Amazon HQ2: The Anatomy of Shocks to Housing Price Expectations *

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Abstract

Utilizing the unanticipated selection of Amazon's second headquarters (*HQ2*) as a quasi-natural experiment, this paper causally identifies the impact of an expectations shock on local housing markets. We find that the shock caused a substantial and immediate increase in house prices without affecting new lease rents. Price appreciation was broad across all market segments and accompanied by a significant increase in housing liquidity. We document that mortgage credit facilitated the capitalization of future demand expectations into current prices, with loan values growing in tandem with price appreciation, thus maintaining a constant loan-to-value ratio. To interpret these dynamics, we develop a tractable general equilibrium housing model with collateralized mortgages, limits on unsecured borrowing, and infrequent refinancing. We show analytically how sufficiently large increases in expected future income raise house prices relative to rents on announcement, and how mortgages amplify the house price response.

Keywords: Housing markets, price-rent ratio, expectations shocks, financial frictions, mortgages.

JEL Codes: E23, E32, E44, G21, R31

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1 Introduction

Housing market dynamics are a central driver of economic cycles and policy worldwide, yet the precise causes of short-run price fluctuations remain debated.[1] Despite their importance, understanding the precise drivers of short-run housing price fluctuations remains an active area of research.[2] A leading explanation is the transmission of expectations regarding future income and rents to current house prices. Ideally, testing this channel requires identifying an exogenous shock about a future event that clearly and immediately impacts present prices. However, a primary challenge lies in the difficulty of obtaining the granular, micro-level data and exogenous source of variation necessary to isolate and measure changes in future housing demand expectations.[3] Because prior research has largely relied on surveys or self-assessed valuations, many studies introduce biases that compromise the validity of causal inference.[4]

Our primary contribution is the novel identification of an expectations shock on current house prices, derived from a change in expected future demand for housing consumption. We achieve this by leveraging the unanticipated selection of Amazon's second headquarters (*HQ2*) as a quasi-natural experiment to test how changes in expectations regarding future economic activity, job prospects, and income affect local housing markets. This unique event allows us to directly provide an anatomical account of an expectations shock using granular transaction and listing data. Through a rigorous empirical approach, we deliver robust causal estimates of a significant and substantial increase in transacted closed prices, sellers' listed prices, and housing liquidity in the selected locations. Notably, these changes occurred without affecting the price of new lease rents. We further document that mortgage credit facilitated the capitalization of these future demand expectations into current prices, with loan values growing in tandem with price appreciations. Therefore, we observe increased valuations affected all tiers of the housing market, leading to a broad upward shift in the distribution of house prices.

Our second contribution is a tractable general equilibrium housing model that characterizes how expectations about future income affect current house prices and rents. The model features fixed housing supply, collateralized mortgages, limits on unsecured borrowing, and infrequent refinancing. We derive analytical conditions under which an announcement of higher future income raises the price-rent ratio, and show that mortgages amplify the house price response. The

[1] For example, (Leamer, 2007) famously argues that housing drives the business cycle, while (Garriga and Hedlund, 2019) provide a comprehensive review of the central role of housing markets in macroeconomic dynamics, including their links to macroeconomic policy and financial stability.

[2] For detailed and comprehensive review and of the factors that influence house prices and rents see (Davis and Van Nieuwerburgh, 2015; Piazzesi and Schneider, 2016; Cox and Ludvigson, 2021)

[3] Kuchler, Piazzesi and Stroebel (2023) provide a comprehensive review and synthesis of the literature on housing market expectations.

[4] Surveys and self-assessed valuations are prone to several biases, including projection bias, where individuals extrapolate recent trends; cognitive dissonance, which can inflate homeowners' valuations; and anchoring effects to recent prices. These methods also face issues with strategic reporting and sample selection bias, highlighting their inherent endogeneity and the value of a quasi-natural experiment.

comparison with an unconstrained user-cost benchmark identifies the role of financial frictions in separating the responses of prices and rents. Numerical illustrations examine how these responses depend on refinancing frequency, borrowing limits, and household preferences.

The highly publicized and consequential selection process of Amazon's *HQ2* offers a rare opportunity to isolate and measure the impact of a significant, largely unanticipated shock to expectations about future local economic prospects and housing demand. The campaign for *HQ2* was unique in the sheer scale of the promised benefits: up to 50,000 high-paying jobs and $5 billion in investment. The process began in 2017 with an open call for proposals from cities across North America. After narrowing the field to twenty finalists in January 2018, Amazon made a surprising announcement in November 2018, naming two winning locations: Long Island City, New York, and Crystal City, Virginia.[5] The surprise continued three months later when, facing growing local opposition, Amazon abruptly withdrew from the Long Island City location, creating a subsequent negative expectation shock for that housing market.

