Amazon HQ2: A Tale of Shocks to Housing Price Expectations*

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Abstract

The measurement of the contribution of expectations to house prices is unresolved in the macro-housing literature. We leverage a novel quasi-natural experiment using Amazon's unanticipated split location decision for its second headquarters to identify the impact of this expectations shock on local house prices, seller expectations and market liquidity. We find that listed and transacted prices increased on average 7.9% and 7.5%, respectively in the six months following the announcement. Furthermore, price gains were common across all market segments and the announcement had no effect on rents. We develop a tractable general equilibrium macro-housing model featuring mortgages and endogenous housing supply able to replicate the response of the price-rent ratio to an expectations shock. The model quantifies the differences between credit and expectations shocks and generates testable predictions for identifying the nature of a housing price shock. Our empirical and theoretical results provide a benchmark test for structural models that attempt to incorporate shocks to price expectations.

Keywords: House price-rent, expectations shocks, credit segmentation.

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1 Introduction

The dynamics of housing markets have been an important driver of macroeconomic cycles (Leamer, 2007; Garriga and Hedlund, 2019). Somewhat surprisingly, identifying the exact determinants of housing price fluctuations remains an open question for the macro-housing literature which models housing as an asset that provides services and a resale option. This approach contrasts with the *traditional urban approach* that considers housing as a service good provided by an absentee landlord, and where all the variation in prices (or rents) is solely driven by changes in local conditions with no role for expectations or credit.

The two primary candidates to explain house price volatility are changes in credit conditions and expectations about future price appreciation. (Davis and Van Nieuwerburgh, 2015; Piazzesi and Schneider, 2016; Cox and Ludvigson, 2021). It is challenging to identify the relative importance of each of these forces to house price movements. Persistently loose credit conditions, i.e., low interest rates or loose lending standards, generates housing demand and drives expectations of future price growth. Similarly, expectations about future price growth due to local fundamentals can relax current credit conditions, and further impact house price growth. In practice, identifying changes in credit conditions and expectations of future interest rates can be recovered through financial market information. On the contrary, it is generally infeasible to measure expectations of house price appreciation. In an ideal world, researchers could connect micro data containing exogenous sources of variation of expectations and current house prices for both buyers and sellers as well as non-active market participants. Unfortunately, this information is rarely available, as discussed by Kuchler, Piazzesi and Stroebel (2023) and thus, researchers have to rely on surveys, self-assessed valuations, or randomized control trials to quantify the effect of expectation shocks on house prices.

The paper's first contribution is to provide a novel identification of an expectation shocks to future house prices by leveraging the quasi-natural experiment of Amazon's selection process of its second headquarters (*HQ2*). Our analysis provides a direct empirical measurement of a house price expectations shock using micro transaction and listing data. Our empirical strategy offers robust estimates of significant and persistent effects of an expectations shock on sellers' listed prices, close prices, and housing liquidity in the winning locations. The increased valuations for buyers and sellers affects all segments of the housing market, as the expectations shock shifts the entire distribution of house prices upwards.

The second contribution is the development of a tractable general equilibrium macro-housing model that replicates the response of the price-rent ratio in response to expectation shocks. Our model features collateralized mortgages with loan-to-value (LTV) requirements and endogenous housing supply, and additionally nests other formulations used in the literature as special cases. The model allows for identifying the responses of the price-rent ratio to either unexpected expectations shocks or credit shocks. Quantifying the contribution of expectations on house price movements and its interaction with credit provides a resolution to the discussion of what are the key determinants of house price movements in the macro-housing literature.

The process of selection of the location for Amazon's *HQ2* created a setting uniquely suited to measuring the changes in house price expectations due to an unanticipated shock. Amazon's campaign to choose a second headquarters was unique in terms of its publicity and the size and wealth of the workforce Amazon promised to hire in the selected location. In September 2017, Amazon announced a search for a second headquarters to complement their original location in Seattle, WA. Amazon invited cities across North America to submit proposals. In return, Amazon promised to bring up to 50,000 high-paying jobs and $5 billion in investment which would be implemented in the coming years. In January 2018, the field was narrowed to twenty finalists locations primarily in the Eastern United States. In November 2018, Amazon "unexpectedly" announced two winners for their headquarters sweep- stakes: Long Island City in Queens, New York City, NY, and Crystal City in Arlington, Virginia, VA.[1] Three months after disclosing the winners, Amazon unexpectedly decided to withdrew from Long Island City as political opposition to their development plans and aid package grew locally. This creates another unexpected, potentially negative, expectations shock for that housing market.

We exploit the timing and secrecy of the selection process for Amazon *HQ2* and use transaction level data to measure the magnitude of the expectations shock on housing market conditions using difference-in-differences (*DID*) estimators in the winning locations. The use of transaction data has clear advantages over survey data. The announcement provides critical information on the listing behavior of current and prospective sellers, but also identifies how these prospects are materialized when the transaction is closed.

In order to interpret our estimates from the *DID* estimator as causal treatment effects from the *HQ2* shock we need to reasonably establish that Amazon *HQ2* announcement is exogenous. In Crystal City, VA, we find that house prices experienced a sharp discontinuity upon the winning announcement, suggesting the shock was unexpected. We perform statistical tests of the pre-announcement period which find little evidence that housing market conditions in Crystal City changed prior to the announcement. In Long Island City, NY, there are back to back shocks moving expectations in opposite directions. We find no evidence of pre-trends prior to the winning announcement, and a notable change in trend upon the withdrawal. Moreover, we present narrative evidence using online betting odds and Google trends to corroborate that Amazon's decision was unexpected in either location.

The empirical implementation and identification requires us to specify a control group against the winning *HQ2* locations, as well as pre- and post-periods. We choose a collection of large metropolitan areas that were not *HQ2* finalists as a control group so that they are uncontaminated by Amazon's selection process. We use residential real estate transactions in the year prior to the winning announcement as well as a series of time windows after the announcement to evaluate dynamic treatment effects of the *HQ2* shock on local housing market conditions.

[1] The areas has been renamed National Landing in Arlington County in Virginia, and it includes Crystal City, Pentagon City, and the Potomac Yard neighborhood in the City of Alexandria all near Reagan International airport. For convenience, the exposition uses the name of Crystal City to capture the entire area.

Our baseline results show that the *HQ2* announcement affects transaction prices, i.e., the CLOSE PRICE/SQ.FT, in Crystal City shortly after the announcement. The estimated treatment effects use rolling windows of 1, 3, 6, and 12 months after the announcement to study the dynamics of the housing market in response to the shock. Housing is an non-liquid asset with considerable transaction delay, so we expect transacted prices to adjust slower than the listing prices of sellers. The estimates show that CLOSE PRICE/SQ.FT increased $19 after three months, up to $26 by month six, and around $30 per in a twelve month window due to the *HQ2* shock. The cumulative announcement effect on prices is on average 8.6% of the total home value in a twelve month window. The economically large price appreciation attributed to an expectations shock is a novel contribution and suggests that expectations shocks are capable of generating substantial housing price movements.

The Amazon *HQ2* announcement has a significant impact on LIST PRICE/SQ.FT, the price the seller initially asks for, which can be interpreted as a directly observable measure of the sellers' own home valuation expectation. The average seller's expectations increase immediately after the announcement remained high for an entire year, sustaining an increase in the ask price of approximately $30 per square foot across all time windows. In contrast, buyers' adjusted their valuations more slowly, but within a year converged to nearly the same level as the sellers' valuations with a small gap of $2 per square foot. The results suggest that, over time, realized prices converged to seller expectations. The gap between listing and close prices emphasizes the role of *housing market liquidity* when studying housing markets as transactions occur with a significant lag, as discussed by as discussed by Garriga and Hedlund (2020) and Famiglietti, Garriga and Hedlund (2020). We find that the *HQ2* shock caused the time on the market (in days) to transact houses to decline by 12 days within six months. This amounts to about a 22% increase in the level of housing liquidity, and the effect is persistent for the remaining six months in the sample.[2]

Did the Amazon *HQ2* announcement trigger a gradual increase in rents over time, as lease contracts are renewed to catch up with the house price appreciation? Graphically, we find that relative to the control group there is no discontinuity around the *HQ2* announcement and that rents in Crystal City actually declined. Using the ZIP code as a unit of analysis, *DID* estimates find no significant effects on rental prices within one year after the winning announcement in sharp contrast to the effects on house prices.

What is the size of *house price "pass-through"* of the shock to expectations across the different market segments? Since Crystal City is within an expensive metro area with relatively low levels of home ownership, one might suspect that the shock was concentrated in the upper tier in the housing market. This is in contrast to a credit loosening shock, which one would expect to be concentrated among the lower pricing price tiers as credit expands access to housing markets. We find that the distribution of house prices after the announcement shifts upwards considerably compared

[2] We test the robustness of our baseline results along the dimensions of the validity of the control group and use different estimators. In summary, we find all the empirical results to be robust to alternative control groups and estimators, and it is likely that our baseline results are conservative.

to the pre-announcement distribution. We test and confirm that the house price distribution after the announcement first-order stochastically dominates the pre-announcement distribution. This is not due to nationwide trends, as the price distribution does not change for the control group. This evidence suggests that the expectations shock affected all market segments by a similar magnitude.

In the case of Long Island City, NY, there are two shocks associated with *HQ2* that move expectations in opposite directions (winning and withdrawal announcements three months apart). Estimating effects of the winning announcement on close prices is challenging given that we only observe 99 transactions in Long Island City in the three months after winning the *HQ2* bid, and that the market is very illiquid with an average time to transact of 172 days. The withdrawal announcement significantly reduced close prices and lowered seller optimism measured by listing prices within six months. The estimated treatment effects show a decline of close prices by $36 per square foot and list prices by $52 per square foot within six months of the withdrawal announcement. Because of the data limitations in this neighborhood, we detail our analysis of the withdrawal of *HQ2* from Long Island City in Appendix B and focus on Crystal City in the main text.

Our empirical findings provide a benchmark that structural macro-housing models with expectations shocks should be capable of explaining. We find that while an arbitrage pricing model can replicate the dynamics of the *HQ2* expectations shock, endogenizing prices and quantities provides a more challenging test. Our second contribution is developing a general equilibrium macro-housing model that is capable of replicating the magnitude and dynamics of the price-rent ratio in response to the *HQ2* expectations shock. The model features segmented housing markets using collateralized loans (mortgages) with LTV requirements and endogenous housing supply. The model is general enough to nest common specifications used in the literature as special cases.

In this setting, the expectations shock is modelled as an unanticipated increase in permanent future income. This model delivers predictions, in terms of the response of the price-rent ratio, that are consistent with the empirical findings. We find that upon announcement, house prices sharply increase and rents remain largely flat- causing a large spike in the price-rent ratio. Rents start to increase upon the realization of the expected income, after which the price-rent ratio begins to decline but eventually converges to a new steady state level higher than the initial steady state.

The model has implications for the literature as it provides guidance on the structural asset pricing relationships needed to estimate shocks to expectations. We highlight the main features that are required to replicate the price-rent dynamics empirically: collateralized mortgages and endogenous housing supply. We demonstrate in Appendix C that common macro-housing model specifications that abstract from these features fail to generate empirically plausible price-rent responses to an expectations shock. While other models might be able to consistently replicate the associated dynamics of expectations shock, a singular feature of our model is that the removal of any of these features fails to replicate the empirical patterns documented.

Exploring the model mechanisms highlights the interactive role of housing supply and collateralized mortgage loans in replicating the empirical dynamics. Endogenous housing supply and mortgages together crucially allow for the model to decouple rents from house prices in response to an expectations shock by allowing households to bring expected future income to present housing expenditures. As housing supply is not perfectly elastic in the short-run, the model predicts that expectations shocks in areas with inelastic housing supply will generate some degree of overshooting. In the short-run housing supply is fixed and prices increase due to a shock to future income.

Eliminating credit frictions makes house prices and rents move one-for-one in response to the expectation shock. As a result, the price-rent ratio remains constant which is inconsistent with observed dynamics of the identified empirical shock. Alternatively, one can consider the case of uncollateralized borrowing with a fixed interest rate. This specification counterfactually predicts an increase of borrowing to consume housing and non-housing goods, increasing both prices and rents a year after the announcement when the increase in income is realized.

The role of mortgage loans are to act as a vehicle to channel resources directly towards housing markets allowing expectations shocks to be fully capitalized in house prices and not rents. This mechanism accounts for the the observed decoupling between these two prices. The arbitrage pricing model assumes that rents are driven by a different exogenous process than house prices.

Through the lens of the model, one can ask whether there are testable implications between expectations and credit shocks in terms of the implied dynamics of the price-rent ratio. The model provides an ideal laboratory to test the relative differences between these two types of shocks. Our model shows that while both shocks can move the price-rent ratio in a similar magnitude in the short-run, their long-run implications are different. Credit shocks generate a persistent gap between house prices and rents, whereas expectations shocks cause rents to increase only when expected income is realized, which generates a decline in the price-rent ratio over time. The differential dynamics can prove of interest when attempting to identify the nature of shocks to house prices.

The model clearly highlights the critical importance of mortgages to capitalize expectation shock in house prices. Is there evidence that the transactions in Crystal City used mortgages for their purchases as opposed to cash? If post-announcement purchases use mortgages, one might expect that the growth in mortgage loans is inline with the growth of house prices such that the LTV ratio stays more or less constant. Alternatively, one might expect increases in the LTV if loan growth exceeds house prices. With cash purchases, one might expect a decrease in the LTV and the number of purchases using mortgages to decrease relative to the pre-announcement trends.

We use the dataset ZTRAX which contains housing deeds to for every single transaction before and after the *HQ2* shock. Using different time windows, we find graphically that loan growth per square foot after the announcement is inline with house price appreciation. The similar growth in mortgage growth and house prices implies a relatively constant LTV similar to the pre-announcement trend. These results provide external validity on key model assumptions regarding the use of mortgages and the constant LTV ratio and are of interest for future theoretical work.

The article is organized as follows. Section 2 connects our contributions with the existing literature. Section 3 discusses the data used in the analysis and presents the institutional details of the natural experiment. Section 4 presents the empirical measurements of a housing expectations shocks. Section 5 builds a tractable macro-housing model recovering the shock and highlights how other standard housing models fail to capture all the market dynamics documented in our empirical analysis. Section 6 concludes.

2 Literature Review

In the last two decades, the macro-housing literature has focused in isolating the driving factors of housing valuations, both at an aggregate level and in the cross-section of urban locations. These factors include variation in rents growth, expectations, interest rates, and credit conditions. Shiller (2007) conjectures that exuberant expectations for future price increases were the fundamental driver of the 2000s housing bubble, but the existing empirical evidence is not conclusive in isolating the key driver among these factors for different episodes. The survey article by Kuchler et al. (2023) discusses both the data currently available and the econometric difficulties in measuring expectations about future house prices. They present a very detailed summary of the different approaches to measure the effects of expectations on housing variables. In this section we discuss some of their key ideas and connections with our empirical strategy.

A first approach is to use *self-assessed valuations* and estimate expectations as the difference with transacted prices at the local level. Focusing on the boom-bust cycle in the early 2000s, Davis and Quintin (2017) estimate a Kalman filter model and find that self-assessed house prices did not increase as rapidly as housing price indexes during the boom. However, they did not decline as severely during the bust either, thus highlighting a sluggish response of expectations. The evidence using this approach challenges the notion that housing price expectations can account for rapid variation in prices due to slow adjustment speeds. However, self assessed expectations include valuations of a large number of individuals that are not actively trading in the housing market as current or future buyers and/or sellers. Using listing and transaction information identifies faster adjustment of expectations for both current and future buyers/sellers, rather than using self-assess data.

Another approach for identifying the role of expectations uses *high frequency survey data* from a rotating panel of household heads to understand how expectations are formed and change over time. Using German data, Kindermann, Le Blanc, Piazzesi and Schneider (2021) highlight that expectations may differ significantly across agents when buyers and sellers are different, e.g., expectations are different depending on whether buyers may transition from-rent-to-own relative to those that remain homeowners (Armantier, Topa, Van der Klaauw and Zafar, 2017). A complementary approach solicits questions to panel respondents and allows econometricians to use *randomized control techniques* to measure differences in response between the treated and control groups.Armona, Fuster and Zafar (2019) use this methodology with the New York Fed Survey of Expectations where respondents are presented with factual information about past changes to re-elicit expectations. This approach suggests how expectations differ across the treated and the control group, although it is challenging to extrapolate measured beliefs to actual and prospective market participants. Our findings, using a quasi-experimental approach, indicate that even among the treated group there are different speeds of formation of expectations for buyers and sellers.

A traditional alternative to micro data has been to measure house price expectations using asset pricing relationships such as the user cost model e.g., Rosen (1979), Rosen (1985),Poterba (1984), Poterba (1992), Green and Vandell (1999), Glaeser and Shapiro (2003), Himmelberg, Mayer and Sinai (2005), Campbell, Davis, Gallin and Martin (2009), and Glaeser and Nathanson (2015). This approach exploits a no-arbitrage pricing relationship that uses available data on house prices, rents, and interest rates and measures expectations as an equilibrium residual. Expectations usually play an important role, and the challenge is to find a mechanism that rationalizes the role of expectations with the lack of rent growth that should be consistent with house prices trends.

To overcome these challenges, the macro-housing research has developed an alternative approach to understand the interactions between housing and the business cycle (Iacoviello, 2005; Davis and Heathcote, 2005). Davis and Van Nieuwerburgh (2015) and Piazzesi and Schneider (2016) present extensive summaries. There is a strand of research that has focused on the role of expectations and beliefs as the key driver of house prices (Adam, Kuang and Marcet, 2012; Kahn, 2008; Gelain, Lansing and Natvik, 2018). Other authors have used information frictions to capture housing outcomes (Barlevy and Fisher, 2010; Burnside, Eichenbaum and Rebelo, 2016; Rıos-Rull and Sánchez-Marcos, 2012).

The period 2000-2010 was a challenging hurdle for macro-housing models that endogenously determine house prices and rents. Landvoigt, Piazzesi and Schneider (2015) capture the cross-sectional changes in San Diego County during the boom using credit access. At an aggregate level, Favilukis, Ludvigson and Van Nieuwerburgh (2017) present a model with a shock to expectations on the state of financing constraints which leads to price booms. Garriga, Manuelli and Peralta-Alva (2019) show that the persistence of financing conditions such as mortgage rates and LTV requirements have large implications in matching the decoupling of price-rent dynamics. Kaplan, Mitman and Violante (2020) consider homeowners and investors with shocks to expectations that drive prices. Garriga and Hedlund (2020) interact the role of expectations about future credit conditions and labor market outcomes to replicate the macroeconomic performance during the boom-bust, including default rates. All these papers build on indirect inference that relies on simulation-based methods for estimating the parameters of economic models. They all use a structural framework to capture aspects of the data upon which to base the estimation.

Our paper contributes to structural housing research by presenting novel evidence based on a unique quasi-natural experiment which allows us to identify the causal impact of expectations effects on prices and rents. Our evidence represents a test for a large family of structural models that can be validated if they capture the market dynamics triggered by an identified expectations shock. There may be a large class of models consistent with the observed expectation shock, but we provide a simple macro-housing model including credit, land, and endogenous supply of structures that captures the timing and the disconnect between prices and rents. Our model shows that the inclusion of credit frictions in general equilibrium causes expectations shocks to generate a permanent gap between house prices and rents. While dynamics can be replicated in an arbitrage pricing model, the intuition behind this model does not extrapolate to general equilibrium. Appendix C discusses different specifications common in the literature that are special cases of our model and show that fail the test of replicating the key aspects of the identified shock.

3 Amazon HQ2: The Selection Process and Data Evidence

This section reviews the timeline of the process that Amazon followed to select a second headquarters in 2018. In this section we describe the data used in our empirical analysis. To motivate our use of the election process as a quasi-natural experiment which can be exploited using standard econometric methods, we examine whether the *HQ2* announcement was anticipated by the public or within local housing markets.

3.1 Amazon HQ2: Selection Process Timeline

On September 7, 2017, Amazon announced that it was searching for a second headquarters in North America. The reward for the chosen city providing the location for Amazon's *HQ2* was substantial: Amazon initially promised up to $5 billion in investment and up to 50,000 high quality jobs averaging over $100,000 per salaried worker at the location. In their announcement, Amazon cited a preference for metropolitan areas with over one million people, a friendly business environment, and an urban or suburban location. Furthermore, Amazon explicitly asked municipalities for incentives such as tax breaks and fee reductions.

Between September 17, 2017 and October 19, 2017, cities and metropolitan areas were invited to submit proposals to Amazon explaining why they were the ideal choice. Amazon requested information from respondents on incentives they could offer, potential building sites, community culture, and infrastructure accessibility. During this period, Amazon received 238 proposals from various cities, metropolitan areas, counties, and others. On January 18, 2018 Amazon announced that *20 finalists* had been selected for consideration as the location of *HQ2*. At this point, Amazon started to work closely with the finalists by visiting each city and meeting with local officials to discuss more specific details of their potential selection.[3] After the conclusion of this process, on November 13, 2018 Amazon unexpectedly announced that it would split *HQ2* between two locations: Crystal City in Arlington, VA, and Long Island City in New York City, NY. By splitting the headquarters, Amazon also halved the jobs and investment (25,000 jobs and $2.5 billion in investment) going into each location.

The selection of NY for *HQ2* received significant opposition. Local officials and community activists denounced the package of incentives offered to Amazon by the city and state as corporate

[3] The full list of finalists includes Atlanta, Austin, Boston, Chicago, Columbus, Dallas, Denver, Indianapolis, Los Angeles, Miami, Montgomery County MD, Nashville, Newark, New York City, Northern Virginia, Philadelphia, Pittsburgh, Raleigh, Toronto (Canada), and Washington D.C.

welfare, complained that *HQ2* would gentrify the neighborhood, and that the move would further exacerbate the affordability crisis in New York City. Amazon suddenly announced on February 14, 2019 that it was pulling out from its planned *HQ2* location in Long Island City.

3.2 Data Description

To estimate the effect of the Amazon *HQ2* decision on housing prices we use several public and proprietary data sources that we now describe. In order to account for residence and neighborhood heterogeneity we include covariates in our empirical analyses that affect house prices such as type of residence, square footage and other house attributes, as well as physical and socioeconomic characteristics of the neighborhood where the property is located (ZIP code).

We use the confidential CoreLogic's *Multiple Listing Services* (*MLS*) data which consists of real estate agent listings of individual properties and contains detailed information on residential listings and transactions at the unit level, including timing and pricing at listing and closing[4]. We make use of Close Price and List Price, both measured in dollars per square foot, as well as Time on Market (days), which is a measure of market liquidity. *MLS* also provides detailed information on property characteristics such as Square Footage, number of Bedrooms and Bathrooms, and Age of the structure. Most properties in *MLS* are single family homes or condominiums but to also account for apartments, townhouses, and residential income properties (properties purchased to be rented out), we control for property types[5] in our empirical analyses. Our sample from *MLS* was collected in January 2020 and includes data through October 2019. This provides roughly eleven months of transaction data after the first Amazon *HQ2* announcement.

The *American Community Survey* (*ACS*) five year estimates are used to collect data on ZIP level population, home ownership rate and the percentage of adults with a college degree. Population density (people per sq mile) is constructed by dividing the population by land area provided by the *U.S. Census Gazetteer* files. We use the *Internal Revenue Services' Statistics of Income* database (*IRS SOI*) to construct the average income of the top three income groups in each ZIP code. These top three income classes defined by the *IRS* correspond roughly to households with more than $100,000 in income, which we designate as the likely homeowners in these areas. To control for local housing affordability, we construct a measure of close price to earnings ratio, Price/Earnings, for each ZIP code by dividing the average annual transacted house price in the ZIP code as calculated from *MLS* by the average income of the top three income classes. Finally, to control for financing costs and macroeconomic conditions in the empirical models, we include the weekly 30-year fixed rate mortgage aggregated by Freddie Mac. This is a national rate that gets matched to every transaction held on the same week. During the time period of interest, this mortgage rate ranged between 4.81% and 3.65% (top/bottom decile) with an average of 4.29%.

