs further exploration, as many cities such as Vancouver have levied vacancy taxes on empty units due to concerns that new supply is not being used.

On the price side, we show that when foreign capital dries up, prices differentially fall. Continued declines in capital flows will lead to further differential declines in specific exposed submarkets. The current Covid-19 crisis has largely frozen many U.S. housing markets, so an open question is whether foreign capital will return. Furthermore, if the current foreign investment in the U.S. market relocates abroad, local markets may be oversupplied with investment properties, leading to more volatile price swings. Given the durability of housing, the costs of overbuilding could be large and persistent (e.g. Glaeser and Gyourko (2005)). While our analysis establishes the consequences of capital inflows on U.S. house prices, the impact of capital outflows, if foreign nationals choose to repatriate capital or move their funds elsewhere, remains an area of further research.

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Tables

Table 1: Covariate Balance

Variable	FBC=0	FBC=1	Difference
HPI	1.976	1.888	-0.074
	(1.499)	(0.947)	(0.108)
HPI growth, 1 Year	0.041	0.039	0.002
	(0.473)	(0.379)	(0.015)
HPI growth, 5 Years	0.307	0.359	0.057
	(0.861)	(0.824)	(0.058)
Lagged HPI	1.957	1.894	-0.052
	(1.412)	(0.924)	(0.107)
Sales	47.533	54.946	3.806
	(63.620)	(49.402)	(3.914)
Lagged Sales	50.086	55.463	1.903
	(64.917)	(49.641)	(4.042)
Permits: Single Family Units	1,403.915	2,012.000	478.352
	(3,445.920)	(3,527.558)	(672.553)
Permits: All Units	2,220.648	5,550.035	3,123.131**
	(5,150.242)	(6,479.830)	(1,364.078)
Establishments	390.730	1,055.530	638.459***
	(464.619)	(750.433)	(51.042)
Estab. growth, 1 year	-0.001	0.008	0.008***
	(0.075)	(0.037)	(0.002)
Estab. growth, 5 years	-0.010	0.027	0.034***
	(0.178)	(0.104)	(0.009)
Employment	6,059.159	17,673.475	11,180.679***
	(9,004.811)	(17,567.438)	(1,428.239)
Emp. growth, 1 year	0.003	0.009	0.002
	(0.162)	(0.201)	(0.004)
Emp. growth, 5 years	-0.005	-0.010	-0.007
	(0.378)	(0.196)	(0.016)
Annual Payroll (1000s)	241.617	1,098.492	838.187***
	(546.318)	(1,560.161)	(150.819)
2010 Population	16,280.107	34,627.074	17,224.406***
	(15,861.714)	(21,414.273)	(1,680.279)
2011 Population	16,177.837	34,163.547	16,872.781***
	(15,766.526)	(21,106.248)	(1,612.878)
2011 Median Income	55,501.430	80,097.359	23,285.756***
	(21,700.305)	(34,987.082)	(4,515.230)

Notes: This table shows pre-period balance for housing and labor market characteristics. $FBC_i = 1\left\{\frac{FBCpop_i}{pop_i} \geq 99^{th} percentile\right\}$ for zipcode i. Data is zipcode level, excepting permits data, which is at the county level. Data at the quarterly level, excepting annual employment, establishment, and payroll data. Data through 2011q3 for the housing and permit data, and through 2012 for the annual data. Standard errors in parentheses, clustered by MSA. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Differences-in-Differences Results

	(1) HPI	(2) HPI	(3) HPI	(4) HPI
Post=1 X FBC=1	0.118*	0.167***	0.0781*	0.0950**
	(0.0693)	(0.0316)	(0.0435)	(0.0385)
R^2	0.869	0.899	0.889	0.891
Observations	86768	428903	163355	176224
		Fixed Effects		
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
MSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficient β from $hpi_{it} = \alpha + \beta FBC_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. FBC=1 defined as $FBC_i = 1\left\{\frac{FBCpop_i}{pop_i} \geq 99^{th} percentile\right\}$ for zipcode i. All data at the zipcode by quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Binned Dose Response Results, FBC

	(1) HPI	(2) HPI	(3) HPI	(4) HPI
Post = 1 X 50th-90th ptile	0.0377	0.0411**	0.0393	0.0223
	(0.0372)	(0.0143)	(0.0310)	(0.0317)
Post = 1 X 90th-95th ptile	-0.00285	0.0762	0.0379	0.0246
	(0.0750)	(0.0498)	(0.0557)	(0.0536)
Post = 1 X 95th-99th ptile	0.0555	0.139**	0.0771	0.0644
	(0.0641)	(0.0590)	(0.0665)	(0.0572)
Post = 1 X Above 99th ptile	0.200**	0.234***	0.131*	0.130**
	(0.0972)	(0.0462)	(0.0686)	(0.0515)
R^2	0.869	0.899	0.889	0.891
Observations	86768	428877	163355	176224
		Fixed Effects		
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
MSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficients β_k from $hpi_{it} = \alpha + \sum_k \beta_k FBCbin_{ik} \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. All data at the zipcode by quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Differences-in-Differences Results, by house price quintile

	(1) HPI	(2) HPI	(3) HPI	(4) HPI
Post = 1 X FBC X quintile 1-4	-0.262***	-0.00115	0.0240	0.0102
	(0.0932)	(0.0711)	(0.0775)	(0.0740)
Post = 1 X FBC X quintile 5	0.140*	0.178***	0.0819*	0.101**
	(0.0710)	(0.0369)	(0.0456)	(0.0405)
R^2	0.867	0.899	0.889	0.890
Observations	86779	428879	163331	176178
		Fixed Effects		
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
MSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficient β from $hpi_{it} = \alpha + \beta FBCquint_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. All data at the zipcode by quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Differences-in-Differences, Supply Response

	(1) New Units	(2) New Units	(3) New Units	(4) New Units
Post=1 X FBC=1	1.535***	2.210***	2.111***	1.850***
	(0.530)	(0.492)	(0.573)	(0.448)
R^2	0.715	0.773	0.813	0.830
Observations	21164	18425	7731	7262
		Fixed Effects		
Quarter	X	X	X	X
County	X	X	X	X
State X Quarter		X		
MSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficient β from $NewUnits_{it} = \alpha + \beta FBC_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. $NewUnits_{it} = 100 \times \sum_{\tau=2011}^{t} \frac{Permits_{i,\tau}}{Stock_{i,2011}}$ All data at the county by quarter level. $FBC_i = 1\left\{\frac{FBCpop_i}{pop_i} \geq 95^{th} percentile\right\}$ for county i. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Matching Results

	(1)	(2)	(3)	(4)
Post=1 X FBC=1	0.168	0.122	0.119	0.0817
	(0.127)	(0.0902)	(0.104)	(0.108)
R^2	0.745	0.772	0.773	0.774
Observations	11550	11550	11550	11550
		Fixed Effects		
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
MSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficient β from $hpi_{it} = \alpha + \beta FBC_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$, using the matched sample. All data at the zipcode by quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Expected Capital Flow IV: First Stage

	(1)	(2)	(3)
	ln(ECF$_{it}$)	ln(ECF$_{it}$)	ln(ECF$_{it}$)
Post X Frac. FB	1.007***	0.983***	1.097***
	(0.0639)	(0.0541)	(0.123)
R^2	0.989	0.999	0.997
F	248.0	330.4	80.03
Observations	787303	92438	26398
		Fixed Effects	
Zip	X		
Quarter	X	X	X
County		X	X
Zone X Quarter	X	X	X

Notes: This table shows the first stage results from $ln(ECF_{it}) = \alpha + \beta frac\_FB_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. All data mean collapsed at the quarter level. Standard errors in parentheses, clustered by geography. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Expected Capital Flow IV: Second Stage

	(1) ln(HPI)	(2) ln(HPI)	(3) $\frac{dQ_{it}}{Q_{it}}$
ln(ECF$_{it}$)	0.274***	0.542***	0.00423***
	(0.0895)	(0.0856)	(0.000920)
Root MSE	1.454	0.128	0.00174
Observations	787303	92438	26398
		Fixed Effects	
Zip	X		
Quarter	X	X	X
County		X	X
Zone X Quarter	X	X	X

*Notes:*The table shows the second stage results from $Y_{it} = \delta + \gamma ln(\widehat{ECF}_{it}) + \zeta_i + \theta_t + \epsilon_{it}$, where Y_{it} is either $ln(hpi_{it})$ or $\frac{dQ_{it}}{Q_{it}}$, and i is either zipcode or county. All data mean collapsed at the quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

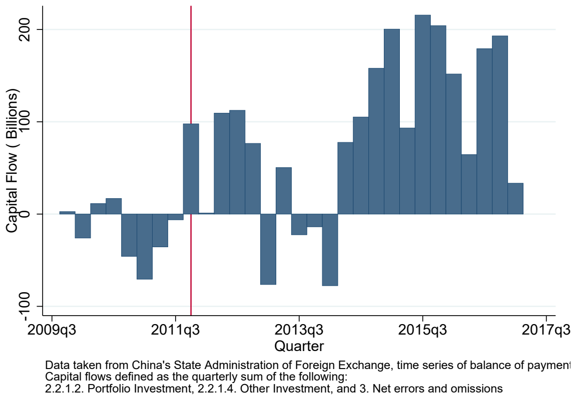
Table 9: Most Inelastic and Elastic CBSA's

Top 5 Most Inelastic	
Providence, RI	0.02
San Francisco, CA	0.03
Boston, MA	0.03
Minneapolis – St. Paul, MN	0.03
Sacramento, CA	0.03
Top 5 Most Elastic	
Salisbury, MD	0.69
Ocala, FL	0.60
Lakeland, FL	0.46
Virginia Beach - Norfolk, VA	0.35
Trenton, NJ	0.27

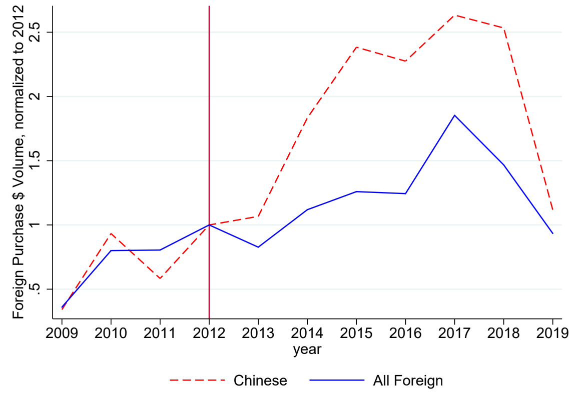
Note: This table shows 10 of the 100 price elasticities of supply, for the most inelastic and elastic CBSA's in the country.

Figures

Figure 1: International Capital Flight and U.S. House Prices



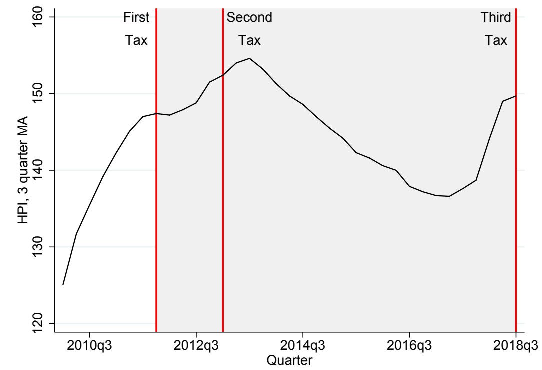
(a) Net Capital Flows from China



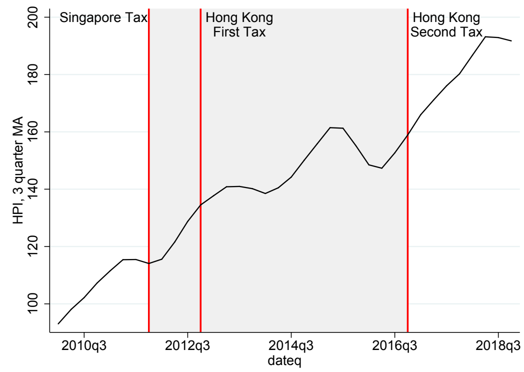
(b) Sales Volume, NAR

Source: Capital outflows from SAFE, time series of balance of payments, and are defined as the quarterly sum of: 2.2.1.2. Portfolio Investment, 2.2.1.4. Other Investment, and 3. Net errors and omissions. Transaction volume from annual editions of the National Association of Realtors' (NAR) "Profile of International Activity in U.S. Residential Real Estate."