Our empirical analysis exploits the carefully timed and confidential nature of the Amazon *HQ2* selection process to establish causality. By using transaction-level data and a difference-in-differences (*DID*) strategy, we directly measure the magnitude of the resulting expectations shock on local housing market conditions in the winning locations. Our strategy uses as control groups either large metropolitan areas that were not *HQ2* finalists, and therefore unaffected by the selection process. We extend our analysis to use the finalist cities *not* selected as winners by Amazon. The results are robust to either implementation.

The main results of the analysis reveal that the Amazon *HQ2* announcement had a significant and substantial impact on housing prices in Crystal City. The estimated effects on transacted prices are that the price per square foot (Close Price/Sq.Ft) increased by $19 within three months, rising to $26 by month six, and reaching approximately $30 within a twelve-month window. This cumulative effect amounts to an average price appreciation of 7.5% of the total home value. Long Island City, NY, experienced two opposing shocks—winning and withdrawal—just three months apart. The winning announcement's impact is challenging to estimate due to the market's low liquidity and limited transaction data in the short period before withdrawal. We do find some evidence that the withdrawal, however, reduced close prices by a significant amount within six months of the withdrawal announcement.

Given the considerable transaction delays in real estate markets, one would ex ante expect closed transaction prices to adjust more slowly to the *HQ2* shock than sellers' initial listing prices. The Amazon announcement had an immediate and significant impact on sellers' valuations, as measured by the listing price per square feet, which serves as a directly observable, unilateral measure of their home valuation and encodes their sentiment. The close price, conversely, measures the bilateral agreement—the equilibrium reached by both seller and buyer—and is therefore a

[5] The area 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.

lagging indicator of the realized expectations shock. The average seller's valuations remained high for an entire year, sustaining an increase of approximately $30 per square foot across all time windows. In contrast, buyers' valuations adjusted more slowly but converged to nearly the same level within a year, with only a small gap of $2 per square foot remaining between listing and closed prices.

The documented convergence of realized prices to seller expectations highlights the critical role of liquidity as a mechanism for housing market shocks. How rapidly listing and close prices converge should be affected by available housing market liquidity, as property transactions occur with a significant lag after listing (Garriga and Hedlund, 2020; Famiglietti, Garriga and Hedlund, 2020). Therefore, we also study the transmission of the *HQ2* shock to local housing market liquidity, measured by the amount of time a property spends on the market after listing before selling. We find that the shock caused the time properties spent on the market to decline by 12 days within six months—a 22% increase in liquidity that persisted for the remainder of the sample.

To assess the house price "pass-through" of the expectations shock across different market segments, we study the distribution of prices. Given that both winning locations are expensive metropolitan areas with relatively low homeownership, one might hypothesize that the shock would be concentrated in the upper tier of the housing market. This dynamic is distinct from a credit loosening shock, which typically benefits lower-price tiers. However, we find that the distribution of house prices shifted upwards considerably after the announcement in Crystal City. We confirm this by testing and finding that the post-announcement house price distribution first-order stochastically dominates the pre-announcement distribution. This effect is not due to nationwide trends, as the price distribution for our control group remained unchanged. This evidence suggests that the expectations shock affected all market segments locally. This feature within the anatomy of a well-identified expectations shock may be used in further research decomposing the relative roles of the effects of credit and expectations in passing-through to housing prices.

Our analysis then explores the market mechanisms that transmitted the *HQ2* shock to the housing market. While standard asset pricing models assume a tight relationship between house prices and the expected future path of rents, a significant body of empirical literature documents a disconnect between these two series (Shiller, 2007; Garriga, Manuelli and Peralta-Alva, 2019). This tension motivates our investigation into the rental market's response to the announcement. We find no evidence of a gradual increase in rents over time. Instead, our analysis shows that new lease rents in Crystal City actually had a slight declined relative to the control group. Using ZIP codes as the unit of analysis, our *DID* estimates find no significant effect on rental prices within nearly 12 months of the winning announcement, a finding that stands in sharp contrast to the substantial effects on house prices. Our analysis focuses on a 12-month window following the announcement because data beyond this period is contaminated by the arrival of the COVID-19 pandemic in early 2020. This constraint, however, does not hinder a key finding: newly leased rents did not react to the announcement within this period.

Another market mechanism we explore is whether the announcement generated a selection shift in the types of buyers, such as the arrival of large capital investors drawn by the centrality and attractiveness of the location. While directly tracking buyer identity is challenging, we use the ZTRAX dataset, which contains housing deeds for every transaction, to document the evolution of mortgage financing as a complementary approach. Our analysis shows that loan growth per square foot moved in line with house price appreciation after the announcement, implying a relatively constant Loan-to-Value (LTV) ratio similar to the pre-announcement trend. This result suggests the shock did not fundamentally alter the composition of buyers.

Building on these empirical findings, we model the HQ2 shock as an unanticipated announcement of a permanent increase in future income. Housing supply is fixed, and households can borrow through both unsecured loans and collateralized mortgages. An unsecured borrowing limit and infrequent mortgage refinancing restrict households' ability to bring future income and housing wealth forward into current consumption. For a sufficiently large income announcement, households cannot smooth their consumption against this future income shock. House prices then rise relative to rents because the value of future housing ownership increases while the contemporaneous response of consumption and rents is constrained by credit frictions for both housing and unsecured borrowing.