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[4] We use a personal consumption expenditure index to deflate all prices to real values.

[5] The types of properties for-sale considered in the analysis include: single family, multi family, condos, townhouses, apartments, and residential income properties (properties purchased to be rented out).

Table 1: **Summary Statistics**

	Mean	Std. Dev.	Percentile 10%	Percentile 50%	Percentile 90%
Crystal City, VA					
Close Price/Sq.Ft	348.46	90.06	245.05	337.95	472.32
List Price/Sq.Ft	379.22	98.42	265.52	366.62	514.67
Time on Market	53.34	40.33	21.00	41.00	105.00
Square Footage	1,512.86	778.15	767.00	1,374.00	2,617.00
Age	56.44	26.58	12.00	67.00	80.00
Bathrooms	2.16	1.08	1.00	2.00	4.00
Bedrooms	2.41	1.07	1.00	2.00	4.00
Population Density	10,971.92	2,418.01	6,440.80	9,810.13	13,443.20
College	66.36	13.84	50.94	74.33	83.44
Price/Earnings	1.40	0.62	0.90	1.21	2.72
Ownership Rate	44.30	12.02	24.30	39.23	65.81
Non-Finalist Cities					
Close Price/Sq.Ft	151.65	82.90	77.51	131.39	250.01
List Price/Sq.Ft	170.45	95.42	87.83	146.93	279.50
Time on Market	86.76	68.48	32.00	64.00	174.00
Square Footage	1,932.09	871.52	1,037.00	1,736.00	3,085.00
Age	36.04	28.07	3.00	31.00	72.00
Bathrooms	2.29	0.87	1.00	2.00	3.10
Bedrooms	3.22	0.93	2.00	3.00	4.00
Population Density	2,544.12	2,164.86	293.12	1,999.01	5,498.90
College	32.92	15.19	14.69	30.26	54.34
Price/Earnings	1.55	1.58	0.50	1.07	2.95
Ownership Rate	67.53	14.83	47.23	69.75	85.40

Notes: Variables and units of measurement are defined in the text. The sample period is a twelve month window around Amazon's *HQ2* announcement date and it includes information for 2,734 in VA and 2,447,887 in a set of other cities that were not selected by Amazon. This is the sample with complete regressor information. We also conduct the econometric analysis in the text with larger samples not including some or all residence attributes.

For market-specific rental prices, we use Zillow's *Observed Rent Index* (*ZORI*). Zillow describes this dataset as a smoothed, seasonally adjusted measure of their observed market rate rent in a given ZIP code. This rental rate is weighted to ensure rental home representativeness within the ZIP code and is the mean of rents in the 40th to 60th percentile price range. This data is available for ZIP codes on a monthly basis.

In Table 1, we report summary statistics of our sample in Crystal City and in a set of large control metro areas unrelated to the *HQ2* selection process further documented in Section 4.1. Crystal City, VA is a relatively expensive and densely populated market where houses are similar in size, about 1,500–1,600 square feet. We find that close prices (always smaller than list prices on average) exceed $348 per square foot. In general, sellers across cities are in general overoptimistic for the entire sample period. For the control cities close prices are about 10% lower than listing prices when measured in dollars per square foot as compared to 8.8% in our VA sample. Notably, Crystal City is a more liquid housing market than cities that were not finalists for *HQ2*, with 53 rather than 87 days on the market for sales to take place.

3.3 A Quasi-Natural Experiment

We argue that Amazon's *HQ2* announcement amounts to an unanticipated shock. We first document Google search data around the announcement windows and find no preexisting trends hinting at a public anticipation of the decision. We corroborate this lack of anticipation by reviewing betting odds of finalists to win the *HQ2* lottery. When examining price trends around the announcement date, we find that there is a large discontinuity in transacted close prices in Crystal City relative to other metropolitan areas when the *HQ2* announcement occurred.

3.3.1 An Unanticipated Shock: Evidence from Google Search and Betting Odds

Amazon successfully kept the identity of the winner secret from the wider public until the announcement day. Moreover, there is no indication anyone anticipated Amazon's decision to split the location of its *HQ2* in two cities. Amazon valued discretion in selecting their *HQ2* and went as far as signing non-disclosure agreements with local officials to prevent details of potential preference for particular locations from leaking. If Amazon had not succeeded in their secrecy goal, the public could have had opportunity to act upon the leaked information. Widespread anticipation of the announcement would challenge the identification of our empirical strategy which relies on the exogeneity of the announcement to market participants.

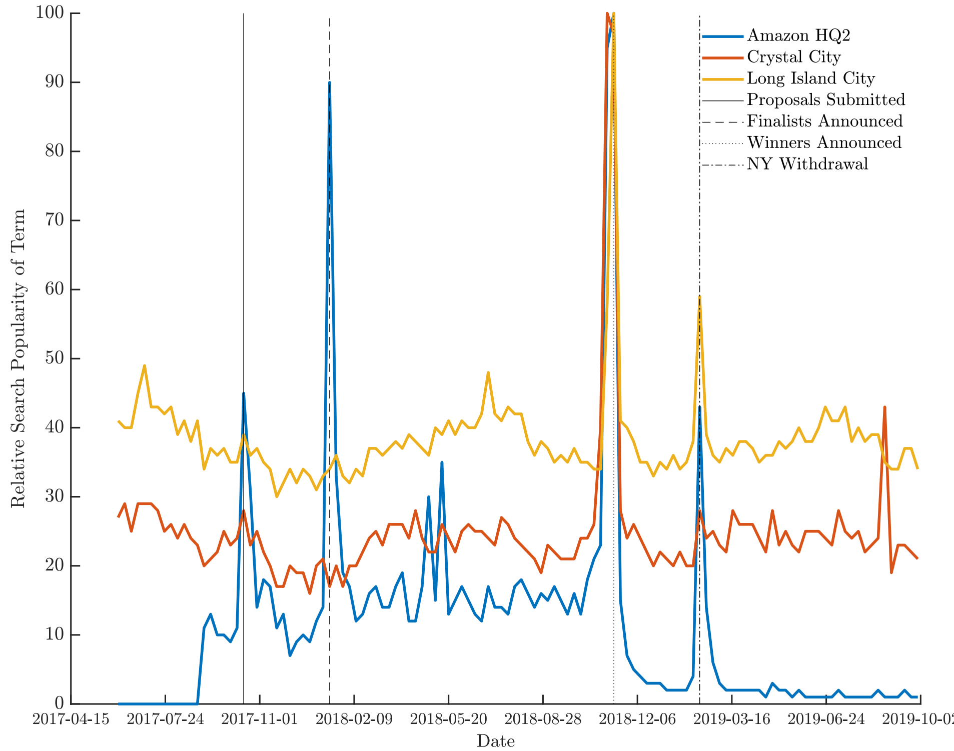
To assess whether the broader public anticipated Amazon's *HQ2* announcements, Figure 1 plots data from Google Trends that shows the relative Internet search popularity of the winning locations and the term "Amazon *HQ2*."[6] Each line represents the popularity of the search term relative to itself, so a value of 100 represents peak popularity. The figure shows that search popularity "Amazon *HQ2*" peaked four times. These occasions (marked on the plot) correspond to the announcement of a search for *HQ2*, the unveiling of the 20 finalists, the selection of the winning locations, and the withdrawal from NY. By contrast, the search term "Crystal City" only peaks once with its selection as a winning location for *HQ2*, whereas "Long Island City" surges in popularity for the winning and withdrawal announcements.

We further document anecdotal evidence from betting odds between the 20 finalist that also indicate that the announcements were mostly unanticipated for the winning locations. For example, Moody's Analytics placed strong odds for Austin, Atlanta, Philadelphia, Rochester, and New York City. The Irish gambling site *Paddy Power* marked Atlanta and Austin as the most likely winners, and the *New York Times* identified Denver, Boston and D.C. as the strongest contenders.[7]

[6] Variants of the search term "Amazon *HQ2*" such as "*HQ2*" or "Amazon Headquarters" yield identical results.

[7] We also study the listing behavior of residential properties as an alternative approach to evaluate the anticipation of Amazon's announcement. Potentially seller's *en masse* anticipated their houses would become more valuable and the number of listings in Crystal City would increase as some homeowners would seek to cash out their capital gains. We use *MLS* data to show that the number of listings expressed as a percent change from the previous year ago does not change significantly around Amazon's *HQ2* announcement date. We plot the time series listings of the winning cities, their surrounding metropolitan areas and the listings in other finalist locations in Figure D.1 in Online Appendix D.

Figure 1: Google Trends for Amazon HQ2



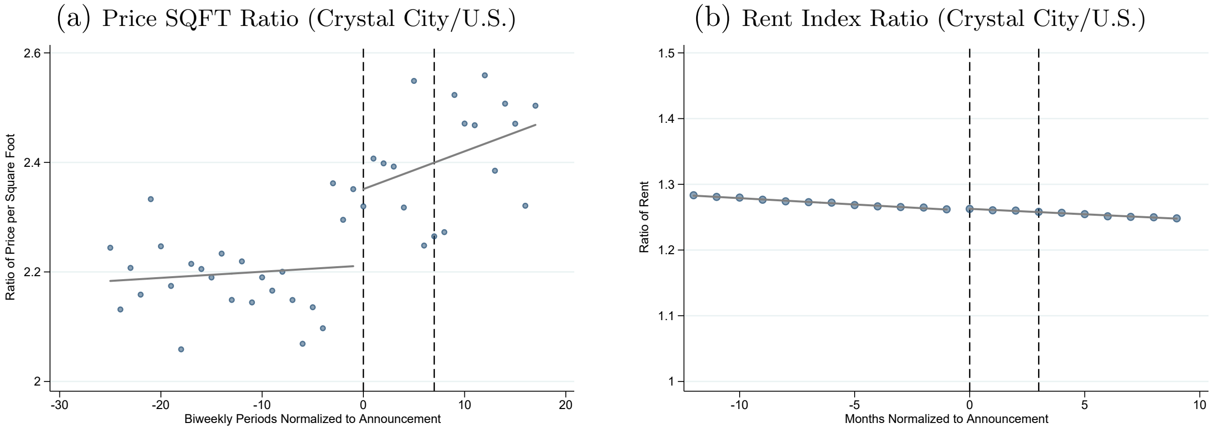
Notes: Relative search popularity of "Amazon HQ2," "Crystal City," and "Long Island City" during Amazon *HQ2* selection process.

The combination of Google search term popularity and betting odds suggests that at least for the public in general, the announcements were unanticipated.

3.3.2 Price Changes Around HQ2 Announcement

We now complement this narrative evidence regarding the public's anticipation of the *HQ2* announcement with the effect of the announcement on transacted real estate prices. The impact of the choice of *HQ2* can be visualized in Figure 2. Panel (a) depicts the ratio of average close prices in Crystal City relative to close prices in a large set of non-finalist metropolitan areas (to be described below in Section 4.1) for biweekly periods around the announcement. Panel (b) plots the relative rental indices of Crystal City relative to the same set of metro areas. We plot linear regression lines over the ratios to illustrate trends around the announcements, with a trend estimated for both the pre- and post-shock periods. The first dashed vertical line represents the original winning *HQ2* announcement, while the second dashed vertical line represents Amazon's withdrawal from the Long Island City location. After the initial announcement, Crystal City experienced an immediate and notable increase in close prices and a notable increase in the upwards trend in housing prices. Both the raw ratios and trend lines through them indicate that the period around the announcement

Figure 2: House Prices and Rents Relative to the Control, Crystal City



Notes: The left panel show bi-weekly average close price per square foot, Close Price/Sq.Ft, ratio in Crystal City relative to the control group around Amazon's *HQ2* announcement date and the withdrawal from Long Island City, respectively. The right panel show the monthly rent price ratios relative to the control group around Amazon's *HQ2* announcement dates.

represents a visual discontinuity in the preexisting trend[8]. The graphical evidence suggests that the *HQ2* announcement was associated with an immediate and clear change in price trends in Crystal City.

In contrast to the changes in house prices, rent price indices did not experience any type of discontinuity in Crystal City around the announcement periods. The lack of movement in rents may not be unexpected as our sample consists of a period before the expected income gains become realized. Without real income changing in the neighborhood during our sample, rent prices largely continue on their pre-announcement trends. The rent indices estimated by Zillow increased during this period for the treated and control groups, but rent growth was faster in the latter, which explains the slightly declining trend for VA relative rent index ratio.

4 Evidence of a House Price Expectations Shock

In this section we address the impact of Amazon's *HQ2* decision on the real estate market, with particular attention to its influence on price expectations. We compare the performance of the real estate market of Crystal City against a set of non-finalist cities to estimate causal effects of the *HQ2* shock. We consider different post-announcement time windows to measure the dynamic reaction of local housing markets to the shock. In particular, we study realized (close) prices, seller expectations (listing prices), and housing liquidity (time on the market).

[8] We plot in Appendix Figure B.1 Panel a) the same price ratios for the winning location Long Island City, NY neighborhood. On both the winning and withdrawal announcements, there is a clear change in trend lines reflecting the two shocks in this neighborhood. However, the price data is significantly more volatile than in Crystal City, VA. We find no evidence of anticipatory price pre-trends for either shock in the Long Island City neighborhood, but also do not graphically confirm discontinuities of trends and levels of prices as clearly.

We utilize different estimation methods, control groups, and time frames to convey a simple but robust message: an expectations shock about future economic activity and housing demand have quantitatively important effects on current house prices. Amazon's *HQ2* announcement had a real impact on real prices, seller expectations and market liquidity. Six months after the announcement, housing prices in Crystal City, VA increased at least $26 per square foot relative to similar residences in other markets. Seller's increased their expectations of the values of their homes upon impact and persistently priced their homes $30 additional dollars per square foot relative to before the announcement. These increases are very significant, and for the average-sized residence it amounts to an increase in realized house values of between $40,000 and $55,500, or about 7.5%.

4.1 Treatment and Control Groups

Our analysis compares the dynamics of expected and realized housing prices and liquidity across cities in the US before and after the announcement of Amazon's *HQ2* choice. Our first task is to define what geographic locales should be considered treated by this announcement and which locales are appropriate control groups. We define the treated areas here as the ZIP codes of the neighborhoods Amazon chose to be *HQ2* locations, along with neighbors adjacent to these ZIP codes[9]. In our baseline, this means the treated area is Crystal City and any ZIP codes geographically adjacent to Crystal City.[10] These treated area is relatively narrowly defined, representing a collection of neighborhoods within a broader metro area. Using a precise definition of the Crystal City neighborhood allows us to estimate the direct effect of the *HQ2* announcement on the geography directly associated with the *HQ2* shock.

For our empirical analyses we need a suitable control group of cities not affected by Amazon's *HQ2* decision to locate in Crystal City. Using other neighborhoods in Washington D.C. or the broader Northern Virginia region might seem an obvious first choice as a control group. These surrounding neighborhoods are similar from a demographic and regulatory standpoint and, in addition, regional housing and economic trends would likely be similar even after controlling for different price and income levels. However, these very close neighborhoods are likely affected by Amazon's *HQ2* decision to locate nearby. Since we cannot rule out the existence of significant spillovers on the surrounding areas, using them as controls could easily lead to important econometric bias in the estimation of the effect of the expectation shock on housing market prices and liquidity. As we believe it is likely that the broader metro area experienced price changes due to the announcement of *HQ2* we instead focus our analysis on changes in prices relative to other metro areas.

To alleviate concerns of regional price spillovers, our preferred control group consists of neighborhoods in metropolitan areas that Amazon *did not seriously* consider for *HQ2*, i.e., non-finalist cities[11]. Despite not being considered by Amazon in the preliminary selection round, our

[9] The list of ZIP codes is available in Online Appendix Table D.1

[10] Appendix B uses the same methodology in defining the treated area of Long Island City, NY.

[11] The list of metro areas used in the non-finalist control group is available in Online Appendix Table D.2

control group consists of large metropolitan areas with characteristics that Amazon valued in their selection process, such as access to universities, public transportation, and a skilled labor force. Furthermore, these areas were not affected by any of the Amazon announcements as they were not finalist contenders for *HQ2*. We prefer this control group to an alternative control group comprised by finalist cities, which are more likely to have experienced anticipatory real estate market effects due to the *HQ2* selection process.[12] The control group used in the empirical analysis includes one year of real estate transaction data before Amazon's *HQ2* announcement and up to eleven months afterwards in 75 Core-Based Statistical Areas (*CBSA*'s).[13]

4.2 Treatment Effects: Expectations, Prices, and Housing Liquidity

We use a standard difference-in-differences (*DID*) estimator to evaluate the effect of the *HQ2* shock on the local housing market is on three variables measuring the performance of real estate markets: $Y = \{\text{Close Price/Sq.Ft}, \text{List Price/Sq.Ft}, \text{Time on Market}\}$. Each element of Y captures a different aspect or interest to evaluate the *HQ2* shock affected the performance of local housing markets. Close Price/Sq.Ft measures transacted real estate prices. List Price/Sq.Ft measures housing price expectations, andTime on Market is a measure of housing liquidity given by the time it took for a home to sell. The model specification for these three variables of interest is:

$$\begin{aligned} Y_{ijzmt} = \alpha \; &+ \beta_{\text{Announcement}} \mathbb{1}(t > T) + \beta_{\text{Winner}} \mathbb{1}(m = \text{Winner}) \\ &+ \beta_{HQ2} \mathbb{1}(t > T) * \mathbb{1}(m = \text{Winner}) + \lambda_j + \mu_m + \tau_t \\ &+ \gamma X_{ijt} + \theta W_z + \psi Z_t + \varepsilon_{ijzmt} \, , \end{aligned} \tag{1}$$

where the outcome variable Y_{ijzmt} refers to property i of type j in ZIP code z of metropolitan area m at time t. We also include fixed effects for residence type, λ_j; metropolitan area, μ_m; and month, τ_t, to account for seasonality. Amazon announced its *HQ2* decision at time T. A dummy variable identifies observations before and after this date to measure the effect of the announcement on price (or any other variable of interest) across different locations in the country. Another dummy variable identifies the observations from the winning city to account for city-specific unobservable reasons behind higher prices or lengthier times of vacant houses on the market. The product of these two dummies capture our object of interest: the effect of Amazon's *HQ2* announcement on the subsequent performance of the real estate market in the selected location.

In addition, to account for other sources of observable market heterogeneity, we include other covariates, X, to control for property characteristics such as square footage, age of residence, number

[12] For the sake of completeness, we repeat the analysis with the set of finalist cities as an alternative control group to evaluate the robustness of our results in Section 4.3 and Appendix A.

[13] *CBSA* is a U.S. geographic area defined by the Office of Management and Budget that consists of one or more counties anchored by an urban center of at least 10,000 people plus adjacent counties that are socioeconomically tied to the urban center by commuting.

of bathrooms, and number of bedrooms at the transaction date. We also include time invariant socioeconomic characteristics, W, comprising population density, rate of college attainment, the price-to-earnings ratio, and the home ownership rate at the ZIP code level. Finally, we also account for the national time-variant financial indicator Z, the weekly mortgage rate.

We argue that β_{HQ2} identifies the average treatment effect of the *HQ2* expectations shock on the outcomes in Crystal City. For this identification to be valid, it must be the case that the parallel trends hypothesis holds in our sample e.g., Abadie (2005) and Angrist and Pischke (2009, §5.2). For identification we assume the difference in outcomes between Crystal City and the non-Finalist control group would have remained unchanged had the *HQ2* shock not occurred. We showed in Section 3.3.1 that the public was unlikely to have known the winning locations prior to the announcement. More formally, we test whether there existed significant price effects associated with Crystal in the pre-announcement period by implementing Laporte and Windmeijer (2005) dynamic treatment estimator and test for treatment effects for every month in our sample. These results can be viewed as a placebo for whether prices were changing prior to the announcement. We report the estimated treatment effects in Table D.3. In summary, there is no sign of any pre-trend for close prices. For list prices, some months in the pre-announcement sample report significant effects but they are not persistent and are dwarfed in magnitude by the size of the treatment effects estimated after the announcement. Section 4.3 implements an additional synthetic controls estimator matching prices during the pre-announcement period between the treated and control local housing markets. We find even stronger evidence to discard that Amazon's *HQ2* announcement was anticipated. Given the cumulative weight of the evidence we believe the parallel trends assumption is reasonable and thus we interpret the estimated β_{HQ2} as the causal treatment effect of the *HQ2* shock to price expectations.

Table 2 presents the estimates of the average treatment effects (ATT) of Amazon's *HQ2* announcement on the closing price of residential housing in VA evaluated six months after the announcement. This time frame appears reasonable to capture the short term effect of this economic news given the fact that we documented that average time on the market for properties in Crystal City is 53 days. The different specifications aim to produce robust estimates of the key variable of interest, *HQ2*. Model VI is our preferred specification, using all covariates described in equation (1).

The results are robust across various specifications and indicate that houses are more expensive in highly dense areas where college graduates tend to live.[14] These educated residents, likely with higher income, favor more expensive neighborhoods, perhaps because of their associated (non-observable) amenities.[15] Indeed, the price effect of the price-to-earnings nearly doubles

[14] We Winsorize the sample for the top/bottom 1% for all endogenous and exogenous regressors, separately for treatment and control groups, to accommodate that our neighborhoods of interest are among the most dense and expensive in the country.

[15] House prices are partially explained by observable and unobservable attributes in addition to expectations, the focus of our research. Bajari, Fruehwirth, Kim and Timmins (2012) show how past transaction prices can be used to control time-varying unobservable attributes. Our analysis evaluates the short term effect of expectations on housing prices. It is unlikely that housing features or neighborhood amenities change significantly in less than a year. We thus assume that unobservable attributes are invariant to the *HQ2* announcement. Controlling for

Table 2: Crystal City: Amazon's HQ2 and Close Prices

	I	II	III	IV	V	VI
Announcement	-0.242*	-0.259	1.704***	1.945***	3.223***	4.291***
	(0.132)	(0.337)	(0.293)	(0.309)	(0.298)	(0.285)
Winner	180.178***	173.413***	14.044	13.961	88.601***	88.467***
	(2.247)	(16.379)	(14.087)	(14.078)	(13.234)	(13.259)
HQ2	30.195***	30.508***	27.580***	27.520***	26.717***	26.286***
	(4.235)	(3.749)	(2.415)	(2.449)	(2.596)	(2.371)
Square Footage		-0.004***	-0.017***	-0.017***	-0.013***	-0.013***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age		0.014	-0.398***	-0.399***	-0.099***	-0.099***
		(0.031)	(0.030)	(0.030)	(0.020)	(0.020)
Bathrooms		19.732***	14.128***	14.129***	11.632***	11.585***
		(1.128)	(1.067)	(1.067)	(0.539)	(0.539)
Bedrooms		-10.974***	-5.608***	-5.607***	-7.554***	-7.533***
		(0.839)	(0.724)	(0.724)	(0.490)	(0.490)
Population Density			0.008***	0.008***	0.003***	0.003***
			(0.001)	(0.001)	(0.000)	(0.000)
College			2.566***	2.567***	2.290***	2.291***
			(0.110)	(0.110)	(0.059)	(0.059)
Price/Earnings			8.115***	8.117***	3.874***	3.865***
			(0.550)	(0.550)	(0.408)	(0.408)
Ownership Rate			-0.483***	-0.484***	-0.225***	-0.228***
			(0.114)	(0.114)	(0.071)	(0.071)
Mortgage Rate				2.750***	7.175***	6.423***
				(0.447)	(0.336)	(0.360)
Constant	150.832***	147.437***	65.020***	52.818***	18.155**	22.238***
	(0.075)	(9.106)	(9.840)	(9.899)	(8.602)	(8.599)
Property Type FE	No	No	Yes	Yes	Yes	Yes
Metro FE	No	No	No	No	Yes	Yes
Month FE	No	No	No	No	No	Yes
R-Squared	0.006	0.050	0.266	0.266	0.608	0.609
Observations	1,828,120	1,828,120	1,828,120	1,828,120	1,828,120	1,828,120
Treated Obs.	2,049	2,049	2,049	2,049	2,049	2,049

Notes: Endogenous variable is Close Price/Sq.Ft. Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus six months after. *OLS* regression. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

the positive effect of the college attainment rate and is several orders of magnitude higher than population density, all of which supports the idea of gentrification following the arrival of thousands of highly paid employees working at Amazon's *HQ2*. As for house features, there is a clear premium for additional bathrooms that exceeds the discount for an additional bedroom. We also find that larger and older houses are sold at a very small but significant discount.

unobserved house attributes is not possible with a cross-section of housing transactions. Instead, we control for unobserved neighborhood amenities by means of location fixed effects.