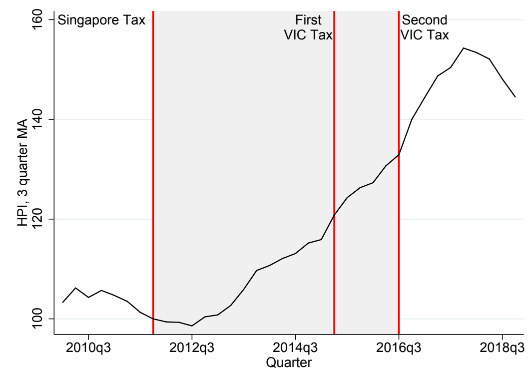
Figure 2: International House Prices



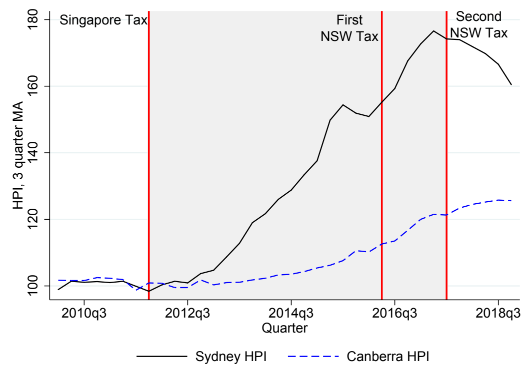
(a) Singapore



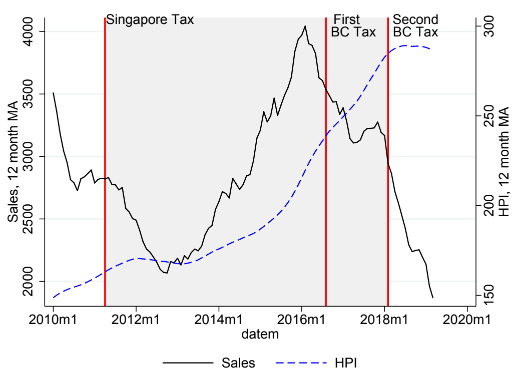
(b) Hong Kong



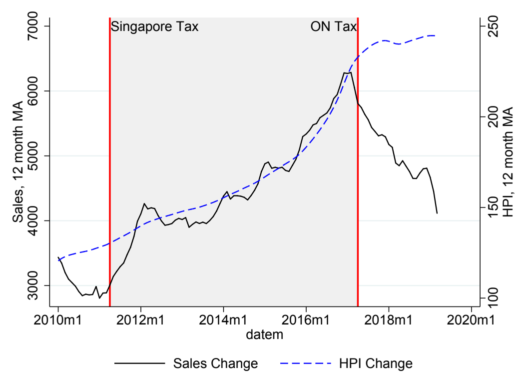
(c) Victoria, Australia



(d) New South Wales, Australia



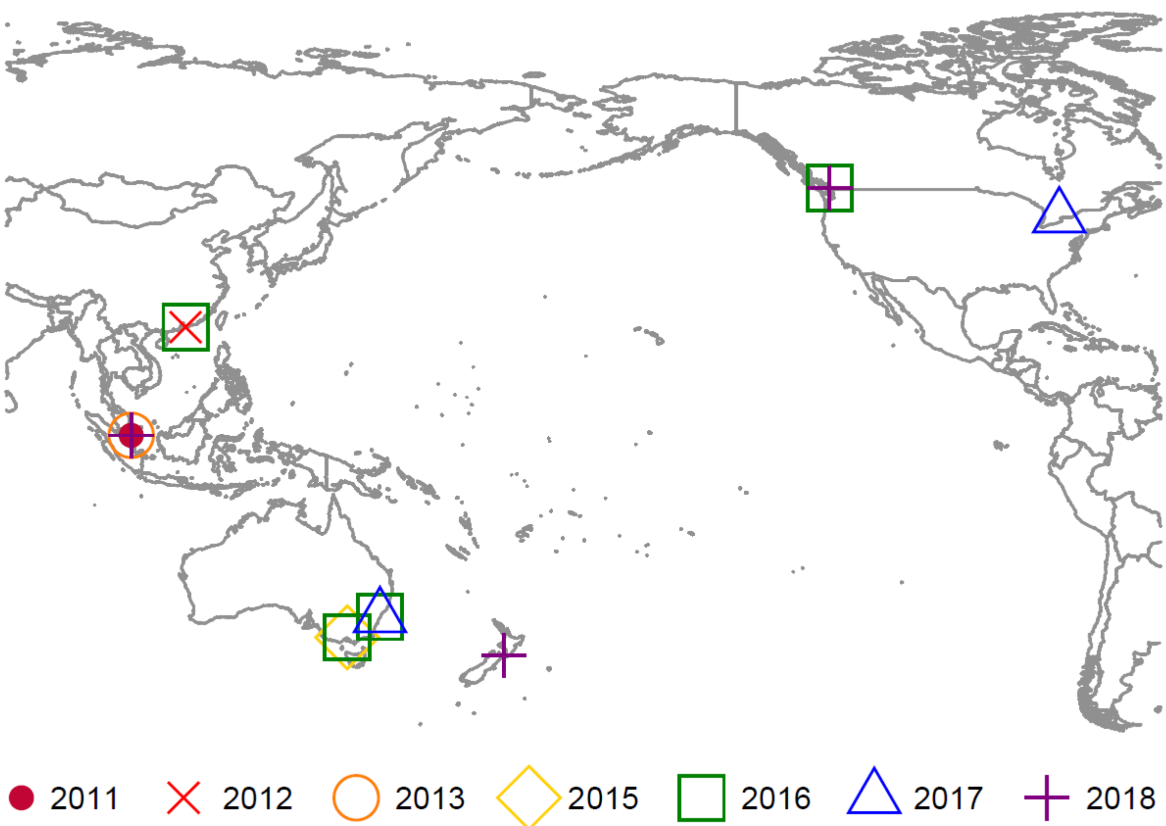
(e) British Columbia, Canada



(f) Ontario, Canada

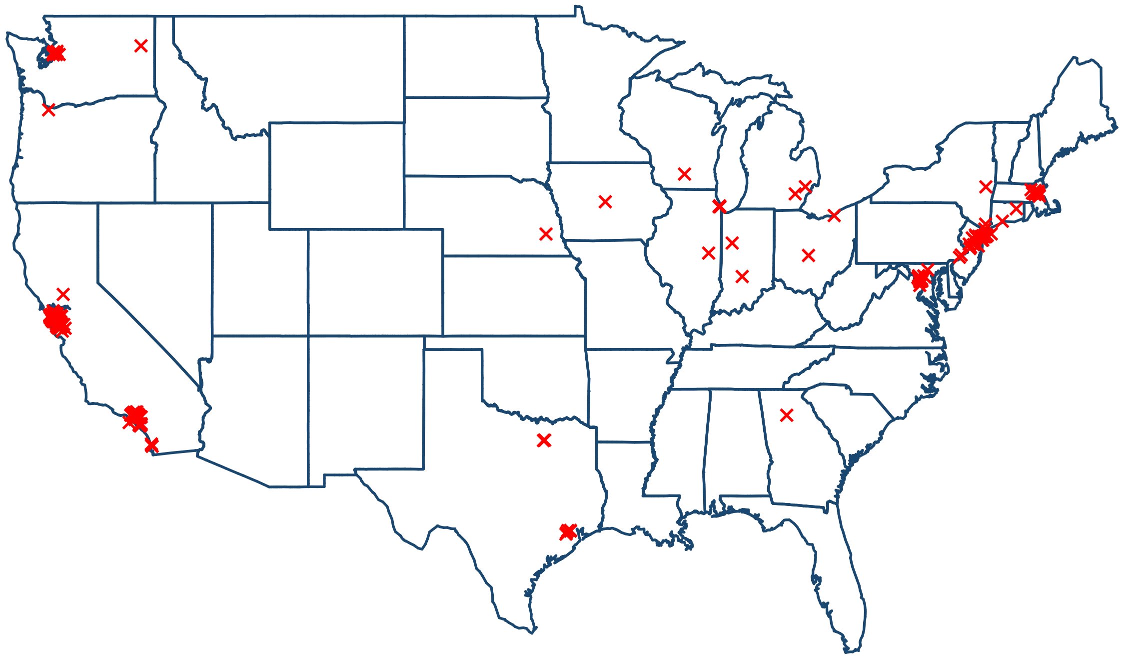
Source: For Singapore, data from data.gov.sg for private residential property price index. For Hong Kong, data from the Bank for International Settlements via St. Louis Fred, source code Q:HK:R:628, real residential property prices. For Australia, data from Australian Bureau of Statistics, residential property price indexes by city. For Canada, data from Teranet and National Bank of Canada, residential property price indexes by city.

Figure 3: Map of Tax Policy Changes

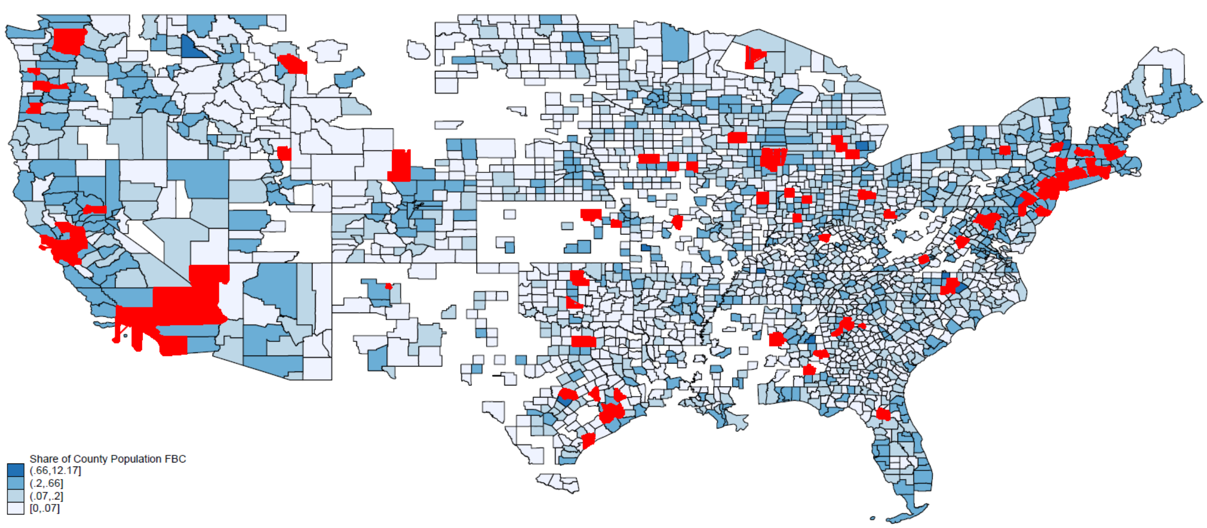


Notes: Singapore: 10% in **2011m12**, 15% in 2013m1, 20% in 2018m7; Australia: 3 % in 2015m6 (VIC), 4% in 2016m6 (NSW), 7% in 2016m7 (VIC), 8% in 2017m7; Canada: 15% in 2016m8 (BC), 15% in 2017m4 (ON), 20% in 2018m2; New Zealand: banned all non-resident foreigners from purchasing existing SFHs, may still purchase up to 60% of new construction multiunit condos, 2018m8. Other policies include taxes on vacant units, often at lower rates. The United Kingdom and Malaysia are currently considering imposing similar policies.

