Global Capital and Local Assets: House Prices, Quantities, and Elasticities *

Caitlin Gorback† Benjamin J. Keys‡

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Abstract

This paper exploits foreign demand shocks to the U.S. housing market to estimate local price elasticities of supply. Other countries introduced foreign-buyer taxes beginning in 2011, intended to deter foreign housing investment. We show house prices grew 6 to 9 percentage points more in U.S. zipcodes with high foreign born populations after 2011, subsequently reversing with the cooling of global-U.S. relations post-2017. We use these international tax policy changes as a U.S. housing demand shock and estimate local house price and quantity elasticities with respect to international capital. The ratio of these two elasticities yields a new estimate of the local house price elasticity of supply, which we construct for 100 large U.S. cities. These supply elasticities average 0.26 and vary between 0.06 and 0.9, suggesting that local housing markets are currently inelastic and exhibit substantial spatial heterogeneity.

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†National Bureau of Economic Research. Email: gorback@nber.org

‡The Wharton School, University of Pennsylvania, and NBER. Email: benkeys@wharton.upenn.edu

1 Introduction

U.S. housing markets have become increasingly unaffordable, as prices have risen faster than incomes, while new supply has declined (Joint Center for Housing Studies, 2020). Between 2010 and 2020, home prices (as measured by the Case-Shiller index) grew 3.8% per year, or 45% between 2010 and 2020, while new housing starts fell below 1 million per year, 37% lower than the long-run average since 1960. Understanding the drivers of this historic decline in new supply first requires estimating the key parameter that characterizes housing development: the house price elasticity of supply.

We provide new local estimates of this parameter by exploiting a novel macroprudential shock abroad, the imposition of foreign buyer taxes on housing, that exogenously varied housing demand in U.S. markets. First, we show in the reduced form that this shock is sizable. Next, we use the shock to estimate the house price elasticity of supply for 100 U.S. cities, which cover 60% of the U.S. population, and are 16.4% foreign born on average. We find that over the decade 2009-2018, housing markets across the U.S. exhibit significant inelasticity, in line with increases in regulation and unaffordability.

How housing supply responds to changes in price affects the choices made by households and firms regarding home equity, loan collateral, and ultimately consumption and production decisions.[1] Because the tightness of housing supply impacts the entry cost to a location, the supply elasticity ultimately governs the equilibrium of spatial models that allow for worker and firm sorting across space, commuting, migration patterns, and contribute to misallocation of resources.[2] Thus, well-estimated housing elasticities play a central role in a broad range of reduced form and structural applications.

To measure the reduced form impact of increased foreign capital on domestic housing markets, we exploit time-series variation in international tax policy and cross-sectional variation in the likely destinations for these investments. Singapore first imposed foreign buyer taxes in December 2011, largely in response to an influx of Chinese capital driving up house

[1] See Chaney, Sraer and Thesmar (2012); Mian, Rao and Sufi (2013); Mian and Sufi (2014); Adelino, Schoar and Severino (2015); Favara and Imbs (2015); Charles, Hurst and Notowidigdo (2018); Stroebel and Vavra (2019); Kaplan, Mitman and Violante (2020).

[2] See Head, Lloyd-Ellis and Sun (2014); Eeckhout, Pinheiro and Schmidheiny (2014); Ahlfeldt et al. (2015); Diamond (2016); Ganong and Shoag (2017); Restuccia and Rogerson (2017); Monte, Redding and Rossi-Hansberg (2018); Hornbeck and Moretti (2019); Hsieh and Moretti (2019).

prices. Hong Kong, Australia, Canada and New Zealand subsequently adopted their own barriers to foreign investment in local real estate. We therefore define our policy intervention date based on Singapore's adoption of a foreign buyer tax, as it ushered in a regime change in how many countries tax or restrict foreign ownership of domestic assets.

We use cross-sectional variation in predicted foreign investment destinations under the assumption that foreign capital, akin to foreign labor, is expected to flow to foreign born enclaves. This variation exploits the importance of "preferred habitat" in immigrant investment, as documented in Badarinza and Ramadorai (2018). Since the U.S. government does not track country of origin for real estate transactions, this variation builds on the immigration literature that finds differential likelihoods of immigrant destination based on the pre-existing mix of foreign born residents in a local market (Card, 2001). Using data from over 48 million housing transactions, we compare house price growth in neighborhoods with larger shares of foreign born residents to those less likely to attract foreign capital.

After three years of parallel growth, we find that house prices in immigrant enclaves grew 6–9% more after these tax policies were adopted than did other neighborhoods within the same city, while housing supply grew an additional 1% in areas with high immigrant shares. Given the housing and labor market recoveries in the U.S. concurrent with our sample period, we use a variety of methods to confirm that our results are driven by external capital flows rather than labor market conditions or gentrification. Additionally, as global sentiment towards the U.S. cooled during the recent trade war, we document a decline in both foreign capital flows and relative house prices. These findings provide new evidence on global demand shocks contributing to price volatility in inelastic markets (Gyourko, Mayer and Sinai, 2013).

In order to trace out new estimates of local housing supply curves, we first measure the elasticity of house prices and quantities with respect to foreign capital. However, we must overcome two key sources of potential endogeneity. First, a local demand shock such as labor market growth could vary the housing supply schedule as well the demand schedule by changing construction costs or political support for zoning regulation. To avoid concerns about local supply responses, we construct our elasticities using global variation in foreign capital inflows to shift the demand curve independently from the supply curve.

Second, expected housing returns may attract foreign capital, introducing bias from reverse causality into our elasticity estimates. Therefore, we use the tax policy shock to instrument for foreign capital inflows, the demand shifter. The instrument's validity requires that immigrant enclaves saw an increase in house prices and quantities after the tax policy adoption (relevance), as shown in the reduced form results. The approach also requires making the exclusion restriction assumption that the instrument be uncorrelated with changes in house prices except for its impact through foreign investment. We carefully examine whether more immigrant-concentrated neighborhoods were on a differential trend in house prices, which would therefore violate this identification assumption. Once we have identified the price and quantity elasticities with respect to foreign capital, the ratio of these two elasticities provides new estimates of the price elasticity of housing supply for each of the largest 100 cities in our sample.

Consistent with our reduced form findings, we show that house prices are much more elastic with respect to foreign capital inflows than are house quantities. Taking the ratio of these elasticities, we find price elasticities of supply that average 0.26 and vary between 0.06 and 0.9 for the largest 100 US cities in our sample. These estimates are about two and a half times smaller than the relationships in the raw data would suggest, confirming the importance of addressing our endogeneity concerns. Overall, our new measure shows that over the ten-year period from 2009-2018, local housing markets were highly inelastic and exhibited substantial spatial heterogeneity.

Our elasticities produce a metro ranking with coastal cities such as San Francisco as the least elastic metros, and relatively unconstrained or recovering cities like Grand Junction, CO and Baltimore, MD as the most elastic. These estimates are correlated with existing measures of supply elasticities, though they consistently exhibit more market tightness. We find that increasing measures of regulatory stringency or the share of unavailable land by one standard deviation decreases our estimated elasticities by 0.03 to 0.04, or 12-15% of the mean city's elasticity of supply. The correlations with our new elasticities confirm that local regulation and land availability are key determinants of housing supply.

Our work contributes to a growing literature on cross-country capital flows and their impact on asset markets such as housing. Li, Shen and Zhang (2019) find that a Chinese demand shock in three California cities between 2007 and 2013 raised house prices in areas exposed to more Chinese immigrants, with the largest impacts after 2012, in line with our post–period results. Agarwal, Chia and Sing (2020) document how offshore wealth drives up local house prices, Badarinza and Ramadorai (2018) examine inflows to the London housing market from countries experiencing political risk, and 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. An extensive literature has 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). While much of the literature identifies out-of-town purchases through name-matching or address differences on deeds, we instead draw on the immigration literature to connect novel aggregated data on foreign housing purchases to domestic neighborhoods, overcoming the lack of capital origin data in U.S. housing transactions.

By exploiting variation in pre-existing population shares, as in Card (2001), we expand the applicability of this strategy beyond the flow of migrants to the flow of capital, informing the literature on immigration's impact on local housing affordability. Many papers have examined the impact of immigrants on house prices directly, such as Saiz (2003, 2007), Saiz and Wachter (2011), Akbari and Aydede (2012), Sá (2014), Pavlov and Somerville (2016), and Badarinza and Ramadorai (2018). Pellegrino, Spolaore and Wacziarg (2021) highlight the importance of cultural distance in determining bilateral capital positions.

Additionally, our work documents an important consequence of capital regulation: Foreign buyer taxes in one country induce capital to flow to another. Hundtofte and Rantala (2018) find that regulating anonymity leads to large housing capital flight. Claessens (2014) provides an overview of many macroprudential policy tools and their relationship with housing markets. Within China, Deng et al. (2020) find that home purchase restrictions spill over into neighboring cities. While earlier work has linked international shocks to exposed domestic sectors, including real estate (e.g. Peek and Rosengren, 2000), we innovate by using recent non-U.S. macroprudential policies as a shock to U.S. housing markets.

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 local measures 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 the survey-based results of Gyourko, Hartley and Krimmel (2019) 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 (2021) use Bartik labor demand shocks and theory to construct census tract level house price elasticities. While a local labor market shock exploits intensive-margin variation in demand from wage or employment improvements, we instead exploit extensive-margin variation in demand originating from foreign countries. Both approaches provide new directions for estimating timely and more locally-relevant house price elasticities of supply.

In the next section, we describe our data. Section 3 introduces our reduced form research design and results. We present our instrumental variables design and results in Section 4. House price elasticity results and their context are discussed in section 5. Section 6 concludes.

2 Data

2.1 Treatment Definition

In order to measure exposure to foreign capital flowing into the U.S. housing market, we draw on the methods from Altonji and Card (1991); Card and DiNardo (2000) 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, we anticipate that foreign capital is most likely to flow to locations with *ex-ante* high shares of foreign born residents, immigrant enclaves, similar to the "preferred habitat" identification strategy in Badarinza and Ramadorai (2018). We rely on this approach because there is no buyer registry in the U.S. that tracks whether purchasers are foreign or domestic.

Foreign purchasers may seek to invest their capital in neighborhoods with initially high foreign born populations, and purchase real estate by employing an agent who has worked with foreign 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, 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. These areas are likely attractive to foreign buyers as they already have familiar language, other cultural infrastructure, and pre-established 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.

While our IV analysis uses a continuous measure of foreign born population share, for ease of visual inspection in event studies as well as checks for pre-trends, we begin by splitting our sample into discrete treatment and control groups. 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.[3] For our difference-in-differences analysis, we define as "treated" those zipcodes i whose foreign born immigrant share in 2011 is above the 95^{th} percentile, denoted as "foreign born" zipcodes, FB_i:

$$FB_i = 1\left\{\frac{FBpop_i}{pop_i} \geq 95^{th} percentile\right\}. \tag{1}$$

The treatment indicator equals 1 for those zipcodes with at least 29% foreign born residents, the 95^{th} percentile cutoff, with 1,004 FB=1 zipcodes and 19,078 FB=0 zipcodes. Nationally, the average zipcode in our sample is 7.4% foreign born, with the median zipcode being 3.5% foreign born. In contrast, the mean and median FB zipcodes had 38% and 36% foreign born shares, respectively.

For our instrumental variables approach used in estimating the price elasticity of supply, we employ a measure of the fraction of the local population born abroad:

[3] We use zipcodes as our preferred geography when possible, as they are small enough to provide considerable within labor market variation, while large enough to encapsulate a neighborhood and its characteristics. Supply data is available at the county level, requiring analysis at the larger geography.

$$fracFB_i = \frac{FBpop_i}{pop_i}. \tag{2}$$

Figure 1 shows the geographic distribution of our treatment variable, $FB_i = 1$. Panel (a) shows that treated zipcodes are clustered in many coastal cities such as New York City, Seattle, San Francisco, Los Angeles, Washington, D.C., and Boston. Note however that our treatment definition is not restricted to the coasts; large immigrant communities are also present in Omaha, Atlanta, Salt Lake City, and Minneapolis. Panel (b) shows the fraction of a county's population that is foreign born (used in our housing supply analysis). Counties shaded in red are treated and are distributed across 24 out of 48 states in our sample (we limit to the contiguous 48 states). We use the 95^{th} percentile for county cutoffs, yielding 117 treated and 2,243 control counties. Treated counties have at least 16% of their population foreign born, with the average treated county having 24% of its population born abroad. Across the entire sample, the median county has 3% born abroad, while the mean has 5%.

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, Canada and the United Kingdom have all introduced taxes in recent years.[4] 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.

The reported political motivations for these taxes have focused on the macroprudential stability of housing markets and affordability for domestic residents. Notably, the implementation of these taxes have predictably responded to an influx of foreign capital sharply driving up the cost of housing. Appendix Figure E1 shows the time series of price indices

[4] See Appendix A for details of these tax policies. In addition, New Zealand has recently banned non-resident foreigners from buying homes.

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. Singapore and Hong Kong experienced rising prices from 2010 to 2012, as shown in panels (a) and (b). Investment moved east to Australia, shown in panels (b) and (c), then further east to Canada, shown in panels (d) and (e).[5] Figure 2 summarizes the timing and location of the enactment of these taxes.

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

$$Post_t = 1\{t \geq 2011q4\} \tag{3}$$

We select 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. This date thus began the regime change in which global foreign capital increasingly landed in the U.S. housing market, as one of the final remaining untaxed markets with high immigrant shares from a variety of countries.

2.3 House Prices

We use CoreLogic's transactions database to construct quarterly 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 account for differences in 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 4, for each transaction j in zipcode i we control for lot size, living square footage, year built, number of bedrooms, number of bathrooms, and whether the house has a garage:

$$ln(Price^i_{jt}) = \beta^i_t qtr_t + \delta Acres^i_{jt} + \gamma Sqft^i_{jt} + Built^i_{jt} + Bed^i_{jt} + Bath^i_{jt} + Garage^i_{jt} + \eta^i_{jt} \tag{4}$$

[5] For direct evidence that these taxes deterred foreign investment, potentially pushing it to other markets, see Botsch and West (2020) on Vancouver's foreign homebuyer tax.

After constructing these indices for each zipcode, we focus our sample on the decade spanning 2009–2018.[6] This yields a zipcode-by-quarter panel of house price indices, $HPI_{it} = \beta_t^i$, for 19,830 zipcodes across 1,856 counties, covering 48.7 million transactions. Appendix Table D1 shows the housing characteristics for the zipcode-quarters in our data prior to 2012. We also use Zillow's Home Value Index (ZHVI) and Rent Index (ZRI) in our analysis to validate the robustness of our hedonic methodology, to examine the most recent time periods after 2018, and to study rental markets.

2.4 Housing Supply

To measure the supply of new housing, we use data from the Census' Building Permits Survey, 2009–2018, in conjunction with county-level housing stock data from the 2009 American Community Survey This data covers 632 of the roughly 3,120 counties in the continental United States. 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. We construct a time-varying measure of housing supply by summing up the flow in new housing units, anchored to the 2009 stock as in Equation 5:

$$Units_{it} = Stock_{i,2009} + \sum_{\tau=2009}^{t} Permits_{i,\tau} \tag{5}$$

2.5 Expected Capital Flows

As our measure of capital flows, we collect aggregate data on foreign sales volume from 2009–2019 from the National Association of Realtors' (NAR) "Annual Profiles of International Home Buyers" from 2011 to 2019. The 2019 survey was completed by about 12,000 realtors, 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 (Yun, Smith and Cororaton, 2011-2015). This pattern is likely due to language and cultural familiarity

[6]Data on expected capital flows (the demand shifter) and zip level data from the American Community Survey (used in robustness check) are not available prior to 2009. In the reduced form analysis, which does not utilize capital flow data, robustness checks extend back to 2005.

among a subset of realtors, as in Badarinza, Ramadorai and Shimizu (2019), 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) and Sakong (2021), 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.