Table 3: **Crystal City: Amazon's HQ2 Event Study (Non-Finalists)**

	1 Month	3 Months	6 Months	12 Months
Y = Close Price/Sq.Ft				
Announcement	-0.200	1.471***	4.291***	5.211***
	(0.307)	(0.365)	(0.285)	(0.278)
Winner	88.190***	88.510***	88.467***	90.741***
	(13.257)	(13.220)	(13.259)	(13.278)
HQ2	18.021***	18.627***	26.286***	30.400***
	(3.600)	(3.897)	(2.371)	(2.690)
R^2	0.615	0.612	0.609	0.607
Observations	1,345,775	1,498,820	1,828,120	2,533,959
Treated Obs.	1,475	1,687	2,049	2,734
Y = List Price/Sq.Ft				
Announcement	7.010***	3.388***	-1.024*	-2.369***
	(0.442)	(0.490)	(0.607)	(0.625)
Winner	89.505***	88.708***	88.937***	90.807***
	(14.257)	(14.293)	(14.356)	(14.382)
HQ2	31.102**	11.645**	29.796***	32.589***
	(14.586)	(5.153)	(1.633)	(3.303)
R^2	0.544	0.541	0.543	0.548
Observations	1,722,820	1,933,490	2,384,564	2,898,327
Treated Obs.	1,679	1,834	2,321	2,878
Y = Time on Market				
Announcement	13.675***	22.115***	12.052***	4.117***
	(0.324)	(0.283)	(0.207)	(0.206)
Winner	-42.513***	-42.669***	-43.214***	-43.566***
	(4.691)	(4.549)	(4.497)	(4.454)
HQ2	-1.416	2.549	-11.941***	-11.976***
	(2.825)	(3.142)	(2.427)	(2.135)
R^2	0.100	0.113	0.115	0.094
Observations	1,767,417	1,991,587	2,414,427	3,337,129
Treated Obs.	1,747	1,979	2,364	3,143

Notes: Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus the indicated number of months after. *OLS* regression including transactional covariates as well as property type, metropolitan area, and month fixed effects. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

The positive effect of Announcement captures the fact that house prices were increasing slightly nationwide at the time of Amazon's *HQ2* split decision, resulting in a \$4 increase in the six months following Amazon's *HQ2* announcement. This corresponds to a 1.2% price increase or \$6,492 for the average sized house in Crystal City relative to the control group of non-finalist cities. Crystal City is however significantly more expensive, about \$88 per square foot as captured by the estimate of Winner. This indicates that regardless of Amazon's actions, the average VA sized house is nearly $134,000 more expensive than a similar house in a non-finalist city.

The main variable of interest of our study is *HQ2*, the interaction of the dummies Winner and Announcement, which captures the *ATT* of Amazon's decision to locate in VA, measured as the additional increase in price of transacted residences in Crystal City relative to similar transactions after the announcement decision in neighborhoods of non-finalist metropolitan areas. Our preferred Model VI indicates that Amazon's *HQ2* decision causes the price per square foot in VA to increase by $26 six months after the split headquarters decision was announced. The top panel of Table 3 shows that most of the real estate appreciation is realized within six months of Amazon's announcement. This 7.5% appreciation, the Amazon effect, increases the value of the average sized home in VA by almost $40,000 relative to houses in non-finalist cities.

Table 3 summarizes the causal effect of Amazon's *HQ2* announcement on other real estate related variables for various time windows. We find that close prices increased significantly within three months after the *HQ2* shock. The point estimate of the effect on close prices is always significant and it increases as we consider longer time windows. This indicates that over time, the *HQ2* shock was priced into the market. It is likely that close prices within three months of the *HQ2* announcement are likely lower than later observed values due to frictions associated to time needed to purchase a house since many of the houses in the first three month window were listed *prior* to the *HQ2* announcement.

The gradual adjustment of transacted prices contrasts to seller expectations of their home values as measured by list prices. List prices increased in Crystal City by $31 additional dollars per square foot almost immediately after the announcement. This effect was persistent and largely constant over our sample period, with an effect on list prices of over $32 per square foot within a year of the announcement. The are no frictions to adjust list prices and thus, these larger list price effects indicate sellers were immediately more optimistic due to the *HQ2* shock. The list price results indicate seller expectations of home value increased significantly and consistently over the sample in response to the announcement.

We also find that the *HQ2* shock improved market liquidity, as measured by Time on Market. Although effects are insignificant in the first 3 months after the shock, Time on Market gets reduced by nearly 12 days six months after the announcement. This reduction corresponded to a 22% increase in housing liquidity in Crystal City during this period. This finding highlights how liquidity can adjust even more rapidly than prices in areas with low housing supply elasticity as in Famiglietti, Garriga, Hedlund (2020). The effects on liquidity were persistent after six months over our sample when the existing inventory cleared and remained largely unchanged after nearly 12 months after the *HQ2* announcement.

4.3 Robustness of the Expectations Shock Effects

We conduct a battery of robustness checks on these results. Our robustness analysis falls broadly under two categories: i) definition of the control group and ii) estimation methods. We discuss the robustness results for each category of market performance. We only report the main conclusions of this robustness analysis in the main text to ease the exposition. Appendix A provides additional estimates and offers a much more detailed discussion.

4.3.1 Control Group

Our preferred control group consists of metropolitan areas not selected as finalists for *HQ2* by Amazon. This group could be questioned on the basis of sample selection: Amazon choosing not to include them among the finalists might be revealing of some fundamental differences, observable or otherwise, across local real estate markets that might be relevant for the *HQ2* selection. The key econometric issue is whether these differences affect the estimates of the effect of the *HQ2* announcement on price expectations.

To evaluate this potential shortcoming, we repeat the analysis of Section 4.2 using the metropolitan areas of the 14 finalists we have data for. The advantage of using the finalist control group is that it is comprised of the metro areas with strong cultural, business, and demographic similarities or qualities that Amazon found desirable. Some of them, e.g., Boston, Chicago, Miami or Los Angeles, are also similarly expensive to the winning location of Crystal City. Using similarly priced cities cities as a control group would provide a close counterfactual to the Crystal City area.[16]

Results using the finalists as a control group are reported in Table A.2 in Appendix A, where we report and discuss them extensively. To summarize, results are highly consistent with those of Section 4.2. Six months after the *HQ2* announcement, close prices increased \$27, list prices increased nearly \$30, and time on the market declined by 12 days. These results are nearly identical compared to using the non-finalist cities as a control group, which speaks to the robustness of our estimated effect of the *HQ2* expectations shock on housing market conditions.

4.3.2 Estimator Robustness

To test the robustness to our preferred *DID* estimator, we recover causal effects of the *HQ2* shock using a variety of parametric and non-parametric estimators. Our primary motivation for checking the robustness of the estimator is to evaluate whether more precise counterfactual groups still validate the *DID* results presented in Section 4.2.

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[16] See Online Appendix E for a detailed discussion of whether the *HQ2* shock or announcement of finalist locations generated significant effects in the housing markets of these finalist cities. In summary we find no evidence that the announcement that the locations were to become finalists had any significantly positive effect on residential markets.

Our first alternative is to use Coarsened Exact Matching (*CEM*) to estimate the causal effects of the *HQ2* shock on close prices, list prices, and liquidity in Crystal City.[17] *CEM* is a nonparametric matching algorithm that returns a weighted counterfactual control group highly similar to Crystal City based on ex-post *HQ2* shock covariates. This alternative estimator also produces results that are very similar to the *DID* estimates, with close prices per square foot rising \$28.4, list prices per square foot appreciating \$31.1, and Time on Market declining 10.9 days.

To complement the post-shock *CEM* algorithm, we also use a synthetic control estimator and match the aggregated Crystal City neighborhood to a weighted combination of either non-finalist or finalist neighborhoods, respectively, based on pre-shock observables.[18] The synthetic control method has an additional benefit, in that it allows us to match our counterfactual group to pre-announcement characteristics of Crystal City, including prices leading up to the announcement. We find that, if anything, *DID* understates the *ATT* in Crystal City, as the average post-*HQ2* effect on close prices is \$41.3 per square foot, with a maximum effect reached approximately 5 months after the shock. The size of this effect while directly matching our counterfactual to prices before the announcement speaks to the limited ability of any pre-trend in driving our results and provides indirect evidence for our identification assumptions.

Finally, we documented that house prices in VA increase slightly ahead of Amazon *HQ2* announcement, both using the raw data in Figure 2(a) or the the synthetic control methods as illustrated in Figure A.1. We thus test whether this price movement is significant relative to our control group by implementing Laporte and Windmeijer's (2005) dynamic treatment estimator and testing for treatment effects in every month of our sample. We find no statistically significant change in prices prior to the announcement that cannot be explained by other location conditions. The results are reported online in Table D.3. In summary, our estimates are robust to alternative control groups and to estimators that generate counterfactuals by matching characteristics on ex ante or ex post housing market characteristics.

4.4 Treatment Effects: Rental Prices

The standard user cost model or the canonical macro-housing model relies on a tight connection between current house prices and the path of rents. Empirically, there is evidence of a disconnect between prices and rents (Shiller, 2007; Garriga et al., 2019). Hence, a natural question is to what extent did rental prices move during after the *HQ2* announcement? We use rental price data in the treated and non-finalist control locations to address this issue. Our primary data source of *MLS* is highly concentrated in the for-sale real estate market, so for disaggregated rental market data we use monthly information from *ZORI*, the Zillow Observed Rent Index. We assign treatment to the same set of ZIP codes as the baseline analysis and use the non-finalist set of MSA's as the control group. The right panel of Figure 2 depicts the ratio of monthly rent data in the treatment and

[17] For a full discussion of the estimator and results see Appendix A.2.

[18] We describe our implementation of the synthetic control method and present detailed results in Appendix A.3.

Table 4: **Event Study of Crystal City Rents (6 Months Post-Announcement)**

	HQ2 Announcement	
	Unweighted	**Weighted**
Y = Rents		
ATT	0.598	2.518
	(3.207)	(2.587)
City	191.458	137.120
	(127.865)	(132.282)
Announcement	68.416***	68.691***
	(1.749)	(1.522)
Metro FE	Yes	Yes
Month FE	Yes	Yes
R-Squared	0.754	0.789
Observations	24,462	24,462
Treated Obs.	54	54

Notes: Sample includes one year of monthly ZIP code observations prior to Amazon's *HQ2* announcement date plus six months afterwards for the columns "Unweighted" and "Weighted". *OLS* regression including mean transactional covariates as well as share, metropolitan area, and month fixed effects. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

control ZIP codes. Unlike the trends in house price, the rental prices do not appear to experience any discontinuity or change during any of the key announcements dates.

To more explicitly test whether there was a significant effect on rents during this period, we use the ZORI data and modify model (1) to a monthly ZIP-aggregated *DID* estimator:

$$\begin{aligned} R_{jzmt} = \alpha \ &+ \beta_{\text{Announcement}} \mathbb{1}(t > T) + \beta_{\text{Winner}} \mathbb{1}(m = \text{Winner}) \\ &+ \beta_{HQ2} \mathbb{1}(t > T) * \mathbb{1}(m = \text{Winner}) + \omega_j + \mu_m + \tau_t \\ &+ \gamma X_{jt} + \theta W_z + \psi Z_t + \varepsilon_{jzmt} \, , \end{aligned} \tag{2}$$

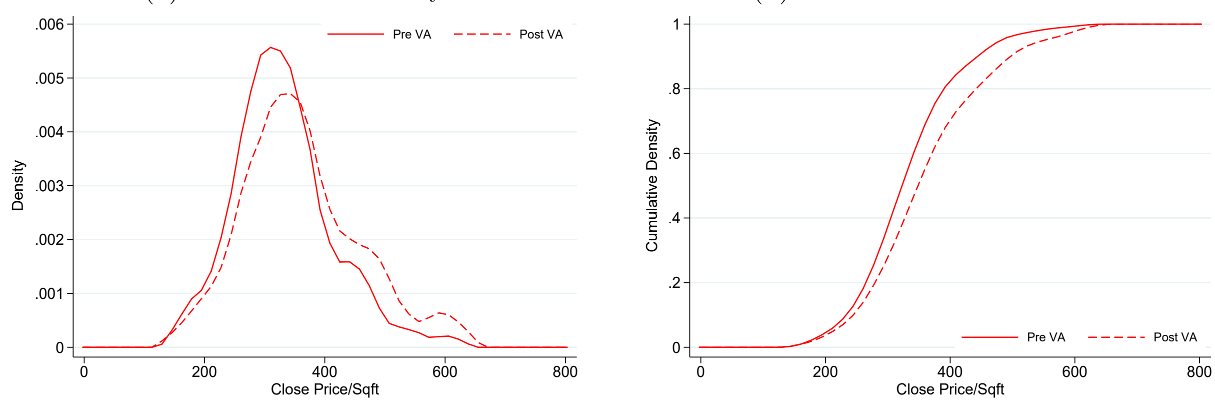
where R_{jzmt} is the observed rent in ZIP code j given by the *ZORI* data, and ω_j is the share of single-family homes for sale in a given ZIP code in month. For the purpose of identification, we assume that property characteristics within a ZIP code do not differ between tenant and owner-occupied housing units. We designate November 2018 as the period t where the announcement took place to conform to the monthly nature of the rental data. We estimate Equation 2 using one year (12 months) of data prior to the *HQ2* announcement and six months of post-announcement data. We estimate two specifications, one where each ZIP code is given an equal weight and the alternative where the rent index of each ZIP code is population weighted. The causal effect estimates on rental prices for the model are reported in Table 4. Both weighted and unweighted treatment effects suggest that there is no significant effect on rent appreciation in the treated neighborhoods around the announcement. This evidence suggests a disconnect between house prices and rents, which is a topic that we further explore the implications of theoretically in Section 5.

4.5 Heterogeneity in Price Distribution Response to an Expectations Shock

Our empirical analysis thus far has obtained robust *average* effects of the impact of Amazon's decision to locate in Crystal City, VA. By construction, these estimates are averages across all residential homes and ignore that causal effects may be heterogeneous across different real estate market segments. This is an important issue as the macro-housing literature argues that credit shocks can generate different responses across price tiers due to market segmentation[19].

Figure 3: Pre/Post Close Price Distribution in Crystal City

(a) VA Close Price Density (b) VA Close Price Distribution



Transaction data allow us to document the shifts in the distribution of prices. Figure 3 presents the probability density and distribution functions of Close Price/Sq.Ft in Crystal City before and after Amazon's *HQ2* winning announcement. The distributions shift for to the right for Crystal City, but remain virtually unchanged for the set of other finalist and non-finalist cities.[20]

How significant are the distribution shifts? Table 5 reports simulation-based, first and second order, Kolmogorov-Smirnov tests of Barrett and Donald (2003) for stochastic dominance. We test whether the distribution of Close Price/Sq.Ft before the announcement dominates the distribution of Close Price/Sq.Ft after the announcement as well as the opposite hypothesis.

We reject the null whenever the reported p-values exceed $\alpha = 0.05$. We thus conclude that the post-announcement distribution of Close Price/Sq.Ft first (and therefore second) order stochastically dominates the pre-announcement distribution in Crystal City, VA. In other words, the increase in price expectations after Amazon's *HQ2* decision is widespread and benefits sellers of all kind of residential homes, big and small, expensive or affordable. The shift in prices affects all market price segments with perhaps a slightly larger effect at the upper end.

[19] Landvoigt, Piazzesi, and Schneider (2015) use micro-data on San Diego County during the 2000s boom, and find that cheaper credit for poor households was a major driver of prices, especially concentrated at the bottom price tier of the market.

[20] Figure D.3 in Online Appendix D calculates the shifts in the distribution for the control cities.

Table 5: Stochastic Dominance Tests

	$H_0 : F(P_{pre}) \leq_i F(P_{post})$		$H_0 : F(P_{post}) \leq_i F(P_{pre})$	
HQ2 Location	**FOSD**	**SOSD**	**FOSD**	**SOSD**
Crystal City, VA	0.000	0.000	1.000	0.808

Notes: Test reports p-values of the Kolmogorov-Smirnov test of stochastic dominance obtained by simulating the maximal difference of two cumulative distribution functions over an evenly spaced grid of 100 points covering the whole support of each simulated distribution of Close Price/Sq.Ft using 1000 replications. This is the "KS1" test in the notation of Barrett and Donald (2003).

The visually larger shift of the right tails of Figure 3 is perhaps not surprising as the jobs created by Amazon are expected to be highly paid. Given that approximately half of households are homeowners in Crystal City, the *HQ2* decision likely provides a large windfall to many in this community in the form of higher realized housing value due to an expectation shock. Therefore, while there may be some heterogeneity by market segment, because the *HQ2* shock significantly shifted the entire house price distribution higher, we interpret our causal ATT estimates to be a meaningful measure for the entire housing market within this geography. This has implications for macro-housing research, as even if segmentation is an intrinsic feature of housing markets, prices across segments can respond to expectations shocks similarly.

4.6 Long Island City: We Had Everything Before Us, We Had Nothing Before Us

The case of Long Island City, NY is equally interesting, but presents unique and additional challenges. In this market, real estate prices are three times the average income, which more than doubles the price-to-earnings ratio of Crystal City. Long Island City also has a very high population density, with New York City being the most densely populated city in our sample, and with low levels of home ownership and housing market liquidity. Still, what makes differentiates Long Island City for our study is the fact that Amazon caved to opposition by residents and very particularly to local and national political pressure and decided to withdraw from building *HQ2*.This policy reversal presents a unique opportunity to evaluate whether markets responds symmetrically to good and bad economic news. Our analysis is briefly summarized here and detailed extensively in Appendix B.

1. *Winning announcement*: We find there was an immediate increase in the close price per square foot of \$14.82 and the list price per square foot of \$44.40, but neither price effect is statistically significant. The direction of prices and time-on-the-market are consistent with Crystal City trends.

2. *Withdrawal announcement*: This shock arrives three months after the winning announcement, and six months later, seller's expectations have significantly declined since the withdrawal announcement as List Price/Sq.Ft have declined by nearly \$52. For transacted prices, effects are slightly less clear. We find that prices per Square Footage in NY have declined by \$37 Close Price/Sq.Ft at a marginal significance level relative to the withdraw decision

date of 14 February, 2019. Our robustness checks suggest that this is a lower bound of the loss of housing value inflicted by Amazon's withdrawal decision as the *CEM ATT* estimate of *HQ2* on Close Price/Sq.Ft indicates that six months later was \$64 lower rather than just \$37.[21] *CEM* estimates show a similar reduction in the List Price/Sq.Ft as the *DID* estimate.

Amazon's decision to withdraw from NY not only erased any potential housing capital gains generated by its early decision to locate in Long Island City but potentially caused declines in housing value. The implied average decline of \$58,500 in house values is only a lower bound estimate of the economic impact induced by the withdrawal. If Amazon had not withdrawn and one assumes the housing market in Long Island City would have evolved similarly to Crystal City (7.5% price appreciation six months after the winning announcement) the increase in the price per square foot would implied a hypothetical Close Price/Sq.Ft of \$613 in NY by mid-May 2019, \$42 more than by mid-November 2018. The price gap between the actual Close Price/Sq.Ft in NY six months after Amazon's withdrawal amounted to \$80 per square feet, i.e., an average effect of \$127,000 of the average house property value lost due to the withdrawal using simple back of the envelope calculations when one extrapolates from the price appreciation observed in Crystal City.

5 Measuring Expectations Shocks in Macro-Housing Models

The causal impact of expectations shocks on housing prices and rents has important implications for macro-housing modeling. We build a general equilibrium macro-housing model capable of replicating the empirical price-rent ratio dynamics[22] Critical to the model's success are the features of endogenous housing supply and collateralized mortgages. Due to the model's consistency with the empirical results for expectations shocks, our model provides an ideal laboratory setting to compare and contrast the implications of an expectations shock relative to a credit shock.

5.1 Arbitrage Pricing Models and Expectation Shocks

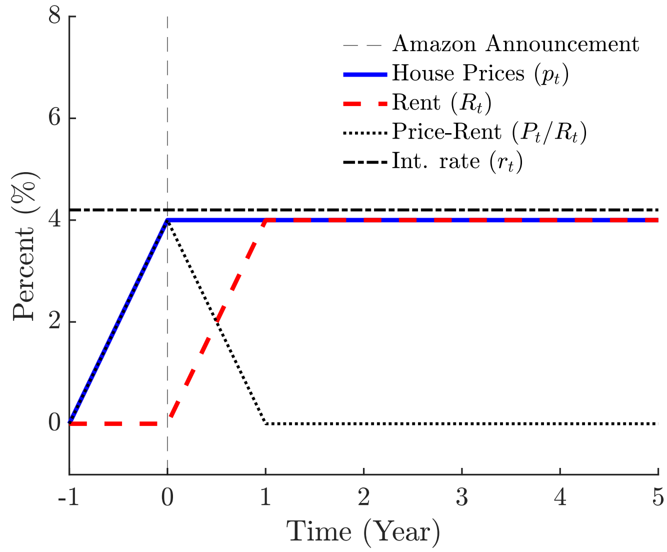
The idea of retrieving expectations shocks from house pricing relationships based on the concept of no-arbitrage opportunities is very common in the literature (Kuchler et al., 2023). Housing is viewed as an asset with valuation p_t that delivers a cash-flow of rents denoted by R_t where the cash-flows are discounted at the rate r_t^d. A simple representation is described in the arbitrage pricing equation (user-cost) below for consecutive periods.

$$p_t^h = R_t + \frac{p_{t+1}^h}{1 + r_{t+1}^d} = R_t + \frac{R_{t+1}}{1 + r_{t+1}^d} + \frac{p_{t+2}^h}{(1 + r_{t+1}^d)(1 + r_{t+2}^d)}, \; \forall t \tag{3}$$

[21] Table D.11 in Appendix D shows that the *CEM* control sample is much closer to NY than the original sample used for *DID* estimation across all house characteristics and demographics except Population Density.

[22] In Appendix C, we explore special cases of the model and find that common structural pricing models cannot replicate the dynamics if they abstract from these essential model features.

Figure 4: Arbitrage Pricing and Expectation Shocks



If the dividend of housing, $R_{t+1} > R_t$, increases because of an *HQ2* shock (additional income in the winning location), then arbitrage pricing causes outside funds to be invested in housing to increase the valuation and closes the arbitrage gap. This approach is agnostic about any connection between rents, interest rates, and house prices, and does not account for the sources of funds used to arbitrage investments as it is a partial equilibrium analysis. Figure 4 provides a graphical representation of an expectation shock that is consistent with the evidence presented in Section 4. It shows the path for house prices, rents, and the price-rent ratio as a result of the future rent increase of 4.0% upon the announcement at $t = 0$ with interest rate r_t^d fixed at 4.2%.

House prices react on impact to the expectations shock and converge to a higher level as the increase in rents is permanent. Rents change in $t = 1$, and the price-rent ratio falls to the initial level. All the variation of the price-rent ratio is driven by the expectations shock. This type of arbitrage pricing model assumes the existence of external investors with deep pockets to close arbitrage opportunities (Kaplan et al., 2020). In this user-cost approach, the shock that generates an increase in future rents does not impact interest rates or feedback in the path of rents. While an arbitrage pricing model can easily replicate the dynamics of the *HQ2* expectations shock, for the macro-housing literature it is important to ensure that general equilibrium asset pricing models replicate our empirical findings and can be applied generally to other settings.