Figure 4: Geographic Spread of Treated Zips and Counties



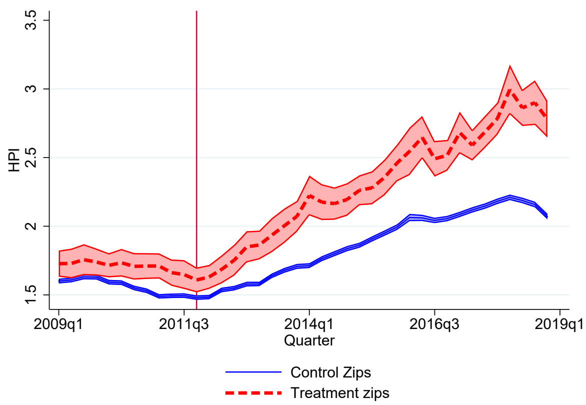
(a) Zip Codes



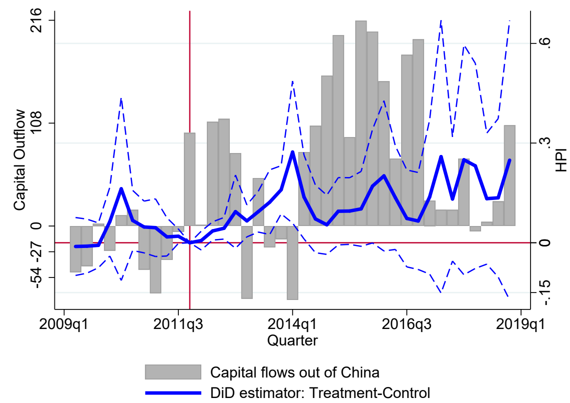
(b) Counties

Note: Panel (a) plots the FBC=1 zipcodes. Panel (b) plots the fraction foreign-born Chinese by county, breakpoints correspond to the 50^{th}, 75^{th} and 95^{th} percentiles. Treated counties shaded in red.

Figure 5: Differences-in-Differences Event Study, 2009-2018



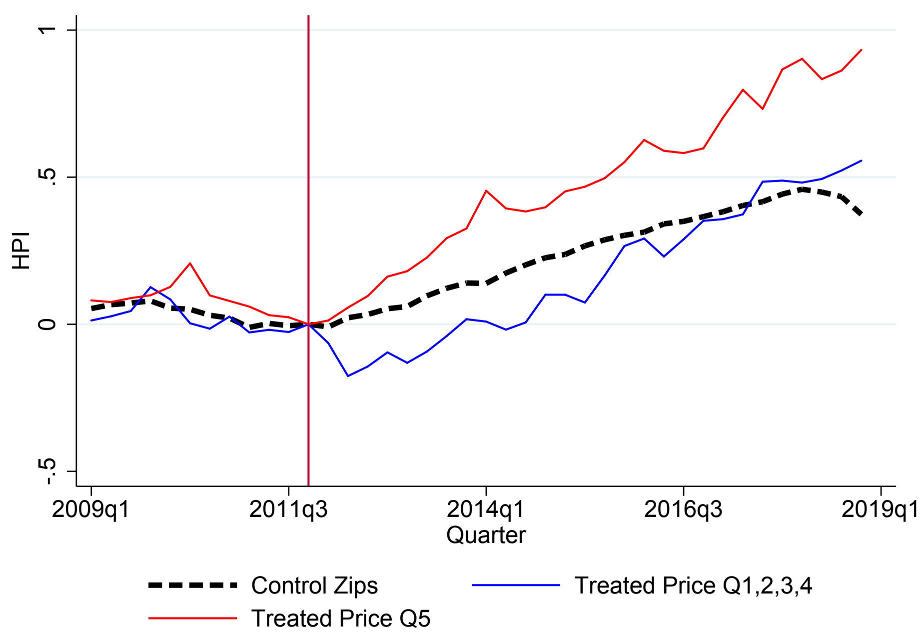
(a) Raw Data



(b) DiD Estimator

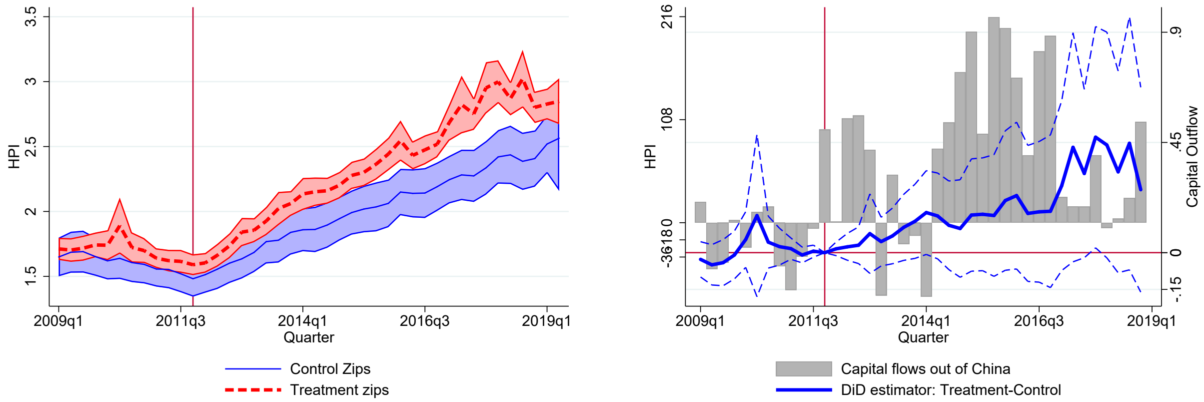
Note: Panel (b) uses regression estimates from the baseline DiD, adding city-level trends, as in column (4) of the DiD results: $hpi_{it} = \beta FBC_i \times qtr_t + \zeta_i + \theta_t + MSA_i \times t + \varepsilon_{it}$.

Figure 6: Differences-in-Differences Event Study, by House Price Quintile



Note: The figure uses regression estimates from the price qauntile DiD, adding city-level trends, as in column (4) of the DiD results: $hpi_{it} = \beta FBC_i \times qtr_t + \zeta_i + \theta_t + MSA_i \times t + \varepsilon_{it}$.

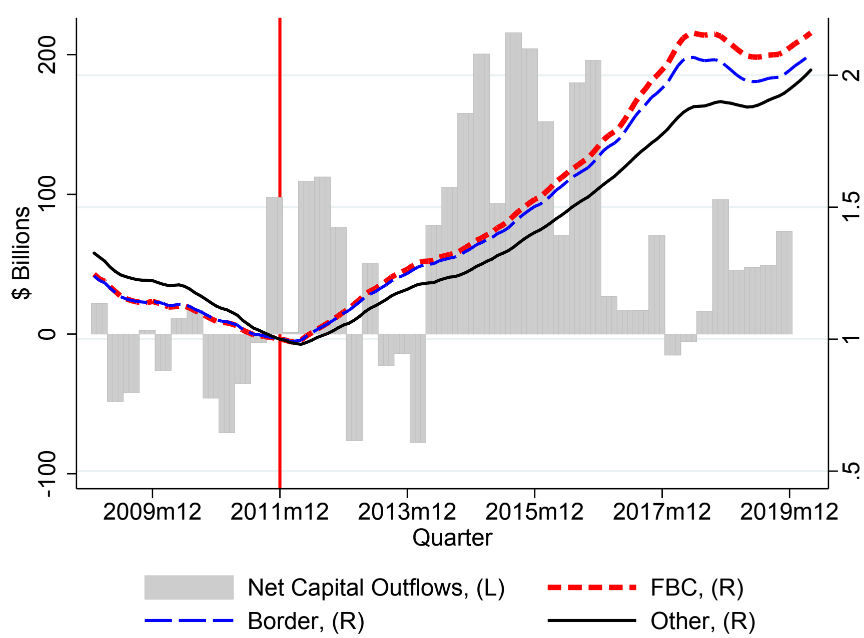
Figure 7: Matching Results: Event Study



(a) Raw Data (b) DiD Estimator

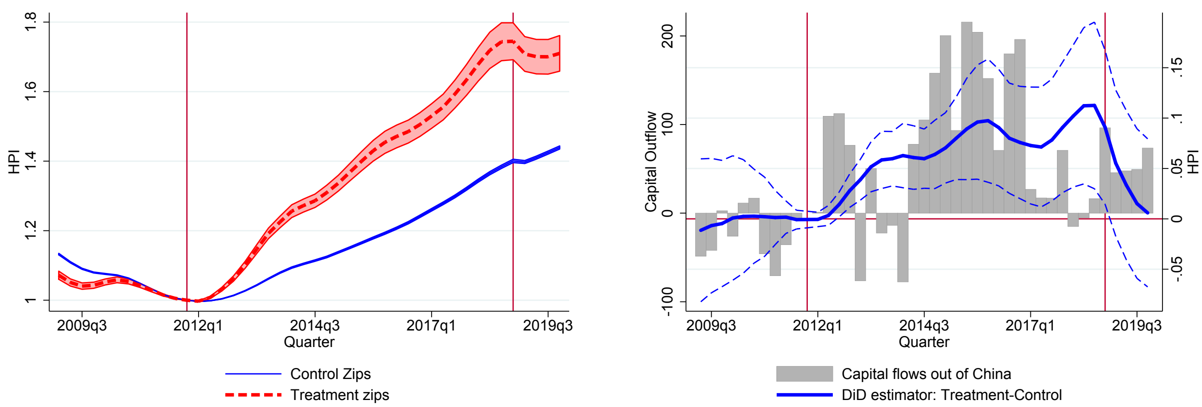
Note: Panel (b) uses regression estimates from the baseline DiD, adding city-level trends, as in column (4) of the matching results: $hpi_{it} = \beta FBC_i \times qtr_t + \zeta_i + \theta_t + MSA_i \times t + \varepsilon_{it}$.

Figure 8: Event Study: Seattle's Housing Market 2009-2019



Note: Seattle's monthly Zillows Home Value Index, by zipcode foreign-born status.