Each report provides a national estimate for the sales volume purchased by international clients originating from Canada, China, India, Mexico, 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.[7] The NAR profiles do not distinguish between sales volume going to the two types of international clients; however, 40–50% of foreign buyers on average report residing primarily outside of the U.S. over our sample period (Yun, Ratiu and Cororaton, 2018-2019).

Figure 3 presents the time series of foreign home sales from the National Association of Realtors from 2009 to 2019. Foreign purchase volume nearly doubled between 2012 and 2017. The decline after 2017 is marked by two important developments which lowered interest in U.S. housing.[8] First, at the end of 2016, China tightened capital controls by requiring banks to report on large overseas transfers and limiting foreign property purchases.[9] Second, after 2017, relations between the U.S. and the rest of the world cooled as the Trump administration renegotiated major trade agreements such as the North American Free Trade Agreement (NAFTA) with Canada and Mexico. Many foreign governments introduced retaliatory tariffs, and Figure 3 suggests foreign citizens also reduced their purchase activity in the U.S. housing

[7] Motivated by the inability to distinguish capital flows by international client type, in unreported analysis, we find no differential immigration into treatment and control areas over our time period.

[8] While 2017 saw significant dollar depreciation against the Chinese Yuan, generally since 2014, the dollar has exhibited significant appreciation relative to the currencies in the countries specified in our NAR data. All else equal, a strengthening dollar would have been expected to reduce foreign demand for U.S. housing.

[9] Olsen, Kelly "Beijing's capital controls are weighing on Chinese investors looking to buy property abroad," CNBC, February 26, 2019.

market.[10]

Figure 4 shows the contribution of the top 5 international client groups to the overall international sales volume using NAR data from 2010 to 2019. The darkest bar, shown 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 "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 only a fraction of total foreign investment. Investment from Canada increased by approximately 50% between 2011 and 2017, and Mexican investment more than doubled. By 2019, both of these countries saw investment in U.S. housing decline to their lowest levels since the NAR reports began in 2009. We use this aggregate sales volume data to construct a metric of expected capital flows at the local level apportioned based on pre-existing foreign born population shares; the details are described in section 4.1 where we develop our instrument.

2.6 Additional Economic Data

For robustness checks, we collect a number of real economic variables to control for local economic characteristics. We use county level annual employment, establishment counts, and payroll data from the County Business Patterns, 2009–2018. We also include county level population and immigration data from the 2010 Decennial Census and the 2011-2018 American Community Survey. Finally, we collect zipcode level data on population and median income from the American Community Survey 2009-2018.

3 Reduced Form Analysis

Our reduced form approach examines whether changes in tax policy interacted with local immigrant shares impacts house prices and quantities, highlighting the relevance condition. For ease of inspection, we first compare treated zipcodes, those with high immigrant shares, to control zipcodes, those with lower shares, in a difference-in-differences framework. Next,

[10] See, e.g., "Timeline: Key dates in the U.S.–China trade war," Reuters, January 15, 2020.

we check to see that these patterns increase with immigrant concentration, motivating a binned dose response analysis. While the exclusion restriction is not directly testable, by showing that house prices and supply in immigrant enclaves respond differentially after foreign buyer tax policy adoption, we support the argument that foreign capital inflows move house prices and supply.

Our first specification in Equation 6 uses a generalized difference-in-differences design for zipcode i in quarter t:

$$ln(Y_{it}) = \alpha + \beta FB_i \times post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it} \tag{6}$$

where $Y_{it} \in \{HPI_{it}, Units_{it}\}$. The parameter of interest is β, which measures the percent change in the house price index (housing stock) in treated versus control zipcodes (counties) after the introduction of the first foreign buyer tax abroad. This design estimates an average treatment effect over a time period in which treatment intensity increased with adoption of more policies; β establishes the average impact of a tax policy regime change, not the impact of a single tax policy, on the U.S. housing market.

We also include zipcode (or county), ζ_i, and quarter, θ_t, fixed effects. In order to address concerns that our design is capturing broader local labor market trends instead of level differences in means, we additionally control for linear state-by-quarter, commuting zone-by-quarter, or CBSA-by-quarter trends, λ_{gt}, with trend geography denoted by g. When controlling for trend geography, we also limit the sample to include only states, commuting zones, or CBSAs that have at least one treated zipcode (or county). By controlling for geography-by-time trends, as well as year and geography fixed effects, we directly address labor market or investment sorting concerns to make comparisons exclusively within the same geography in the same quarter. 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. Panel (a) in Figure 5 and Appendix Table D1 support parallel trends in the pre-period for house prices.

3.1 Reduced Form Results: Prices and Quantities

Figure 5(a) presents the comparison between the house prices of high fraction foreign born (FB) zipcodes and all other zipcodes. The figure first shows smooth and parallel house price trends prior to the start of 2012, after which foreign capital flows 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.

Panel A of Table 1 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 5(b). Column (1) of the table includes both quarter and zip fixed effects, and each column adds progressively more restrictive, linear geography-by-time trends to flexibly account for different patterns in house prices in local markets. The estimated differences in house prices between treated and control zipcodes are consistently large and statistically significant, ranging from 6–9% higher in FB zipcodes, when allowing for local time trends.[11] Our preferred estimate is presented in column (4), where even after flexibly conditioning on commuting zone-specific time trends, we estimate that after 2012, house prices in high foreign born zipcodes were 6.7% higher on average than in control zipcodes in the same commuting zone.[12]

To assess whether these price impacts increase monotonically with expected attractiveness to foreign capital, we can replace the binary treatment indicator for a more continuous measure of exposure. Panel B in Table 1 shows the house price changes for zipcodes with foreign born 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. The results show that house prices rose monotonically with higher shares of foreign

[11] 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. This allows errors to be correlated across zipcodes and time within a state, CBSA, or Commuting Zone, respectively.

[12] Treated zipcodes have mean house prices of around $345,000 in the pre-period, and experience an additional $8.44 million in quarterly expected foreign capital inflows between 2009 and 2020. This would imply that foreign buyers purchased an average of 25 homes per zipcode per quarter, or 700 between 2012 and 2018. With an average population of 42,000 and assuming the U.S. average of 2.35 residents per housing unit, this implies 17,872 residential structures per zipcode. A back-of-the-envelope calculation then estimates that foreign purchasers bought about 4% of the existing stock (and substantially more of the flow) in these neighborhoods over a 7 year period, driving the price wedge.

born residents. In our preferred specification in column (4), we find that zipcodes in the 99^{th} percentile of foreign born share see house prices 13.5% 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 13.2% house price increase, the $90^{th} - 95^{th}$ percentiles an 11% increase, and $50^{th} - 90^{th}$ a 5% increase.

For the final price analysis, shown in Panel C of Table 1, we implement a continuous dose-response design, using fraction foreign born instead of the top 5 percentile or percentile bins. We use this continuous source of cross-sectional variation in the IV analysis in Section 4, so the dose-response can also be interpreted as our IV's reduced form. Using our preferred specification, column (4), moving from a zipcode with the median population foreign born (3.5%) to a zipcode with the 95^{th} percentile foreign born (29%) would increase house prices by $(0.29 - 0.35) \times 0.366 = 9.3\%$, in line with the results from the binned dose response analysis in Panel B. Taken together, these findings provide evidence of a differential house price response in areas most likely exposed to foreign capital flows.

Did this increase in house prices, induced by an influx of foreign capital, translate into real economic effects? In Figure 6 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 2.6. Panel (a) shows a level shift in the raw permitting rate among FB counties after the tax regime change in 2011. Panel (b) implements the same DiD event study from Figure 5 using the number of permits at the county level as the dependent variable. It shows that treated counties had similar permitting rates in the pre-period, while experiencing a differential increase of 500 permits per quarter on average from 2012 through 2018. For context, the average county in our sample prior to the tax changes had 222,000 housing units, and 231 new permits per quarter, for a raw annual permitting rate of about 0.4%. Our point estimates thus suggest a doubling of the (very low) permitting rate in the post-period in high-exposure counties.

In Panel A of Table 2 we study how the housing stock evolves at the county level, summing up permits over time and adding them to baseline stock in 2009 as in Equation 5. The table presents estimates from difference-in-differences specifications similar to those in Table 1, utilizing a county-quarter panel. The dependent variable is defined as the natural log of the stock of housing, $ln(Units_{it})$.

In column (4), our preferred specification that includes commuting zone-specific time trends, we estimate that high foreign born counties experienced an additional 1% increase in supply on average after 2012, which control counties in the same market did not experience.[13] This estimate provides new evidence that foreign capital flows have had a direct and local effect on real construction activity in the United States in those areas most likely to attract foreign investment.

In Panel B of Table 2 we examine whether this increase in supply is monotonically related to the immigrant share. In our preferred specification in column (4), we find that relative to below-median counties, those with higher foreign born shares experience 1.2%-1.8% higher housing supply after the policy change. Finally, we present dose-response results, the reduced form IV for supply, in Panel C of Table 2. In the preferred specification, moving from the median to the 95^{th} percentile county of foreign born share would imply an increase in supply of $(0.16 - 0.03) \times 0.054 = 0.7\%$. In sum, among locations with higher immigrant shares, those attractive to foreign capital, the demand shock resulted in a meaningful housing supply response.

Appendix B discusses two extensions of our reduced form analysis. First, we examine the implications for housing affordability in Appendix B.1. Foreign investment in U.S. housing pushes up not only house prices, but also spills over into the rental market. We also find that foreign capital has the largest price effects in relatively inexpensive neighborhoods (across all types of U.S. cities), potentially contributing to gentrification or affordability concerns. Second, we show a quasi-reversal in treatment in Appendix B.2. Concurrent with the rise of trade disputes under the Trump administration, Figure 3 shows that foreign investment in U.S. housing exhibits a sharp decline, suggesting foreigners found the U.S. to be a less hospitable market. We find that just one year later, immigrant enclaves lost about $1/3^{rd}$ of their relative house price gains achieved over the six prior years. This finding reaffirms the sensitivity of housing markets in immigrant enclaves to foreign capital flows.

Lastly, in Appendix Table D2, we present our dose-response results controlling for a

[13]The estimated R^2's approach 1 in this analysis as permitting variation is small relative to initial housing stock, especially after controlling for local time trends.

variety of alternative explanations, acknowledging the economic recovery after the Great Recession: that this is a sand-state recovery phenomenon, that these housing differences are due to growth in the tech sector happening differentially in locations with immigrant enclaves, or that this is purely a function of population growth in the biggest cities with immigrant enclaves. We also allow for city-specific trend breaks in the pre- and post-periods, which accommodate linear trends over both periods. Finally, we extend the sample back to 2005 to provide a balanced pre- and post-period panel, with 7 years pre and 7 years post.

These reduced form results hold across a variety of specifications, samples, and time periods, supporting the relevance criterion that this tax policy interacted with immigrant enclaves impacts housing markets above and beyond an alternative labor market explanation. Furthermore, the results show trend breaks in the event studies at the time of the tax policy change, even allowing for local labor market trends, supporting the exclusion restriction that immigrant shares matter for house price and quantity growth through foreign capital investment in U.S. housing markets.

4 IV Analysis: Prices and Quantities

In our setting, the series of foreign buyer tax policies adopted by other countries serve as an exogenous demand shifter into the U.S. housing market. We use this tax policy change interacted with the fraction of the zipcode that is foreign born to instrument for capital flows into the U.S. 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 construct a new measure of the price elasticity of supply for local U.S. housing markets.

Using global variation in home purchase capital flows has two primary advantages. First, it confirms that the mechanism through which immigrant share impacts U.S. housing markets is foreign investment, as opposed to other local investment such as FDI. Second, our measure re-weights investment based on a specific location's immigrant mix, not only its immigrant share, introducing additional variation in exposure to the tax instrument. On the other hand, if we estimate our elasticities ignoring the tax experiment, we would be concerned about reverse causality. For example, hot housing markets may attract foreign capital and vice versa, or local variation in demand could be correlated with local supply shifters, such as construction wages or local regulation.

The instrumental variable design relies on both a relevance condition and an 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, $E[ln(ECF_{it})(fracFB_i \times Post_t)] \neq 0$, which we present in our first stage results. The reduced form results for foreign capital presented in Section 3 show that the instrument has a positive correlation on the second stage outcome variable.

The exclusion restriction in our context is that the instrument must be uncorrelated with changes in house prices except for its impact through foreign investment. Our instrument relies on both temporal and cross-sectional sources of variation, $E[\epsilon_{it}(fracFB_i \times Post_t)] = 0$. Section 3 established that foreign born share did not differentially impact house prices or quantities prior to the tax regime change. The second component requires that foreign buyer tax policy changes only affect U.S. house prices by diverting capital into the housing market. If these taxes induced foreigners to invest in local businesses instead of housing, we could suffer a violation. While not directly testable, in Section 3, we control for this concern using geography-specific time trends, and also confirm a lack of trend break in labor market outcomes.[14]

4.1 IV for Expected Capital Flows

As the U.S. does not track country of origin for home purchases, we construct a novel measure of local expected capital flows (ECF_{it}) that "distributes" national home purchase capital flows ($capflow_{ct}$, in billions) from the NAR, presented in Section 2.5, to zipcodes based on pre-existing immigrant composition:

[14] Additionally, in Appendix C.1, we test whether investments in the tech industry violate the exclusion restriction, and find no support.

$$ECF_{it} = 1000 \times \sum_{c \in C} capflow_{ct} \times \frac{FBpop_{ic}^{2011}}{FBpop_{c}^{2011}} \tag{7}$$

where

$$1 = \sum_{i} \frac{FBpop_{ic}^{2011}}{FBpop_{c}^{2011}} \tag{8}$$

and $C = \{\text{Canada, China, India, Mexico, U.K., Other}\}$, 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.

This strategy exploits cross-sectional variation in immigrant shares, analogous to the earlier immigration literature as in Card and DiNardo (2000) as well as the recent "home-bias" literature spurred by Badarinza and Ramadorai (2018). It also incorporates time-series variation in capital flows, as in Sá (2016). The intuition is similar to that of a Bartik instrument, in which the local industry shares are the population shares, and the national industry growth rate is national foreign capital flows. By using differential exposure to a common shock, in our case the foreign–buyer tax policy change, identification relies on the initial population shares being exogenous to house price growth or quantity growth.[15] 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.

We find substantial variation in expected capital flows in the cross-section, as well a large increase in local capital flows over time based on this measure. Appendix Figure E2 shows the ECF_{it} distributions for 2009q1 and 2017q1, based on the pre–period composition of

[15] Goldsmith-Pinkham, Sorkin and Swift (2020) suggest testing this assumption by examining how much the initial shares are correlated with confounders in the pre–period. Our difference-in-differences empirics above directly address this concern.

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.[16] In 2009q1, the 99^{th} percentile zipcode in our sample received \$3.8m in ECF_{it}, rising to \$20m in 2017q1, as the entire distribution shifted to the right.

Our multinational IV design proceeds as follows:

$$ln(ECF_{it}) = \alpha + \beta fracFB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it} \tag{9}$$

$$ln(HPI_{it}) = \delta + \gamma^P ln(\widehat{EC}F_{it}) + \zeta_i + \theta_t + \lambda_{gt} + \epsilon_{it} \tag{10P}$$

$$ln(Units_{it}) = \delta + \gamma^Q ln(\widehat{EC}F_{it}) + \zeta_i + \theta_t + \lambda_{gt} + \epsilon_{it} \tag{10Q}$$

In the first stage, β measures the percent change in capital (in millions of dollars) per foreign born share 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. We index i to zipcodes for price analysis, and i to counties for price and quantity analysis. We continue to include zipcode or county fixed effects, ζ_i, quarter fixed effects, θ_t, as well as linear commuting-zone time trends, λ_{gt}.

4.2 IV Results: Price and Quantity Elasticities

Table 3 presents the results from the expected capital flows estimation strategy. We show 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 specific linear time trends, as in our preferred specification from the difference-in-differences analysis.