In the canonical macro-housing model prices are determined through market pricing by supply and demand in all markets. Therefore, future income increases impact rents and house prices, and additionally interest rates as well as quantities of housing produced. Endogenizing prices and quantities provides a more challenging test as we show in Appendix C that many standard model specifications fail to retrieve the causal effect of the expectations shock on house prices, rents, and the price-rent ratio. We present a general equilibrium macro-housing model that is consistent with the empirical evidence in subsequent subsections.

5.2 A General Equilibrium Macro-Housing Model

The canonical general equilibrium macro-housing model derives structural relations connecting house prices, rents, and interest rates. We generalize it beyond its simplest version to accommodate endogenous housing supply and collateralized credit. Our model nests the canonical user-cost version as a particular case. To generate an expectations *HQ2* shock in the context of the model, it is useful to compare the housing outcomes relative to a non treated control location. To ease notation we abstract from providing a location index to each variable (i.e., $p^s_{j,t} = p^s_t$ for all j).

Household preferences are represented by a standard utility function $U(c_t, h_t)$ over consumption and housing that satisfies the usual assumptions (continuous, twice differentiable, time-separable, and strictly concave). The flow of utility is discounted by the factor $\beta \in (0, 1)$. Housing services are generated by combining physical structures, S_t which depreciate at rate δ_s, and land, L_t according to an aggregator function, $h_t = G(S_t, L_t)$, that also satisfies the standard assumptions. The price per unit of structures and land are defined by p^s_t and p^ℓ_t, respectively. Aggregated together, the price of housing services is given by p^h_t. In this model, arbitrage will ensure that the value of residential housing, $V^h_t = p^h_t h_t = p^s_t S_t + p^\ell_t L_t$, equals the value of structures and land. Production takes place with a constant-returns to scale technology, $Y_t = z_t F(K_t, N_t)$, where K_t and N_t represent capital and labor respectively, and z_t is the level of productivity with capital depreciating at the rate $\delta_k \in (0, 1)$. To simplify the exposition, the baseline assumes linear production, $Y_t = z_t N_t$, which under perfect competition equates wages to productivity: $w_t = z_t$.

For the most general specification in terms of financial assets, it is convenient to consider a specification that includes mortgage (collateralized), loans (non-collateralized), and physical capital. Formally, B_t denotes the stock of mortgages with the interest rate denoted by r^m_t. The stock of debt is represented by D_t, and the associated rate is denoted by r^d_t. The rate of return on physical capital K_t is given by r^k_t. In the absence of segmentation and uncertainty, the rates of returns would these assets will be equal. Garriga et al. (2019) includes the presence of asset segmentation introduces a wedge between r^d_t and r^*_t, as arbitrage forces are limited by the requirement that borrowing can only be collateralized by the stock of housing. We will consider different specifications that differ on whether some rates are endogenous or exogenous.[23]

The law of motion for mortgage debt B_t is given by $B_{t+1} = b_{t+1} + (1 - \triangle)B_t$, where $0 \leq \triangle \leq 1$ is the fraction of the stock of debt that must be repaid/amortized every period.[24] To prevent arbitrage, it is necessary to restrict the amount of mortgage debt to a fraction of the net market value of the stock of housing given by the parameter ϕ_t which measures the maximal loan-to-value ratios at time t according to $b_{t+1} \leq \phi_t V^h_t - (1 - \triangle)B_t$. With a positive spread of

[23] One case allows for the international determination of borrowing rates for uncollateralized and collateralized loans. See Favilukis et al. (2012) for a discussion of the role of international lenders.

[24] This specification is a simple approach to capturing the real-world heterogeneity in the average duration of mortgage contracts. The parameter Δ can be chosen to approximate the average maturity of mortgage loans.

interest rates, borrowers have an incentive to rollover balances which implies that the next-period's stock of debt equals:

$$B_{t+1} = \phi_t(p_t^s S_t + p_t^\ell L_t) = \phi_t p_t^h h_t = \phi_t V_t^h \,. \tag{4}$$

The capital and structure investments, x_t and s_t, determine the evolution of the stock of productive capital $K_{t+1} = x_t + (1-\delta_k)K_t$ and physical structures $S_t = s_t + (1-\delta_s)S_{t-1}$.

The representative agent solves:

$$U = \max \sum_{t=0}^{\infty} \beta^t U(c_t, G(S_t, L_t)), \tag{5a}$$

$$\begin{aligned} s.t. \quad & c_t + (r_t^* + \triangle)\, B_t + p_t^\ell \ell_t + x_t + p_t^s s_t + D_{t+1} \\ & = r_t^k K_t + w_t + p_t^s(1-\delta_s)S_{t-1} + b_{t+1} + (1+r_t^d)D_t, \end{aligned} \tag{5b}$$

$$L_t = L_{t-1} + \ell_t, \tag{5c}$$

$$b_{t+1} \leq \phi_t V_t^h - (1-\triangle)B_t, \tag{5d}$$

together with the standard non-negativity constraints in choice variables and the laws of motion for mortgages and investments (i.e., capital and structures). The optimality conditions yield pricing relationships for user-cost/rents $R_t = U_{h_t}/U_{c_t}$ and interest rates $1 + r_{t+1}^d = U_{c_t}/\beta U_{c_{t+1}} = 1 + r_{t+1}^k$ due to a no-arbitrage condition where $r_t^k = F_{kt} + \delta_k$. The pricing expressions for land and structures are given by:

$$p_t^\ell = R_t G_{\ell,t} + \frac{p_{t+1}^\ell}{1+r_{t+1}^d} + \phi_t p_t^\ell \frac{(r_{t+1}^d - r_{t+1}^m)}{1+r_{t+1}^d}, \tag{6a}$$

$$p_t^s = R_t G_{S,t} + \frac{(1-\delta_s)}{1+r_{t+1}^d} p_{t+1}^s + \phi_t p_t^S \frac{(r_{t+1}^d - r_{t+1}^m)}{1+r_{t+1}^d}, \tag{6b}$$

where $G_{\ell,t}$ and $G_{s,t}$ denote the partial derivatives with respect to $G(S_t, L_t)$, the housing production function. Note that in the pricing equation land is not subject to depreciation, unlike structures.

The notion of equilibrium is standard for a macro-housing model with competitive pricing, as it solves for sequences of prices $\{p_t^s, p_t^\ell, r_t^d, R_t)$ and allocations $\{c_t, b_t, K_t, D_t, s_t, \ell_t)$ consistent with the optimality conditions of the households, firms, and market clearing conditions. The exogenous variables are the path of productivity, the quantity of land, the interest rate of mortgage borrowing, and the loan-to-value requirement.

This general specification of the model introduces more detailed features such as collateralized mortgage loans and endogenous housing supply. Appendix C explores the implications of eliminating housing supply and mortgages from the general model. In summary, these models are not successful in recovering the dynamics of an expectations shock.

5.3 The Amazon HQ2 Expectations Shock

In the context of the macro-housing model, the Amazon HQ2 expectations shock is an unanticipated announcement in period τ about a future permanent increase income in the subsequent period $(Y_\tau = z_\tau N_\tau < Y_{\tau+1} = Y_{\tau+2} = ...)$, in the winning locations relative to the control group that has no change in job opportunities $(Y_\tau = Y_{\tau+1} = Y_{\tau+2} = ...)$. As in the empirical specification, all the variation comes from an income increase given by $z_{\tau+1} > z_\tau$.[25]

The model needs to be parameterized to explore the response of house prices and rents to an expectation shock. The parametrization is fairly conventional and conservative for a macro-housing model. Agents preference have a CRRA structure for intertemporal utility with a base case where $\sigma = 1$. For the intratemporal utility between consumption and housing, the elasticity of substitution between consumption and housing is set to $\Sigma_{c,h} = 0.67$, the share of housing is set to $\gamma = 0.8$ and the discount rate $\beta = 0.96$. Following Garriga et al. (2019), the mortgage rate, r_t^m is set to 3%, the loan-to-value $\phi = 75\%$, and the maturity rate $\Delta = 0.10$. The depreciation rate of structures $\delta_s = 0.02$, the endowment of land is normalized to one, and for the base case the labor share is equal to one (no capital). The permanent income increase in the winning locations is 4% of income[26] (which is a conservative estimate of the income increase in the average income of the winning location). We model that the announcement of future income occurs in $t = 0$, but the income is not realized until $t = 1$.

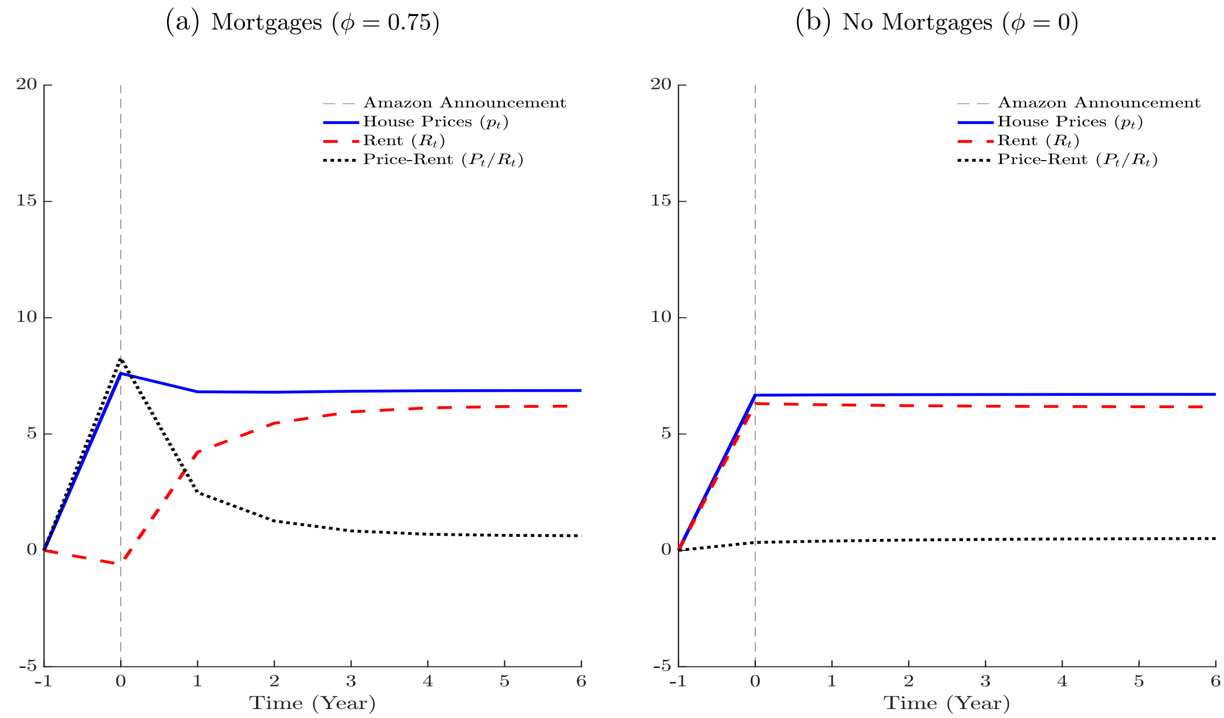
Figure 5 describes the dynamics of prices and rents in our baseline model. Panel a) plots the dynamics of the case with collateralized mortgages ($\phi = 0.75$), whereas Panel b) plots the case with no mortgages ($\phi = 0$), in both cases the housing supply is endogenous.

The response to the *HQ2* shock in Panel a) indicates that the presence of mortgages and housing supply generates a response consistent with the empirical estimates as house prices react upon announcement to the expectations shock. This occurs as the inclusion of endogenous housing supply in the presence of collateralized mortgages allow for households to bring expected future income to present housing expenditures. These model features allow for the decoupling of rents from house prices on announcement. We observe a slight overshooting of prices as the housing supply is less elastic in the short-run relative to the long-run. The model also predicts that a response of the housing supply in the future attenuates the initial price appreciation, however the increase in prices is highly persistent and the new steady state level is well above the initial price. We find that on impact, the expectations shock generates a large increase in the price-rent ratio, and rents are almost flat for the period prior to the realization of the expected income. These features are all consistent with the empirical evidence documented in Section 4. Rents do increase slowly over

[25] We assume that the population of households remains constant in our treated location in the model despite the increase in productivity. Work by Davis, Fisher and Veracierto (2021) suggests that in the short-run this assumption is valid as migration occurs slowly.

[26] The choice of the increase of $z_{\tau+1}$ to be 4% is calculated as a lower bound of the size of the impact. This lower bound is calculated by estimating the increase in income that would occur to the commuting area within 20 minutes of the proposed *HQ2* location if $2.5 billion was added to local income.

Figure 5: Model: Mortgages and Endogenous Housing Supply



time after the arrival, but ultimately the price-rent ratio converges to a higher level than before the expectations shock. The equilibrium features a permanent increase in the price-rent ratio due to the accumulation of mortgage debt to finance the initial housing appreciation.

What is the role of mortgages in determining the dynamics of the price-rent ratios? To answer this question, Panel b) of Figure 5 eliminates mortgages, but maintains endogenous housing supply. The elimination of the loan-to-value mortgage results in a case where rents and prices simultaneously increase at the time of the announcement, and there is a much smaller change in the price-rent ratio. That prices and rents do not move one-for-one in Panel b) is entirely driven by the elasticity of substitution between consumption and housing is 0.67 instead of 1.[27]

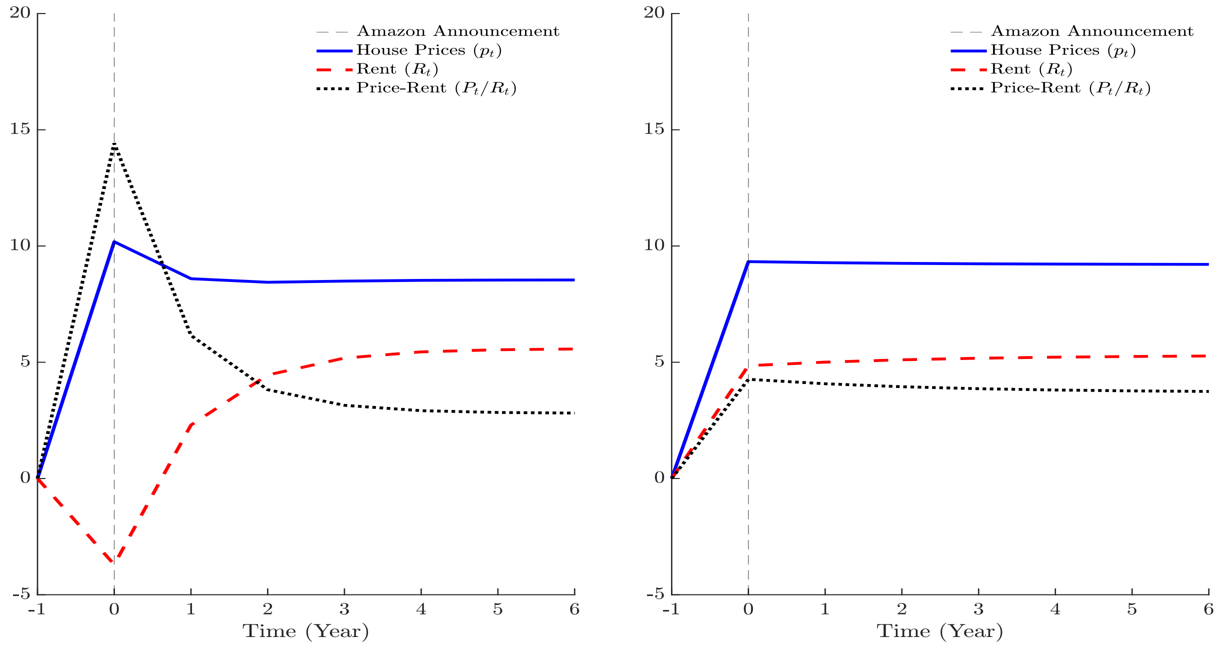
A key feature of both the empirical evidence and the model is that rents are flat on announcement, and the model suggests they should increase after the arrival of Amazon to the location. The model predicts the the price-rent ratio declines over time. To explain this phenomenon, note that the response of prices and rents with mortgage loans is very different from the model with a fixed interest rate (uncollateralized credit) as seen in Figure C.3. The financial friction of mortgages through the LTV ($\phi > 0$) drives the dynamics of the price-rent ratio.[28]

[27] We have performed numerous simulations exploring the role of this elasticity and its interaction with different model features. The response in Panel b) provides a good summary of the role of this parameter. If we had log utility preferences, the house prices and rents would move one-for-one in this specification.

[28] As a robustness test, we have analyzed delaying the timing of arrival of income one additional period. This modification generates an additional incentive to borrow due to the longer delay before the expected income is realized. The delay reduces the expected present value of the expectations shock, as the increase of life-time

Figure 6: Dynamics under Different Credit Constraints

(a) High LTV ($\phi_t = 0.85$) (b) Low LTV ($\phi_t = 0.5$)



How much does the level of the LTV ratio for mortgages (ϕ) influence the path of the price-rent ratio? In Figure 6 we consider the implied price-rent paths for a cases where the loan-to-value ratio is higher (Panel a) or lower (Panel b) relative to the baseline depicted in Figure 5. As Panel a) shows, the expectations shock has a larger impact on house prices and the price-rent ratio when credit conditions are relatively loose ($\phi = 0.85$). With tighter credit limits, as indicated in Panel b), the response of the price-rent ratio is substantially reduced albeit either case has qualitatively similar long-run levels of prices and rents. But even the case of less credit generates a large increase of the price-rent ratio relative to the pre-announcement level. Comparing the response of rents in the various cases, there is an intermediate level of ϕ that would be consistent with our baseline calibration of $\phi = 0.75$. The insignificantly declining ratio of rents in the treated group relative to the control group in Figure 5 is consistent with the evidence presented in Section 4.4.

Extending the discussion of LTV ratios, our model has a limiting case when credit conditions are not relevant. As credit vanishes from the model, $\phi \to 0$, the long-run gap between prices and rents closes. This can be seen by comparing the right panel of Figure 5 with both panels in Figure 6. The level of credit causes the short-run dynamics of the shock to impact the long-run gap between house prices and rents. This dimension is absent in the traditional urban housing framework where permanent income changes closely link both variables. It also becomes clear that allowing for

income is not as large. However, the quantitative impact on prices is negligible, but the price-rent ratio increases more than without the delay. This is due to a short-term decline of rents resulting from a relative decline in consumption to purchase more housing. A similar compositional effect in the price-rent ratio occurs in Favilukis et al. (2017).

more credit borrowing allows for a stronger decoupling of house prices and rents. A value close to $\phi = 0.75$ appears to be consistent with the data, whereas a higher or a lower value generates an excessive reaction of the price-rent ratio.

5.4 Expectations Shocks vs. Credit Shocks

One of the most important questions in the macro-housing literature is to what extent house prices movements are driven by expectations shocks or changes in credit conditions. The previous analysis demonstrates that there are important theoretical connections between the two types of shocks. Given the salience of the two shocks to the literature, one can ask to what degree price-rent ratios respond differently in response to either of the shock types. That the price-rent ratio is dynamic is well known, for example Shiller (2006) argues that there exists a clear disconnect between house price and rents during boom-bust cycle episodes.

Our macro-housing model with mortgages and endogenous housing supply provides an ideal setting to answer whether the implied price-rent ratio associated with a housing boom driven by expectations shocks differ from those driven by credit shocks[29]. Figure 7 compares housing booms that are driven by expectations shocks (Panel a) to the response of prices and rents to a positive credit shock that reduces the mortgage rate (Panel b). For the expectations shock, we use the same *HQ2* shock and calibration as the previous section. For a shock to credit, we permanently reduce mortgage rates from 2.5% to 2.1% which generates an equivalent increase in the price-rent ratio.

While both shocks can generate similar in the price-rent ratio, the underlying dynamics both prices and rents implied by each shock are different. With expectation shocks the price-rent ratio decreases over time converging close to the initial level before the announcement. For credit shock this ratio remains elevated over time above the pre-announcement level.

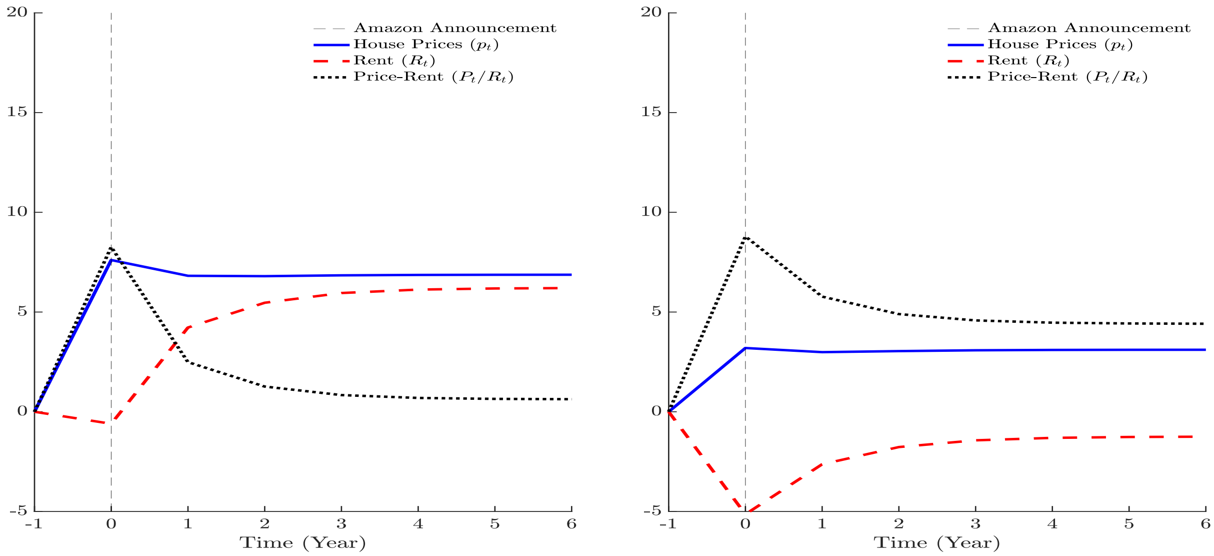
We find that, qualitatively, both shocks increase house prices but generate different responses for rents. Critically, we find that shocks to credit conditions generate an initial negative correlation between rents and prices. Part of the increase in the price-rent ratio due to a credit shock is a decrease in rents, a mechanism that is present in Favilukis et al. (2017). After the initial decline, rents increase over time, but the credit shock scenario predicts a large permanent gap between prices and rents. As discussed earlier, for the scenario of expectations shocks rents have a muted reaction initially. Over time the path of rents approaches to house prices, but does not converge. The muted response of rents to the Amazon *HQ2* shock documented in Section 4.4 is consistent with the expectations shock of Figure 7 panel a).

Clearly the response of rents to each type of shock provide clear testable implications for future structural and empirical work. In these two cases, both shocks have in common that the

[29] Garriga et al. (2019) show that changes in the persistent of credit conditions generate a income and price effect that can move the price-rent ratio along the observed behavior in the United States between 2000-2010.

Figure 7: Expectations vs. Credit Shocks

(a) Expectations Shocks *HQ2* (b) Credit Shock ($r^m = 2.5\% \to 2.1\%$)



long-run relationship between prices and rents is influenced by credit conditions[30] This role of credit is often absent in the traditional urban literature, where income effects tend to move prices one-for one. In the context of our setting and model, we find that one cannot abstract away from modeling mortgages and housing supply to generate realistic responses of housing to future income shocks.