Figure 9: Differences-in-Differences Event Study, 2009-2019, Treatment Reversal

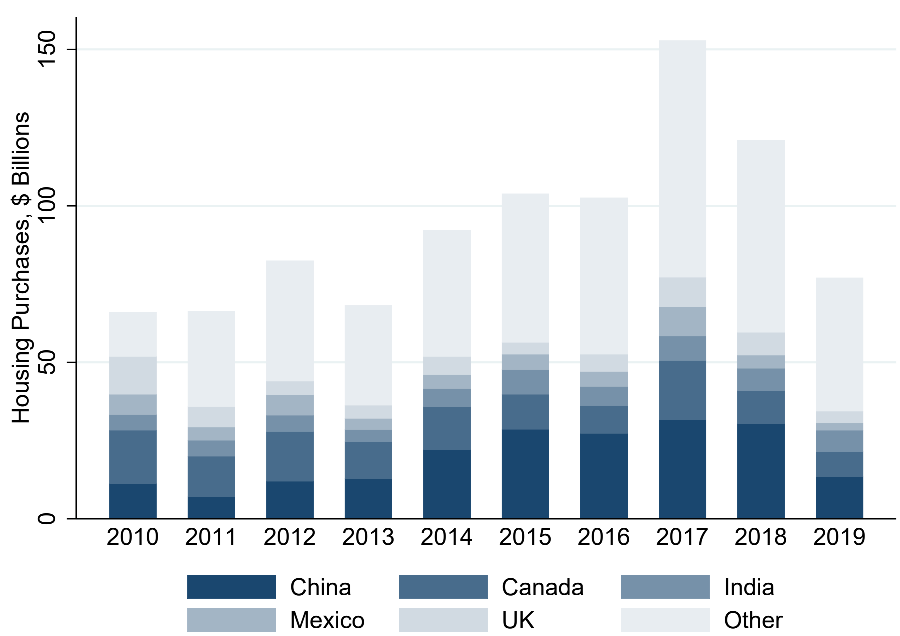


(a) Raw Data

(b) DiD Estimator

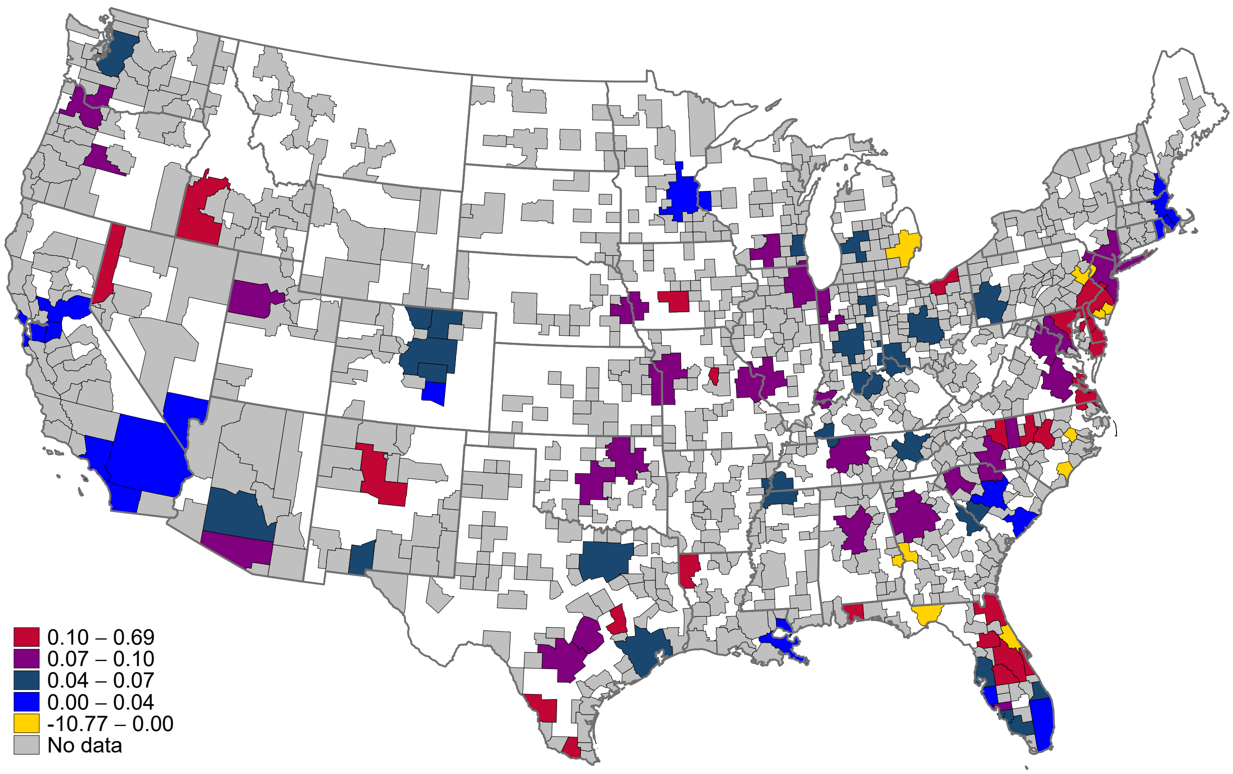
Note: Panel (b) uses regression estimates from the baseline DiD, adding city-level trends, as in column (4) of the DiD results: $hpi_{it} = \beta FBC_i \times qtr_t + \zeta_i + \theta_t + Czone_i \times t + \varepsilon_{it}$. hpi_{it} constructed using Zillow's Home Value Index, averaged quarterly.

Figure 10: Expected Capital Flow Index Inputs



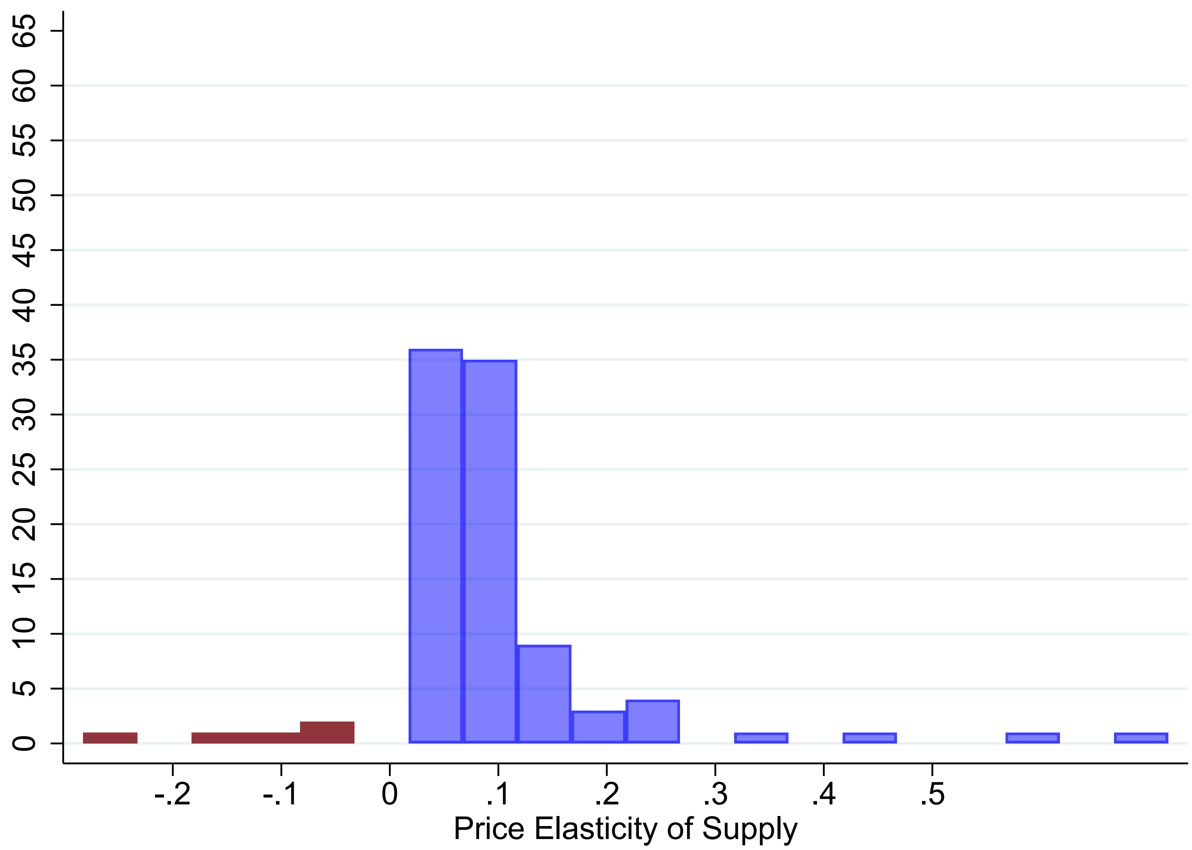
Source: Transaction volume by country from NAR's annual "Profile of International Activity in U.S. Residential Real Estate." Capital outflows from SAFE, time series of balance of payments, and are defined as the quarterly sum of: 2.2.1.2. Portfolio Investment, 2.2.1.4. Other Investment, and 3. Net errors and omissions.

Figure 11: Geographic Distribution of Local House Price Elasticities



Note: This map shows the distribution of house price elasticities. Blue CBSA's are the most observably inelastic (top quartile), followed by navy, then purple, and finally the red are the most elastic qaurtile of CBSA elasticities. Yellow CBSA's denote negative elasticities. Gray CBSA's are those we see in the data but which are not in the top 100 CBSA's by population. White regions have no data in any of our samples.

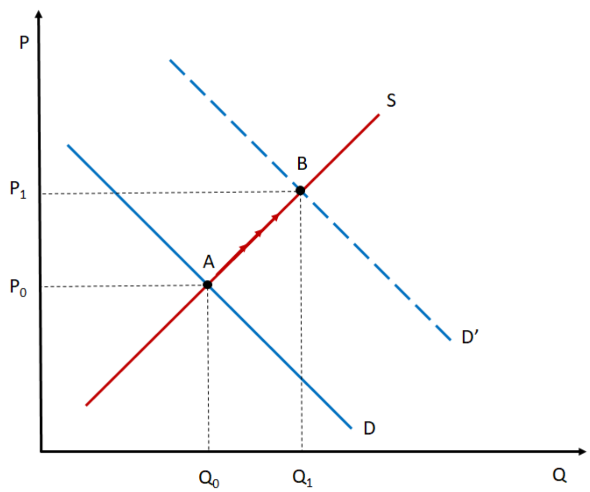
Figure 12: Distribution of Local House Price Elasticities



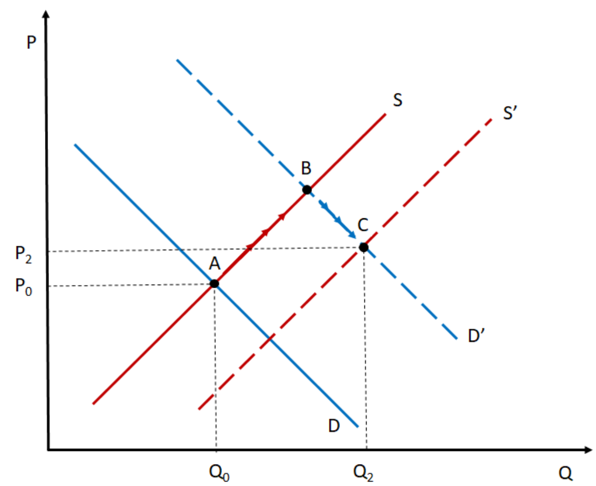
Note: This histogram shows the distribution of house price elasticities. Red bars denote negative elasticities.

Figure 13: Endogeneity Issues in Estimating House Price Elasticities

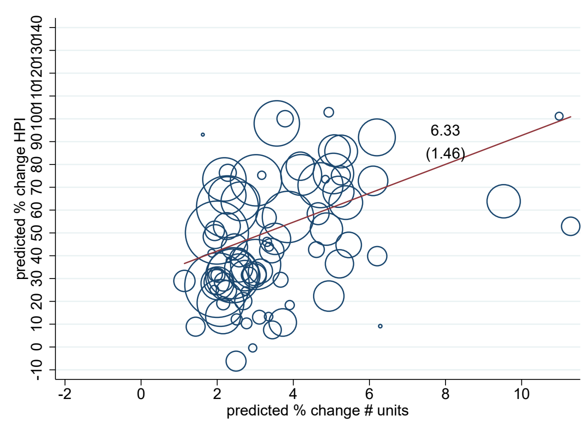
(a) Isolating Demand Response



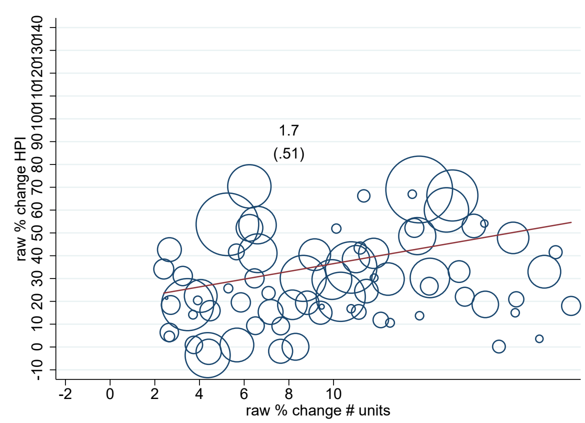
(b) Supply and Demand Response



(c) Predicted Equilibria Changes



(d) Observed Equilibria Changes



Note: This figure highlights the endogeneity problem of using observed house price and quantity changes to estimate local house price elasticity of supply. Panel (a) shows the ideal experiment, an exogenous demand shifter. Panel (b) shows the problem in extrapolating the slope from observational data; drawing a line between points A and C creates a falsely flatter supply curve. The left hand scatter in panel (c) shows the top 91 CBSA's price and quantities estimated using our IV design strategy, while the right hand side scatter shows the raw data, without isolating the demand shifter from the supply shifter.

A Tax Policy Appendix

We have identified 10 policy events across five countries that make the U.S. housing market relatively cheaper to invest in from 2011 to 2018, as summarized in Figure 3. In response to sharply rising house prices, Singapore initiated the first tax on foreign buyers in December 2011. All foreigners and entities (buyers who are not individuals) were charged a 10% Additional Buyer's Stamp Duty (ABSD) on top of the Buyer's Stamp Duty levied on all real estate purchases. In January 2013, Singapore raised the ABSD to 15% for foreigners and entities, and introduced a 5% ABSD of 5% on Singapore Permanent Residents. The ABSD increased again in July 2018 to 20% for foreigners, 25% for entities, and 30% for housing developers.