Panel (a) of Table 3 shows that ECF_{it}, the expected foreign capital flowing to a zipcode, is strongly associated with the interaction of the foreign born share of the population and an indicator for post-2012 time periods. This instrument yields a first-stage F-statistic of 219, even after the inclusion of zipcode and quarter fixed effects and commuting zone time trends. The median zipcode has a fraction foreign born of 3.5%, and the 95^{th} percentile is 29% foreign born. The estimated semi-elasticity of 0.97 reported in column (1) implies that

[16] Appendix Table D7 provides a numerical example of ECF_{it} construction.

moving between these two zipcodes would increase expected capital flows by 25%. Column (2) shows similar first-stage results with the panel of counties for the price outcomes. 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 0.98 with an F-statistic of 54.

Panel (b) of Table 3 reports estimates of the elasticity of zipcode house prices and quantities with respect to 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 0.37% when using a panel of zipcodes. This instrumented price increase represents the response to capital without a concurrent change in the supply schedule, showing prices are quite sensitive to foreign capital. Column (2) shows the equivalent result for the county panel; as counties are less substitutable than zipcodes for homebuyers, the point estimate rises from column (1) to column (2). Column (3) in Table 3 reports the comparable quantity elasticity; a 1% increase in expected capital flows to a county increase the stock of units by 0.04%, showing that quantities are much less responsive than prices. Taken together, these results imply that the U.S. housing market is highly inelastic over the span of roughly a decade.

We should note that this regression specification assumes a shock to expected capital inflows increases supply concurrently. Since we measure new supply by permitting activity each quarter, we would expect a more immediate supply response relative to a measure of actual unit construction or completion. However, we can analyze whether supply responds at longer horizons, as developers may need time to select projects, acquire land, or purchase units to renovate, among other activities that could delay the addition of new permits. In effect, we acknowledge that housing supply has a longer time horizon than many other products. We thus run the following specification:

$$ln(ECF_{it}) = \alpha + \beta fracFB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it} \tag{11}$$

$$ln(Units_{i,t+\tau}) = \delta + \gamma^Q ln(\widehat{ECF}_{it}) + \zeta_i + \theta_t + \lambda_{gt} + \epsilon_{it} \tag{12Q}$$

to examine the supply response τ quarters in the future relative to a current capital inflow shock. Figure 7 shows estimates of how supply responds to a change in current capital inflows over the subsequent 6 years. The intercept at 0 corresponds to the concurrent supply elasticity w.r.t. capital inflows, as reported in Table 3 to be 0.04. Moving over the future horizon, we see that supply responds the most by 3.5 years from the current capital shock; this means that developers today incorporate the capital inflow shock and it takes between 0 and 3.5 years for new supply to show up. After about 5 years, there is no differential supply response, putting an upper bound on how forward-looking developers are, and providing guidance to the time it takes to bring new supply online after a demand shock.

While we identify γ^P and γ^Q from changes in local capital inflows, if changes in capital flows are small, we could be identifying off of immigrant composition instead. To check that this is not the case, and we do have enough variation in local flows, we can control for the initial immigrant composition in each city, as shown in Appendix Table D8, column (2). 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 population and income in the IV regressions in Appendix Table D8, column (3). Moreover, if immigrant neighborhoods are also lower income, they may exhibit more house price volatility as in Hartman-Glaser and Mann (2021). These time-varying controls further proxy for changes in the quality and composition of neighborhood-level local amenities. Controlling for population and income does not impact the baseline results in Table 3, whose results are recorded in column (1) of the appendix table.[17] Given that we also include geography-specific fixed effects, based on these specifications we can be confident that zipcode-level income growth within a metro area is not driving our results.

We also explore whether these results are robust to alternative approaches of constructing ECF_{it}. Our baseline approach uses variation in the foreign born population in a U.S. zipcode, regardless of source country. However, as Li, Shen and Zhang (2019) document notable Chinese investment over our period in California, and Figure 4 shows a stark increase in Chinese investment in the U.S. housing market, we check whether these elasticity results are

[17]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.

driven solely by Chinese investment.[18] Appendix Table D8 column (4) partitions the first stage fraction foreign born into the fraction of foreign born residents originating in China, and those originating in any other country. Column (5) excludes Chinese capital flows and immigrants entirely from the analysis. In both cases, we recover similar estimates to the baseline.

Finally, in Appendix Section C.2, we weight the ECF_{it} by the fraction foreign born in the zipcode, analogous to the exposure treatment measure in Abramitsky et al. (2019) on the impact of immigration quotas on local economies. This alternative weighting scheme considers the overall number of people in a zipcode, as a zipcode 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 0.88, which is not statistically different from the baseline price elasticity (see Appendix Table D8, column (6)), and a quantity elasticity of 0.03, 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 of 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 to capital flows than the supply of new housing units.

5 Estimating Local House Price Elasticities of Supply

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

[18] Home purchase restrictions began in Beijing in 2010, limiting the number of homes a given household could pruchase. This later spread to more cities, and by 2016 limits on home-ownership were expanded to require higher downpayments Sun et al. (2017).

price elasticity of supply, η:

$$\frac{\frac{\partial ln(Q)}{\partial ln(f)}}{\frac{\partial ln(P)}{\partial ln(f)}} = \frac{\partial ln(Q)}{\partial ln(P)} = \eta \tag{13}$$

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 measuring 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 plausibly exogenous variation in housing demand to estimate the slope of the supply curve. While Baum-Snow and Han (2021) 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. As such, it leverages variation in demand plausibly unrelated to local housing authorities' regulatory decision-making or local construction costs correlated with employment changes.

To obtain local house price elasticities η^M for each CBSA M, we modify the instrumental variables strategy discussed in Section 4. First, we use the county as the unit of observation, as this is the granularity available for building permits, our measure of $\partial ln(Q_{ct})$. An additional benefit of studying counties is that we would expect spillovers at smaller geographies such as the zipcode, where neighborhoods are more substitutable. 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:[19]

$$ln(HPI_{ct}) = \gamma^P ln(\widehat{EC}F_t^c) + \gamma_M^P ln(\widehat{EC}F_t^c) \times CBSA_c + \eta^{ct} \tag{2PM}$$

$$ln(Units_{ct}) = \gamma^Q ln(\widehat{EC}F_t^c) + \gamma_M^Q ln(\widehat{EC}F_t^c) \times CBSA_c + \nu^{ct} \tag{2QM}$$

[19]For ease of exposition, we omit the first stage here; however, we also instrument for $ln(ECF_t^c) \times CBSA_c$ with $fracFB_c \times Post_t \times CBSA$.

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^k_M for the CBSA–specific additional elasticity.[20] To recover the distribution of price elasticities of supply, for each CBSA we then calculate

$$\eta^M = \frac{\gamma^Q + \gamma^Q_M}{\gamma^P + \gamma^P_M} \tag{14}$$

with η^M providing the CBSA–specific house price elasticity of supply. We construct η^M for the largest 100 CBSAs by population in 2010 available in our Building Permits Survey data. This sample covers counties which include just under 60% of the total U.S. population in 2011. All 100 cities had exposure to foreign buyers; on average, these cities were 16.4% foreign born.

5.1 Estimated Elasticities

The map in Figure 8 shows the geographic distribution of the elasticities, dividing the 92 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.[21] The middle of the country remains relatively more elastic, though large areas of the Mid–Atlantic are also elastically supplied over this period.

Table 4 provides a list of the most elastic and most inelastic cities based on our approach; the full table of elasticities by CBSA is provided in Appendix Table D9. The most inelastic cities in our sample have price elasticities of supply of about 0.06, while the most elastic have an elasticity closer to 0.9.[22]

[20]For CBSA's with more than one county, we estimate a sales-weighted house price elasticity of supply.

[21]Using an entirely different methodology, Aastveit, Albuquerque and Anundsen (2019) also find that Minneapolis is highly inelastic, so much so that in 2019 Minneapolis passed the "Minneapolis 2040 comprehensive plan" intending to abolish single family zoning. See Trickey, Erick, "How Minneapolis Freed Itself From the Stranglehold of Single-Family Homes." *Politico*, July 11, 2019.

[22]Based on our methodology, eight CBSAs have negative elasticity estimates: Allentown, PA, Salisbury, MD, Columbus GA, Daytona Beach, FL, Albany, GA, Vineland, NJ, Tallahassee, FL, and Atlantic City, NJ. These CBSAs 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. We also find two CBSAs with sufficiently higher elasticities to be considered outliers: Virginia Beach, VA and Trenton, NJ.

Over a ten-year period, the U.S. housing market appears to be highly inelastically supplied, with the bulk of short-run elasticities falling below 0.5.[23] 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.

5.2 Addressing Endogeneity Through IV: Estimates Compared

We motivated our identification strategy with two factors that would bias downwards our estimated supply slopes (the slope is the inverse house price elasticity of supply, or $\frac{1}{\eta}$) if we used OLS, as illustrated in Figure 9(a). First, labor market or recovery conditions may have shifted both supply and demand curves for housing (simultaneity bias). Second, foreign capital may be attracted to high house prices (reverse causality). We can use global variation in expected capital flows to isolate a foreign demand shock independent of the local supply schedule as illustrated in Figure 9(b). To mitigate reverse causality concerns, we instrument for foreign capital with the tax policy change interacted with foreign born shares.

As a test that our instrument works as intended, namely to isolate a demand shifter along a fixed supply curve, we compare the predicted house price changes against the changes observed in the raw data for our sample of cities. If the instrument has mitigated the simultaneity problem, the slope for predicted changes should be steeper than for the raw data. Panels (c) and (d) in Figure 9 plot the raw and predicted price and quantity changes from the data, between 2011q4 and 2018q4. As expected, we observe that the slope for the predicted values (panel (d)) is much steeper than the slope for the raw change (panel (c)).

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;

[23] Motivated by Figure 7, we also construct elasticities looking up to 3 years in the future for supply. The distribution shifts marginally to the right as we would expect since longer term elasticities should be flatter than shorter run ones; notably the mean city remains highly inelastically supplied. The average price elasticity of supply grows from 0.25 to 0.37 (keeping a consistent sample of 86 cities with reasonable elasticities), but the distributions are not statistically significantly different.

however, if we do not hold the supply of housing fixed, and fail to isolate a 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 mitigates 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 is lower than that for the raw equilibria, $\overline{\eta}^M_{predicted} = 0.44 < 1.07 = \overline{\eta}^M_{raw}$, highlighting the need for an instrument to isolate the demand shock. Without the instrument, one would erroneously conclude that U.S. housing markets are nearly 2.5 times more elastic than we find.

Finally, we note that a key component in the supply of housing is developers' expectations around future demand. With a short-run shock to demand, we might expect developers to move along the demand schedule. However, as more countries impose foreign buyer taxes, a permanent change in expectations could shift the supply curve out by raising developers' expected returns. Thus, while our instrumental variables method removes significant downward bias in the slope over the course of a decade, it may not account for adjustments to the supply curve when estimated over longer horizons, suggesting housing markets may be even tighter than estimated here.

5.3 Estimates in Context

To further assess the plausibility of our methodology, we compare our estimated elasticities against three other existing measures of supply: the house price elasticities of supply estimated by Saiz (2010), those estimated by Baum-Snow and Han (2021), and the Wharton Residential Land Use Regulatory Index (WRLURI). To ensure consistency of comparison, we restrict our sample to the 31 CBSAs with data in all four datasets.

The Saiz elasticities are constructed using data on buildable land and the WRLURI filtered through a model of housing price evolution, using data on CBSAs from 1970–2000. The estimated elasticities average 1.75, with major metropolitan areas having elasticities below 1. Figure 10(a) shows that our elasticities are strongly correlated with the Saiz elasticities, having a correlation coefficient of 0.48. While highly correlated, note that our elasticities vary between 0.06 and 0.34, while the Saiz elasticities range between 1 and 4. We posit two reasons for the high correlation combined with the level shift downward in magnitude.

First, the supply of housing takes time to evolve, which explains the magnitude of difference between our elasticities and those in Saiz (2010); we estimate changes in supply over 10 years instead of 30. Given that housing is highly durable and expensive to construct, developers may take a few years to ramp up their supply pipelines in response to a price shock. Indeed, within our time period, we find that the average house price elasticity of supply grows from 0.17 in 2015 to over 0.26 by the end of 2018.

Second, the U.S. housing market has become increasingly more regulated over time, as noted in Gyourko, Hartley and Krimmel (2019), who study changes in regulation between 2008 and 2018. Taking California as an example, Krimmel (2021) documents that only 5% of CA jurisdictions had supply restrictions in 1964, growing to 24% in 1980. Given that the Saiz elasticities were estimated over the least-regulated period in modern U.S. history, as well as during the rise of suburbanization driven in part by the expansion of highways (Baum-Snow, 2007), we would expect earlier magnitudes to be considerably larger than ours, estimated in the most-regulated environment.

Baum-Snow and Han (2021) construct elasticities for ≈50k census tracts using variation in local labor demand shocks between 2000 and 2010, a period that covers both a rapid supply expansion and construction collapse. To compare their tract-level estimates to our CBSA-level ones, we take the average census tract unit elasticity ($gamma01b_TYPE_FMM$), and plot it against our own estimates, as shown in Figure 10(b). Despite using global, rather than local, housing demand shocks and covering different decades, our elasticities are highly correlated, though on average 10% smaller than the Baum-Snow and Han estimates. In contrast to prior estimates in the literature, our timeline covers an era of notable housing supply constraints and subsequent lack of affordability. Both methodologies, over recent and shorter time periods, produce elasticities an order of magnitude smaller than those of Saiz (2010).

Figure 10 also plots our elasticities against the $WRLURI^{08}$ and $WRLURI^{18}$ in panels (c) and (d). A higher WRLURI value implies that the location is more tightly regulated when it comes to building new housing stock (Gyourko and Summers, 2008; Gyourko, Hartley and Krimmel, 2019). As expected, Figure 10 shows that our elasticities are negatively correlated with both the 2008 and 2018 WRLURI indices, implying that more tightly regulated housing markets have lower estimated elasticities of housing supply.

Table 5 shows the univariate relationships between our elasticities, the Saiz elasticities, components of the Saiz elasticities, Baum-Snow & Han averaged elasticities, $WRLURI^{08}$, $FlatShare$, $UnavailableLand$, as well as the updated $WRLURI^{18}$ and a meaure of population density. We regress our elasticity on the variable in the first column to test which are statistically related, and find that the Saiz elasticities, Baum-Snow & Han elasticities, $WRLURI^{08}$, and geographic variables all have statistically significant relationships. We do not interpret these coefficients as causal, but they may guide researchers interested in the determinants governing housing supply. For example, geography appears to have more promise as a fundamental input than does population density, which has no statistical relationship with our elasticities.

These correlations with independent sources of market tightness support the assumptions underlying our estimation strategy: if our approach were contaminated by simultaneous correlated shocks (e.g. gentrification, housing market recovery), then it is unlikely our estimates would have a meaningful relationship with local regulatory restrictions or prior estimates based on entirely different sources of variation, namely regulation and geography.

5.4 Applications for Supply Elasticities

House price elasticities of supply, beyond providing a measure of the nature of urban development, are also commonly used to provide variation in housing wealth or house price growth. Some examples of research exploiting variation in housing supply elasticities include the role of housing equity in entrepreneurship, firms' financing decisions, college attainment, credit supply, household consumption, non-tradable employment, and retail price growth (Chaney, Sraer and Thesmar, 2012; Mian, Rao and Sufi, 2013; Mian and Sufi, 2014; Adelino, Schoar and Severino, 2015; Favara and Imbs, 2015; Charles, Hurst and Notowidigdo, 2018; Stroebel and Vavra, 2019). Comparing the dispersion of the Saiz elasticity to ours, we find similar coefficients of variation, but with estimates an order of magnitude smaller, suggesting less available variation in the most recent context.