5.5 Amazon HQ2 Shock: Trends in Transactions with Mortgage

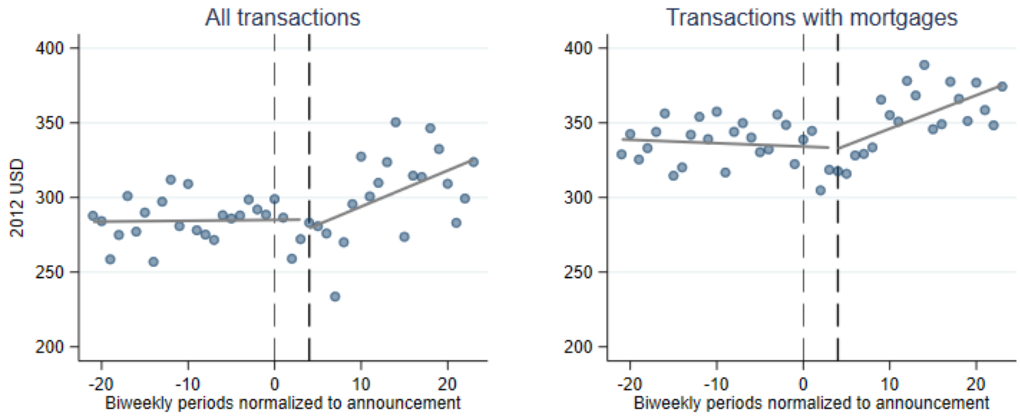
We finish our analysis by studying the purchasing behavior of buyers around the *HQ2* shock, particularly whether buyers financed their purchases with mortgages. Our model implicitly makes two assumptions regarding purchasing behavior that can be empirically validated: 1) buyers finance purchases using mortgages after the expectations shock and 2) the LTV ratio remains constant. We assess the validity of both of these assumptions in this subsection.

To assess the role of mortgage financing to purchase housing after the Amazon HQ2 expectation shock we use transaction data from Zillow transaction data (ZTRAX). This dataset includes transactions for properties that transfer ownership between buyers and sellers, potentially with or without a mortgage lien against the property. It also includes observations which are changes in the lien (refinances) for which ownership of the property is retained.[31]

[30] For work consistent with rents and credit conditions, Garriga et al. (2019) document the U-shape pattern of rents during the 2000-2010 housing boom in the United States.

[31] The data contains about 90 million transacted properties with a mortgage lien assigned within two weeks of registering the property. The key variables used are the sale price, the square footage of the property, and the loan amount. The data has a high degree of coverage with over 88 million of transactions that include the loan amount, and 84 million the sales price amount). For the location identifiers, the FIPS is always available, and the zip code of the property is only missing in 3.6 percent of the observations.

Figure 8: Evidence Mortgage Use in Transactions



If buyers after the *HQ2* announcement continue to finance purchases with mortgages as they did prior to the announcement, as opposed to cash or other sources of income, we would expect to see the loan-to-value ratio of Crystal City remain fairly constant. Because prices increased as a result of the *HQ2* shock, this would correspond to an increase of similar magnitude of the value of loans buyers used for purchases.

We examine whether there is evidence that the mortgage loan amount increased after the *HQ2* shock similar to the price appreciation. Using the ZTRAX data, one can calculate for each biweekly period around the *HQ2* announcement the average mortgage amount per square footage. We do this for all transacted properties, as well as for transactions which were financed with mortgages. Figure 8 depicts the trends in transactions with mortgage for each variable before and after the *HQ2* shock in Crystal City. As is usual, the first dashed line represents the period with the *HQ2* shock. The second dashed line marks 8 weeks after the announcement, from where we measure the second trend line in the figures. Given that the average time in the market is 53 days, or 7.5 weeks, we think it is a sensible period to assess any change in trends. The results are robust to changes in the window.[32]

Both measures in Figure 8 show that the mortgage value per square foot was flat 40 weeks before *HQ2* shock (the left side of the first vertical dashed line), as calculated by the trend line. There is a positive change in trend 8 weeks after the winning announcement (2nd dashed line) in both panels. The change in the average mortgage value per unit of size for all transacted properties or transactions with mortgage 40 weeks after the announcement is approximately 10% for each measure. These values are roughly consistent with the measured appreciation of price per square foot relative to the control group, and suggest that the expectation shock was largely capitalized by purchases that use mortgage loans. In summary, we find that loan value growth roughly matched price appreciation after the *HQ2* announcement which held loan-to-value ratios relatively constant.

[32] We have calculated the change in trend for different time windows after the winning announcement from 4 to 10 weeks.

6 Concluding Remarks

An unresolved question in the macro-housing literature is to determine what is the magnitude of the contribution of expectations shocks to house prices. This analysis provides a precise measurement of house price expectations by leveraging a quasi-natural experiment. The selection process of the location for Amazon's *HQ2* provides a unique setting to measure the changes in close and list prices as well as housing liquidity to an unanticipated shock. The secrecy and timing of the selection process allows us to use transaction level data to estimate standard *DID* techniques to identify causal treatment effects of Amazon's decision to locate in Crystal City, VA. Both sellers' listing prices and the transacted close prices increase significantly in the winning location in a short window after the announcement. We find that the shock was associated with a shift of the entire price distribution upwards. Notably, rents did not increase upon announcement, prior to the realization of the expected income.

These empirical findings provide a benchmark test of price and rent dynamics that macro-housing models should be consistent with. We provide a tractable structural model that replicates the dynamics of price-rent ratio after the *HQ2* shock. The model features collateralized mortgages and endogenous housing supply as key features to generate realistic dynamics to the *HQ2* shock in terms of the response of the price-rent ratio. The model provides an ideal setting to study the interaction of credit with expectations shocks providing a resolution to the unresolved question of the relative magnitude of these forces in determining house price movements. An important implication of the exercise is to emphasize that expectations need to be capitalized with resources, in our case mortgages, but possibly diverting funds from other assets or investments.

Extending our analysis to test the dynamics of the housing market further than one year of the *HQ2* shock is likely not feasible. The COVID-19 pandemic arrived in March 2020, while our transaction data covers the period until December 2019. Further extension of the sample will likely muddy the identification by including the uncertain period during the onset of COVID-19.

The pandemic has had an important impact in work practices and on-site work. In March 2023, Amazon announced a pause in the construction on its sprawling second headquarters near Washington, a decision that coincided with the company's largest job cut historically and a re-assessment of office needs to account for remote work. The fact that the *HQ2* plans for job growth and business investment have been derailed strengthens our conclusion that expectations shocks are capable of driving local housing booms as the *HQ2* income never materialized.

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Appendix

A Empirical Analysis: Robustness

In this appendix we examine the robustness of our empirical results detailed in Section 4. Our tests can be summarized broadly under two categories: robustness using an alternative control group and robustness under alternative estimators. When considering alternative estimators we implement estimators that match either ex ante or ex post on observables to generate counterfactuals. In summary, our baseline results of Section 4 are highly robust to alternative control groups and estimators.

A.1 Alternative Control Groups

As an alternative control group, we gather together housing transactions that occurred in the finalist CBSA's in the year prior to and up to a year post the *HQ2* announcement. Other than the change in CBSA's we pull transactions from, we make no changes to the baseline empirical analysis .We report the finalist CBSA's that we have data coverage in Table A.1.

The baseline *DID* specification of Equation 1 is estimated using the described finalist control group. We report the results in Table A.2. Focusing first on CLOSE PRICE/SQ.FTpoint estimates, results are highly similar to the baseline when using the finalist control group. Over a window of six months, the *HQ2* announcement increased CLOSE PRICE/SQ.FT by \$27.0 using finalists vs. \$26.3 using non-finalists. The results for other windows are highly similar. When examining LIST PRICE/SQ.FTresults, the similarities hold and the point estimate of the ATT with finalists is \$29.9 compared to \$29.8 with non-finalists. The results for market liquidity, TIME ON MARKET, are remarkably similar at the six month window with the point estimate using the finalists being -11.9 days, and the point estimate using non-finalists being -11.9 days. Results are highly similar over all time windows and indicate that our point estimates are consistent and robust using a completely different control group.

Table A.1: Amazon Finalist Group

CBSAs	
Atlanta-Sandy Springs-Marietta, GA	Austin-Round Rock, TX
Bethesda-Frederick-Gaithersburg, MD	Chicago-Naperville-Joliet, IL
Boston-Quincy, MA	Cambridge-Newton-Framingham, MA
Columbus, OH	Dallas-Plano-Irving, TX
Denver-Aurora, CO	Durham, NC
Essex County, MA	Indianapolis, IN
Los Angeles-Long Beach-Santa Ana, CA	Miami-Miami Beach-Kendall, FL
Nashville-Davidson–Murfreesboro, TN	Raleigh-Cary, NC
Newark-Union, NJ-PA	Philadelphia, PA
Pittsburgh, PA	

Notes: We report all the finalist CBSA's (excluding the winning locations) for which we have data coverage in *MLS*.

Table A.2: **Crystal City: Amazon's HQ2 Event Study (Finalists)**

	1 Month	3 Months	6 Months	12 Months
Y = Close Price/Sq.Ft				
Announcement	-1.789***	-0.351	3.384***	4.395***
	(0.504)	(0.384)	(0.304)	(0.295)
Winner	50.528***	51.026***	50.866***	51.268***
	(14.211)	(14.194)	(14.186)	(14.188)
HQ2	14.547***	19.109***	27.048***	31.560***
	(3.835)	(4.352)	(1.871)	(2.266)
R^2	0.719	0.717	0.715	0.715
Observations	669,243	743,452	904,565	1,258,259
Treated Obs.	1565	1785	2172	2885
Y = List Price/Sq.Ft				
Announcement	7.708***	2.698**	-3.630***	-5.769***
	(1.191)	(1.154)	(1.279)	(1.404)
Winner	52.131***	51.372***	51.211***	51.563***
	(14.706)	(14.752)	(14.785)	(14.763)
HQ2	27.476*	12.739*	29.857***	32.566***
	(15.838)	(6.784)	(2.652)	(3.891)
R^2	0.536	0.534	0.539	0.540
Observations	971,815	1,085,000	1,350,127	1,648,292
Treated Obs.	1816	1981	2514	3105
Y = Time on Market				
Announcement	16.049***	24.916***	12.323***	4.278***
	(0.421)	(0.354)	(0.296)	(0.313)
Winner	-13.548***	-14.084***	-12.948***	-12.144***
	(3.280)	(3.208)	(3.278)	(3.498)
HQ2	2.716	2.380*	-11.944***	-11.927***
	(1.792)	(1.310)	(1.706)	(1.386)
R^2	0.094	0.111	0.111	0.089
Observations	961,425	1,085,558	1,315,583	1,832,393
Treated Obs.	1874	2118	2537	3374

Notes: Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus the indicated number of months afterwards. *OLS* regression including transactional covariates as well as property type, metropolitan area, and month fixed effects. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

A.2 Coarsened Exact Matching

As a complementary approach to the standard *DID* estimates reported, we implement an alternative estimator: Coarsened Exact Matching (*CEM*). This estimation technique allows to evaluate causal effects that makes use only of post-treatment transaction information, i.e., after Amazon's *HQ2* announcement. Specifically, *CEM* is a nonparametric matching algorithm that minimizes multivariate imbalance between the control and treatment groups by coarsening control variables into a set of user-defined strata. The algorithm then matches treated and control observations on their sets of unique strata of control variables (Iacus, King and Porro, 2011, 2012). The estimator removes observations from the treatment and control groups that cannot be matched and derives a weight for each observation to provide causal estimates of the treatment effect.[33] For identification we assume outcomes differences in outcomes between treated and matched counterfactual observations are driven by the *HQ2* announcement.

Table A.3: Crystal City: CEM Balance Analysis

	CEM-matched		All Data	
	Control	Treated	Control	Treated
Crystal City, VA				
Square Footage	1,508.22	1,498.10	1995.72	1508.45
Age	60.33	55.71	36.30	55.27
Bathrooms	2.10	2.12	2.34	2.18
Bedrooms	2.46	2.45	3.25	2.45
Population Density	7,164.92	10,851.99	2,541.90	10,864.56
College	57.89	67.02	32.72	66.86
Price/Earnings	1.36	1.40	1.60	1.40

Notes: Sample means six months after the winning announcement. Treated observations in the *CEM* sample receive weight of 1. Control observations in the *CEM* sample receive a weight equal to the ratio of the number of treated and control observations in their specific stratum multiplied by the ratio of the total number of matched treated and control observations.

In order to obtain a control group of house transactions in other markets with similar features to those transacted in Crystal City, we allocate each residential home sold after Amazon's *HQ2* announcement to a stratum defined by decile bins of the continuous variables square footage, the price-earnings ratio, and the college attainment rate; a discrete version of age where the new/old cutoff is set at 20 years; number of bathrooms (with four or more binned into one group); and number of bedrooms (with five or more binned into one group). Similarly we bin population density for every thousand persons per square miles and group areas with more than 4,000 people per square mile into the top bin. Strata with at least one transacted residence in Crystal City and one of the cities in the control group are kept, defining the *CEM* sample. With these criteria, we match over 90% of the treated observations with house transactions in the control group for time windows of six months from the shock or greater.[34]

Table A.3 shows that house and sociodemographic characteristics of properties sold in Crystal City and other cities are much more similar than those in the original data used for *DID* estimation, i.e., the *CEM* matching significantly improved the covariate balance. The only

[33] Recent applications of *CEM* in economics include the works of Azoulay, Graff Zivin and Wang (2010), Jaravel, Petkova and Bell (2018), and Álvarez and Argente (2019).

[34] It notable that the largest number of matches for the treated group in Crystal City are control observations from Seattle, followed by Minneapolis, San Diego and Portland. When using a nonparametric matching algorithm, the winning neighborhoods of *HQ2* most closely resemble neighborhoods in Seattle, the original headquarters. This is consistent with Amazon either implicitly or explicitly selecting locations similar to the location of their original headquarters.

Table A.4: **Crystal City: Amazon's HQ2 CEM Estimates (Non-Finalists)**

	1 Month	3 Months	6 Months	12 Months
Y = Close Price/Sq.Ft				
HQ2	35.34***	30.82***	28.43***	31.46***
	(9.85)	(5.96)	(4.11)	(2.92)
R^2	0.029	0.017	0.009	0.009
Observations	429	1,543	5,069	13,029
Treated Obs.	128	359	735	1,486
Treated Matched	82%	92%	95%	96%
Y = List Price/Sq.Ft				
HQ2	27.16**	21.72***	31.16***	37.31***
	(10.52)	(9.93)	(4.69)	(2.34)
R^2	0.011	0.004	0.006	0.007
Observations	600	2,193	6,868	15,906
Treated Obs.	133	359	735	1,486
Treated Matched	79%	92%	95%	96%
Y = Time on Market				
HQ2	-4.44	1.30	-10.91***	-8.21***
	(4.80)	(2.95)	(2.08)	(1.33)
R^2	0.001	0.000	0.004	0.002
Observations	601	2,194	6,870	17,885
Treated Obs.	128	359	735	1,486
Treated Matched	82%	92%	95%	96%

Notes: Sample includes transactions after Amazon's *HQ2* announcement date in Crystal City and across non-finalist cities. Observations are matched as described in the text and estimates are given by *OLS* using optimal *CEM* weights. Robust standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

exception is population density, with Crystal City being significantly more densely populated than most cities in our control group. *CEM* does not aim to match transactions that are identical but just *similar enough* across treated and control groups. Thus, table A.4 reports *CEM*-weighted *OLS* estimates of the casual *ATT* obtained by comparing residential houses sold in Crystal City and other cities in the *CEM* sample.[35]

The *CEM* causal *ATT* estimates of the effect of *HQ2* on Close Price/Sq.Ft are now more similar across time windows. They converge with *DID* estimates as we consider longer post-announcement evaluation horizons. This suggests that the baseline specifications of Section 4.2

[35] Notice that *CEM* does not make use of transactions prior to Amazon's *HQ2* announcement. Using post-announcement transactions only, makes impossible to separate the *ATT* and the VA price level differential irrespective of Amazon's decision. To circumvent this difficulty we regress the price appreciation since Amazon's *HQ2* announcement. The reference values for Close Price/Sq.Ft, List Price/Sq.Ft, and Time on Market are the average values of these magnitudes in VA for the quarter prior to the announcement: 329.56, 364.04, and 61.05, respectively. Table A.4 recast the results in the original level *ATT* to ease comparison with *DID* estimates. The *CEM* results do not include mortgage rates in the regression, as they only capture time variation, but their inclusion leads to almost no quantitative difference in results.

adequately control for differences between housing markets at least for Close Price/Sq.Ft. Results are similarly conclusive for other variables. *CEM* estimates of the effect of *HQ2* on List Price/Sq.Ft are slightly larger than the corresponding *DID* estimates of Table 3, indicating that homeowners in Crystal City were more optimistic than homeowners of similar houses in other metropolitan areas. Finally, the results for Time on Market show a slightly smaller increase in liquidity around the *HQ2* announcement.

A.3 Synthetic Control Method

Despite many appealing features, researchers still need to determine arbitrarily the degree of coarseness of *CEM* estimation as well as those variables where it should be exact or approximate. In this section we move to a more data-driven approach to address the robustness of our estimates by comparing the behavior of the close price in Crystal City to a synthetic control group resulting from a convex combination of all other locations available in our sample. We follow Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010) and let the data define the weights given to each observation in this synthetic control group to minimize the mean squared prediction error (*MSPE*) of Close Price/Sq.Ft over the sample prior to Amazon's *HQ2* announcement in the case of Crystal City. It is interesting to pursue this approach as the basic assumption of *no interference* between local housing markets is likely to hold, i.e., Amazon's decision to locate in Crystal City is unlikely to have affected most other housing markets (positively or negatively) outside of the very closest geographical neighbors.

We proceed as follows: We use bi-weekly neighborhood panel data for nearly two years centered around Amazon's *HQ2* announcement decision[36]. All ZIP codes in Crystal City are consolidated into a single neighborhood and average Close Price/Sq.Ft, house features, and other covariates are recorded for each two-week period. The goal is to define a balanced panel of control neighborhoods smaller than the whole metropolitan area to allow for sufficiently heterogeneous housing market behavior so that the synthetic control sample is not unnecessarily constrained in its design.

To build the synthetic control sample we consider all other cities, finalists and non-finalists, that we have used in our empirical analysis before. Individual transactions are winsorized at the top and bottom 1% for Close Price/Sq.Ft and all other covariates, and then averaged at the ZIP code level for each two-week period. To obtain a balanced panel we eliminate any ZIP code with missing information and those at the top and bottom 1% of the control nationwide sample distribution of Close Price/Sq.Ft. ZIP codes are grouped into four neighborhoods for each *CBSA* using a K-means clustering algorithm (Hastie, Tibshirani and Friedman (2017, §14.3)) using average Close Price/Sq.Ft and all other covariates of Table 3 for transactions occurred before Amazon's *HQ2* announcement. This defines a panel with a control group comprising 220 neighborhoods across finalist and non-finalist cities in the U.S. We use an algorithm to minimize the pre-treatment

[36] As in our *DID* specifications, we use 12 months prior to the winning announcement and approximately 11 months after the winning announcement as our sample

Table A.5: VA: Synthetic Control Sample

	Crystal City, VA		Average of 220
	Real	Synthetic	control neighborhoods
Square Footage	1,596.92	1,487.17	2,012.85
Age	55.21	44.09	40.97
Bathrooms	2.28	2.07	2.29
Bedrooms	2.54	2.34	3.26
Population Density	11,015.53	14,054.29	3,362.05
College	65.87	52.32	34.41
Price/Earnings	1.45	1.78	1.66
Ownership Rate	43.06	37.48	65.52
Mortgage Rate	4.44	4.44	4.44
Close Price/Sq.Ft ($t = -1$)	350.10	334.89	166.88
Close Price/Sq.Ft ($t = -9$)	323.68	325.97	169.08
Close Price/Sq.Ft ($t = -18$)	309.21	311.83	164.14

Notes: Variable means over the twelve months period leading to Amazon's *HQ2* announcement date. Time t is measured in biweekly periods.

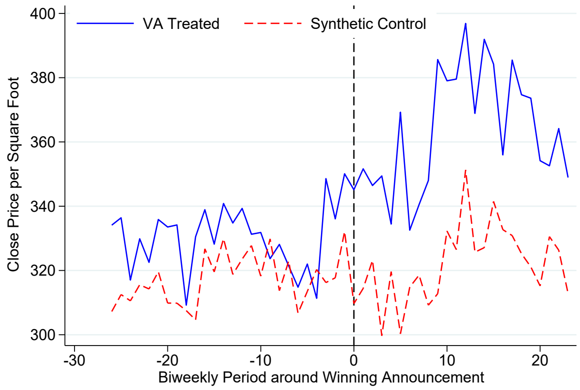
mean squared prediction error of Close Price/Sq.Ft to define the synthetic control. Table A.5 reports the features of the original control housing markets as well as the actual and synthetic VA market. Four neighborhoods out of the 220 local control housing markets comprise the synthetic control group for Crystal City: Denver–Boulder, CO (0.671); Miami-Miami Beach–Kendall, FL (0.259); Los Angeles–Long Beach–Santa Ana, CA (0.035); and Bethesda-Frederick-Gaithersburg, MD (0.029), with all others receiving zero-weight. The procedure generates synthetic control sample that is much closer across all variables to Crystal City than pooling together all the finalist and non-finalist cities.

Figure A.1 plots the biweekly *ATT* estimates of the impact that Amazon's *HQ2* announcement on November 13, 2018 had on the Close Price/Sq.Ft of residential homes sold in Crystal City afterwards, i.e., the difference between close prices in Crystal City and the predicted close prices using the constructed synthetic control. On average, Close Price/Sq.Ft increases \$41.32 after the winning announcement, with the increase peaking in the eleventh biweekly period after the announcement, i.e., about five months or by late-April 2019. This estimate is more than 25% higher than the \$28 *CEM* estimate and therefore suggests that our base \$26 *DID* estimate of the housing appreciation triggered by Amazon's decision to locate its *HQ2* in Crystal City is likely a conservative one.

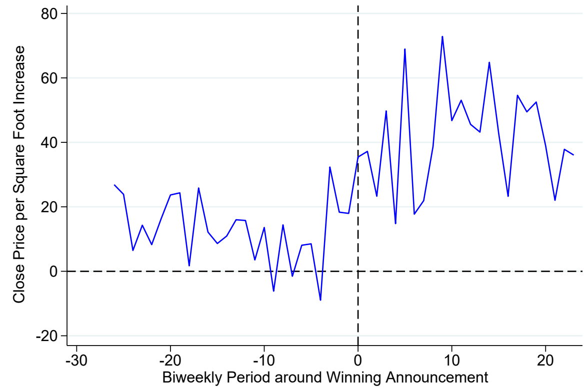
To conclude we conduct a placebo test (Abadie and Gardeazabal, 2003; Bertrand, Duflo and Mullainathan, 2004) to rule out the possibility that the estimated *ATT* is the result of pure chance. The idea is to apply the synthetic control method for each of the 220 neighborhoods (including Crystal City in their respective control group) and compute the *ATT* for each placebo as if Amazon had decided to locate in one of these neighborhoods. Placebo estimates for housing markets that are not affected by Amazon's *HQ2* decision will fit equally well before and after the winning announcement while those really affected will perform worse after it. We run these 220 synthetic control placebos to obtain the empirical distribution of the ratio of post/pre-Amazon's

Figure A.1: Synthetic Control: VA House Close Prices

(a) VA and Synthetic VA Close Price



(b) VA – Synthetic VA Close Price Gap



Notes: Figures show bi-weekly average close price per square foot, Close Price/Sq.Ft, in VA relative to Amazon's *HQ2* announcement date.

HQ2 decision *MSPE* and compare it to the same *MSPE* ratio for VA. There are 8 out 220 instances where this ratio exceeds the threshold value of Crystal City, so that the probability of obtaining a *MSPE* as large as Crystal City if evaluating Amazon's decision randomly in the sample of local housing markets is just 0.036. In other words, we have estimated a causal *ATT* for Crystal City with confidence probability of 96.36%.