Hong Kong introduced a 15% buyer stamp duty (BSD) for non-residents in October 2012. Under the policy, any buyer who was not a Hong Kong permanent resident paid the tax on top of their purchase price. The policy extended to include companies buying properties, regardless of their local or nonlocal status. In addition to the purchase tax, Hong Kong raised the special transactions tax, which is levied on housing sales that occur within three years of initial purchase, from 10% to 20% to discourage speculation in the housing market. In November 2016, the Hong Kong government raised the stamp duty for all non first-time residential property buyers, applicable to both residents and non-residents, from 8.5% to 15%. This effectively raised the taxes paid by foreign parties from 23.5% to 30%.

The state of Victoria, Australia (home to Melbourne) introduced the Foreign Purchaser Additional Duty, applicable to foreign persons, corporations, and trusts purchasing residential property (or non-residential property with the intent of conversion) in June 2015. An additional duty at 3% of the dutiable value (the higher of the price paid for the property or the market value) was imposed from June 2015 to July 2016. It was subsequently raised to 7% in July 2016. In June 2016, the state of New South Wales, Australia (home to Sydney) introduced a 4% surcharge purchaser duty (SPD) applicable to residential real estate purchases by foreign persons. The state raised the SPD to 8% in July 2017. All duties are paid on top of the original duties paid by any purchaser of residential real estate.

The provincial government of British Columbia, Canada (home to Vancouver) passed Bill 28 in August 2016, which introduced a foreign-buyer tax, as well as a vacancy tax to specific communities in B.C. From August 2016 until February 2018, foreign buyers in the Greater Vancouver Regional District paid an additional 15% of the fair market value in tax. In February 2018, the tax amount increased to 20% of the fair market value and expanded geographically. At the same time, the city of Vancouver initiated a vacant homes tax of 1% of the assessed taxable value on residences not occupied for at least 6 months of the year.

Ontario, Canada's provincial government implemented the Non-Resident Speculation Tax (NRST) in April 2017. As per NRST, foreign entities pay a 15% tax on the residential property value for any property located in the Greater Golden Horseshoe Region of Ontario, which covers approximately $1/5^{th}$ of the population of Canada (and includes Toronto).

Most dramatically, in August 2018, New Zealand barred non-residents from purchasing real estate, excepting Singaporeans and Australians due to existing trade agreements. A number of national and local governments continue to tighten restrictions for foreign buyers. In October 2018, Theresa May announced plans to implement a foreign buyer tax in the United Kingdom, and Governor Andrew Cuomo has included a pied-a-tierre tax in his proposed 2019 New York State budget. In July 2018, the Chief Executive of Hong Kong suggested she was open to further policies aimed at limiting non-resident housing purchases.

Figure 2 documents the effects of these foreign-buyer taxes on their respective local markets; all graphs plot house price indexes, and include sales volume when available. For instance, Figure 2d displays one of the more recent policy interventions in British Columbia, and the results in the local housing market. After the enactment of the taxes, the 12-month sales volume moving average fell by 54% between its peak in February 2016 and March 2019. Although the tax has had little effect on the level of Vancouver housing prices, with the 12-month moving average falling only 1%, house price growth has effectively ceased.

B Difference-in-differences Appendix

B.1 Synthetic Control

In addition to vigorous temporal and geographic controls and the within-city, donut matching estimator, we construct a synthetic control for each treated zipcode. The synthetic control method builds a synthetic control zipcode for each treated zipcode, using pre-period demographic, economics and housing market characteristics. This synthetic control need not map to a real zipcode, and is instead a weighted combination of true zipcodes best fitting to the relevant treated zipcode. We construct our synthetic controls in a series of four steps. First, we limit the sample of zipcodes used in control construction to those within the MSA of the treated zipcode of interest. Next, we restrict to zipcodes with full histories, using data from 2005q1-2011q4. Third, we restrict the potential control sample to those with fraction FBC below the 75^{th} percentile, to ensure we do not select all control zips from those around the treatment threshold. Finally, for each zipcode we generate the synthetic control using the restricted sample.

Table D3 shows the covariate balance using the synthetic control method. While many covariates still show statistically significant differences in real economic outcomes such as establishments and population, the magnitudes are smaller than in table 1. Once we have our synthetic control sample, we again run specification 4. We cannot include any geographic trends as these controls do not have a geography, although they are constructed within a MSA.

The results of our synthetic control estimation are shown in table D4. Our preferred specification in column (2) includes quarter fixed effects, and show that house prices in high foreign-born Chinese share zipcodes rise by 10.8pp more than synthetic control zipcodes, consistent with our primary differences-in-differences results and our matching results.

B.2 Zillow Rent and House Price Data

We replicate the difference–in–differences design for the house price analysis using the Zillow Home Valuation Index and the Zillow rent Index. Figure E1 shows the raw price and rent paths for the ZHVI and ZRI for treated and control zipcodes from 2009m1 for ZHVI and 2011m1 for ZRI. Both series show much higher gains in FBC zipcodes than in control zipcodes. Additionally, we plot the ZHVI/ZRI ratio indexed to 1 in 2011q4. This price-rent ratio falls for control zipcodes over 2012, eventually recovering close to 1 by 2018, while it increases for the treated zipcode. These results suggest that purchasers were more optimistic about future house price growth in treated zipcodes than in control zipcodes; prices growing faster than rents reflect expectations of future growth, rather than any current change in the local economic environment, which would also drive up rents.

Our preferred specification in column (4) of Tables D5 and D6 control for commuting–zone by time trends. Table D5 shows that average quarterly house values are 3pp higher in the post–period in FBC zipcodes than in control zipcodes. This lower point estimate is likely due to different coverage samples; the CoreLogic data covers the entire US, while the Zillow data favors more urban areas that are more dense and hence, where neighborhoods are more substitutable for foreign homebuyers. Table D6 shows that rents have not been as responsive as house prices, with rents increasing 1.7pp more per month in FBC zipcodes relative to control zipcodes in the post period. Finally, Table D7 shows how the price-rent, or ZHVI/ZRI, ratio responds to the tax policy in zipcodes with high foreign-born Chinese zipcodes. These treated zipcodes see their price-rent ratio increase by 1.14 more than their peers, on average. This represents a significant increase in the ratio, up from 1 in 2011q4, where we normalize it, though not necessarily a doubling as the ratio is falling for the control group over this time period. Regardless, the divergence in the price-rent ratios between treated and control groups reflects optimism about price growth that is not coupled with improvements in fundamentals, such as employment, which would concurrently drive rent growth.

B.3 Other Economic Outcomes

B.3.1 Employment, Establishment Counts, Payroll

Thus far, the paper has established large price impacts resulting from the influx of foreign capital. We have not addressed whether these price impacts reflect large enough local investments to spill over into the real economy; there are a variety of other economic outcomes tied to the large inflows of international capital. The price results show that house prices rose on average by 8pp in FBC zipcodes in the post period, and the ECF results show that a 1% increase in ECF increases house prices by 0.27%. Higher house prices can positively shock local economies through a variety of channels. In this section, we explore results for real economic outcomes, changes in housing supply, and population and immigration characteristics.

First, higher housing demand can lead to an increase in construction jobs, raising demand for labor and increasing wages. These can spill over to employment in the broader local economy as more money circulates, driving higher consumption of housing and non-housing goods alike. Additionally, numerous news outlets have noted a penchant for Chinese firms investing in local businesses.[30] Foreign direct investment from China to the U.S. increased from \$3.3 Billion to \$39.5 Billion from 2010 to 2017, a nearly 1100% increase. [31]

To test whether the foreign buyer tax policies funneled money into the real economic sector, we can study the difference-in-differences for zipcode level employment, establishments, and annual payroll from the County Business Patterns. Table D8 shows the results of this analysis. Panel (a) shows that employment increases between 0.4 and 1.8% in the post period, depending on the geographic trends controlled for. Looking to the event study associated with column (3), figure E2 panel (a) shows that the average treated zipcode sees more than a 2% increase in employment by the end of 2015, with no action in the pre-period. Panel (b) in table D8 suggests an increase in establishment count by 1-2%, but the associated event study suggests this is a continuation of a pre-period trend. Finally, panel (c) in table D8 shows a 1-2% increase in annual payroll, however these results are not statistically significant. The figure associated with column (3) in panel (c) of D8, panel (c) in figure

[30] https://www.nytimes.com/2019/07/21/us/politics/china-investment-trade-war.html
https://www.nytimes.com/2018/07/12/us/politics/trade-war-china-michigan.html?module=inline
https://www.npr.org/sections/money/2017/10/27/560407035/episode-802-the-hotel-at-the-center-of-the-world

[31] Foreign Direct Investment in the U.S., Foreign Direct Investment Position on a Historical-Cost Basis, U.S. Bureau of Economic Analysis

E2, shows that by the end of 2015, annual payroll has risen by just over 3% for the average treated zipcode, relative to the average control zipcode. Annual payroll takes a few years to respond, leading to the measured insignificance in the table; however, by 2015 the growth is borderline significantly different from zero.

Taken together, the real economic results suggest that the influx of capital post-tax has impacted more than just house prices. An increase in local demand driven by house prices, or concurrent local investment, has increased employment by 2% and payroll by 3% by the end of the post-period. Because there is not much annual increase in employment or payroll, the housing demand shock stimulating the local economy seems a better match to the data than does FDI in local businesses. As FDI tends to be more concentrated in large factories or industries, it seems reasonable that this cannot be driving such geographically dispersed real economic growth.

B.3.2 Immigration vs. Foreign Purchases

Finally, we check to see whether the rise in house prices in FBC zipcodes seems to be driven strictly by investment in the housing market, or by the concurrent move of people and capital, namely immigration. Table D9 checks to see how populations, and population shares have responded to the foreign buyer tax and capital outflows. Panel (a) checks to see how local populations, foreign born populations, and FBC populations have changed in the post-period in FBC treated zipcodes. Column (1) suggests that the total population has increased by 0.8% on average. This growth seems to come from FBC immigrants, as their population increased by 4.4% on average, while the total foreign-born population, declines by 2.2%. These results suggest that FBC locations add very little population, but the FBC population grows significantly; however, since the number of FBC residents is small in the pre-period, this 4% growth is not very tangible; the FBC population adds about 2.5k new residents, relative to the average county population of 1.5 million in the pre-period. This is reflected in panel (b), which studies how the fraction of foreign born populations change in the post-period. Column (1) says that the total fraction foreign born has declined by 0.0025, from 0.276 in the pre-period to 0.273 in the post-period. Fraction FBC has increased from 0.039 in the pre-period, on average, to 0.042 in the post-period; foreign-born Chinese residents still make up only about 4% of even FBC treated counties. All told, it does not seem feasible that this increase in prices is entirely due to immigration of wealthy Chinese to the U.S.; instead foreign ownership of local real estate as an investment property matches the data better.

Taken altogether, these other outcomes paint a picture of foreign investment in U.S. housing stimulating local economic demand in the real economy, through the expansion of jobs, annual payroll, and the building of new housing units, all for a relatively unchanged population. The results are not consistent with immigrants leaving China and setting up local firms driving our findings.

C IV Approach Appendix

C.1 IV Exclusion Restriction

A plausible violation of the exclusion restriction is that investment in the technology sector drives house price results. As foreign countries impose foreign buyer taxes, foreigners choose to invest in U.S. tech stocks instead of in foreign real estate. This leads to economic growth in tech-heavy cities, which tend to be inelastically supplied with housing, increasing house prices. Therefore, the tax policy change $\implies E[\epsilon_{it}(FBC_i \times Post_t)] \neq 0$, where $Post_t$ is the tax policy change, and the city's high-tech status is in ϵ_{it}, which is the second stage error term, and thus correlated with the second stage left-hand-side variable, $ln(hpi_{it})$.

We test for this mechanism by excluding San Francisco, San Jose and Seattle from our estimation sample. If tech drives our house prices, the difference-in-difference estimator should fall to zero. If tech investment is not the destination for all of the foreign capital and instead it moves into the housing market, the coefficient for our difference-in-difference estimator should be similar to the unrestricted sample.