Supply elasticities can also be used to categorize locations and compare conditions in elastic vs. inelastic markets (Gyourko, Mayer and Sinai, 2013; Robb and Robinson, 2014). Importantly, the distribution of which cities are most and least elastic has changed over time; while coastal markets have historically been constrained by both geography and regulation, Gyourko, Hartley and Krimmel (2019) find that many cities in the center of the country are becoming increasingly regulated. Thus, using an outdated classification of elastic vs. inelastic cities may bias downwards any hypothesis that relies on their recent (i.e. post-2010s) differences in trajectory.

Finally, different locations can accommodate more or fewer residents, making one's entry cost to a city, county, or zipcode a function of the housing supply elasticity. The relative cost of entry into a location helps explain migration patterns, commuting trends, divergence in skill patterns across cities, and even the misallocation of labor to less productive locations (Head, Lloyd-Ellis and Sun, 2014; Eeckhout, Pinheiro and Schmidheiny, 2014; Ahlfeldt et al., 2015; Diamond, 2016; Ganong and Shoag, 2017; Restuccia and Rogerson, 2017; Monte, Redding and Rossi-Hansberg, 2018; Hornbeck and Moretti, 2019; Hsieh and Moretti, 2019). Therefore, house price elasticities, or a more fundamental land price elasticity (that allows for inelastic land supply combined with potentially elastic land use intensity), are often a key component of general equilibrium models. By providing updated measures of housing supply elasticities, we hope to contribute to the wide variety of applications in which these elasticities play a central role.

6 Conclusion

Fluid international capital flows have the potential to rapidly inflate the value of assets, especially illiquid ones. 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 and exacerbate affordability concerns. In this paper, we first document the effect of international capital on the U.S. housing market, emphasizing that a series of foreign-buyer taxes in other countries may have made American cities more attractive investments. Using a difference-in-differences design and data on over 48 million housing transactions, we estimate that house prices rose 6–9% more in zipcodes with a larger share of foreign born residents prior to the capital shock, and subsequently fell following the chilling of U.S.-global relations, exposing these markets to significant price volatility.

Estimating the housing market's sensitivity to global capital, we find that a 1% increase in instrumented foreign capital raises house prices at the zipcode level by 0.37%, and housing supply at the county level by 0.04%. We then use this demand shock to provide new estimates of the price elasticity of housing supply. We find that U.S. housing markets seem relatively inelastic in the short run and exhibit substantial heterogeneity correlated with existing measures of supply constraints, such as zoning and land use rules.

Our findings have two primary implications. First, 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, and those nearby, are less accessible to existing U.S. residents as prices and rents 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.

Second, 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. If foreign demand remains persistently high, we would expect price growth to abate only with more housing supply. These low elasticities are substantially smaller than those found using supply data from earlier periods, emphasizing the importance of context when applying supply elasticities to models of urban development.

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 towards being overly elastic. 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 occupied.

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 Covid-19 crisis has prevented investors abroad from touring U.S. housing opportunities, so an open question is to what degree foreign capital will return. Furthermore, if the current foreign investment in the U.S. market relocates elsewhere, 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.

Finally, as housing costs play a major role in where people choose to locate, a broad range of economic disciplines incorporate versions of housing supply measures into their research. With housing making up a significant part of the household balance sheet, and the importance of housing equity and collateral, changes in housing wealth can spill over into consumption choices, small business formation, and household credit decisions. Given the variety of contexts, time periods, and applications in which housing supply parameters are used, we emphasize the importance of working towards a wide variety of context-appropriate housing supply estimates.

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Tables

Table 1: IV Reduced Form Results, Prices

	(1) ln(HPI)	(2) ln(HPI)	(3) ln(HPI)	(4) ln(HPI)
Panel A: Difference-in-Differences				
Post = 1 X FB	0.121***	0.0923***	0.0613***	0.0671***
	(0.0167)	(0.0244)	(0.0169)	(0.0181)
R^2	0.864	0.876	0.872	0.872
Observations	462678	462678	223576	240240
Panel B: Binned Dose Response				
Post = 1 X 50th-90th ptile	0.105***	0.0594***	0.0510***	0.0532***
	(0.0151)	(0.0127)	(0.0126)	(0.0136)
Post = 1 X 90th-95th ptile	0.194***	0.125***	0.0992***	0.109***
	(0.0179)	(0.0183)	(0.0153)	(0.0158)
Post = 1 X 95th-99th ptile	0.217***	0.157***	0.123***	0.132***
	(0.0230)	(0.0298)	(0.0227)	(0.0230)
Post = 1 X Above 99th ptile	0.227***	0.168***	0.131***	0.135***
	(0.0251)	(0.0364)	(0.0390)	(0.0255)
R^2	0.866	0.876	0.873	0.873
Observations	462678	462678	223576	240240
Panel C: IV Reduced Form (Continuous Dose Response)				
Post = 1 X Fraction Foreign Born	0.619***	0.465***	0.336***	0.366***
	(0.0595)	(0.0936)	(0.0688)	(0.0722)
R^2	0.866	0.877	0.873	0.873
Observations	462678	462678	223576	240240
	Fixed Effects and Trends			
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
CBSA X Quarter			X	
Zone X Quarter				X

Notes: Panel A shows the coefficient β from $ln(HPI_{it}) = \alpha + \beta FB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. Panel B shows the coefficients β_k from $ln(HPI_{it}) = \alpha + \sum_k \beta_k FBbin_{ik} \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. Panel C shows the coefficients β from $ln(HPI_{it}) = \alpha + \beta FracFB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. All data at the zipcode by quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Analysis spans 2009-2018. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 2: IV Reduced Form Results, Quantity

	(1) ln(Units)	(2) ln(Units)	(3) ln(Units)	(4) ln(Units)
Panel A: Difference-in-Differences				
Post = 1 X FB = 1	0.00330	0.0117***	0.0107*	0.00957*
	(0.00479)	(0.00351)	(0.00577)	(0.00481)
R^2	1.000	1.000	1.000	1.000
Observations	13799	13799	5468	5031
Panel B: Binned Dose Response				
Post = 1 X 50th-90th ptile	0.0166***	0.0118***	0.0172***	0.0116**
	(0.00257)	(0.00310)	(0.00542)	(0.00492)
Post = 1 X 90th-95th ptile	0.0213***	0.0167***	0.0254**	0.0161
	(0.00548)	(0.00491)	(0.0120)	(0.00992)
Post = 1 X 95th-99th ptile	0.0128**	0.0183***	0.0213***	0.0152**
	(0.00547)	(0.00425)	(0.00696)	(0.00605)
Post = 1 X Above 99th ptile	0.00443	0.0122*	0.0224**	0.0182**
	(0.00343)	(0.00616)	(0.0101)	(0.00843)
R^2	1.000	1.000	1.000	1.000
Observations	13799	13799	5468	5031
Panel C: IV Reduced Form (Continuous Dose Response)				
Post=1 X Fraction Foreign Born	0.0286	0.0623***	0.0619**	0.0541**
	(0.0176)	(0.0143)	(0.0271)	(0.0203)
R^2	1.000	1.000	1.000	1.000
Observations	13799	13799	5468	5031
	Fixed Effects and Trends			
Quarter	X	X	X	X
County	X	X	X	X
State X Quarter		X		
CBSA X Quarter			X	
Zone X Quarter				X

Notes: Panel A shows the coefficient β from $ln(Units_{it}) = \alpha + \beta FB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. Panel B shows the coefficient β_k from $ln(Units_{it}) = \alpha + \sum_k \beta_k FBbin_{ik} \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. Panel C shows the coefficients β from $ln(Units_{it}) = \alpha + \beta FracFB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. All data at the county by quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Analysis spans 2009-2018. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Expected Capital Flow IV

	(1) ln(ECF$_{it}$)	(2) ln(ECF$_{it}$)	(3) ln(ECF$_{it}$)
Post X Frac. FB	0.974***	0.933***	0.979***
	(0.0658)	(0.0671)	(0.133)
R^2	0.988	0.998	0.997
F	218.8	193.4	54.34
Observations	628694	20564	20564
		Fixed Effects	
Zip	X		
Quarter	X	X	X
County		X	X

(a) First Stage

	(1) ln(HPI)	(2) ln(HPI)	(3) ln(Units)
ln(ECF$_{it}$)	0.369***	0.534***	0.0403***
	(0.0636)	(0.0845)	(0.0138)
Root MSE	0.183	0.112	0.0135
Observations	628694	20564	20564
		Fixed Effects	
Zip	X		
Quarter	X	X	X
County		X	X

(b) Second Stage

Notes: This table shows the first stage results from $ln(ECF_{it}) = \alpha + \beta fracFB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. All analysis includes linear commuting-zone time trends. Standard errors in parentheses, clustered by commuting zone. Analysis spans 2009-2018. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Most Inelastic and Elastic CBSA's

Top 5 Most Inelastic	
San Francisco-Oakland-Hayward, CA	0.06
Minneapolis-St. Paul-Bloomington, MN-WI	0.07
Riverside-San Bernardino-Ontario, CA	0.07
Miami-Fort Lauderdale-West Palm Beach, FL	0.07
Los Angeles-Long Beach-Anaheim, CA	0.07
Top 5 Most Elastic	
Wilmington, NC	0.621
McAllen-Edinburg-Mission, TX	0.650
Roanoke, VA	0.679
Grand Junction, CO	0.728
Baltimore-Columbia-Towson, MD	0.902

Notes: This table shows 10 of the 100 estimated price elasticities of supply, for the most inelastic and elastic CBSA's in the country, estimated between 2009-2018.

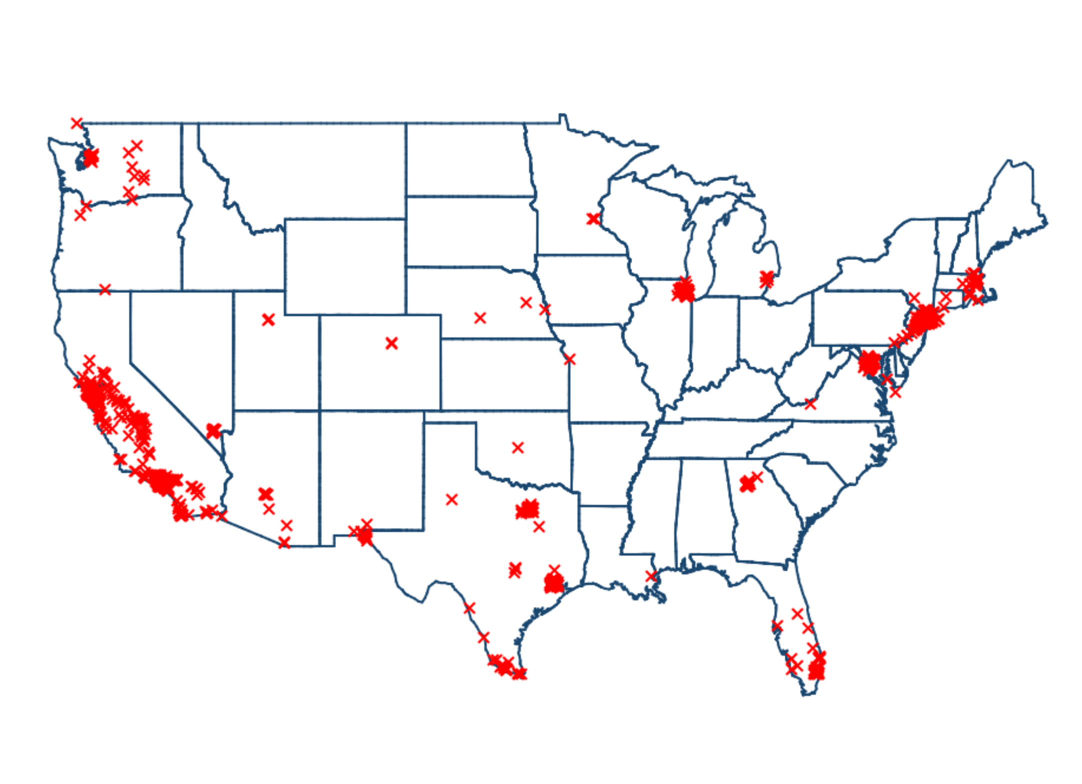
Table 5: Elasticities: Univariate Correlations with Other Measures

	Elasticity	St. Error	Correlation	Indepvar Mean	Indepvar Std. Dev.
$Saiz$	0.047***	0.016	0.48	1.77	0.89
$Mean(B-S\&H)$	0.60**	0.23	0.44	0.20	0.06
$WRLURI'^{08}$	-0.050*	0.025	-0.34	0.19	0.59
$WRLURI'^{18}$	-0.034	0.035	-0.18	0.23	0.45
$FlatShare$	0.201*	0.114	0.31	0.91	0.13
$UnavailableLand$	-0.235***	0.07	-0.52	0.23	0.19
$Ln(PopDensity)$	-0.021	0.021	-0.18	-0.50	0.74

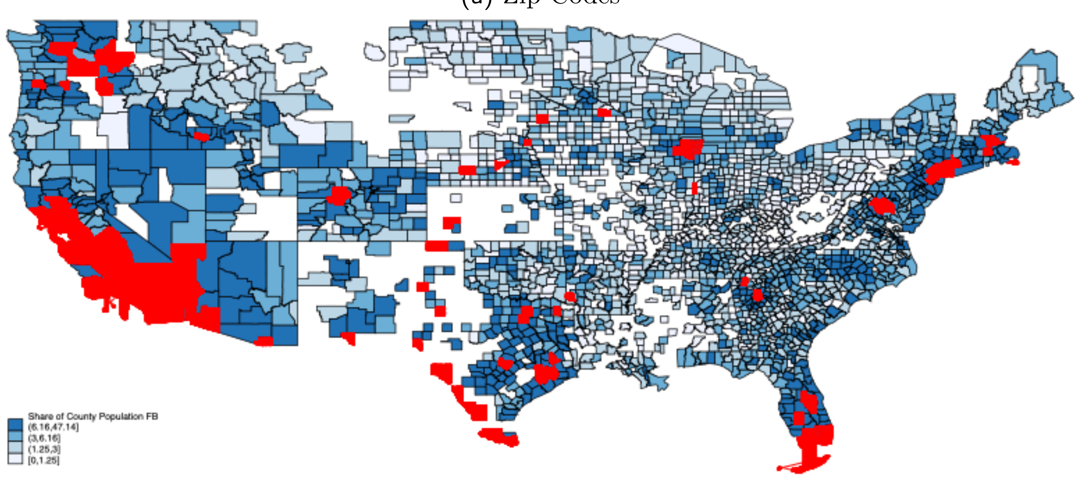
Notes: $Saiz$ elasticities, $FlatShare$ and $UnavailableLand$ from Saiz (2010). $Mean(B-S\&H)$ averaged tract elasticities within CBSAs from Baum-Snow and Han (2021). $WRLURI'^{08}$ and $WRLURI'^{18}$ from Gyourko and Summers (2008) and Gyourko, Hartley and Krimmel (2019). $Ln(PopDensity)$ from US Census. CBSA's limited to those with full data across all datasets, leaving us with 32 CBSA's.