B Long Island City: Implications for the Housing Market

This appendix reports statistical analysis of the effect of the *HQ2* shock on the Long Island City housing market. As outlined briefly in Section 4.6, we find that the New York City housing market surrounding Long Island City is idiosyncratic relative to other metropolitan areas in the United States. The most notable feature of the Long Island City housing market is the long time to transact. In addition, we find that the number of residential real estate transactions in *MLS*for this particular neighborhood is small. Because of these features, our point estimates treatment effects for the *HQ2* announcement in the Long Island City neighborhood are not as precise and clear cut as our analysis of Crystal City in Section 4. With this caveat in mind, our results indicate that in all likelihood, Long Island City was adversely affected by the withdrawal announcement from *HQ2* and that property values declined as a result. Our analyses suggests that the average home in Long Island City declined in value considerably and that the declines affected almost all pricing tiers.

We begin our analysis of Long Island City by reporting summary statistics in Table B.1. The summary statistics demonstrate that the Long Island City housing market is idiosyncratic as it features the highest metro prices in our sample (see Table 1), a significantly older housing stock, and a much smaller ownership rate. This comparison holds for both our control group and when compared to Crystal City. Most importantly for our analysis, the Long Island City neighborhood is illiquid, with properties on the market for nearly six months (172 days). Between the time to transact and other *MLS* data coverage idiosyncrasies, we suffer from a small sample size when performing analysis on Long Island City.

We re-estimate (1) using the methodology described in Section 4.2 now using Long Island City, rather than Crystal City, as the treatment group[37] using the non-finalist control sample. We estimate (1) separately using two different timing partitions. Per our usual methodology, we estimate our specification on a window of three months after the *HQ2* announcement, we can estimate the effect of the announcement on the Long Island City housing market attributable to the winning announcement. We further estimate (1) shifting the treatment date to be February 14, 2019 to retrieve the effect of the withdrawal announcement in a window of six months after that date. For consistency, we use 12 months worth of transactions prior to either date for the pre-period.

Table B.2 summarizes the causal effects of Amazon's *HQ2* announcement three months after 13 November 2018, as well as six months after Amazon decided to withdraw from NY on 14 February 2019. We report our results using both our *DID* estimator in (1), as well as the *CEM* results for the specification described in Section A.2[38]. Although we do not report transactional covariates, the results are nearly identical to our analysis for Crystal City in Table 2 for socioeconomic variables such as Population Density, College, and Price/Earnings.[39]

Table B.1: Summary Statistics for Long Island City

	Mean	Std. Dev.	Percentile 10%	Percentile 50%	Percentile 90%
Long Island City, NY					
Close Price/Sq.Ft	569.99	185.04	370.15	522.65	845.33
List Price/Sq.Ft	666.20	246.56	414.09	606.94	983.87
Time on Market	171.78	79.11	86.00	150.00	284.00
Square Footage	1,591.03	925.65	665.00	1,400.00	2,836.00
Age	71.60	31.04	12.00	80.00	99.00
Bathrooms	2.08	1.17	1.00	2.00	3.50
Bedrooms	3.40	2.20	1.00	3.00	6.00
Population Density	33,099.30	14,239.67	14,466.05	34,064.25	50,829.82
College	36.83	10.47	26.87	28.90	49.00
Price/Earnings	2.92	0.95	1.67	3.15	4.49
Ownership Rate	29.46	13.10	15.87	29.86	52.51

Notes: Variables and units of measurement are defined in the text. The sample period is a twelve month window around Amazon's *HQ2* announcement date and it includes information for 395 transactions in Long Island City with complete regressor information.

Our analysis on treatment effects in the NY market is intended to be suggestive only. Our analysis is econometrically sound, but due to the idiosyncratic nature of the NYC housing market, our sample size is small. Our calculations nevertheless suggest that a similar, symmetric negative

[37] Crystal City is excluded completely from the analysis, and is not part of the control group.

[38] We report *CEM* balance statistics in Table D.11.

[39] We include here only the treatment effects of the *HQ2* variable. The full set of estimates for different time horizons and control groups is included in Tables D.6-D.10 in Online Appendix D.

Table B.2: **Long Island City: Amazon's HQ2 Summary (Non-Finalists)**

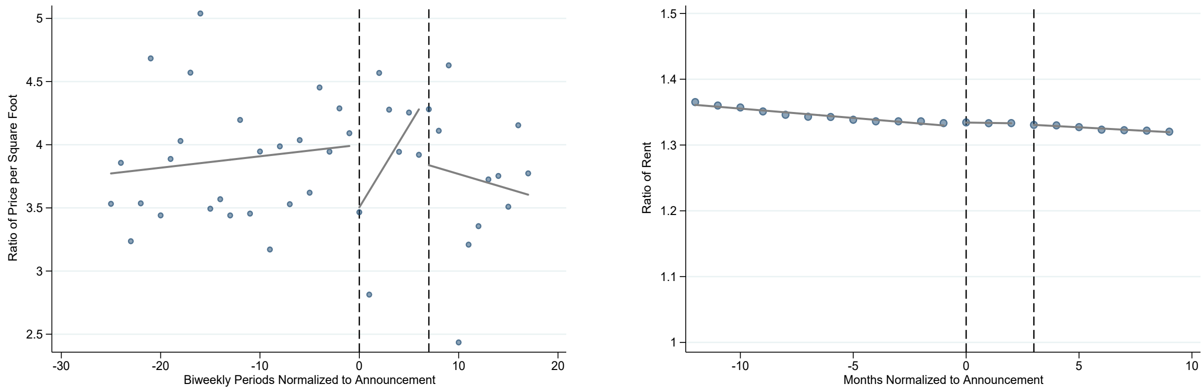
	3 Months after Winning		6 Months after Withdrawal	
	DID	*CEM*	*DID*	*CEM*
Y = Close Price/Sq.Ft				
HQ2	14.82	-6.46	-36.78*	-64.04***
	(18.77)	(35.49)	(21.89)	(23.11)
R^2	0.612	0.000	0.608	0.005
Observations	1,497,408	321	1,980,698	1,421
Treated Obs.	275	73	295	148
Treated Matched	—	74%	—	86%
Y = List Price/Sq.Ft				
HQ2	44.40	-26.53	-51.54***	-55.89***
	(36.27)	(33.00)	(12.89)	(21.04)
R^2	0.543	0.001	0.541	0.003
Observations	1,932,104	482	2,478,000	2,129
Treated Obs.	448	73	514	148
Treated Matched	—	74%	—	86%
Y = Time on Market				
HQ2	3.66	-10.57	2.81	33.11***
	(11.63)	(16.90)	(12.70)	(9.79)
R^2	0.113	0.001	0.121	0.005
Observations	1,990,085	482	2,590,160	2,145
Treated Obs.	477	73	565	148
Treated Matched	—	74%	—	86%

Notes: Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus three months to evaluate the winning announcement and nine for the withdrawal (six months after the withdrawal decision). *ATT* estimation includes transactional covariates as well as property type, metropolitan area, and month fixed effects and uses ZIP code clustered standard errors. For *CEM*, observations are matched on covariates given in Table 4, and the estimates are given by *OLS* using optimal *CEM* weights. Standard errors are reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

shock to residential housing markets occurred due to the withdrawal. We find that the Long Island City experienced no statistically significant effects on housing markets due to the initial winning *HQ2* announcement in the three month window prior to the withdrawal announcement. The point estimates suggest that the withdrawal announcement caused housing market conditions to deteriorate moderately. The *DID* point estimates show that at marginal significance, close prices declined in the six month period after the announcement, whereas the *CEM* estimates indicate larger effects. The evidence is mixed regarding whether housing liquidity deteriorated as a result of the withdrawal. Notably, our *DID* and *CEM* estimates give consistent magnitudes of declines in List Price/Sq.Ft due to the withdrawal. Therefore, we conclude that that seller expectations declined significantly due to the shock. In a more liquid housing market with a longer data sample, we would expect to see stronger realized price effects.

Figure B.1: House Prices and Rents Relative to the Control, Long Island City

(a) Price SQFT Ratio (Long Island City/U.S.) (b) Long Island City Mean Rent Price



Notes: The left panel show bi-weekly average close price per square foot, Close Price/Sq.Ft, ratio in Long Island City relative to the control group around Amazon's *HQ2* announcement date and the withdrawal from Long Island City, respectively. The right panel show the monthly rent price ratios in Long Island City relative to the control group around Amazon's *HQ2* announcement dates.

As in Section 4.4, we extend our analysis of the Long Island City neighborhood to its rental market. Since the rental data is monthly, the November 2018 date is designated as the period t where $\beta_{\text{Announcement}} \mathbb{1}(t > T) = 1$ for the winning announcement in Long Island City, and the February 2019 date is designated as the announcement shock for the withdrawal announcement. For brevity we estimate Equation 2 using one year (12 months) of data prior to the winning in question, and three and six months of data post-announcement. In this case, we further estimate the treatment effect from the withdrawal announcement using 12 months of data prior to the announcement, and six months of data post-announcement. In all cases, the month of the shock is included as post-announcement. The findings are summarized in B.3.

In Long Island City there is no evidence in a change in rental prices after the winning announcement. After the withdrawal announcement, when weighting by population, we find a significant decrease in rental prices of \$64. Similarly, average rental prices in Long Island City are \$2021 implying a reduction in rent prices of less than 3%. These results are much less evident graphically (displayed in Figure B.1 Panel b)), and of smaller magnitude than treatment effects estimated for house prices after these announcements. Similar to Section 4.4 with Crystal City, we do not find much evidence that rental markets were impacted by the *HQ2* shock, indicating segmentation between residential property and rental markets.

Finally, we conclude our analysis for the Long Island City neighborhood by testing whether we observe any distributional shifts in close prices per square foot before and after the withdrawal announcement. We plot the PDF and CDF of close prices per square foot for Long Island City in Figure B.2. Most of the post-withdrawal distribution of Close Price/Sq.Ft shifts to the left, i.e., most housing prices are reduced after Amazon decides not to go ahead with plans to locate in Long Island City. First order stochastic dominance is however rejected, e.g., see Table B.4. Second order

Table B.3: Event Study of Rents in Long Island City

	Winning Announcement		Withdrawal Announcement	
	Unweighted	**Weighted**	**Unweighted**	**Weighted**
Y = Rents				
ATT	66.984	0.262	-9.968	-63.542***
	(82.888)	(21.111)	(37.199)	(17.595)
City	9.197***	9.079***	9.046***	10.240***
	(1.510)	(1.395)	(2.193)	(2.027)
Announcement	-38.703**	-36.486**	-39.346	-52.943**
	(18.946)	(17.121)	(25.682)	(24.016)
Metro FE	Yes	Yes	Yes	Yes
Month FE es	Yes	Yes	Yes	Yes
R-Squared	0.754	0.789	0.751	0.787
Observations	20,383	20,383	24,512	24,512
Treated Obs.	43	43	53	53

Notes: Sample includes one year of monthly ZIP code observations prior to Amazon's *HQ2* announcement date. For the columns under "Winning Announcement", we use three months of data post-announcement, whereas for the "Withdrawal Announcement" columns we use six months of data post-announcement. *OLS* regression including mean transactional covariates as well as share, metropolitan area, and month fixed effects. The "weighted" columns weight ZIP codes by population. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

Table B.4: Stochastic Dominance Tests

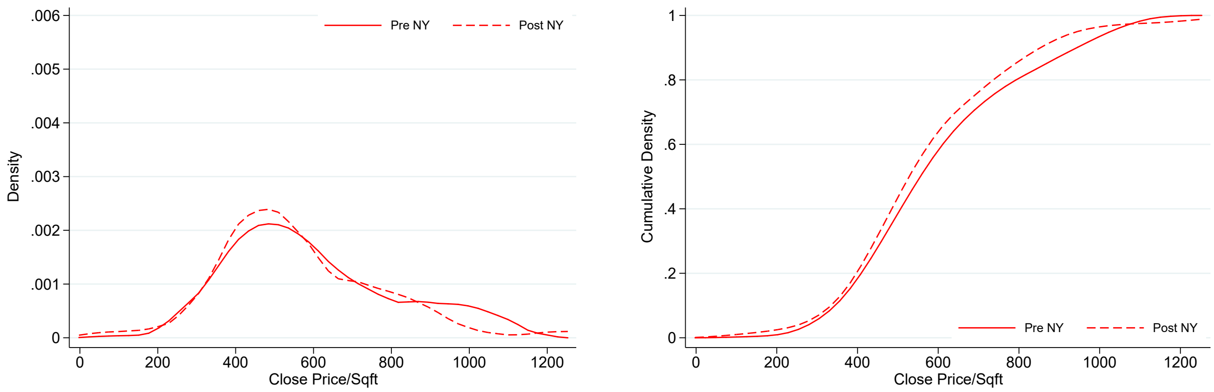
	$H_0 : F(P_{pre}) \leq_i F(P_{post})$		$H_0 : F(P_{post}) \leq_i F(P_{pre})$	
HQ2 Location	**FOSD**	**SOSD**	**FOSD**	**SOSD**
Long Island City, NY	0.944	0.805	0.302	0.151

Notes: Test reports p-values of the Kolmogorov-Smirnov test of stochastic dominance obtained by simulating the maximal difference of two cumulative distribution functions over an evenly spaced grid of 100 points covering the whole support of each simulated distribution of Close Price/Sq.Ft using 1000 replications. This is the "KS1" test in the notation of Barrett and Donald (2003).

stochastic dominance is also rejected although relatively marginally. Overall, Amazon's withdrawal did not create all losers: sellers of very expensive real state, with Close Price/Sq.Ft > 900 witnessed increased prices in their market segment. However, a large majority of losers appear to have been generated by the withdrawal with price reductions for residential houses priced Close Price/Sq.Ft < 700. Thus, we find an uneven effect across housing segments, with owners of very large and expensive houses unaffected Amazon's withdrawal. However, it appears likely that there was some cost to homeowners in the lower and middle ends of the housing distribution in NY after the withdrawal.This analysis supports our conclusion that there is suggestive evidence that the withdrawal announcement was a negative expectation shock to homeowners in Long Island City, and it is likely that the withdrawal disproportionately affected lower-value homeowners.

Figure B.2: Pre/Post Close Price Distribution in Long Island City

(a) NY Close Price Density (b) NY Close Price Distribution



C The Amazon HQ2 Expectations Shock: Other General Equilibrium Model Specifications

Our general equilibrium of Section 5 can be modified slightly to nest special cases. Common specifications in the literature have housing supply fixed and no market segmentation (mortgages) with either fixed or endogenous interest rates. These versions of the model are popular formulations of a traditional user-cost formulation. In this appendix we show that these special cases cannot recover the observed empirical dynamics of an expectations shock.

C.1 The Specification with Fixed Housing Supply, No Mortgages, and Endogenous Interest Rate

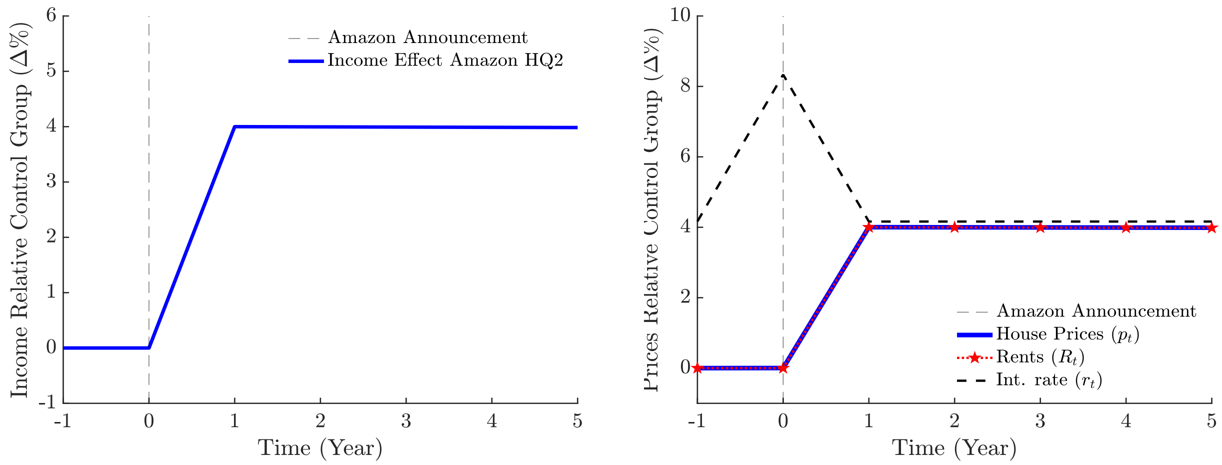
The literature often explores a version of the model with fixed housing supply, $h_t = G(S_t, L_t) = \overline{L}$, which implies, in terms of our model, that housing is land. In this perfectly inelastic case, one expects large effects on prices as a result of increases in demand with no ability to expand supply. For a specification of the model with no mortgages ($\phi = 0$) and log-preferences $U(c_t, h_t) = \ln c_t + \gamma \ln h_t$, the implications of the expectations shock can be derived analytically for house prices, rents, and interest rates. Using the analytic solutions we describe the implications in Figure C.1.

The left panel describes the Amazon *HQ2* shock as a future permanent increase in income $z_{\tau+1} > z_\tau$ in period $\tau = 0$ relative to the control group with no increase[40], $z_{\tau+1} = z_\tau$. The pre-announcement interest rate $r_t^d = (1-\beta)/\beta$ is set a standard value of 4% which implies that $\beta = 0.96$. The share of housing is set to $\gamma = 0.8$. In terms of aggregates, total employment, N_t, is normalized to one, and the stock of housing, $\overline{L}$, to factor proportional to income. Since it is a fixed value, it does not influence the time variation of house prices and rents. The right panel shows house prices, rents, price-rent, and the endogenous interest rates. House prices and rents are calculated relative to the control group, with the pre-trend being the same.

[40] As in our baseline, we model a 4% increase in income.

Figure C.1: Model with Fixed Housing Supply and Log Utility

(a) Amazon *HQ2* Shock (b) House Prices, Rents, Interest Rates



The lack of response of prices is a feature of the log-case where price and income effects cancel out. To prevent prices from increasing after the announcement, the endogenous interest rate increases, reducing cash-flow valuations and offsetting the incentive to buy before house prices increase in the future. The higher interest rate captures that income today is relatively low compared to the higher future income, but the endogenous interest rate effect normalizes to a constant when the income is realized in $t = 1$ and thereafter. This is an extreme case where the interest rate response cancels the increase of house price movements with the announcement, and house prices and rents move one-for-one with the expectations shock. For this reason it is useful to consider a more general specification with constant relative risk aversion (CRRA), $U(c_t, h_t) = ((c_t^\gamma h_t^{1-\gamma})^{1-\sigma} - 1)/(1-\sigma)$, where log-preferences are a special case when $\sigma = 1$.

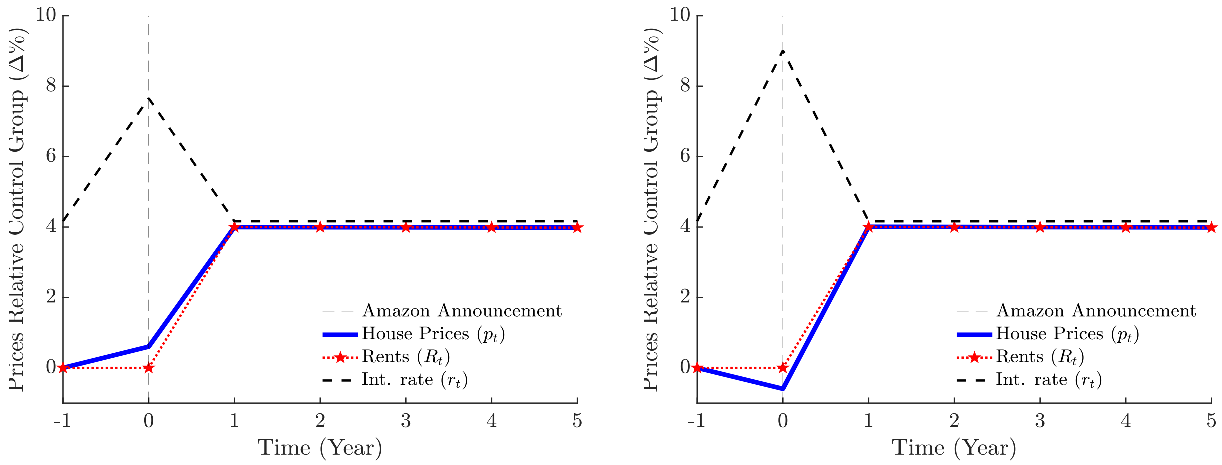
The left panel of Figure C.2 shows the case of when degree of risk aversion is $\sigma = 0.8$, and the left right is $\sigma = 1.2$. A relatively lower value of risk aversion mitigates the interest rate effect, as consumers are more comfortable with changes in consumption over time. This generates a small positive response of house prices with the *HQ2* shock, and a muted rent response. In this case, there is an incentive to pay more for housing today before prices increase next period. Lowering the value of risk aversion increases the announcement effect, but it does not generate overshooting of a stable level where prices converge to the permanent change in income. Not surprisingly, increasing the relative risk aversion yields the opposite effect as can be seen in the right panel of Figure C.2.[41]

This version of the canonical macro-housing model fails to capture the impact of the *HQ2* shock on prices at the time of the announcement, as well as the dynamics of the price-rent ratio, as price and rents move closely together with fixed housing supply. The failure to retrieve the

[41] We have also explored the implications of changing the elasticity of substitution between housing and consumption to values around the unitary elasticity in the baseline and this specification of the model still fails to replicate the dynamics of the expectations shock.

Figure C.2: Model with Fixed Housing Supply and CRRA Utility

(a) σ=0.8 (b) σ=1.2



expectations shock should provide a cautionary tale to empirical research that attempts to estimate expectations shocks using this class of structural asset pricing relationships related to the canonical user-cost specification.

C.2 The Specification with Fixed Housing Supply, No Mortgages, and Exogenous Interest Rate

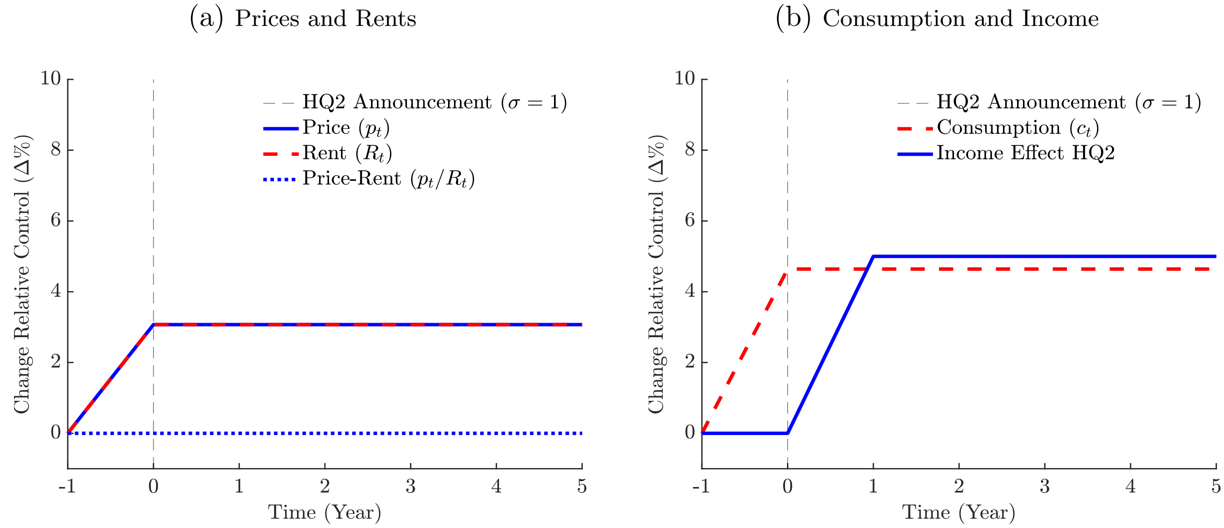
The previous model specification demonstrates that the simultaneous determination of house prices and interests rate may be an excessive test for that case of the model. We change the canonical user cost specification by assuming an exogenous interest rates determined through access to outside credit[42] as is commonly assumed in the macro-housing literature. Using log-preferences, we plot the dynamics of an expectations shock in Figure C.3.