Table D10 shows the results when excluding the big tech hubs from our sample. The point estimats show a 6-13pp increase in house prices in treated zipcodes relative to control zipcodes, in line with our main results showing an 8-17pp increase. This shows that Seattle, San Francisco and San Jose are not driving our results, discrediting this violation of the exclusion restriction.

Instead of removing major housing markets from the data sample, we can also directly control for the tech sector directly. Table D11 shows the results as in Table 2, but also controlling for either employment or establishments, by NAICS category. Employment and establishment data is taken at the county by year level from the County Business Patterns, and so is both local and time-varying. The top panel controls for establishments or employment in levels, while the bottom panel controls by share. In both panels, the baseline point estimate shows that after the Singaporean foreing buyer tax, zipcodes with high fractions of foreign-born Chinese residents see, on average, 10% additional HPI relative to their peers in the same city. Adding controls from the number of firms in the management and professional services industries, as in Ding et al. (2019), column (2) reports that the point estimate falls from 10% to 9%. Controlling just for "Professional, Scientific, and Technical Services" in column (3) yields a similar result. Column (4) controls for the number of businesses in "Computer Systems Design and Related Services", with a similar point estimate. Columns (5) - (7) repeat the controls, but use time-varying employment rather than establishment count.

In sum, no way of accounting for the employment or establishment growth in the tech sector between 2009 and 2019 can undo the impact of foreign capital flowing to U.S. housing markets, and specifically to zipcodes with ex-ante high shares of foreign-born residents. These results are consistent with excising tech-heavy cities from the sample, with all robustness checks yielding differential house price growth in treated zips, ranging from 6% to 10%, in line with our baseline estimate of 10%.

C.2 Expected Capital Flows, Exposure IV

The ECF_{it} exposure IV scales the per-capita capital flows by the fraction foreign-born of the respective country within a zipcode. For example, consider two zipcodes with 3 foreign-born Chinese residents. In the baseline ECF_{it}, each Chinese resident receives the same per-capita share of the national capital flow from China into the U.S. housing market. The exposure index scales this per-capita share by the share of Chinese residents in the total population of the zipcode. If the first zipcode has 10 residents, and the second has 100, then the first zipcode is therefore more exposed to the Chinese capital as it is diluted among fewer non-foreign-born residents:

$$ECF_{it} = \sum_{c \in C} capflow_{ct} \times \frac{FBpop_{ic}^{2011}}{FBpop_{c}^{2011}} fracFB_{ic} \tag{11}$$

where

$$1 = \sum_{i} \frac{FBpop_{ic}^{2011}}{FBpop_{c}^{2011}} \tag{12}$$

and $C = \{$Canada, China, India, Mexico, United Kingdom, Other$\}$, i denotes zipcode, t denotes quarter.

Table D19 shows the results using the exposure ECF_{it}. The second stage yields an price elasticity estimate of 0.85, above but in the same ballpark as our preferred measure.

D Appendix Tables

Table D1: Housing Characteristics for Transactions in FBC vs. Control Zipcodes

	(1)	(2)	(3)
Variable	Control	FBC	Difference
Price ($1000's)	195.91	559.84	110.98***
	(176.74)	(278.86)	(0.00)
Lot (Acres)	3.07	0.80	-1.85***
	(26.56)	(2.94)	(0.00)
Square Feet	1,618.88	1,688.48	-23.80***
	(573.51)	(765.81)	(0.00)
Year Built	1948	1954	2***
	(44)	(34)	(0.00)
Bedrooms	2.60	2.55	-0.28***
	(0.86)	(1.03)	(0.00)
Bathrooms	1.87	2.16	-0.01*
	(0.75)	(0.92)	(0.05)
Garage Spaces	2.20	1.75	-0.17***
	(2.24)	(1.66)	(0.00)
Observations	1,030,756	14,526	1,227,368

Notes: Columns (1) and (2) present raw means for the housing characteristics, with standard errors in parentheses. Significance: *** p<0.01, ** p<0.05, * p<0.1. Column (3) shows the difference in means between FBC and control zipcodes, controlling for commuting zone, with p-values in parentheses.

Table D2: Matching Covariate Balance

Variable	FBC=0	FBC=1	Difference
HPI	1.658	1.753	0.095
	(0.838)	(0.882)	(0.112)
HPI growth, 1 Year	-0.018	0.007	0.025**
	(0.140)	(0.270)	(0.010)
HPI growth, 5 Years	-0.150	-0.036	0.114*
	(0.520)	(0.672)	(0.058)
Lagged HPI	1.688	1.767	0.079
	(0.905)	(0.902)	(0.123)
Sales	62.406	68.825	6.419
	(59.910)	(51.408)	(10.386)
Lagged Sales	63.410	70.177	6.767
	(62.213)	(52.984)	(10.648)
Permits: Single Family Units	1,296.888	1,101.770	-195.118
	(1,814.654)	(1,551.373)	(122.900)
Permits: All Units	3,179.571	3,270.838	91.267
	(3,815.905)	(3,040.635)	(453.422)
Establishments	639.308	1,059.777	420.469***
	(549.566)	(763.174)	(70.664)
Estab. growth, 1 year	0.002	0.006	0.004
	(0.046)	(0.029)	(0.002)
Estab. growth, 5 years	-0.012	0.020	0.031**
	(0.118)	(0.101)	(0.013)
Employment	8,999.520	17,185.475	8,185.956***
	(9,426.618)	(17,234.313)	(1,810.485)
Emp. growth, 1 year	-0.011	-0.000	0.010
	(0.093)	(0.073)	(0.008)
Emp. growth, 5 years	-0.049	-0.021	0.028
	(0.218)	(0.190)	(0.031)
Annual Payroll (1000s)	407.356	1,171.946	764.590***
	(465.063)	(1,686.978)	(176.334)
2010 Population	29,903.615	33,978.137	4,074.522
	(26,508.203)	(21,410.336)	(3,988.653)
2011 Population	29,592.615	33,515.078	3,922.463
	(26,178.000)	(21,102.682)	(3,945.637)
2011 Median Income	75,604.023	80,449.164	4,845.136
	(30,497.830)	(35,586.953)	(5,591.262)

Notes: This table shows pre-period balance for housing and labor market characteristics, using the matched sample of zipcodes. $FBC_i = 1\left\{\frac{FBCpop_i}{pop_i} \geq 99^{th} percentile\right\}$ for zipcode i. Data is zipcode level, excepting permits data, which is at the county level. Data at the quarterly level, excepting annual employment, establishment, and payroll data. Data through 2011q3 for the housing and permit data, and through 2012 for the annual data. Standard errors in parentheses, clustered by MSA. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D3: Synthetic Control Covariate Balance

Variable	FBC=0	FBC=1	Difference
HPI	1.854	1.884	0.029
	(0.856)	(0.933)	(0.049)
HPI growth, 1 Year	0.001	0.038	0.037***
	(0.197)	(0.377)	(0.011)
HPI growth, 5 Years	0.281	0.358	0.078
	(0.612)	(0.824)	(0.047)
Lagged HPI	1.890	1.890	-0.000
	(0.799)	(0.910)	(0.039)
Sales	67.583	54.984	-12.599
	(58.632)	(49.399)	(10.288)
Lagged Sales	67.201	55.502	-11.700
	(58.778)	(49.638)	(10.080)
Permits: Single Family Units	2,210.344	2,008.318	-202.025
	(3,498.401)	(3,524.137)	(135.465)
Permits: All Units	4,848.725	5,548.749	700.024*
	(6,198.069)	(6,481.169)	(349.221)
Establishments	705.786	1,056.400	350.614***
	(378.453)	(750.135)	(100.694)
Estab. growth, 1 year	0.001	0.008	0.007**
	(0.026)	(0.033)	(0.003)
Estab. growth, 5 years	-0.016	0.027	0.043***
	(0.057)	(0.104)	(0.008)
Employment	9,436.474	17,688.168	8,251.694***
	(6,283.615)	(17,567.346)	(2,464.070)
Emp. growth, 1 year	-0.004	0.005	0.009***
	(0.054)	(0.157)	(0.002)
Emp. growth, 5 years	-0.031	-0.010	0.020
	(0.117)	(0.196)	(0.015)
Annual Payroll (1000s)	377.082	1,099.842	722.760***
	(263.612)	(1,560.644)	(178.663)
2010 Population	34,189.008	34,651.656	462.648
	(20,708.795)	(21,402.146)	(4,886.639)
2011 Population	33,767.156	34,187.805	420.650
	(20,216.633)	(21,094.246)	(4,834.596)
2011 Median Income	67,904.367	80,136.805	12,232.438***
	(16,403.313)	(34,968.406)	(4,066.386)

Notes: This table shows pre-period balance for housing and labor market characteristics, using the synthetic control sample of zipcods. $FBC_i = 1\left\{\frac{FBCpop_i}{pop_i} \geq 99^{th} percentile\right\}$ for zipcode i. Data is zipcode level, excepting permits data, which is at the county level. Data at the quarterly level, excepting annual employment, establishment, and payroll data. Data through 2011q3 for the housing and permit data, and through 2012 for the annual data. Standard errors in parentheses, clustered by MSA. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D4: Synthetic Control Results

	(1) HPI	(2) HPI
Post=1 X FBC=1	0.392***	0.108**
	(0.0388)	(0.0386)
R^2	0.089	0.167
Observations	13407	13407
	Fixed Effects	
Quarter		X

Notes: This table shows the coefficient β from $hpi_{it} = \alpha + \beta FBC_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$, using the synthetic control as FBC. All data at the quarter level. Standard errors in parentheses, clustered by quarter. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D5: Difference–in–Differences Results: Zillow Home Value Index

	(1) ZHVI	(2) ZHVI	(3) ZHVI	(4) ZHVI
Post=1 X FBC=1	0.176***	0.0894***	0.0298	0.0341
	(0.0160)	(0.0227)	(0.0331)	(0.0345)
R^2	0.732	0.841	0.879	0.873
Observations	491259	326948	146391	155260
		Fixed Effects		
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
MSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficient β from $ZHVI_{it} = \alpha + \beta FBC_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. FBC=1 defined as $FBC_i = 1\left\{\frac{FBCpop_i}{pop_i} \geq 99^{th} percentile\right\}$ for zipcode i. All data at the zipcode by quarter level. Standard errors in parentheses, clustered by month, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D6: Difference–in–Differences Results: Zillow Rent Index

	(1) ZRI	(2) ZRI	(3) ZRI	(4) ZRI
Post=1 X FBC=1	0.0908***	0.0676***	0.00260	0.0173*
	(0.00899)	(0.0185)	(0.0126)	(0.0101)
R^2	0.730	0.776	0.855	0.848
Observations	396774	266290	126366	132152
		Fixed Effects		
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
MSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficient β from $ZRI_{it} = \alpha + \beta FBC_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. FBC=1 defined as $FBC_i = 1\left\{\frac{FBCpop_i}{pop_i} \geq 99^{th} percentile\right\}$ for zipcode i. All data at the zipcode by quarter level. Standard errors in parentheses, clustered by month, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D7: Difference–in–Differences Results: Zillow Price/Rent Ratio

	(1) ZHVI/ZRI	(2) ZHVI/ZRI	(3) ZHVI/ZRI	(4) ZHVI/ZRI
Post=1 X FBC=1	1.273*	0.883	1.474***	1.136**
	(0.662)	(0.595)	(0.496)	(0.529)
R^2	0.453	0.419	0.467	0.457
Observations	283618	191514	99671	103899
		Fixed Effects		
Quarter X	X	X	X	
Zip	X	X	X	X
State X Quarter		X		
MSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficient β from $ZHVI/ZRI_{it} = \alpha + \beta FBC_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. $ZHVI/ZRI_{it}$ normalized to 1 in 2011q4. FBC=1 defined as $FBC_i = 1\left\{\frac{FBCpop_i}{pop_i} \geq 99^{th} percentile\right\}$ for zipcode i. All data at the zipcode by quarter level. Standard errors in parentheses, clustered by month, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D8: Impact on Zipcode Log Employment, Establishments, and Annual Payroll