Figures

Figure 1: Geographic Distribution of Treated Zips and Counties



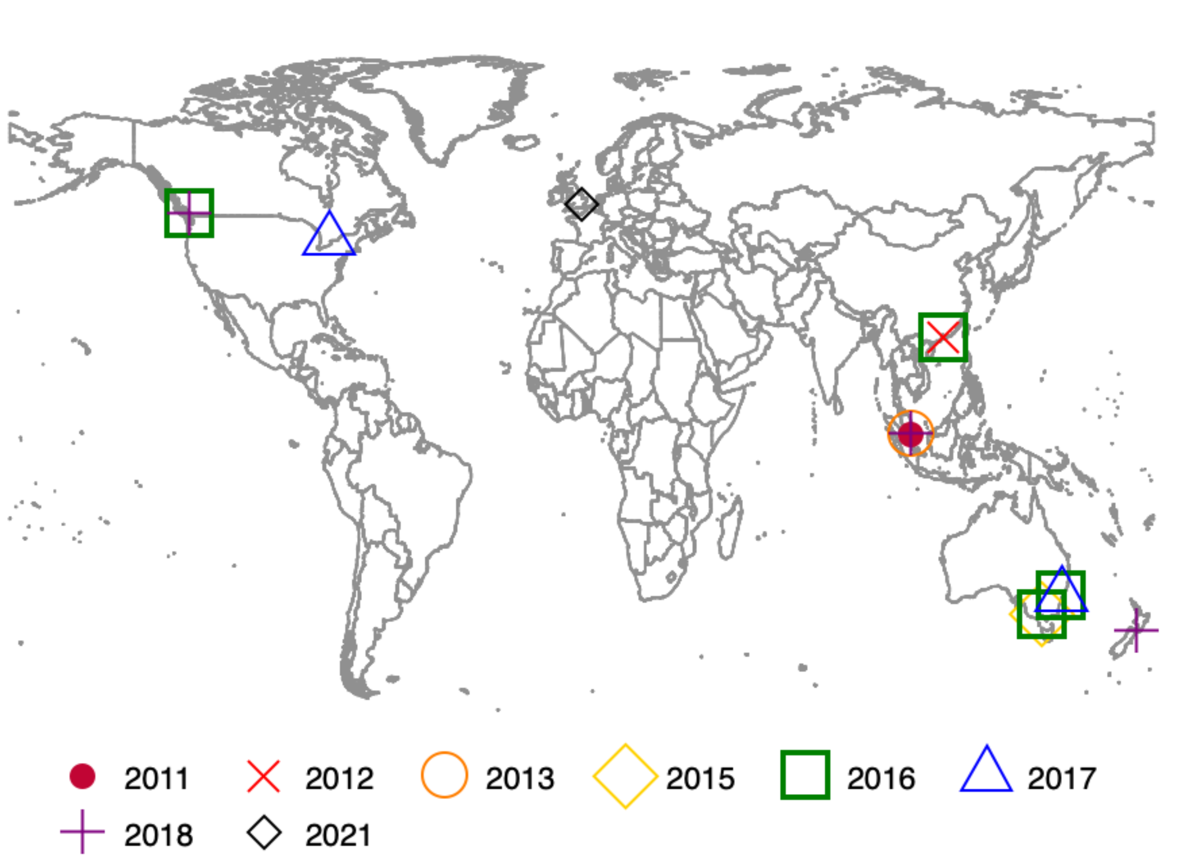
(a) Zip Codes



(b) Counties

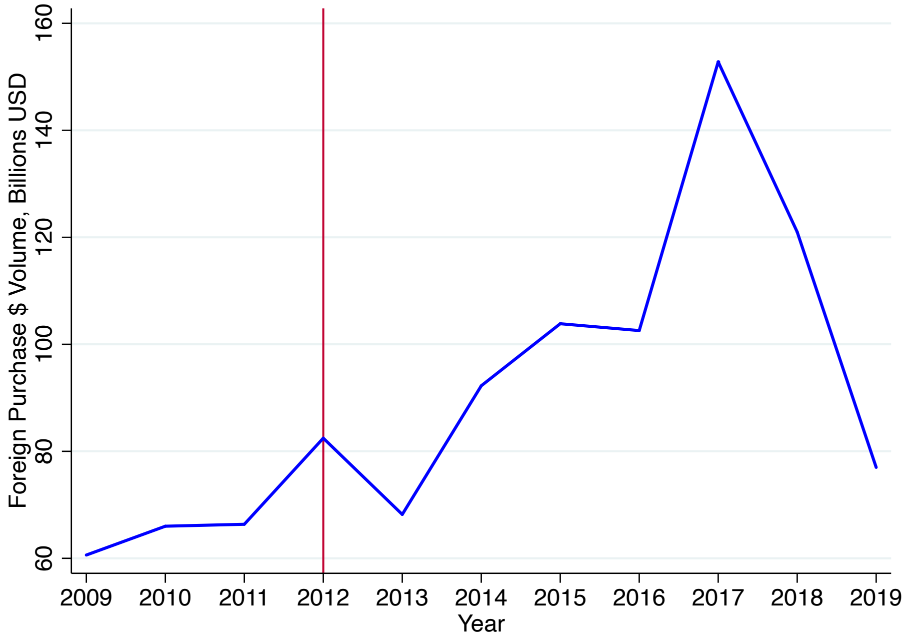
Note: Panel (a) plots the FB=1 zipcodes. Panel (b) plots the fraction foreign born by county, breakpoints correspond to the 50^{th}, 75^{th} and 95^{th} percentiles. Treated counties shaded in red.

Figure 2: Map of Tax Policy Changes



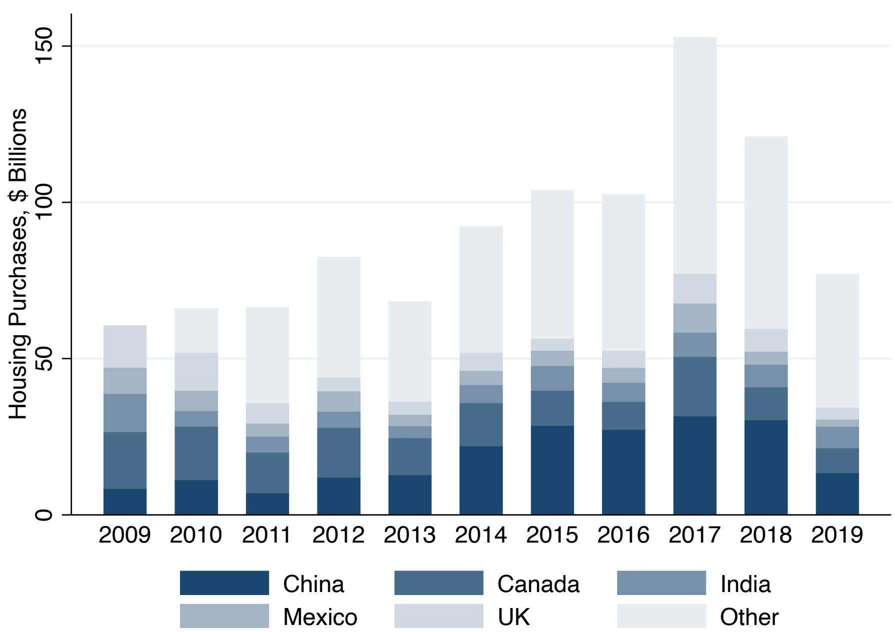
Notes: Singapore: 10% in **2011m12**, 15% in 2013m1, 20% in 2018m7; Australia: 3 % in 2015m6 (VIC), 4% in 2016m6 (NSW), 7% in 2016m7 (VIC), 8% in 2017m7; Canada: 15% in 2016m8 (BC), 15% in 2017m4 (ON), 20% in 2018m2; New Zealand: banned all non-resident foreigners from purchasing existing SFHs, may still 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 3: International Capital in the U.S. Housing Market



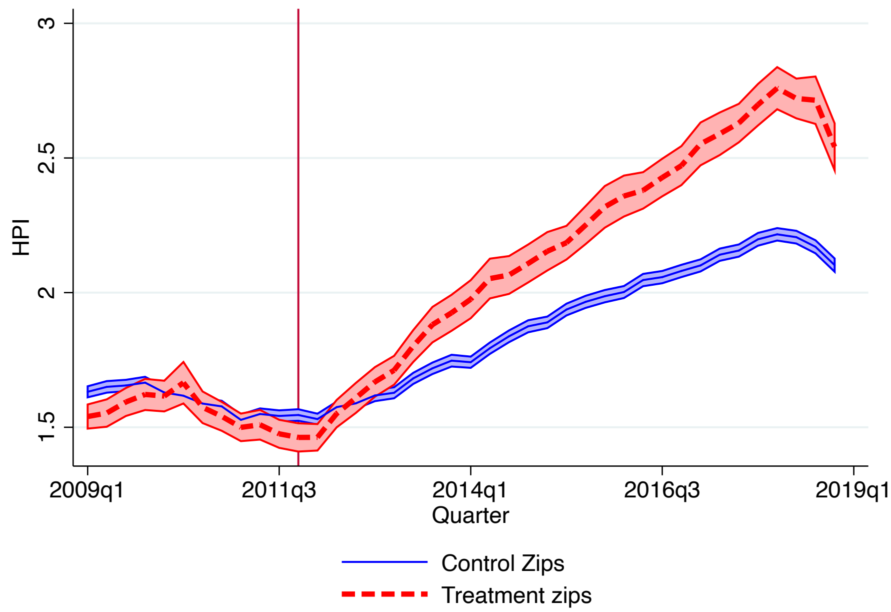
Source: Transaction volume from annual editions of the National Association of Realtors' (NAR) "Profile of International Activity in U.S. Residential Real Estate."

Figure 4: Expected Capital Flow Index Inputs

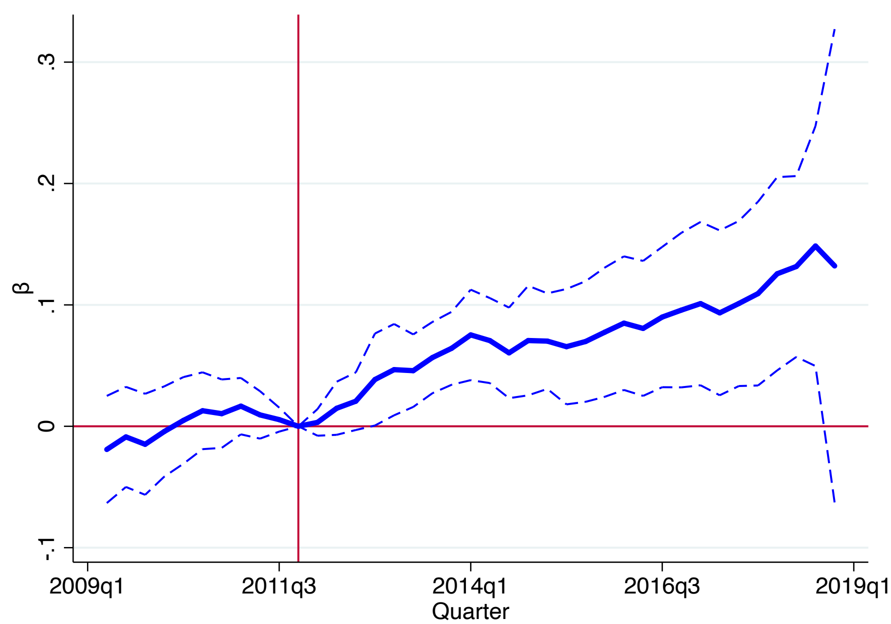


Source: Transaction volume by country from NAR's annual "Profile of International Activity in U.S. Residential Real Estate."

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



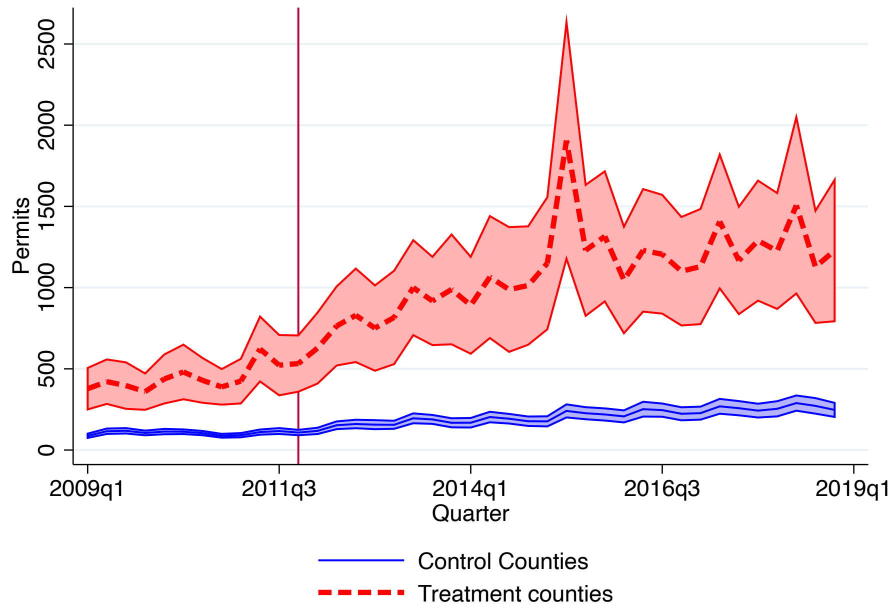
(a) Raw Data



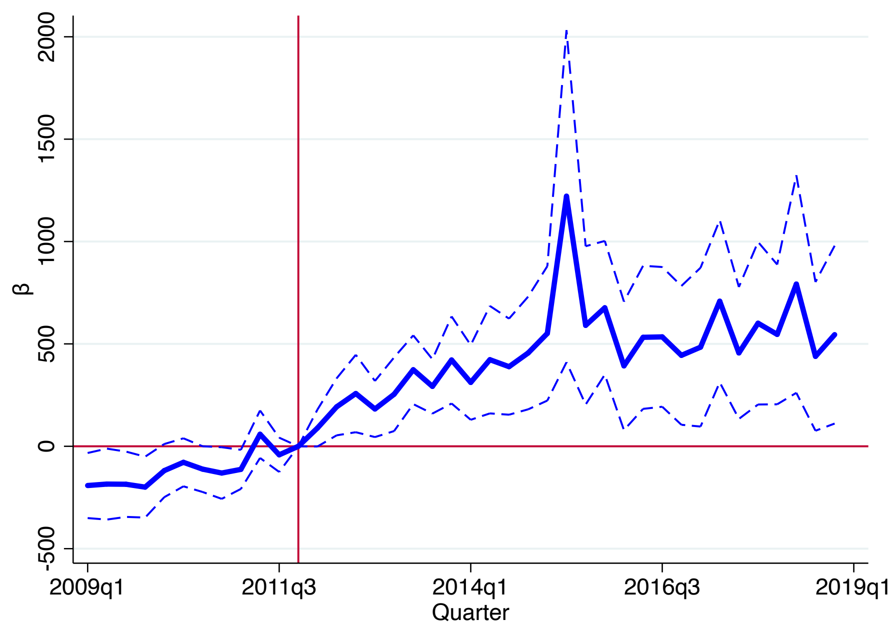
(b) DiD Estimator

Note: Panel (b) uses regression estimates from the baseline DiD, adding linear commuting-zone-level trends, as in column (4) of the DiD results: $ln(HPI_{it}) = \beta FB_i \times qtr_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. Dashed lines denote 95% confidence intervals.