Panel a) shows that the Amazon HQ2 shock generates an announcement effect for house prices which does not occur when the expected income is realized in $t = 1$. Therefore eliminating the offsetting effect of the interest rate response helps to recover the timing of the observed price effect. Unfortunately the house price increase is matched by a one-for-one increase in rents. This leaves the price-rent ratio constant, which is inconsistent with the evidence presented in Section 4.4. With fixed interest rates, prices and rents increase immediately, however they converge to a lower steady state level than when interest rates are endogenous.

It is obvious that fixing the interest rate eliminates the jump response of the interest rate in response to future income growth. In this scenario agents can now trade an asset with external agents. This trade at a given interest rate allows agents to bring future spending opportunities to the time of the announcement, τ. The increase in future income causes consumers to increase

[42] For comparability with the results in Section C.1, the exogenous interest rate, $r_t^* = 1/\beta - 1$, is set the same 4% value. Therefore, in the absence of the *HQ2* shock the agents are not trading assets (borrowing or lending) with external agents.

Figure C.3: Model: Fixed Housing Supply/Interest and Log Utility



their consumption at the time of the announcement as seen in Figure C.3 in Panel b). Note that consumption does not increase to the level of future income, as consumers adjust their consumption through uncollateralized borrowing. Potentially, consumers could increase their spending on housing services. Since housing is in fixed supply, the equilibrium price will increase to prevent increased housing expenditure.

It is common in empirical research to use exogenous interest rates in the estimation of factors that drive house prices. The prediction of the model indicates that even asset pricing relationships with exogenous interest rates can fail to identify and recover the size of expectations shocks.

Online Appendix

D Additional Evidence

Table D.1: Amazon HQ2 Announcement Treated ZIP Codes

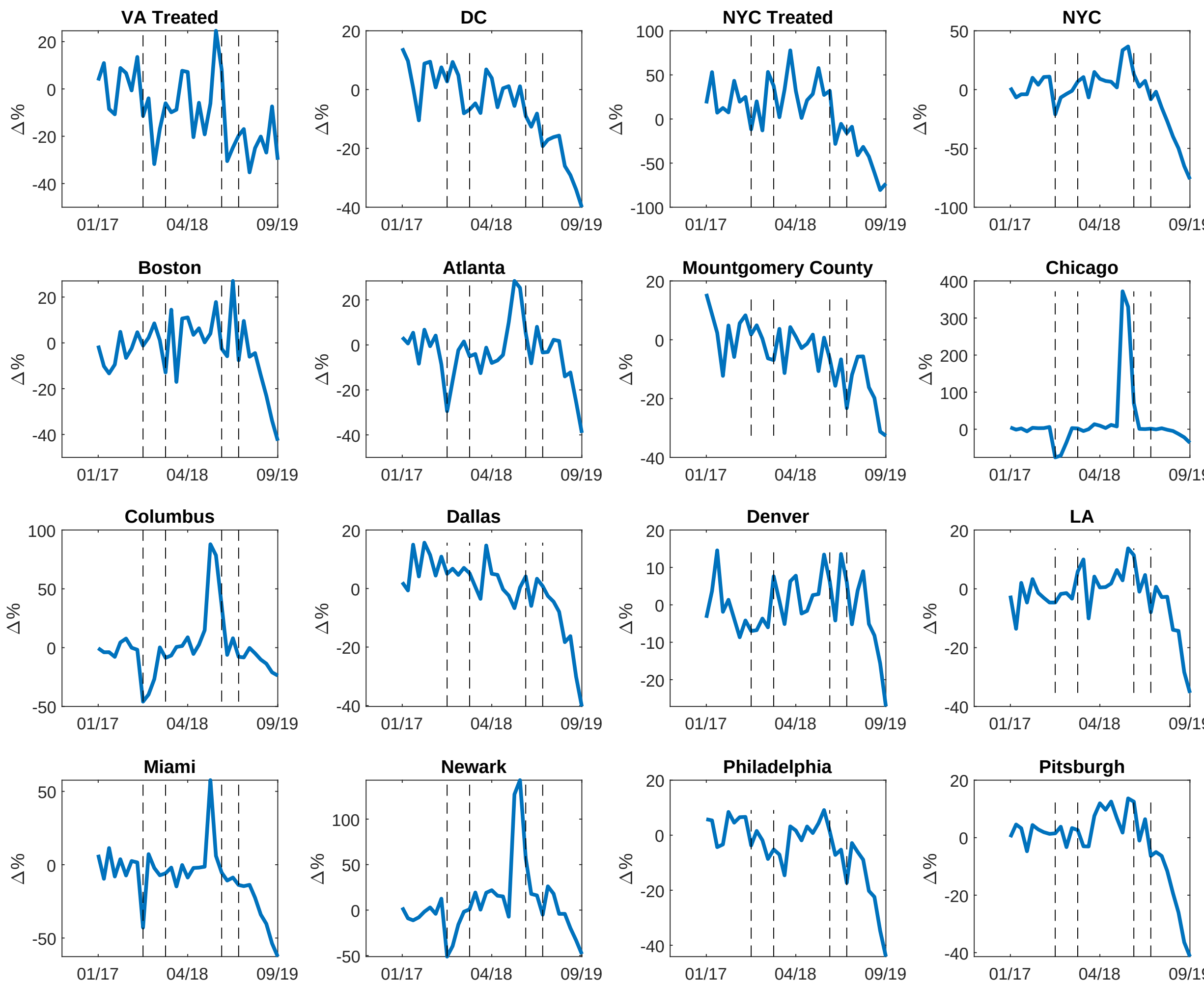
Region	ZIP Codes
Crystal City	22202, 22211, 22305, 22206, 22204, 22301
Long Island City	11101, 11102, 11103, 11104, 11105, 11106, 11109, 11120, 11222 11378, 11377

Table D.2: Uncontaminated Control Group

CBSAs		
Riverside-San Bernardino-Ontario, CA	Virginia Beach-Norfolk-Newport News, VA	Dayton, OH
Tampa-St. Petersburg-Clearwater, FL	Cleveland-Elyria-Mentor, OH	Bridgeport-Stamford-Norwalk, CT
Houston-Baytown-Sugar Land, TX	Detroit-Livonia-Dearborn, MI	Rochester, NY
Phoenix-Mesa-Scottsdale, AZ	Jacksonville, FL	Naples-Marco Island, FL A
Orlando, FL	Cape Coral-Fort Myers, FL	Tacoma, WA
Minneapolis-St. Paul-Bloomington, MN-WI	Providence-New Bedford-Fall River, RI-M	New Haven-Milford, CT
Warren-Farmington-Hills-Troy, MI	Oklahoma City, OK	Albany-Schenectady-Troy, NY
San Diego-Carlsbad-San Marcos, CA	Hartford-West Hartford-East Hartford, C	Buffalo-Niagara Falls, NY
Edison, NJ	Louisville, KY-IN	Ann Arbor, MI
Seattle-Bellevue-Everett, WA	Richmond, VA	Myrtle Beach-Conway-North Myrtle Beach,
St. Louis, MO-IL	Lakeland, FL	Baton Rouge, LA
Baltimore-Towson, MD	Camden, NJ	Flint, MI
Las Vegas-Paradise, NV	Tucson, AZ	Worcester, MA
Portland-Vancouver-Beaverton, OR-WA	Birmingham-Hoover, AL	Manchester-Nashua, NH
Sarasota-Bradenton-Venice, FL	Milwaukee-Waukesha-West Allis, WI	Rockingham County, NH
Cincinnati-Middletown, OH-KY-IN	Boise City-Nampa, ID	Boulder, CO
Kansas City, MO-KS	Tulsa, OK	Greeley, CO
Charlotte-Gastonia-Concord, NC-SC	Allentown-Bethlehem-Easton, PA-NJ	Sacramento–Arden-Arcade–Roseville, CA

Figure D.1 shows that there is no pattern differences in the growth of *MLS* listings around Amazon's *HQ2* announcement across different metropolitan area regardless of whether they are selected or not, or if they are close to the selected locations.

Figure D.1: Change in Listings Around HQ2 Announcement



Notes: Percent change in residential real estate *MLS* listings from the previous year in selected finalist cities, winning locations, and their surrounding metropolitan area.

Table D.3: Dynamic Treatments Estimator for Crystal City, VA

	List Price/Sq.Ft		Close Price/Sq.Ft	
Period × Winner				
Month -10	-4.564	(8.635)	0.889	(15.842)
Month -9	12.373	(10.323)	-4.529	(10.940)
Month -8	10.030	(7.037)	2.074	(12.368)
Month -7	10.766*	(6.323)	6.440	(13.831)
Month -6	7.164**	(3.545)	10.217	(10.421)
Month -5	-3.706	(10.552)	-0.428	(8.331)
Month -4	18.807**	(9.159)	-2.798	(9.093)
Month -3	3.266	(6.976)	-5.166	(10.611)
Month -2	36.671***	(5.940)	-2.859	(8.727)
Month -1	14.310*	(7.779)	11.056	(6.862)
Month 1	28.481**	(13.584)	15.083**	(5.905)
Month 2	1.452	(6.583)	16.367	(10.651)
Month 3	9.722	(12.241)	19.772	(13.950)
Month 4	45.495***	(8.067)	4.199	(9.483)
Month 5	54.618***	(8.114)	45.812***	(11.540)
Month 6	39.501***	(11.283)	49.089***	(11.948)
Month 7	51.691***	(16.046)	51.515***	(13.597)
Month 8	45.008***	(16.174)	37.778***	(10.309)
Month 9	28.847***	(6.861)	44.938***	(12.316)
Month 10	44.135***	(14.896)	28.531***	(10.355)
Month 11	42.057	(25.975)	27.521	(17.988)
Covariates	Yes		Yes	
R^2	0.522		0.600	
Observations	3,622,103		2,797,373	

Notes: Sample includes eleven months of observations prior to Amazon's *HQ2* announcement date plus up to a year after. *OLS* regression including transactional covariates as well as property type, metropolitan area, and month fixed effects. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

Figure D.2: Pre/Post Close Price Distribution in Non-Finalist Control Group

(a) Control Close Price Density (b) Control Close Price Distribution

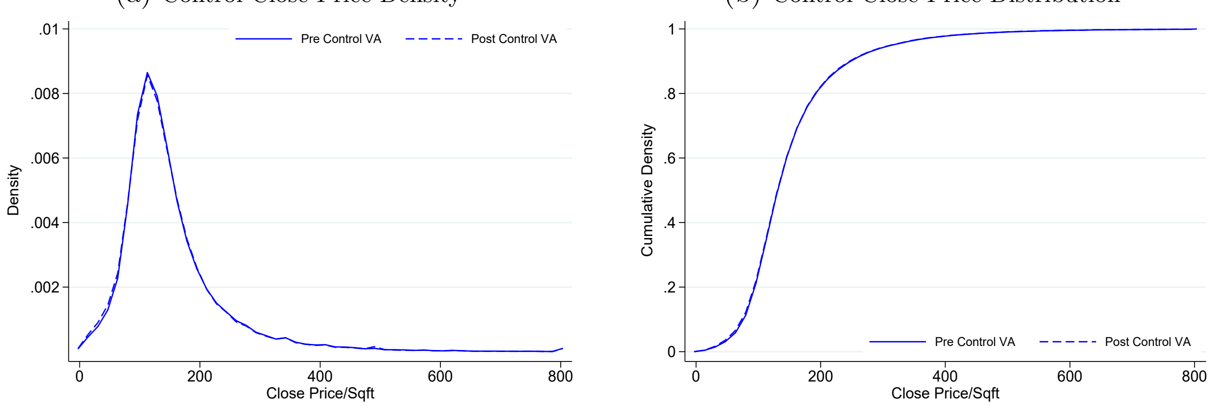


Table D.4: Crystal City: Amazon's HQ2 and Listing Price

	I	II	III	IV	V	VI
Announcement	3.355***	2.947***	3.022***	-3.256***	-2.750***	-1.024*
	(0.142)	(0.287)	(0.254)	(0.708)	(0.589)	(0.607)
Winner	191.686***	182.218***	7.003	7.109	88.909***	88.937***
	(2.275)	(18.484)	(15.134)	(15.234)	(14.385)	(14.356)
HQ2	29.310***	30.971***	30.370***	31.331***	29.893***	29.796***
	(4.695)	(2.515)	(1.973)	(1.916)	(1.641)	(1.633)
Square Footage		0.000	-0.014***	-0.014***	-0.008***	-0.008***
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age		0.110***	-0.331***	-0.332***	0.005	0.005
		(0.036)	(0.038)	(0.038)	(0.026)	(0.026)
Bathrooms		26.431***	20.255***	20.167***	15.573***	15.549***
		(1.388)	(1.313)	(1.310)	(0.669)	(0.670)
Bedrooms		-16.107***	-9.955***	-9.873***	-11.708***	-11.729***
		(1.029)	(0.902)	(0.898)	(0.620)	(0.620)
Population Density			0.008***	0.008***	0.003***	0.003***
			(0.001)	(0.001)	(0.001)	(0.001)
College			2.959***	2.959***	2.680***	2.682***
			(0.133)	(0.133)	(0.077)	(0.077)
Price/Earnings			9.971***	9.925***	5.318***	5.315***
			(0.696)	(0.695)	(0.542)	(0.542)
Ownership Rate			-0.588***	-0.587***	-0.341***	-0.343***
			(0.139)	(0.139)	(0.090)	(0.090)
Mortgage Rate				-12.056***	-12.374***	-13.184***
				(1.166)	(0.750)	(0.768)
Constant	173.708***	164.889***	71.622***	126.740***	105.649***	107.387***
	(0.080)	(13.180)	(12.806)	(13.134)	(12.281)	(12.277)
Property Type FE	No	No	Yes	Yes	Yes	Yes
Metro FE	No	No	No	No	Yes	Yes
Month FE	No	No	No	No	No	Yes
R^2	0.004	0.060	0.252	0.253	0.543	0.543
Observations	2,384,564	2,384,564	2,384,564	2,384,564	2,384,564	2,384,564
Treated Obs.	2,321	2,321	2,321	2,321	2,321	2,321

Notes: Endogenous variable is List Price/Sq.Ft. Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus six months afterwards. *OLS* regression. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

Table D.5: Crystal City: Amazon's HQ2 and Time on the Market

	I	II	III	IV	V	VI
ANNOUNCEMENT	8.624***	8.846***	8.721***	8.016***	7.909***	12.052***
	(0.108)	(0.203)	(0.203)	(0.206)	(0.199)	(0.207)
WINNER	-28.651***	-23.895***	1.642	1.784	-44.423***	-43.214***
	(1.223)	(3.452)	(5.172)	(5.172)	(4.585)	(4.497)
HQ2	-15.837***	-14.632***	-14.783***	-14.612***	-14.233***	-11.941***
	(2.168)	(2.358)	(2.288)	(2.271)	(2.265)	(2.427)
SQUARE FOOTAGE		0.018***	0.018***	0.018***	0.018***	0.018***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
AGE		0.022**	0.078***	0.080***	-0.010	-0.010
		(0.011)	(0.010)	(0.010)	(0.009)	(0.009)
BATHROOMS		2.546***	2.849***	2.838***	1.634***	1.654***
		(0.344)	(0.315)	(0.315)	(0.223)	(0.223)
BEDROOMS		-4.585***	-4.358***	-4.353***	-3.502***	-3.651***
		(0.274)	(0.260)	(0.260)	(0.197)	(0.200)
POPULATION DENSITY			-0.003***	-0.003***	-0.002***	-0.002***
			(0.000)	(0.000)	(0.000)	(0.000)
COLLEGE			-0.050*	-0.054**	0.024	0.022
			(0.026)	(0.026)	(0.019)	(0.019)
PRICE/EARNINGS			2.618***	2.614***	3.084***	3.034***
			(0.186)	(0.186)	(0.133)	(0.134)
OWNERSHIP RATE			-0.032	-0.029	-0.158***	-0.156***
			(0.034)	(0.034)	(0.024)	(0.024)
MORTGAGE RATE				-9.094***	-8.195***	-18.071***
				(0.302)	(0.306)	(0.333)
CONSTANT	91.961***	61.013***	68.855***	109.151***	151.981***	180.277***
	(0.058)	(2.409)	(3.729)	(3.993)	(3.774)	(3.781)
Property Type FE	No	No	Yes	Yes	Yes	Yes
Metro FE	No	No	No	No	Yes	Yes
Month FE	No	No	No	No	No	Yes
R^2	0.003	0.045	0.055	0.056	0.094	0.115
Observations	2,414,427	2,414,427	2,414,427	2,414,427	2,414,427	2,414,427
Treated Obs.	2364	2364	2364	2364	2364	2364

Notes: Endogenous variable is TIME ON MARKET. Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus six months afterwards. *OLS* regression. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

Table D.6: Long Island City: Amazon's HQ2 and Closing Price

	I	II	III	IV	V	VI
Announcement	3.315***	3.156***	3.267***	2.554***	3.274***	4.149***
	(0.122)	(0.175)	(0.159)	(0.349)	(0.312)	(0.308)
Winner	431.911***	444.483***	187.444***	187.429***	355.547***	355.891***
	(13.376)	(40.955)	(41.083)	(41.086)	(28.822)	(28.827)
HQ2	-37.094	-29.373	-42.016*	-42.009*	-37.575*	-36.777*
	(23.033)	(23.040)	(23.996)	(23.991)	(21.831)	(21.889)
Square Footage		-0.005***	-0.017***	-0.017***	-0.013***	-0.013***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age		0.026	-0.394***	-0.394***	-0.090***	-0.090***
		(0.031)	(0.029)	(0.029)	(0.020)	(0.020)
Bathrooms		19.597***	13.902***	13.902***	11.597***	11.586***
		(1.105)	(1.050)	(1.050)	(0.519)	(0.519)
Bedrooms		-10.822***	-5.505***	-5.506***	-7.511***	-7.523***
		(0.818)	(0.710)	(0.710)	(0.477)	(0.477)
Population Density			0.008***	0.008***	0.003***	0.003***
			(0.001)	(0.001)	(0.000)	(0.000)
College			2.552***	2.552***	2.274***	2.272***
			(0.109)	(0.110)	(0.058)	(0.058)
Price/Earnings			7.955***	7.955***	3.832***	3.837***
			(0.533)	(0.533)	(0.388)	(0.388)
Ownership Rate			-0.506***	-0.506***	-0.237***	-0.238***
			(0.113)	(0.113)	(0.070)	(0.070)
Mortgage Rate				-1.179**	-0.706	1.526***
				(0.464)	(0.442)	(0.480)
Constant	150.484***	148.848***	70.690***	76.115***	57.587***	46.861***
	(0.074)	(9.111)	(9.701)	(10.057)	(8.676)	(8.657)
Property Type FE	No	No	Yes	Yes	Yes	Yes
Metro FE	No	No	No	No	Yes	Yes
Month FE	No	No	No	No	No	Yes
R^2	0.004	0.047	0.262	0.262	0.608	0.608
Observations	1,980,698	1,980,698	1,980,698	1,980,698	1,980,698	1,980,698
Treated Obs.	295	295	295	295	295	295

Notes: Endogenous variable is Close Price/Sq.Ft. Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus six months afterwards. *OLS* regression. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

Table D.7: Long Island City: Amazon's HQ2 and Listing Price

	I	II	III	IV	V	VI
Announcement	2.322***	2.681***	1.303***	-9.022***	-0.600	-11.809***
	(0.133)	(0.327)	(0.312)	(1.187)	(0.451)	(0.764)
Winner	556.555***	571.475***	285.685***	283.296***	350.870***	477.041***
	(14.288)	(54.524)	(54.376)	(54.436)	(26.879)	(45.836)
HQ2	-59.212**	-54.946***	-41.485**	-41.273**	-29.578*	-51.538***
	(25.143)	(12.655)	(19.112)	(19.033)	(17.585)	(12.889)
Square Footage		-0.002	-0.016***	-0.016***	-0.014***	-0.009***
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age		0.110***	-0.343***	-0.344***	-0.085***	-0.000
		(0.036)	(0.037)	(0.037)	(0.020)	(0.025)
Bathrooms		25.393***	19.134***	19.009***	11.437***	14.812***
		(1.452)	(1.335)	(1.330)	(0.513)	(0.668)
Bedrooms		-14.997***	-8.842***	-8.720***	-6.995***	-11.118***
		(0.973)	(0.843)	(0.837)	(0.464)	(0.579)
Population Density			0.008***	0.008***	0.003***	0.003***
			(0.001)	(0.001)	(0.000)	(0.001)
College			2.944***	2.949***	2.254***	2.648***
			(0.133)	(0.133)	(0.058)	(0.074)
Price/Earnings			9.865***	9.810***	3.711***	5.128***
			(0.667)	(0.666)	(0.376)	(0.499)
Ownership Rate			-0.638***	-0.638***	-0.229***	-0.357***
			(0.136)	(0.136)	(0.068)	(0.085)
Mortgage Rate				-14.540***	-4.644***	-19.107***
				(1.315)	(0.532)	(0.823)
Constant	174.569***	166.032***	78.651***	144.248***	76.786***	136.401***
	(0.080)	(12.256)	(12.127)	(12.804)	(8.156)	(11.361)
Property Type FE	No	No	Yes	Yes	Yes	Yes
Metro FE	No	No	No	No	Yes	Yes
Month FE	No	No	No	No	No	Yes
R^2	0.006	0.057	0.253	0.254	0.617	0.554
Observations	2,478,000	2,478,000	2,478,000	2,478,000	1,890,931	2,478,000
Treated Obs.	514	514	514	514	514	514

Notes: Endogenous variable is List Price/Sq.Ft. Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus six months afterwards. *OLS* regression. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

Table D.8: Long Island City: Amazon's HQ2 and Time on the Market

	I	II	III	IV	V	VI
Announcement	0.425***	0.288	0.220	6.823***	6.719***	0.324
	(0.100)	(0.189)	(0.190)	(0.278)	(0.271)	(0.271)
Winner	57.300***	61.154***	136.198***	136.359***	76.969***	76.132***
	(4.427)	(4.855)	(13.577)	(13.609)	(9.621)	(9.521)
HQ2	15.422*	12.513	9.584	9.587	10.160	2.809
	(8.826)	(12.206)	(13.063)	(13.022)	(13.120)	(12.701)
Square Footage		0.017***	0.018***	0.018***	0.018***	0.018***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age		0.012	0.065***	0.065***	-0.005	-0.009
		(0.011)	(0.010)	(0.010)	(0.009)	(0.009)
Bathrooms		2.980***	3.384***	3.387***	1.596***	1.648***
		(0.354)	(0.328)	(0.328)	(0.223)	(0.217)
Bedrooms		-5.066***	-4.991***	-4.984***	-3.865***	-3.812***
		(0.289)	(0.274)	(0.274)	(0.203)	(0.196)
Population Density			-0.003***	-0.003***	-0.002***	-0.002***
			(0.000)	(0.000)	(0.000)	(0.000)
College			-0.095***	-0.093***	0.003	0.011
			(0.027)	(0.027)	(0.019)	(0.018)
Price/Earnings			2.610***	2.610***	3.123***	3.070***
			(0.194)	(0.194)	(0.141)	(0.137)
Ownership Rate			-0.028	-0.028	-0.152***	-0.142***
			(0.035)	(0.035)	(0.023)	(0.023)
Mortgage Rate				10.864***	10.772***	-9.296***
				(0.369)	(0.366)	(0.427)
Constant	92.506***	65.602***	73.537***	23.550***	68.330***	155.566***
	(0.059)	(2.579)	(3.965)	(4.287)	(3.932)	(3.976)
Property Type FE	No	No	Yes	Yes	Yes	Yes
Metro FE	No	No	No	No	Yes	Yes
Month FE	No	No	No	No	No	Yes
R^2	0.000	0.040	0.050	0.050	0.092	0.121
Observations	2,590,160	2,590,160	2,590,160	2,590,160	2,590,160	2,590,160
Treated Obs.	565	565	565	565	565	565