	(1) Emp.	(2) Emp.	(3) Emp.	(4) Emp.
Post=1 X FBC=1	-0.000564	0.0179**	0.0110	0.0112*
	(0.0131)	(0.00770)	(0.00712)	(0.00646)
R^2	0.989	1.000	1.000	1.000
Observations	15163	70890	27673	29966
		Fixed Effects		
Year	X	X	X	X
Zip	X	X	X	X
State X Year		X		
Zone X Year			X	
MSA X Year				X

(a) Employment

	(1) Est.	(2) Est.	(3) Est.	(4) Est.
Post=1 X FBC=1	0.00647	0.0224***	0.0129***	0.0158***
	(0.00864)	(0.00381)	(0.00449)	(0.00432)
R^2	0.994	1.000	1.000	1.000
Observations	15487	75778	28516	31050
		Fixed Effects		
Year	X	X	X	X
Zip	X	X	X	X
State X Year		X		
Zone X Year			X	
MSA X Year				X

(b) Establishments

	(1) Pay	(2) Pay	(3) Pay	(4) Pay
Post=1 X FBC=1	0.0122	0.0233	0.00988	0.0134
	(0.0213)	(0.0136)	(0.0133)	(0.0151)
R^2	0.992	1.000	1.000	1.000
Observations	13149	63151	24118	26265
		Fixed Effects		
Year	X	X	X	X
Zip	X	X	X	X
State X Year		X		
Zone X Year			X	
MSA X Year				X

(c) Annual Payroll

Table D9: Impact on County Level Population and Immigration

	(1) ln(Pop.)	(2) ln(Pop. FB)	(3) ln(Pop. FBC)
Post = 1 X FBC = 1	0.00791*	-0.0216***	0.0437*
	(0.00469)	(0.00558)	(0.0225)
Root MSE	0.0269	0.103	0.332
Observations	25140	25033	15222

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(a) Population Changes

	(1) Fraction FB	(2) Fraction FBC
Post = 1 X FBC = 1	-0.00252***	0.00374***
	(0.000223)	(0.000107)
Root MSE	0.00412	0.00158
Observations	25140	15222

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(b) Fraction Foreign Born

Table D10: Results Excluding Tech-Oriented Housing Markets

	(1) HPI	(2) HPI	(3) HPI	(4) HPI
Post=1 X FBC=1	-0.0190	0.132*	0.0616	0.0636
	(0.0523)	(0.0632)	(0.0451)	(0.0569)
R^2	0.876	0.901	0.894	0.892
Observations	76547	415221	162542	149673

(a) Results Excluding San Francisco, San Jose and Seattle

	(1) HPI	(2) HPI	(3) HPI	(4) HPI
Post=1 X FBC=1	0.118*	0.167***	0.0781*	0.0950**
	(0.0693)	(0.0316)	(0.0435)	(0.0385)
R^2	0.869	0.899	0.889	0.891
Observations	86768	428903	163355	176224
		Fixed Effects		
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
MSA X Quarter			X	
Zone X Quarter				X

(b) Baseline Results

Table D11: Results Controlling for County Level Management and Professional Services

		NAICS Establishments			NAICS Employment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	baseline	54 & 55	54	5415	54 & 55	54	5415
Post=1 X FBC=1	0.0952**	0.0886**	0.0887**	0.0909**	0.0858**	0.0864**	0.0924**
	(0.0386)	(0.0385)	(0.0386)	(0.0385)	(0.0389)	(0.0389)	(0.0385)
R^2	0.890	0.891	0.891	0.890	0.891	0.891	0.890
Observations	176202	176202	176202	176202	176202	176202	176202
				Fixed Effects			
Quarter	X	X	X	X	X	X	X
Zip	X	X	X	X	X	X	X
Zone X Quarter	X	X	X	X	X	X	X

(a) Employment and Establishment Levels

		NAICS Establishments			NAICS Employment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	baseline	54 & 55	54	5415	54 & 55	54	5415
Post=1 X FBC=1	0.0952**	0.0986**	0.0983**	0.0980**	0.0932**	0.0955**	0.0967**
	(0.0386)	(0.0384)	(0.0384)	(0.0388)	(0.0384)	(0.0383)	(0.0383)
R^2	0.890	0.890	0.890	0.890	0.890	0.890	0.890
Observations	176202	176202	176202	176202	176202	176202	176202
				Fixed Effects			
Quarter	X	X	X	X	X	X	X
Zip	X	X	X	X	X	X	X
Zone X Quarter	X	X	X	X	X	X	X

(b) Employment and Establishment Share

Notes: Regressions control for county level employment or establishment count in the management and professional services (MPRO) as defined in Ding et al. (2019). Columns (2) and (5) control for NAICS code 54, "Professional, Scientific, and Technical Services" together with NAICS code 55, "Management of Companies and Enterprises", which together form MPRO. Columns (3) and (6) control only for activity in NAICS 54, excluding management services from MPRO; columns (4) and (7) control for a particularly fast growing sub-industry as identified by Ding et al. (2019), NAICS 5415, "Computer Systems Design and Related Services". Panel (a) controls for either establishments or employment in level, for each county and year. Panel (b) controls for either establishments or employment as shares of total establishments or employment in a given county-year. Data from the County Business Patterns 2009-2015.

Table D13: ECF_{it} Intuition: 19104 in 2017q1

c	$FBpop_{ic}^{2011}$	$FBpop_{c}^{2011}$	$capflow_{ct}, \$B$	$Volume_{ict}, \$M$
Canada	140	811,101	4.75	0.82
China	2175	2,241,390	7.9	7.67
India	754	1,896,640	1.95	0.78
Mexico	220	11,604,684	2.325	0.04
UK	185	688,588	2.375	0.64
Other	3845	23,097,640	18.9	3.15
$ECF_{it}, \$M$				13.1

Notes: $ECF_{it} = \sum_{c \in C} capflow_{ct} \times \frac{FBpop_{ic}^{2011}}{FBpop_{c}^{2011}}$, where $1 = \sum_i \frac{FBpop_{ic}^{2011}}{FBpop_{c}^{2011}}$, $C = \{$Canada, China, India, Mexico, United Kingdom$\}$, i denotes zipcode, t denotes quarter. In the table, $Volume_{ict} = 1000 * capflow_{ct} * \frac{FBpop_{ic}^{2011}}{FBpop_{c}^{2011}}$

Table D14: Expected Capital Flow IV, controlling for Population and Income: First Stage

	(1) Baseline	(2) ACS-Census Sample	(3) Population & Income Controls
Post X Frac. FB	1.007***	1.007***	0.968***
	(0.0330)	(0.0330)	(0.0329)
R^2	0.989	0.989	0.989
F	930.4	930.4	369.7
Observations	787303	787224	787224
		Fixed Effects	
Zip	X	X	X
Quarter	X	X	X
Zone X Quarter	X	X	X

Notes: This table shows the first stage results from $ln(ECF_{it}) = \alpha + \beta frac\_FB_i \times Post_t + \zeta_i + \theta_t + Controls_{it} + \varepsilon_{it}$. $Controls_{it}$ are denoted in the column titles. Baseline results from column (1) in Table 7. All data mean collapsed at the quarter level. Standard errors in parentheses, clustered by geography. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D15: Expected Capital Flow IV, controlling for Population and Income: Second Stage

	(1) Baseline	(2) ACS-Census Sample	(3) Population & Income Controls
ln(ECF_{it})	0.274***	0.274***	0.294***
	(0.0670)	(0.0670)	(0.0713)
Root MSE	1.433	1.433	1.433
Observations	787303	787224	787224
		Fixed Effects	
Zip	X	X	X
Quarter	X	X	X
Zone X Quarter	X	X	X

*Notes:*The table shows the second stage results from $Y_{it} = \delta + \gamma ln(\widehat{ECF_{it}}) + \zeta_i + \theta_t + Controls_{it} + \epsilon_{it}$. $Controls_{it}$ are denoted in the column titles. Baseline results from column (1) in Table 8. All data mean collapsed at the quarter level. All data mean collapsed at the quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D16: Expected Capital Flow IV, excluding China: First Stage

	(1) ln(ECF_{it})	(2) ln(ECF_{it})	(3) ln(ECF_{it})
Post X Frac. FB	1.033***	0.971***	1.031***
	(0.0697)	(0.0657)	(0.141)
R^2	0.987	0.998	0.997
F	219.7	218.4	53.62
Observations	784265	92423	26398
		Fixed Effects	
Zip	X		
Quarter	X	X	X
County		X	X
Zone X Quarter	X	X	X

Notes: This table shows the first stage results from $ln(ECF_{it}) = \alpha + \beta frac\_FB_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. All data mean collapsed at the quarter level. Standard errors in parentheses, clustered by geography. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D17: Expected Capital Flow IV, excluding China: Second Stage

	(1) ln(HPI)	(2) ln(HPI)	(3) $\frac{dQ_{it}}{Q_{it}}$
ln(ECF$_{it}$)	0.268***	0.549***	0.00450***
	(0.0884)	(0.0842)	(0.00109)
Root MSE	1.453	0.128	0.00177
Observations	784265	92423	26398
		Fixed Effects	
Zip	X		
Quarter	X	X	X
County		X	X
Zone X Quarter	X	X	X

*Notes:*The table shows the second stage results from $ln(HPI_{it}) = \delta + \gamma ln(\widehat{ECF_{it}}) + \zeta_i + \theta_t + \epsilon_{it}$. All data mean collapsed at the quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D18: Expected Capital Flow IV, Exposure: First Stage

	(1) ln(ECF$_{it}$)	(2) ln(ECF$_{it}$)	(3) ln(ECF$_{it}$)
Post X Frac. FB	0.749***	0.460***	0.509***
	(0.0394)	(0.0494)	(0.0521)
R^2	0.970	0.998	0.996
F	360.4	86.83	95.28
Observations	787303	92438	26398
		Fixed Effects	
Zip	X		
Quarter	X	X	X
County		X	X
Zone X Quarter	X	X	X

Notes: This table shows the first stage results from $ln(ECF_{it}) = \alpha + \beta frac\_FB_i \times Post_t + \zeta_i + \theta_t + \varepsilon_{it}$. All data mean collapsed at the quarter level. Standard errors in parentheses, clustered by geography. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D19: Expected Capital Flow IV, Exposure: Second Stage

	(1) ln(HPI)	(2) ln(HPI)	(3) $\frac{dQ_{it}}{Q_{it}}$
ln(ECF$_{it}$)	0.369***	1.158***	0.00912***
	(0.125)	(0.211)	(0.00215)
Root MSE	1.453	0.124	0.00167
Observations	787303	92438	26398
		Fixed Effects	
Zip	X		
Quarter	X	X	X
County		X	X
Zone X Quarter	X	X	X

*Notes:*The table shows the second stage results from $ln(HPI_{it}) = \delta + \gamma ln(\widehat{ECF}_{it}) + \zeta_i + \theta_t + \epsilon_{it}$. All data mean collapsed at the quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D20: List of CBSAs from Most to Least Inelastic