Figure 6: Event Study: New Permits



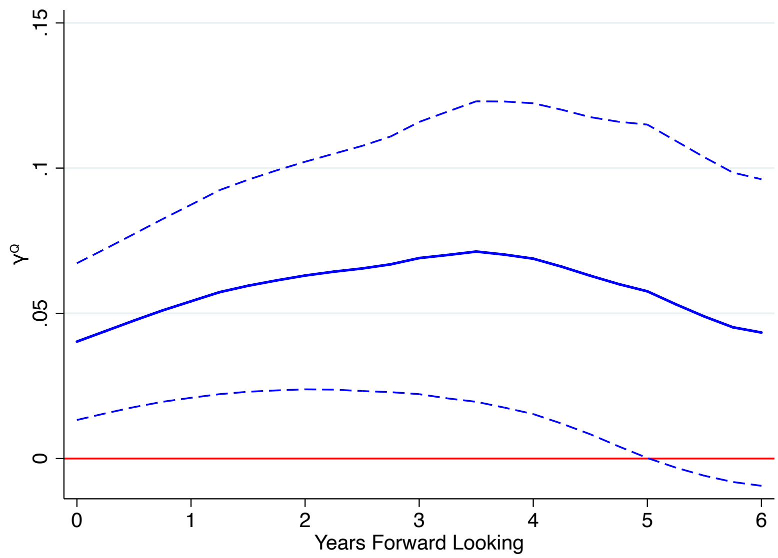
(a) Raw



(b) DiD)

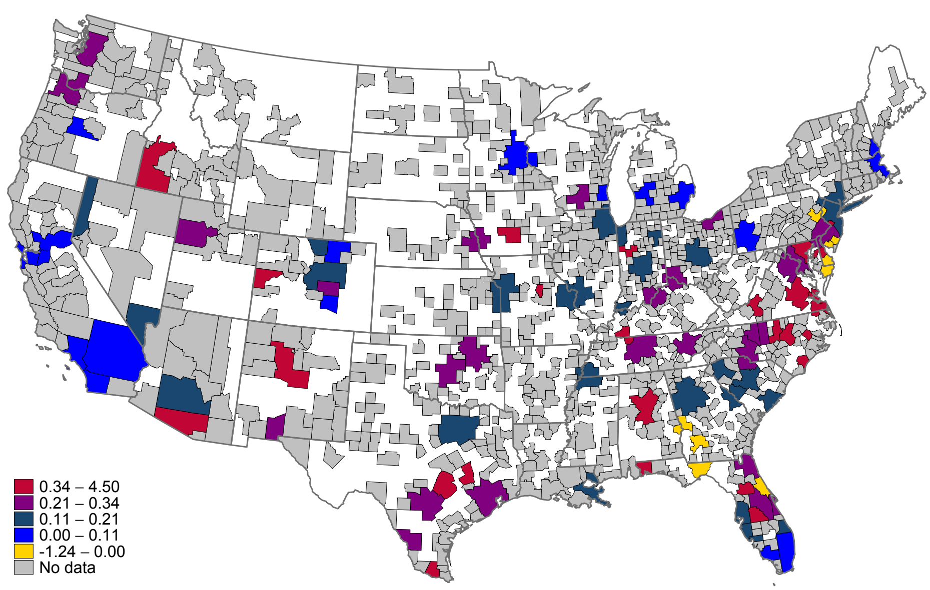
Note: As new supply is so small relative to total supply, for visual inspection we present the event studies for new supply, rather than total units. Figures show event studies for all building permits for all units, multi- and single-family construction. Panel (b) uses regression estimates from the baseline DiD, adding commuting-zone-level trends, as in column (4) of the DiD results: $Permits_{it} = \beta FB_i \times qtr_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. $Permits_{it}$ at the county-by-quarter level from the Building Permits survey 2009-2018. Dashed lines denote 95% confidence intervals.

Figure 7: Forward Looking Supply Response



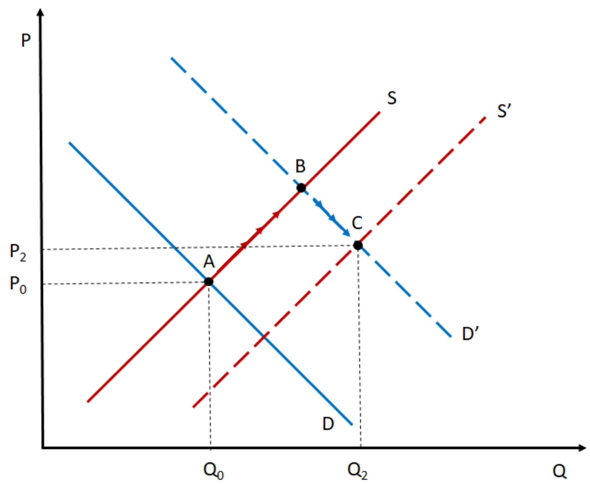
Note: This figure shows the second stage point estimate for γ^Q, as in equation 10Q, varying the decision horizon for developers τ quarters in the future: $ln(Units_{i,(t+\tau)}) = \delta + \gamma^Q ln(\widehat{ECF}_{it}) + \zeta_i + \theta_t + \lambda(gt) + \epsilon_{it}$. Dashed lines denote 95% confidence intervals.

Figure 8: Geographic Distribution of Local House Price Elasticities

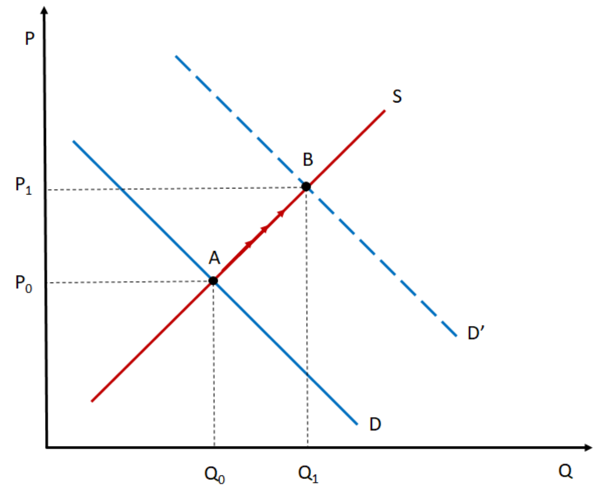


Note: This map shows the distribution of house price elasticities. Blue CBSA's are the most observably inelastic (top quartile), followed by navy, then purple, and finally the red are the most elastic quartile of CBSA elasticities. Yellow CBSA's denote negative elasticities. Gray CBSA's are those we 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.

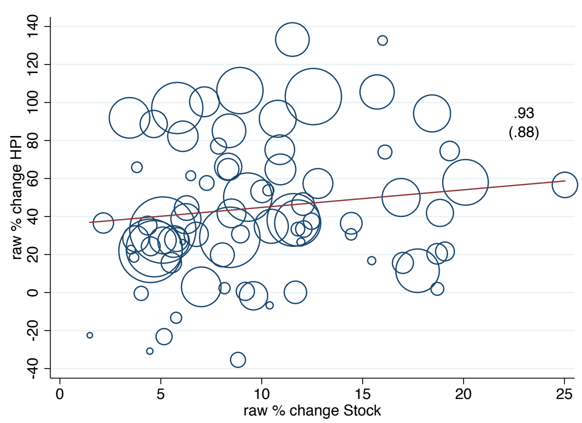
Figure 9: Endogeneity Issues in Estimating House Price Elasticities



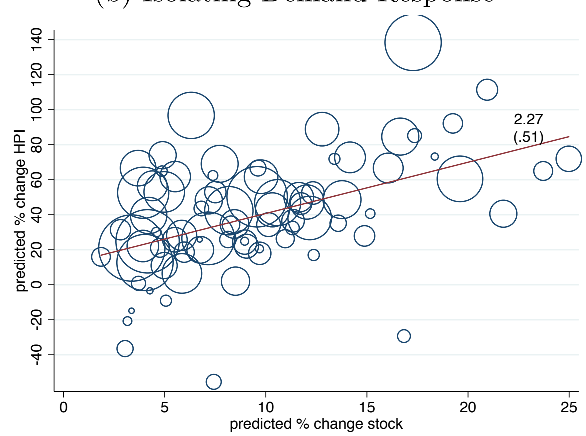
(a) Supply and Demand Response



(b) Isolating Demand Response



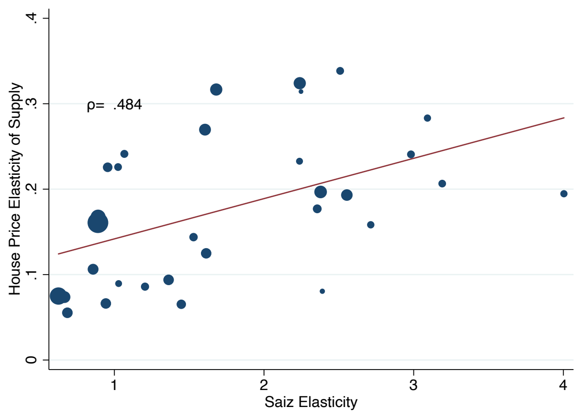
(c) Observed Equilibria Changes



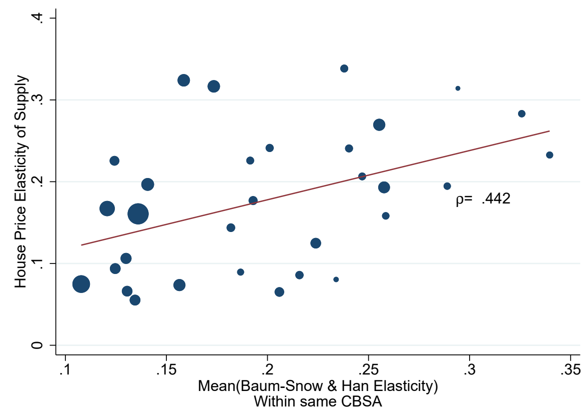
(d) Predicted Equilibria Changes

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 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. Panel (c) and (d) cover the 82/100 CBSA's in our sample with building permits available through 2018q4.

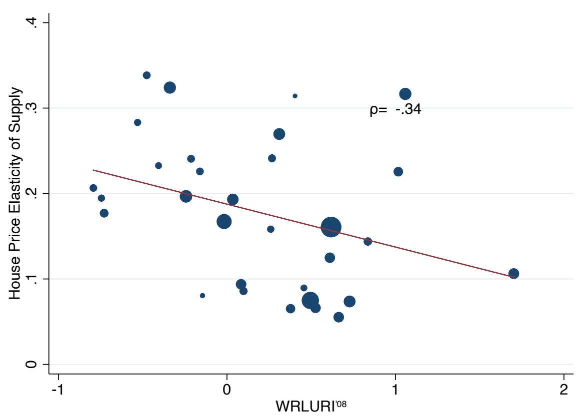
Figure 10: Correlation with other Supply Measures



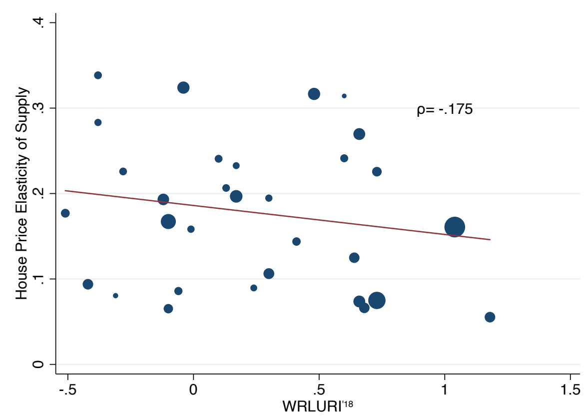
(a) Saiz Elasticity



(b) Baum-Snow & Han Elasticity



(c) $WRLURI^{08}$



(d) $WRLURI^{18}$

Note: The figures show correlation of the estimated elasticities with the Saiz elasticities from Saiz (2010) (panel (a)), as well as the Wharton Real Estate Land Use Regulation Index (WRLURI). Panel (b) shows the correlation between our elasticities and the average of tract level elasticities within a CBSA from Baum-Snow and Han (2021). Panel (c) shows the correlation with the 2008 WRLURI, while panel (d) shows the correlation with the 2018 WRLURI. The sample in the three panels is limited to 32 CBSA's with ≥ 10 $WRLURI^{18}$ responses, as advised by Gyourko, Hartley and Krimmel (2019). The larger the WRLURI, the more highly regulated a local housing market.

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 2. 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 an 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 included a pied-à-terre 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 E1 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 E1d 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 Reduced Form Extensions

B.1 Implications for Housing Affordability

Given the stark price response coupled with the more muted building response, we next examine whether these foreign capital flows affect affordability for renters. To answer this question, we analyze data from Zillow, which provides data on both house values as well as rents. We plot the Zillow Home Value Index (ZHVI) for all homes in Figure E3. Panel (a) confirms that using a different data source for house prices, we still see a sharp divergence in raw prices between foreign born zipcodes and control zipcodes after 2011q4. Panel (b) shows the difference-in-difference estimator's evolution over time, with ZHVI's rising differentially on average by approximately 7% by the end of 2019.

Figure E4 shows the same raw rents and differences-in-differences evolution for the Zillow Rent Index (ZRI). Though limited by a shorter pre-period sample, the rents show similar dynamics as prices, with rents climbing differentially in more foreign born locations by an additional 4% on average by the end of 2019. The respective tables for ZHVI and ZRI are shown in Tables D3 and D4.

We propose three mechanisms by which foreign capital investment in U.S. housing may spill over into rental markets. First, these results are consistent with recent work by Greenwald and Guren (2020) showing incomplete segmentation between home purchase and rental markets. Importantly, surveys of foreign buyers suggest that 43% of purchasers do not plan on using their U.S. home as their primary residence, and only 18% plan to rent it out to tenants (Yun, Ratiu and Cororaton, 2018-2019). This behavior would translate into homes being left unoccupied, many of which may have provided rental units under different purchasers. In short, foreign capital inflows may be shifting home vacancy and who becomes landlords in these communities. Second, it may be that these foreign buyers are outcompeting other potential homebuyers, increasing rent competition as some renters cannot transition to homeownership. Finally, relatively wealthier foreign owners selecting into neighborhoods could draw new amenities, which would also drive up rents. Differentiating between these three hypotheses is beyond the scope of this paper, and we leave it to future work.

To better understand affordability concerns, Appendix Tables D5 and D6 examine which neighborhoods are most affected by these capital inflows. First, we check whether a zipcode transacted above the national median price in 2009. Next, we check whether a zipcode transacted above the local median price in 2009. Taken together, these two tests ask whether capital flows to relatively expensive cities, and within cities, to relatively expensive areas. Appendix Table D5 shows that house prices responded similarly in zipcodes that have either above or below the national median house price in 2009, while Appendix Table D6 finds that house prices respond more in zipcodes with prices below the local median. These results show that capital is flowing to affordable areas within all types of U.S. cities, suggesting international capital may be contributing to gentrification and rental affordability issues in major cities.[24]

B.2 Treatment Reversal

Further examining the Zillow data, Figure E3(a) shows that house prices began to dip nationwide in late 2018 and early 2019. This dip is concurrent with the Trump Administration's focus on domestic policy and renegotiation of many major trade relationships, most critically with China and NAFTA. These choices may have cooled foreign interest in U.S. housing markets, supported by the drastic decline in foreign home purchase volume reported by the NAR in Figure 3.

Implementing the differences-in-differences design, and including commuting zone time trends to ensure we only compare zipcodes within the same labor market, Figure E3 panel (b)

[24]Note that the median priced zipcode in 2009 is \$206,000, so this comparison should not be taken as contrasting extremely high-cost cities with rural housing markets.

plots the difference-in-differences estimate for differential price growth in foreign born areas. Panel (b) shows that on average, FB zipcodes saw 9% additional price growth relative to control zipcodes in the same commuting zone between 2012 and 2018; however, this differential gain falls to 7% by the end of 2019. Complementing prior work on out-of-town buyers by Chinco and Mayer (2015) and Favilukis and Van Nieuwerburgh (2017), our analysis uses variation in both foreign capital increases and decreases to provide new evidence that liquid foreign capital can induce large price changes in domestic housing markets, as hypothesized in Gyourko, Mayer and Sinai (2013).

This reversal in treatment provides additional evidence that the impact of immigrant enclaves on house prices works through foreign capital flows. As the political environment cooled to foreigners, less capital flowed in, and house prices in locations attractive to foreign investors lost one-third of their relative gains through 2018. We conclude that this influx of foreign 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 residents.

C IV Approach Appendix

C.1 Check of Exclusion Restriction

A plausible violation of the exclusion restriction is that investment in the technology sector drives the house price results. As foreign countries impose foreign buyer taxes, foreigners could choose to invest in U.S. tech stocks instead of in foreign real estate. This would lead to economic growth in tech-heavy locations, which tend to be inelastically supplied with housing, increasing house prices. Therefore, the tax policy change $\implies E[\epsilon_{it}(fracFB_i \times Post_t)] \neq 0$, 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 directly controlling for the health of the local technology industry. First, we can remove tech-heavy housing markets from the data. Appendix Table D2 column (3) excludes Seattle, WA, San Jose, CA and San Francisco, CA from our sample and reruns the main analysis. Second, column (4) controls for employment in "Professional, Scientific, and Technical Services" as in Ding et al. (2019). In both panels, after the foreign buyer tax regime change, immigrant enclaves see prices and quantities increase in line with our baseline results.