Notes: Endogenous variable is Time on Market. Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus six months afterwards. *OLS* regression. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

Table D.9: Long Island City: Amazon's HQ2 Event Study (Non-Finalists)

	Winning Announcement		Withdrawal Announcement			
	1 Month	**3 Months**	**1 Months**	**3 Months**	**6 Months**	**9 Months**
Y = Close Price/Sq.Ft						
Announcement	-0.191	1.478***	3.537***	4.654***	4.149***	3.567***
	(0.307)	(0.365)	(0.464)	(0.355)	(0.308)	(0.270)
Winner	342.283***	343.435***	355.419***	354.896***	355.891***	359.149***
	(21.265)	(21.160)	(28.780)	(28.785)	(28.827)	(28.843)
HQ2	-57.422***	14.819	19.641	-33.353	-36.777*	-29.093
	(21.361)	(18.767)	(55.761)	(21.051)	(21.889)	(18.335)
R^2	0.615	0.612	0.611	0.609	0.608	0.606
Observations	1,344,534	1,497,408	1,356,986	1,594,709	1,980,698	2,329,430
Treated Obs.	234	275	225	253	295	339
Y = List Price/Sq.Ft						
Announcement	7.024***	3.396***	-5.772***	-9.726***	-11.809***	-12.141***
	(0.440)	(0.488)	(0.766)	(0.737)	(0.764)	(0.770)
Winner	469.395***	467.988***	474.329***	474.506***	477.041***	477.608***
	(42.122)	(42.102)	(45.789)	(45.810)	(45.836)	(45.833)
HQ2	74.671	44.398	-68.540*	-41.684***	-51.538***	-43.715***
	(61.675)	(36.270)	(36.368)	(12.275)	(12.889)	(14.729)
R^2	0.546	0.543	0.550	0.551	0.554	0.556
Observations	1,721,561	1,932,104	1,770,142	2,089,511	2,478,000	2,594,891
Treated Obs.	420	448	405	470	514	524
Y = Time on Market						
Announcement	13.624***	22.023***	8.441***	0.890***	0.324	1.701***
	(0.323)	(0.282)	(0.338)	(0.287)	(0.271)	(0.260)
Winner	80.426***	79.072***	80.817***	78.686***	76.132***	76.479***
	(9.488)	(9.062)	(10.085)	(9.991)	(9.521)	(9.406)
HQ2	-9.896	3.656	-14.362	15.455	2.809	2.949
	(16.766)	(11.625)	(25.335)	(17.562)	(12.701)	(10.206)
R^2	0.100	0.113	0.102	0.118	0.121	0.109
Observations	1,766,079	1,990,085	1,793,900	2,095,765	2,590,160	3,061,389
Treated Obs.	409	477	416	474	565	668

Notes: Sample includes one year of observations prior to Amazon's *HQ2* announcement date plus the indicated number of months afterwards. *OLS* regression including transactional covariates as well as property type, metropolitan area, and month fixed effects. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

Table D.10: Long Island City: Amazon's HQ2 CEM Estimates (Non-Finalists)

	Winning Announcement		Withdrawal Announcement			
	1 Month	**3 Months**	**1 Months**	**3 Months**	**6 Months**	**9 Months**
Y = Close Price/Sq.Ft						
HQ2	-66.67	-6.46	-13.48	-66.45*	-64.04***	-52.61***
	(67.67)	(35.49)	(105.35)	(35.45)	(23.11)	(18.44)
R^2	0.012	0.000	0.001	0.007	0.005	0.003
Observations	80	321	21	520	1,421	2,623
Treated Obs.	19	73	10	61	148	250
Treated Matched	61%	74%	42%	74%	86%	91%
Y = List Price/Sq.Ft						
HQ2	-48.59	-26.53	7.43	-63.65*	-55.89***	-44.17***
	(70.02)	(33.00)	(105.55)	(35.15)	(21.04)	(16.44)
R^2	0.005	0.001	0.000	0.005	0.003	0.002
Observations	106	482	33	712	2,129	3,908
Treated Obs.	19	81	10	61	148	250
Treated Matched	61%	74%	42%	74%	86%	91%
Y = Time on Market						
HQ2	-12.91	-10.57	66.52*	50.28***	33.11***	18.84**
	((32.33)	(16.90)	(37.060)	(16.36)	(9.79)	(7.84)
R^2	0.002	0.001	0.081	0.013	0.005	0.001
Observations	106	482	36	725	2,145	3,936
Treated Obs.	19	73	10	61	148	250
Treated Matched	61%	74%	42%	74%	86%	91%

Notes: Sample includes transactions after Amazon's *HQ2* announcement date in Long Island City and across non-finalist cities. Observations are matched on covariates given in Table 4, and the estimates are given by *OLS* using optimal CEM weights. Standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

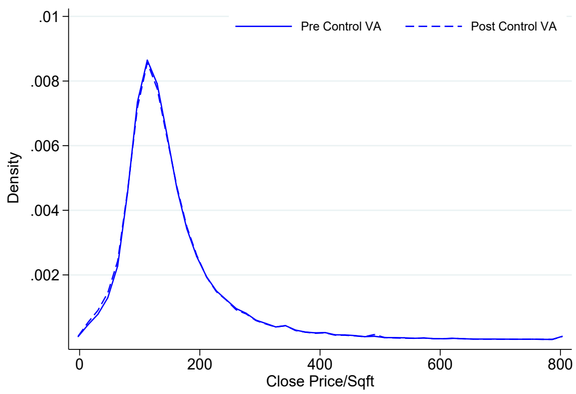
Table D.11: Long Island City: CEM Balance Analysis

	CEM-matched		All Data	
	Control	Treated	Control	Treated
Three Months after Winning				
Square Footage	1,773.30	1,703.86	2008.51	1863.89
Age	59.41	66.00	36.79	69.20
Bathrooms	2.20	2.24	2.34	2.48
Bedrooms	3.08	3.62	3.27	4.05
Population Density	6,127.27	31,275.02	2,545.19	31,977.29
College	35.88	35.40	332.65	37.19
Price/Earnings	3.59	2.60	1.61	2.77
Six Months after Withdrawal				
Square Footage	1,871.51	1,860.94	2,016.92	1,916.71
Age	65.10	67.74	35.99	67.64
Bathrooms	2.25	2.48	2.36	2.60
Bedrooms	3.42	3.91	3.27	4.09
Population Density	6,572.06	33,519.66	2,512.47	33,366.01
College	39.39	38.86	33.32	38.25
Price/Earnings	4.16	3.35	1.59	3.40

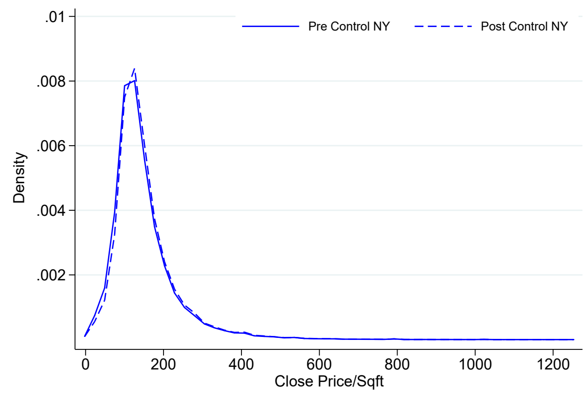
Notes: Sample means for Long Island City, NY. Treated observations in the *CEM* sample receive weight of 1. Control observations in the *CEM* sample receive a weight equal to the ratio of the number of treated and control observations in their specific stratum multiplied by the ratio of the total number of matched treated and control observations.

Figure D.3: Pre/Post Close Price Distribution in Control Cities

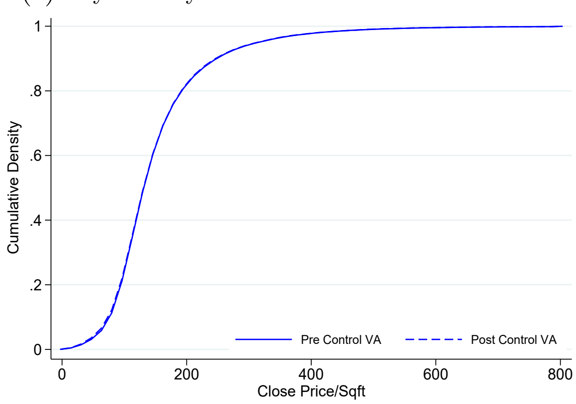
(a) Crystal City Control Close Price Density



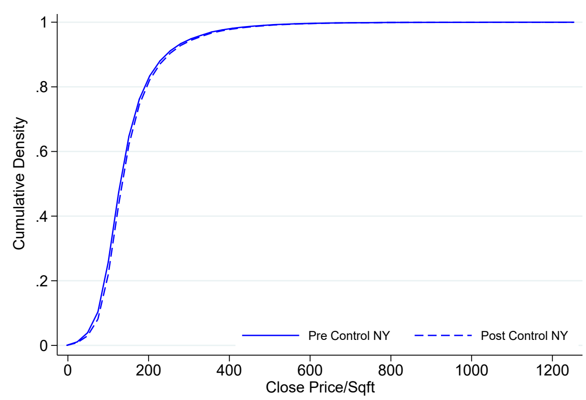
(b) Long Island City Control Close Price Density



(c) Crystal City Control Close Price Distribution



(d) Long Island City Control Close Price Distribution



E Expectations Around Finalist Announcement

Section 4 of the main text argues that the winning announcement shock increased house prices in Crystal City, VA. The expected income shocks to these areas was never realized in our data sample, yet house prices reacted to the expectations shock. A natural extension of our baseline analysis is whether the announcement of the Amazon *HQ2* finalists caused a similar movement in prices as the winning announcement, as one might expect that house prices would rise in anticipation of higher expected future income in a metropolitan area. In this section we test whether the winning areas or any of the most likely finalists experienced significant changes in housing conditions around the finalist announcement that occurred on January 18, 2018.

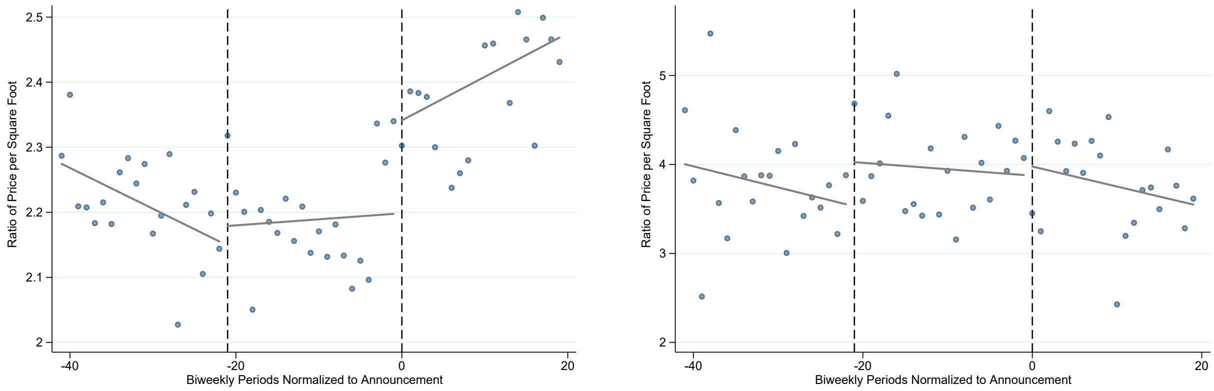
The most natural candidates to experience a significant change in housing conditions as a result of the finalist announcement are the locations that became the winning location. The online oddsmaker Bovada set odds for which area would be chosen as the *HQ2* prior to the winning announcement. The winning locations present a counterfactual to each other as Northern Virginia was a heavy favorite with odds of -290, whereas New York City was one of the less likely finalists to win with odds of +6000.

Graphically, the differences in perceived likelihood of winning the *HQ2* location does not appear to have translated to differential trends of the finalist announcement as we plot the trends in house prices in Figure E.1. This figure is generated using the exact time periods and methodology as Figure 2 by dividing the prices in the treated area by pries in the non-Finalist control group, with the sole difference being that Figure E.1 has been extended to include a longer series of data prior to the finalist announcement. The vertical lines in period -21 represent the finalist announcement on January 18, 2018, whereas the second vertical lines in period 0 are the winning announcements. There does not appear to be any significant price trend change around the finalist announcement in either location, and certainly not a noticeable discontinuity such as the one observed in Crystal City around the winning announcement. This figure would suggest that regardless of the betting odds the eventual winning location did not experience an increase of prices after the finalist announcement, in fact prices fell relative to the control prior to the winning announcement.

Despite the lack of graphical evidence of a change in price trend around the finalist announcement, we estimate a modification of our baseline difference-in-differences specification to formally test whether the finalist announcement generated significant treatment effects for close prices, list prices, and time on market. To do so, we estimate Equation 1 and shift the announcement period to be January 18, 2018 the date of the finalist announcement. For both Crystal City and Long Island City, we estimate (1) with a year of transactions prior to and six months of data after the finalist announcement. We report the results in Table E.1. For Crystal City, there is no significant effect from the finalist announcement on list prices, and close prices actually deteriorated somewhat after the announcement. In addition, liquidity in this market improved significantly after the finalist announcement. For the case of Long Island City, there are no significant effects from the finalist announcement.

Figure E.1: House Prices Before and After HQ2 and Finalist Announcements

(a) Crystal City Mean Close Price | (b) Long Island City Mean Close Price



Notes: Figures show bi-weekly average close price per square foot, Close Price/Sq.Ft, ratio in the VA and NY areas relative to the control group around Amazon's finalist announcement and the *HQ2* announcement date, respectively.

Table E.1: Event Study around Finalist Announcement (Winners)

	List Price	**Close Price**	**TOM**
City = Crystal City			
Announcement	6.370***	8.381***	50.208***
	(0.496)	(0.351)	(0.465)
Winner	88.180***	91.370***	-27.930***
	(15.032)	(15.147)	(2.823)
HQ2	-3.894	-7.970*	-14.640***
	(5.083)	(4.651)	(2.055)
R^2	0.525	0.629	0.121
Observations	2,411,720	1,647,593	2,095,498
Treated Obs.	5397	4054	4854
City = Long Island City			
Announcement	6.327***	8.432***	50.208***
	(0.498)	(0.352)	(0.466)
Winner	474.634***	354.084***	65.242***
	(56.416)	(29.701)	(8.476)
HQ2	5.778	-13.177	8.575
	(27.932)	(22.565)	(8.011)
R^2	0.526	0.629	0.121
Observations	2,407,516	1,644,034	2,091,455
Treated Obs.	1193	495	811

Notes: Sample includes one year of observations prior to Amazon's Finalist announcement date plus six months afterwards. *OLS* regression including transactional covariates as well as property type, metropolitan area, and month fixed effects. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.

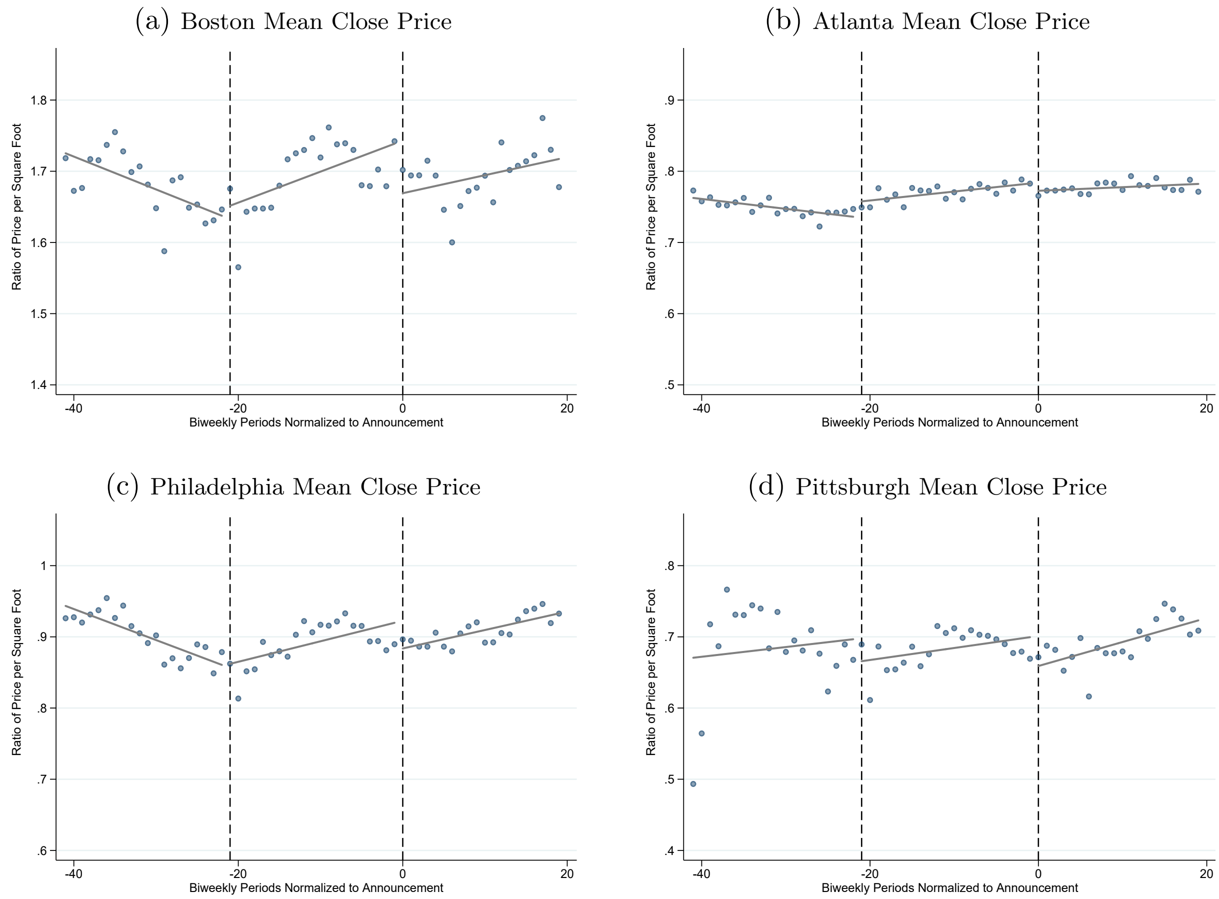
In addition to testing whether the finalist announcement generated movements in housing conditions in the eventual winning locations, we test whether four of the most likely finalist metropolitan areas experienced changes in prices or liquidity around the announcement. To do so we examine housing conditions in the metro Boston area (+600 odds), Atlanta (+1000 odds), Pittsburgh (+2000 odds), and Philadelphia (+4000 odds). These four metro areas are the most likely four candidates we have a significant number of real estate transactions for that are a significant distance from Crystal City, VA and Long Island City, NY.

One important note is that although we know the eventual neighborhoods for Northern Virginia and New York City, Amazon never publicly disclosed specific locations within its chosen finalists for the eventual building site of *HQ2* . Therefore, we use the entirety of the finalist metros in our analysis to reflect the uncertainty regarding an ultimate location, this increases the size of the treatment groups significantly relative to the baseline analyses. Using the same periods and methodology as Figure E.1, we plot close price trends relative to the non-Finalist control group for the four metros in Figure E.2. These four regions all to greater or lesser degrees exhibit some degree of cyclicality in house prices relative to the control group, a finding that is consistent with aggregate housing series. However, no city appears to experience a significant or discontinuous change in trend around either finalist or winning announcements, especially relative to the discontinuity observed in Crystal City.

We further extend the difference-in-differences analysis described above to use the finalist metros as treated areas (excluding other finalists) and report the results in Table E.2. For listing prices, every finalist has a significant effect, but all are of very small magnitude and the only positive effect occurred in Boston. Similarly, there was no significant effect on close prices in the most likely finalists of Boston and Atlanta, while Philadelphia and Pittsburgh experienced small negative effects. The results for time on market are interesting in that Boston did not experience any significant effect, Atlanta witnessed a modest increase in liquidity, whereas Philadelphia and Pittsburgh experienced decreased liquidity around the finalist announcement. However, given the magnitude of these results and the lack of identification of a locality within the metro areas that was intended to be Amazon's *HQ2* location, it is our view that these results are coincidentally significant noise generated by the large sample sizes of the treatment groups. This hypothesis is further supported by the fact that the majority of significant results are in the *opposite* direction of what theory would expect. Residential real estate prices declined in Philadelphia and Pittsburgh upon the announcement of their finalist candidacies. The difference-in-difference results indicate that if there were any significant treatment effects from the finalist announcement on finalists it was exceedingly modest in magnitude.

The lack of significant effect from the finalist announcement is unsurprising considering the lack of information Amazon provided regarding the potential *HQ2* locations and the nature of residential real estate investment. Residential real estate units are expensive, especially in highly expensive metropolitan areas such as the finalist locations. Transacting real estate is expensive from a price perspective and also in the amount of time required to transacting units. This costliness, combined with the lack of certainty regarding the eventual winner appears to have caused would-be investors to sit out from speculating on *HQ2* locations until a winner was announced. Even in the likelist location of Northern Virginia, we observe no change in house prices until around the winning announcement.

Figure E.2: Close Price Expectations in Finalists around Announcements



Notes: Figures show bi-weekly average close price per square foot, Close Price/Sq.Ft, ratio in finalist cities relative to the control group around Amazon's finalist announcement and the *HQ2* announcement date, respectively.

Table E.2: Event Study around Finalist Announcement (Likely Finalists)

	List Price	Close Price	TOM
City = Boston			
ANNOUNCEMENT	6.347***	8.249***	50.271***
	(0.490)	(0.349)	(0.460)
WINNER	93.863***	92.256***	-27.288***
	(9.027)	(7.911)	(1.879)
HQ2	3.811***	0.447	0.003
	(1.194)	(0.952)	(0.821)
R^2	0.532	0.634	0.121
Observations	2,477,674	1,690,161	2,148,815
Treated Obs.	71,351	46,622	58,171
City = Atlanta			
ANNOUNCEMENT	6.137***	8.189***	50.658***
	(0.467)	(0.334)	(0.453)
WINNER	-16.414***	-14.620***	-33.585***
	(5.749)	(5.046)	(1.430)
HQ2	-1.238**	0.506	-6.858***
	(0.572)	(0.442)	(0.573)
R^2	0.527	0.628	0.117
Observations	2,701,352	1,838,413	2,349,526
Treated Obs.	295,029	194,874	258,882
City = Philadelphia			
ANNOUNCEMENT	6.657***	8.478***	50.799***
	(0.478)	(0.343)	(0.457)
WINNER	-2.792	-0.651	-12.519***
	(5.995)	(5.195)	(1.588)
HQ2	-3.435***	-4.942***	8.491***
	(0.605)	(0.530)	(0.589)
R^2	0.520	0.622	0.120
Observations	2,549,753	1,729,878	2,207,111
Treated Obs.	143,430	86,339	116,467
City = Pittsburgh			
ANNOUNCEMENT	6.371***	8.361***	49.583***
	(0.493)	(0.350)	(0.468)
WINNER	-23.839***	-21.826***	-28.477***
	(5.696)	(5.113)	(1.564)
HQ2	-3.268***	-5.826***	28.818***
	(1.131)	(1.174)	(1.550)
R^2	0.522	0.626	0.121
Observations	2,426,167	1,654,374	2,104,482
Treated Obs.	19,844	10,835	13,838

Notes: Sample includes one year of observations prior to Amazon's Finalist announcement date plus six months afterwards. *OLS* regression including transactional covariates as well as property type, metropolitan area, and month fixed effects. ZIP code clustered standard errors reported in parentheses with (*), (**), and (***) indicating p-values less than 0.1, 0.05, and 0.01, respectively.