Rank	CBSA	Elasticity
1	Providence-Warwick, RI-MA	0.0174
2	San Francisco-Oakland-Hayward, CA	0.0298
3	Boston-Cambridge-Newton, MA-NH	0.0298
4	Minneapolis-St. Paul-Bloomington, MN-WI	0.0311
5	Sacramento–Roseville–Arden-Arcade, CA	0.0341
6	Miami-Fort Lauderdale-West Palm Beach, FL	0.0364
7	Riverside-San Bernardino-Ontario, CA	0.0369
8	Charleston-North Charleston, SC	0.0378
9	Columbia, SC	0.0380
10	North Port-Sarasota-Bradenton, FL	0.0382
11	New Orleans-Metairie, LA	0.0393
12	Los Angeles-Long Beach-Anaheim, CA	0.0397
13	Stockton-Lodi, CA	0.0402
14	San Diego-Carlsbad, CA	0.0409
15	Las Vegas-Henderson-Paradise, NV	0.0413
16	Pueblo, CO	0.0421
17	Memphis, TN-MS-AR	0.0425
18	Las Cruces, NM	0.0453
19	Augusta-Richmond County, GA-SC	0.0479
20	Indianapolis-Carmel-Anderson, IN	0.0528
21	Phoenix-Mesa-Scottsdale, AZ	0.0531
22	Greeley, CO	0.0548
23	Tampa-St. Petersburg-Clearwater, FL	0.0558
24	Cincinnati, OH-KY-IN	0.0560
25	Naples-Immokalee-Marco Island, FL	0.0569
26	Grand Rapids-Wyoming, MI	0.0580
27	Pittsburgh, PA	0.0590
28	Knoxville, TN	0.0604
29	Columbus, OH	0.0614
30	Fort Wayne, IN	0.0616
31	Clarksville, TN-KY	0.0642
32	Milwaukee-Waukesha-West Allis, WI	0.0657
33	Cape Coral-Fort Myers, FL	0.0659
34	Port St. Lucie, FL	0.0662
35	Seattle-Tacoma-Bellevue, WA	0.0665
36	Dallas-Fort Worth-Arlington, TX	0.0665
37	Denver-Aurora-Lakewood, CO	0.0674
38	Houston-The Woodlands-Sugar Land, TX	0.0677
39	Fort Collins, CO	0.0693
40	Colorado Springs, CO	0.0698

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Rank	CBSA	Elasticity
41	Louisville/Jefferson County, KY-IN	0.0700
42	Evansville, IN-KY	0.0718
43	St. Louis, MO-IL	0.0719
44	New York-Newark-Jersey City, NY-NJ-PA	0.0737
45	Oklahoma City, OK	0.0741
46	San Antonio-New Braunfels, TX	0.0743
47	Washington-Arlington-Alexandria, DC-VA-MD-WV	0.0746
48	Greensboro-High Point, NC	0.0759
49	Charlotte-Concord-Gastonia, NC-SC	0.0762
50	Greenville-Anderson-Mauldin, SC	0.0766
51	Lafayette-West Lafayette, IN	0.0766
52	Chicago-Naperville-Elgin, IL-IN-WI	0.0772
53	Salt Lake City, UT	0.0796
54	Omaha-Council Bluffs, NE-IA	0.0813
55	Atlanta-Sandy Springs-Roswell, GA	0.0832
56	Austin-Round Rock, TX	0.0837
57	Portland-Vancouver-Hillsboro, OR-WA	0.0850
58	Bend-Redmond, OR	0.0865
59	Kansas City, MO-KS	0.0871
60	Madison, WI	0.0901
61	Birmingham-Hoover, AL	0.0907
62	Richmond, VA	0.0927
63	Nashville-Davidson–Murfreesboro–Franklin, TN	0.0943
64	Tucson, AZ	0.0944
65	Tulsa, OK	0.0952
66	Punta Gorda, FL	0.0998
67	Durham-Chapel Hill, NC	0.108
68	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	0.108
69	Dover, DE	0.109
70	Columbia, MO	0.112
71	McAllen-Edinburg-Mission, TX	0.113
72	Jacksonville, FL	0.122
73	Pensacola-Ferry Pass-Brent, FL	0.124
74	Albuquerque, NM	0.134
75	Raleigh, NC	0.143
76	Orlando-Kissimmee-Sanford, FL	0.149
77	Des Moines-West Des Moines, IA	0.156
78	Reno, NV	0.157
79	Baltimore-Columbia-Towson, MD	0.160
80	Palm Bay-Melbourne-Titusville, FL	0.161

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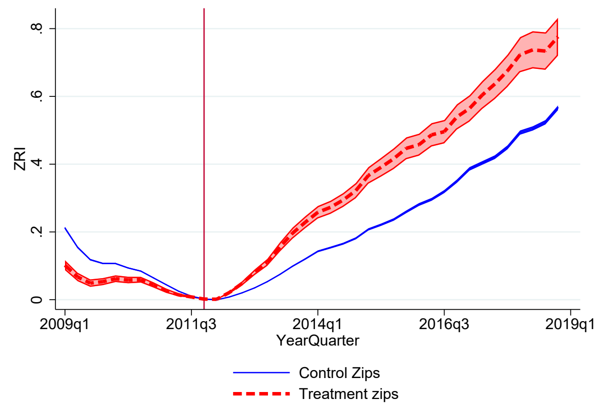
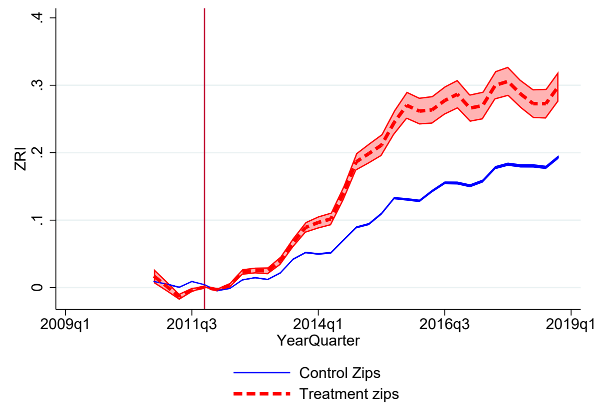
Rank	CBSA	Elasticity
81	Shreveport-Bossier City, LA	0.208
82	College Station-Bryan, TX	0.213
83	Boise City, ID	0.213
84	Cleveland-Elyria, OH	0.221
85	Winston-Salem, NC	0.238
86	Laredo, TX	0.251
87	Trenton, NJ	0.267
88	Virginia Beach-Norfolk-Newport News, VA-NC	0.346
89	Lakeland-Winter Haven, FL	0.461
90	Ocala, FL	0.604
91	Salisbury, MD-DE	0.687
	Greenville, NC	-10.77
	Columbus, GA-AL	-7.193
	Detroit-Warren-Dearborn, MI	-0.925
	Deltona-Daytona Beach-Ormond Beach, FL	-0.403
	Wilmington, NC	-0.283
	Allentown-Bethlehem-Easton, PA-NJ	-0.167
	Tallahassee, FL	-0.105
	Vineland-Bridgeton, NJ	-0.0792
	Atlantic City-Hammonton, NJ	-0.0468

E Appendix Figures

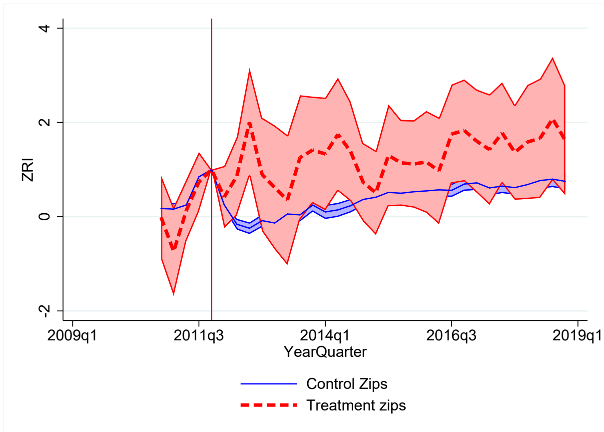
Figure E1: Event Studies for Zillow Home Value Index (ZHVI) and Zillow Rent Index (ZRI)



(a) ZHVI

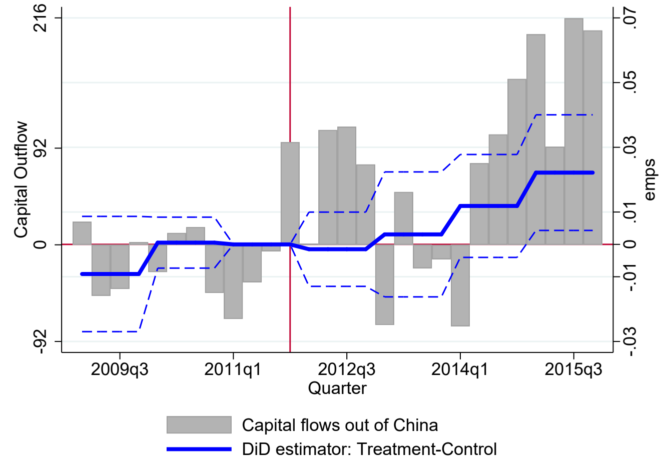


(b) ZRI

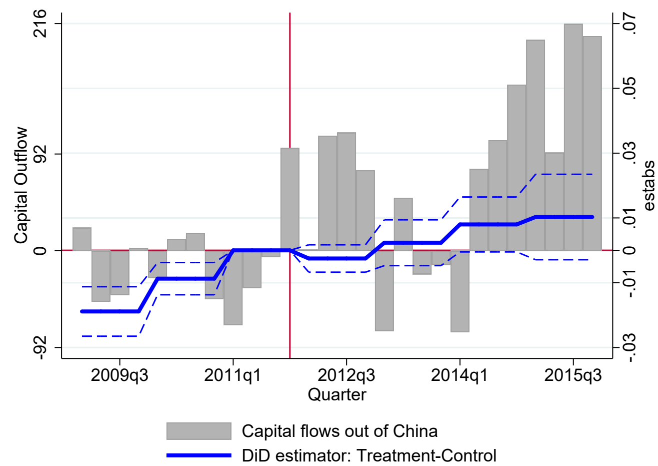


(c) ZHVI/ZRI

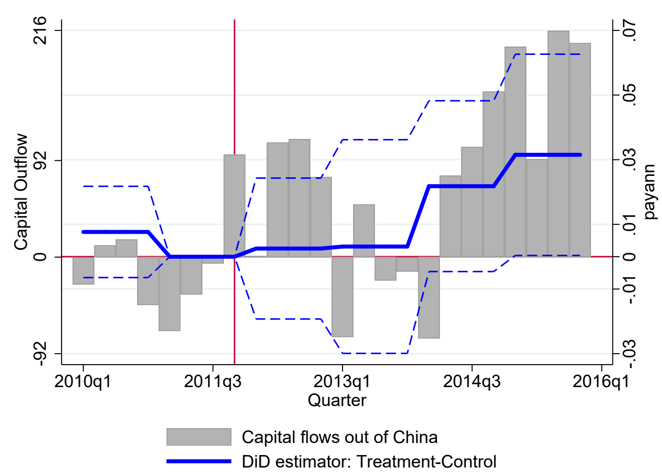
Figure E2: Event Studies for Employment, Establishments, and Annual Payroll



(a) ln(Employment)

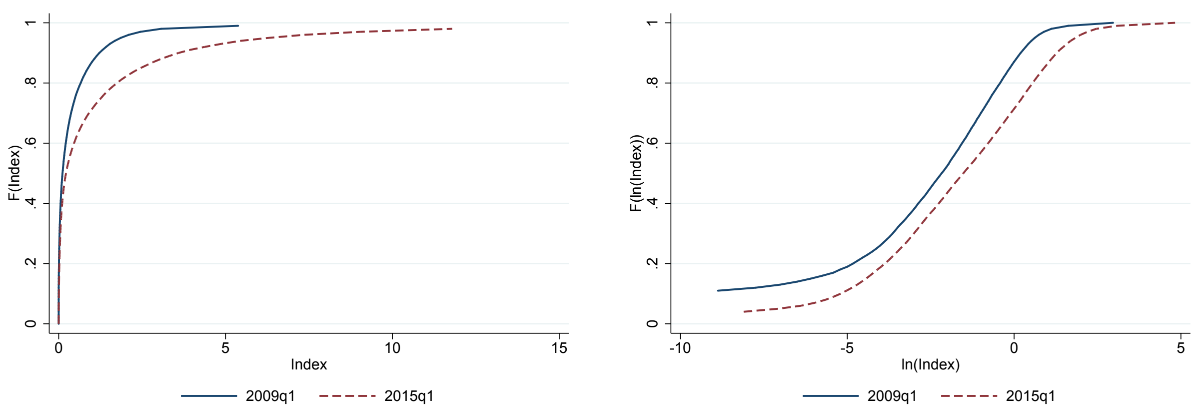


(b) ln(Establishments)



(c) ln(Annual Payroll)

Figure E3: ECF_{it} Summary



(a) ECF_{it} (b) $ln(ECF_{it})$