In sum, accounting for differential trends in the tech sector between 2009 and 2018 does not meaningfully alter our estimates of the impact of foreign capital flowing to U.S. housing markets, and specifically to zipcodes with ex-ante high shares of foreign born residents.

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 residents. In the baseline ECF_{it}, each foeign-born resident receives the same per-capita share of the national capital flow from their origin country into the U.S. housing market. The exposure index scales this per-capita share by the share of foreign born 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 foreign capital as it is diluted among fewer non-foreign born residents:

$$ECF'_{it} = \sum_{c \in C} capflow_{ct} \times \frac{FBpop^{2011}_{ic}}{FBpop^{2011}_{c}} fracFB_{ic} \tag{15}$$

where

$$1 = \sum_{i} \frac{FBpop^{2011}_{ic}}{FBpop^{2011}_{c}} \tag{16}$$

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

Column (6) of appendix Table D8 shows the results using the exposure ECF'_{it}. The second stage yields a price elasticity estimate of 0.54 for the zipcode panel and 0.875 for the county panel, larger but in the same ballpark as our preferred estimates, while the coefficient on units is similar to the main results.

D Appendix Tables

Table D1: Covariate Balance

Variable	FB=0	FB=1	Difference
Fraction Foreign Born	0.063	0.385	0.318***
	(0.067)	(0.076)	(0.008)
HPI	1.878	1.792	-0.062
	(1.687)	(1.225)	(0.119)
HPI growth, 1 Year	-0.002	-0.047	-0.038**
	(0.450)	(0.270)	(0.016)
HPI growth, 5 Years	-0.017	-0.128	-0.096
	(0.756)	(0.845)	(0.092)
Lagged HPI	1.863	1.804	-0.038
	(1.597)	(1.189)	(0.114)
Sales	49.496	80.307	26.513**
	(68.613)	(85.200)	(11.169)
Lagged Sales	51.375	81.828	26.294**
	(69.759)	(87.088)	(11.249)

Notes: This table shows pre-period balance for housing and labor market characteristics. $FB_i = 1\left\{\frac{FBpop_i}{pop_i} \geq 95^{th} percentile\right\}$ for zipcode i. Data is zipcode level, quarterly through 2011q3. Standard errors in parentheses, clustered by commuting-zone. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D2: Robustness Checks for Reduced Form Dose-Response

	(1) Baseline	(2) No Sand	(3) No Big Tech	(4) Tech Controls	(5) Population	(6) Trend Breaks	(7) Back to 2005
Post = 1 X FracFB	0.366***	0.337***	0.378***	0.365***	0.319***	0.375***	0.250***
	(0.0722)	(0.0676)	(0.0821)	(0.0722)	(0.0594)	(0.0920)	(0.0663)
R^2	0.873	0.917	0.872	0.873	0.892	0.877	0.799
Observations	240240	138814	226558	240240	216045	240240	330854
				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

(a) $ln(hpi_{it})$

	(1) Baseline	(2) No Sand	(3) No Big Tech	(4) Tech Controls	(5) Population	(6) Trend Breaks	(7) Back to 2005
Post = 1 X FracFB	0.0541**	0.0576**	0.0528**	0.0564***	0.0541**	0.0549**	0.0541**
	(0.0203)	(0.0250)	(0.0217)	(0.0195)	(0.0203)	(0.0237)	(0.0203)
R^2	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Observations	5031	2938	4671	5031	5031	5031	5031
				Fixed Effects			
Quarter	X	X	X	X	X	X	X
County	X	X	X	X	X	X	X
Zone X Quarter	X	X	X	X	X	X	X

(b) $ln(Units_{it})$

Notes: The table shows the coefficient β from $ln(Y_{it}) = \alpha + \beta FracFB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$, where $Y_{it} \in \{ln(hpi_{it}, ln(Units_{it}))\}$. Panel (a) uses zip-code-by-quarter data, while panel (b) uses county-by-quarter data. The Column (1) provides baseline point estimate. Column (2) drops sand states, Arizona, California, Florida, Nevada, and Texas. Column (3) drops cities with large tech centers: San Francisco, CA, San Jose, CA and Seattle, WA. Column (4) controls for county level employment in a particularly fast growing sub-industry of management and professional services (MPRO), "Computer Systems Design and Related Services", as identified by Ding et al. (2019). Column (5) controls for time-varying zip code population. Column (6) applies linear trends at the commuting zone level, and allows the trends to break before and after 2011q4. Column (7) extends data back to 2005q1. All data at the zipcode by quarter level. Standard errors in parentheses, clustered by quarter, or by geography of time trend. Analysis spans 2009-2018. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D3: Difference–in–Differences Results: Zillow Home Value Index

	(1) ln(ZHVI)	(2) ln(ZHVI)	(3) ln(ZHVI)	(4) ln(ZHVI)
Post=1 X FB=1	0.124***	0.0597***	0.0437***	0.0449***
	(0.00549)	(0.0136)	(0.00755)	(0.0105)
R^2	0.981	0.989	0.990	0.989
Observations	641215	449418	219034	234896
		Fixed Effects		
yq	X	X	X	X
Zip	X	X	X	X
State X yq		X		
MSA X yq			X	
Zone X yq				X

Notes: This table shows the coefficient β from $ln(ZHVI_{it}) = \alpha + \beta FB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. FB=1 defined as $FB_i = 1\left\{\frac{FBpop_i}{pop_i} \geq 95^{th} percentile\right\}$ 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 D4: Difference–in–Differences Results: Zillow Rent Index

	(1) ln(ZRI)	(2) ln(ZRI)	(3) ln(ZRI)	(4) ln(ZRI)
Post=1 X FB=1	0.0405***	0.0295***	0.0109*	0.0219***
	(0.00353)	(0.00876)	(0.00634)	(0.00443)
R^2	0.976	0.981	0.989	0.987
Observations	424388	315011	174674	184613
		Fixed Effects		
yq	X	X	X	X
Zip	X	X	X	X
State X yq		X		
MSA X yq			X	
Zone X yq				X

Notes: This table shows the coefficient β from $ln(ZRI_{it}) = \alpha + \beta FB_i \times Post_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. FB=1 defined as $FB_i = 1\left\{\frac{FBpop_i}{pop_i} \geq 95^{th} percentile\right\}$ 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 D5: Differences-in-Differences Results, above/below national median price

	(1) ln(HPI)	(2) ln(HPI)	(3) ln(HPI)	(4) ln(HPI)
Post = 1 X FB X Below Median	0.141***	0.105**	0.0744**	0.0898***
	(0.0286)	(0.0414)	(0.0293)	(0.0299)
Post = 1 X FB X Above Median	0.113***	0.0869***	0.0561***	0.0574***
	(0.0215)	(0.0202)	(0.0185)	(0.0167)
R^2	0.864	0.876	0.872	0.872
Observations	461147	461147	222061	238699
		Fixed Effects		
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
CBSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficients β_n from $ln(HPI_{it}) = \alpha + \beta_1 FB_i \times Post_t \times Above_i + \beta_2 FB_i \times Post_t \times Below_i + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. 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 D6: Differences-in-Differences Results, above/below local median price

	(1) ln(HPI)	(2) ln(HPI)	(3) ln(HPI)	(4) ln(HPI)
Post = 1 X FB X Below Median	0.137***	0.108***	0.0743***	0.0802***
	(0.0190)	(0.0239)	(0.0177)	(0.0181)
Post = 1 X FB X Above Median	0.0876***	0.0578**	0.0337	0.0379*
	(0.0201)	(0.0254)	(0.0220)	(0.0218)
R^2	0.864	0.876	0.872	0.872
Observations	461147	461147	222061	238699
		Fixed Effects		
Quarter	X	X	X	X
Zip	X	X	X	X
State X Quarter		X		
CBSA X Quarter			X	
Zone X Quarter				X

Notes: This table shows the coefficients β_n from $ln(HPI_{it}) = \alpha + \beta_1 FB_i \times Post_t \times Above_i + \beta_2 FB_i \times Post_t \times Below_i + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. 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 D7: 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 D8: Expected Capital Flow IV Robustness, 2^{nd} Stages

	(1) Baseline	(2) Comp. Controls	(3) Pop. & Inc. Controls	(4) FB. Breakdown	(5) Excluding China	(6) Exposure
ln(ECF_it)	0.534***	0.455***	0.491***	0.522***	0.532***	0.875***
	(0.0845)	(0.126)	(0.0930)	(0.0854)	(0.0857)	(0.327)
Root MSE	0.112	0.104	0.110	0.111	0.112	0.246
Observations	20564	20550	20333	20564	20564	20562
$1^{st} Stage F-Stat$	193.4	92.67	87.58	103.4	131.6	8.871
Fixed Effects						
Quarter	X	X	X	X	X	X
County	X	X	X	X	X	X

(a) $ln(HPI_{it})$

	(1) Baseline	(2) Comp. Controls	(3) Pop. & Inc. Controls	(4) FB. Breakdown	(5) Excluding China	(6) Exposure
ln(ECF_it)	0.0403***	0.0596***	0.0591***	0.0425***	0.0435***	0.0301***
	(0.0138)	(0.0201)	(0.0175)	(0.0140)	(0.0163)	(0.0110)
Root MSE	0.0135	0.0144	0.0141	0.0136	0.0140	0.0154
Observations	20564	20550	20333	20564	20564	20562
$1^{st} Stage F-Stat$	54.34	42.61	48.53	39.80	33.63	20.57
Fixed Effects						
Quarter	X	X	X	X	X	X
County	X	X	X	X	X	X

(b) $ln(Units_{it})$

Notes: The table shows the coefficients γ^P and γ^Q from the second stage equations 10P and 10Q. Column (1) provides baseline point estimates as in columns (2) and (3) of panel (b), Table 3. Column (2) controls for CBSA-level composition of various immigrant groups identified in the NAR data as of 2011 (Canadian, Chinese, Indian, Mexican, British, and Other). Column (3) includes time-varying income and population controls. Column (4) differentiates between foreign born Chinese and foreign born non-Chinese population shares in the first stage. Column (5) excludes Chinese foreign born resident from the foreign born shares, and constructs $ln(ECF_{it})$ without Chinese housing purchases. Column (6) uses $ln(ECF'_{it})$, constructed according to Appendix B.2, scaling per capita foreign capital inflows by fraction foreign born. All data at the county by quarter level, and includes linear commuting zone trends. Standard errors in parentheses, clustered commuting zone. Analysis spans 2009-2018. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table D9: List of CBSAs from Most to Least Inelastic

Rank	CBSA	Elasticity
1	San Francisco-Oakland-Hayward, CA	0.0553
2	Minneapolis-St. Paul-Bloomington, MN-WI	0.0652
3	Riverside-San Bernardino-Ontario, CA	0.0661
4	Miami-Fort Lauderdale-West Palm Beach, FL	0.0736
5	Los Angeles-Long Beach-Anaheim, CA	0.0748
6	Pueblo, CO	0.0750
7	Stockton-Lodi, CA	0.0751
8	Sacramento–Roseville–Arden-Arcade, CA	0.0775
9	Grand Rapids-Wyoming, MI	0.0805
10	Pittsburgh, PA	0.0858
11	San Diego-Carlsbad, CA	0.0893
12	Milwaukee-Waukesha-West Allis, WI	0.0895
13	Greeley, CO	0.0911
14	Detroit-Warren-Dearborn, MI	0.0938
15	Naples-Immokalee-Marco Island, FL	0.105
16	Bend-Redmond, OR	0.106
17	Boston-Cambridge-Newton, MA-NH	0.106
18	Las Vegas-Henderson-Paradise, NV	0.112
19	New Orleans-Metairie, LA	0.116
20	Greenville-Anderson-Mauldin, SC	0.118
21	Phoenix-Mesa-Scottsdale, AZ	0.125
22	Port St. Lucie, FL	0.128
23	Punta Gorda, FL	0.141
24	Columbia, SC	0.142
25	Denver-Aurora-Lakewood, CO	0.144
26	Reno, NV	0.144
27	Memphis, TN-MS-AR	0.149
28	Evansville, IN-KY	0.152
29	Fort Wayne, IN	0.156
30	Columbus, OH	0.158
31	New York-Newark-Jersey City, NY-NJ-PA	0.161
32	Chicago-Naperville-Elgin, IL-IN-WI	0.167
33	St. Louis, MO-IL	0.177
34	Augusta-Richmond County, GA-SC	0.186
35	North Port-Sarasota-Bradenton, FL	0.188
36	Tampa-St. Petersburg-Clearwater, FL	0.190
37	Atlanta-Sandy Springs-Roswell, GA	0.193
38	Indianapolis-Carmel-Anderson, IN	0.195
39	Dallas-Fort Worth-Arlington, TX	0.197
40	Fort Collins, CO	0.204

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Rank	CBSA	Elasticity
41	Kansas City, MO-KS	0.206
42	Charleston-North Charleston, SC	0.209
43	Louisville/Jefferson County, KY-IN	0.214
44	Seattle-Tacoma-Bellevue, WA	0.226
45	Cleveland-Elyria, OH	0.226
46	Palm Bay-Melbourne-Titusville, FL	0.232
47	Nashville-Davidson–Murfreesboro–Franklin, TN	0.233
48	Colorado Springs, CO	0.235
49	San Antonio-New Braunfels, TX	0.241
50	Portland-Vancouver-Hillsboro, OR-WA	0.241
51	Knoxville, TN	0.241
52	Oklahoma City, OK	0.243
53	Las Cruces, NM	0.251
54	Salt Lake City, UT	0.253
55	Jacksonville, FL	0.254
56	Winston-Salem, NC	0.260
57	Washington-Arlington-Alexandria, DC-VA-MD-WV	0.270
58	Charlotte-Concord-Gastonia, NC-SC	0.283
59	Tulsa, OK	0.295
60	Omaha-Council Bluffs, NE-IA	0.310
61	Madison, WI	0.314
62	Laredo, TX	0.316
63	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	0.317
64	Orlando-Kissimmee-Sanford, FL	0.319
65	Greensboro-High Point, NC	0.320
66	Houston-The Woodlands-Sugar Land, TX	0.324
67	Cincinnati, OH-KY-IN	0.338
68	Austin-Round Rock, TX	0.347
69	Clarksville, TN-KY	0.349
70	Lafayette-West Lafayette, IN	0.360
71	Boise City, ID	0.365
72	Tucson, AZ	0.367
73	Dover, DE	0.373
74	Durham-Chapel Hill, NC	0.386
75	College Station-Bryan, TX	0.387
76	Ocala, FL	0.387
77	Greenville, NC	0.398
78	Richmond, VA	0.410
79	Albuquerque, NM	0.416
80	Birmingham-Hoover, AL	0.453

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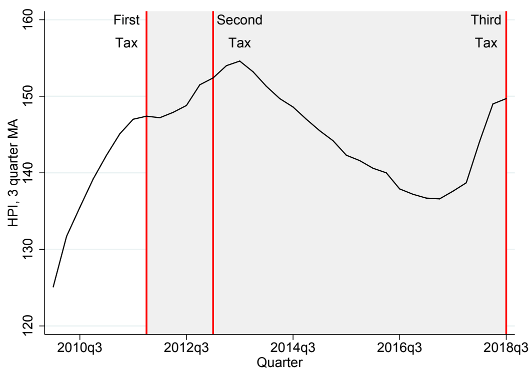
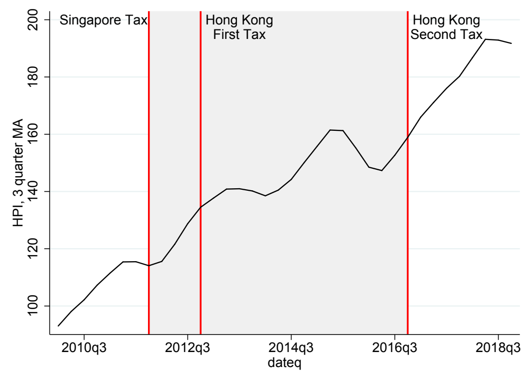
81	Columbia, MO	0.471
82	Lakeland-Winter Haven, FL	0.495
83	Des Moines-West Des Moines, IA	0.533
84	Raleigh, NC	0.536
85	Pensacola-Ferry Pass-Brent, FL	0.539
86	Wilmington, NC	0.621
87	McAllen-Edinburg-Mission, TX	0.650
88	Roanoke, VA	0.679
89	Grand Junction, CO	0.728
90	Baltimore-Columbia-Towson, MD	0.902
	Virginia Beach-Norfolk-Newport News, VA-NC	4.109
	Trenton, NJ	4.502
	Allentown-Bethlehem-Easton, PA-NJ	-1.238
	Salisbury, MD-DE	-0.574
	Columbus, GA-AL	-0.557
	Deltona-Daytona Beach-Ormond Beach, FL	-0.267
	Albany, GA	-0.226
	Vineland-Bridgeton, NJ	-0.152
	Tallahassee, FL	-0.134
	Atlantic City-Hammonton, NJ	-0.0833

E Appendix Figures

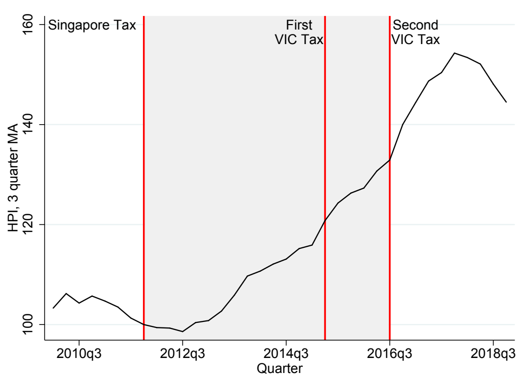
Figure E1: Foreign Born Taxes in Domestic Markets



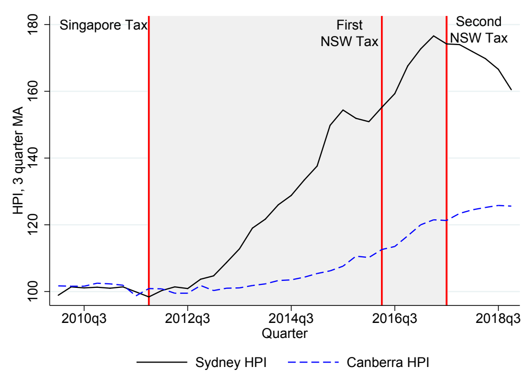
(a) Singapore



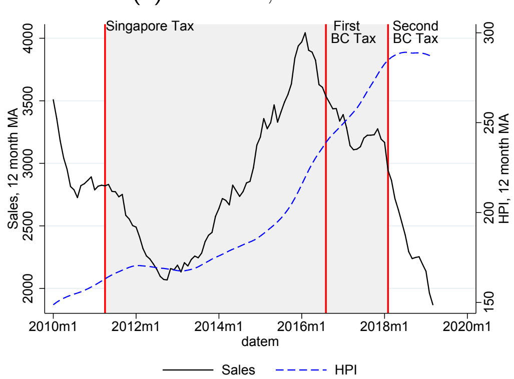
(b) Hong Kong



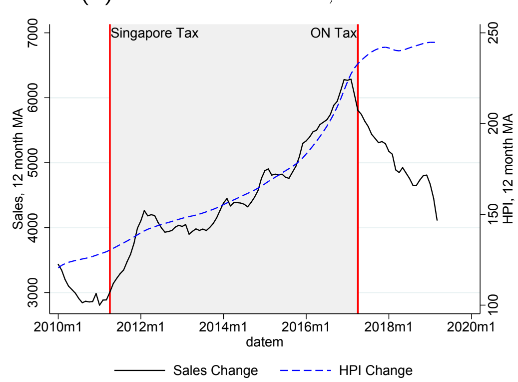
(c) Victoria, Australia



(d) New South Wales, Australia



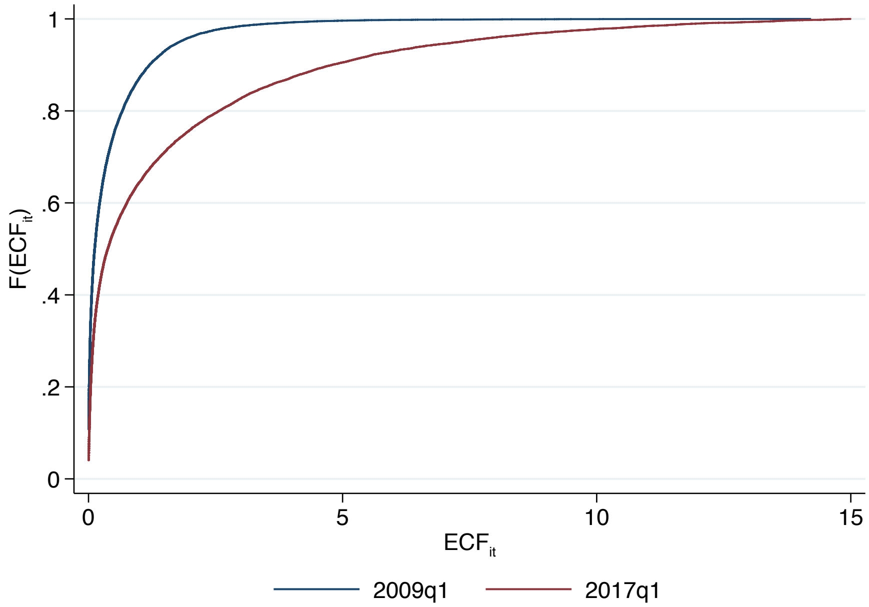
(e) British Columbia, Canada



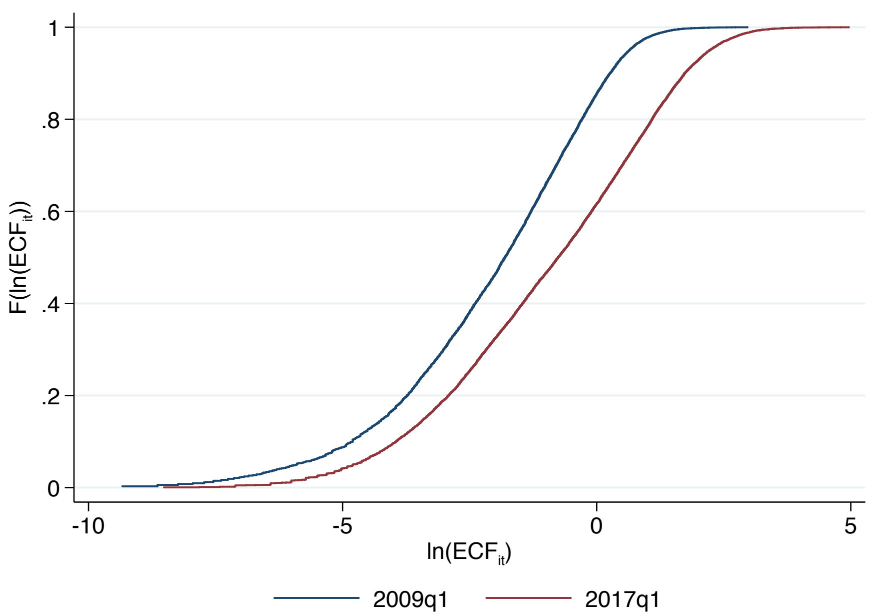
(f) Ontario, Canada

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 E2: ECF_{it} Summary



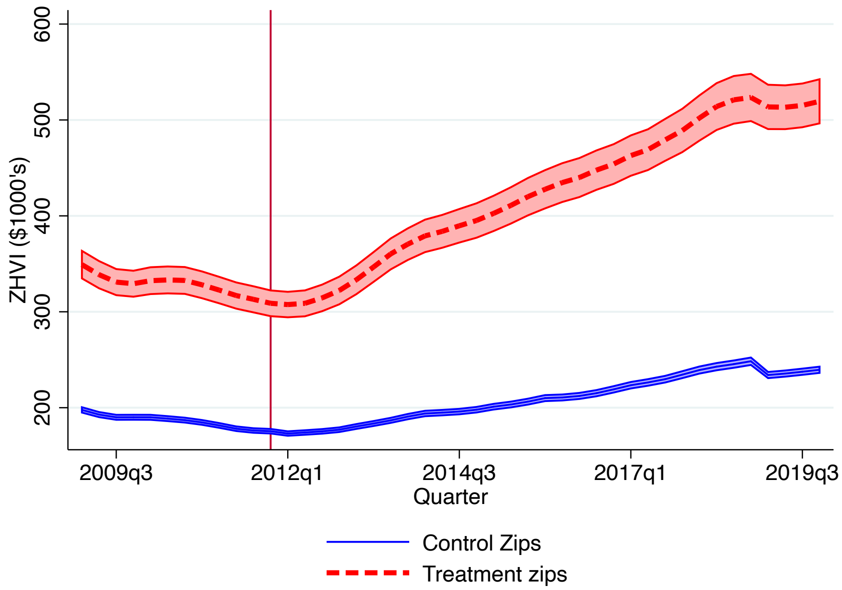
(a) ECF_{it}



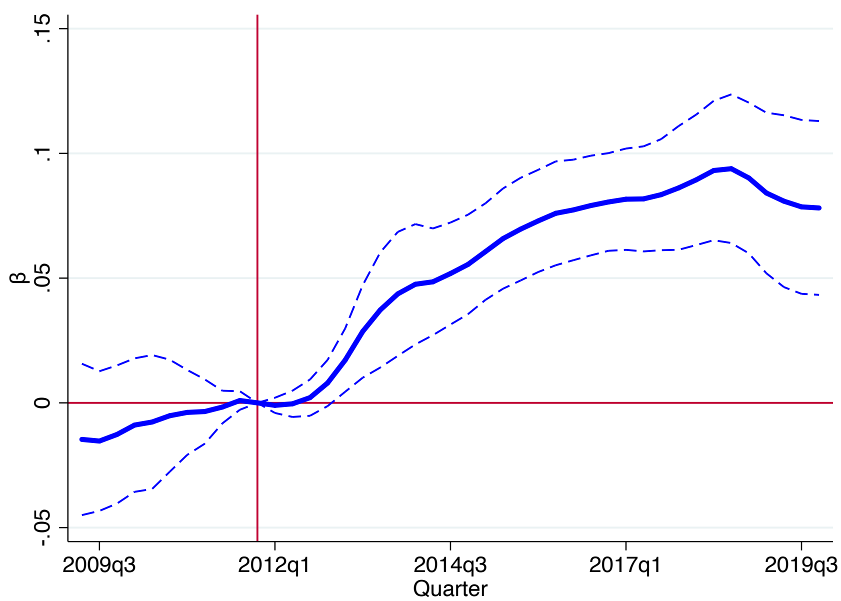
(b) $ln(ECF_{it})$

Note: Figures show distribution of expected capital flows to zipcodes in 2009q1, the beginning of the sample, and 2017q1, the quarter at which foreign investment in the U.S. housing market peaked. Panel (a) shows the distribution in millions of dollars along the x-axis, while panel (b) shows the distribution of $ln(ECF_{it})$, our measure of foreign capital used in IV analysis to back out elasticities of prices and quantities with respect to foreign capital.

Figure E3: ZHVI Event Study



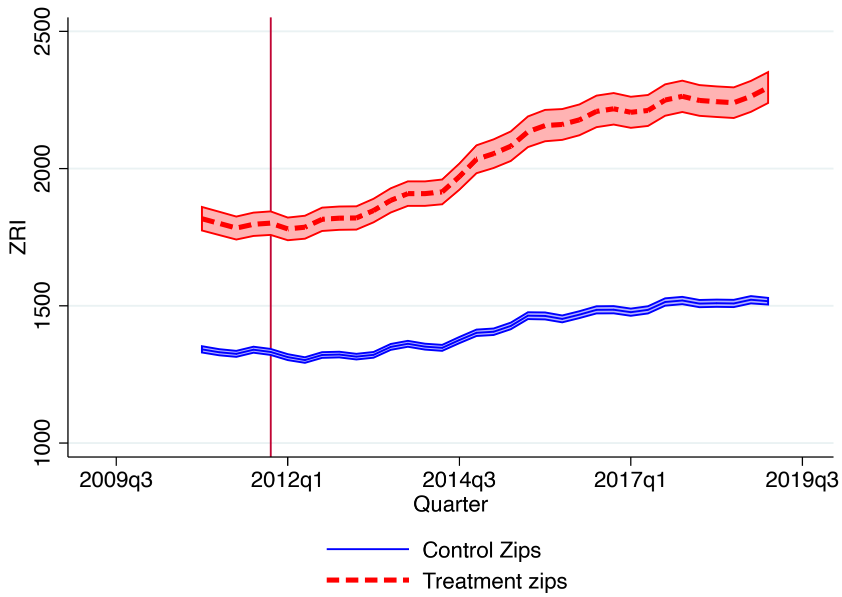
(a) Raw Data



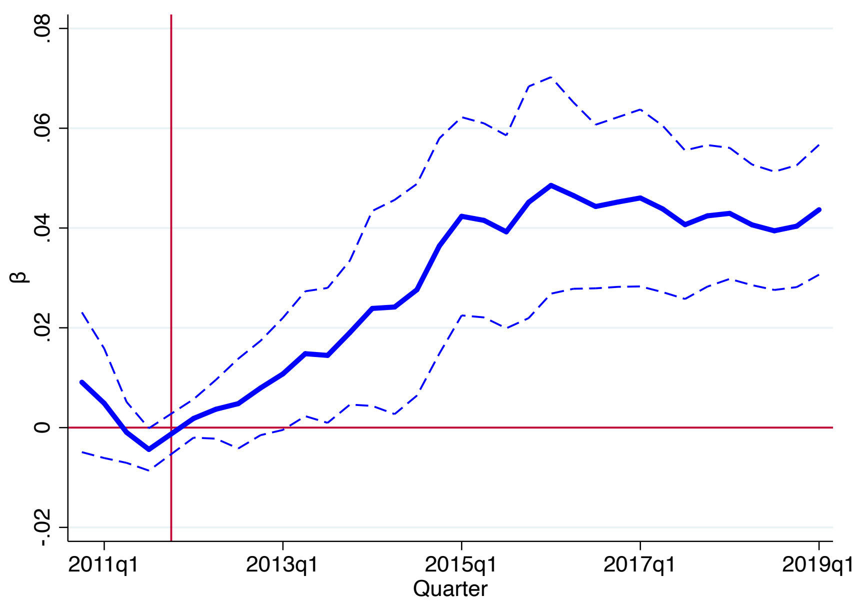
(b) DiD Estimator

Note: Panel (a) shows raw time series of the Zillow Home Value Index. Panel (b) uses regression estimates from the baseline DiD, adding commuting-zone-level trends, as in column (4) of the DiD results: $ln(ZHVI_{it}) = \beta FB_i \times qtr_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. Dashed lines denote 95% confidence intervals.

Figure E4: ZRI Event Study



(a) Raw Data



(b) DiD Estimator

Note: Panel (a) shows raw time series of the ZillowRent Index. Panel (b) uses regression estimates from the baseline DiD, adding commuting-zone-level trends, as in column (4) of the DiD results: $ln(ZRI_{it}) = \beta FB_i \times qtr_t + \zeta_i + \theta_t + \lambda_{gt} + \varepsilon_{it}$. Dashed lines denote 95% confidence intervals. Rent data only available from 2011.