We show analytically that mortgages amplify the house price response relative to an economy without mortgage borrowing. We also compare this outcome with an unconstrained user-cost benchmark, commonly used in the literature, in which households can smooth consumption and the price-rent ratio does not change following the announcement. Numerical illustrations show how the response varies with refinancing frequency, unsecured borrowing limits, preferences, and mortgage loan-to-value ratios. These results characterize how financial frictions affect the capitalization of expected future income into current housing prices. This theoretical analysis demonstrates that financial frictions play a crucial role in transmitting an expectations shock to the price-rent ratio.

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 housing expectation shocks, whereas Section 5 explores different estimators and market mechanisms that influence rents and credit. Section 6 develops the general equilibrium housing model and characterizes the effects of the HQ2 announcement. Section 7 concludes.

2 Literature Review

The macro-housing literature has long sought to isolate the key drivers of housing valuations, focusing on factors such as rent growth, interest rates, credit conditions, and, crucially, expectations about the future. While prominent work, such as Shiller (2007), conjectures that expectations were a fundamental driver of the 2000s housing bubble, the existing empirical evidence is often not conclusive in isolating the causal impact of expectations from other contemporaneous factors. This tension highlights a central challenge in the literature: the difficulty of identifying a clean, exogenous shock to expectations to study its effect on house prices and rents.

As Kuchler et al. (2023) highlight in their comprehensive review of the literature on housing market expectations, a key challenge for studies that use survey data is the measurement of expectations themselves. Although several complementary approaches exist, each has distinct limitations that our work seeks to overcome. One approach is to use *self-assessed valuations* in low-frequency data 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 on self-assessed valuations, Davis and Quintin (2017) find that expectations exhibit a sluggish response, failing to fully capture the rapid swings of housing booms and busts. This evidence challenges the notion that expectations can account for rapid price variation, given that these valuations often include individuals not actively trading. Consequently, using listing and transaction information provides a more accurate measure of the faster-adjusting expectations of current and future buyers and sellers. Another approach for identifying the role of expectations uses *high frequency survey data*. Kindermann, Le Blanc, Piazzesi and Schneider (2021) highlight that expectations can differ significantly across agents, for example, between those transitioning from renting to owning versus existing homeowners ((Armantier, Topa, Van der Klaauw and Zafar, 2017)). A complementary approach uses *randomized control techniques* in survey data where, for instance, respondents are presented with factual information to re-elicit expectations (Armona, Fuster and Zafar (2019)). While these methods are useful, it remains challenging to extrapolate measured beliefs to actual market behavior. Our findings, using a quasi-experimental approach together with listing and transaction data, indicate that even among active participants, expectations for buyers and sellers form at different speeds.

A traditional alternative to the survey data has been to measure house price expectations using asset pricing relationships, such as the user cost model, with local, state, or national data 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. While influential, this methodology struggles to rationalize a large role for expectations in driving prices in the absence of corresponding rent growth—a disconnect that our empirical findings and structural model directly address.

To overcome these challenges, the structural equilibrium 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 et al. (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.

3 Amazon HQ2: The Selection Process and Data Evidence

This section reviews the timeline of the process Amazon followed to select its second headquarters (*HQ2*). By examining whether the *HQ2* announcement was anticipated by the market, our analysis motivates the use of the selection process as a **quasi-natural experiment** that can be employed for causal estimation of the impact of expectation shocks about future housing demand on prices.

3.1 Amazon HQ2: Selection Process Timeline

On September 7, 2017, Amazon announced its search for the *HQ2*. The reward for the chosen city was substantial: Amazon initially promised up to $5 billion in investment and up to 50,000 high-quality jobs, with an average salary of over $100,000. In its 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 and October 19, 2017, cities and metropolitan areas were invited to submit proposals to Amazon. Amazon requested information from respondents on potential incentives, building sites, community culture, and infrastructure accessibility. During this period, Amazon received 238 proposals. On January 18, 2018, Amazon announced that 20 finalists had been selected.

Figure 1: Amazon Finalist Cities



Notes: A map of the 20 finalist metropolitan areas considered by Amazon for its second North America headquarters. The list was announced in January 2018 and includes locations across the United States and Canada.

Following this announcement, Amazon began a closer collaboration with the finalists by visiting each city and meeting with local officials to discuss more specific details. On November

13, 2018, Amazon unexpectedly announced it would split its headquarters between two locations: Crystal City in Arlington, VA, and Long Island City in New York City. By splitting the headquarters, Amazon also halved the promised jobs and investment (25,000 jobs and $2.5 billion in investment) for each location.

Right after the announcement, Amazon's choice of New York for a headquarters was met with significant political opposition. Local officials and community activists denounced the offered incentives as corporate welfare, raised concerns about gentrification, and argued the move would worsen the city's affordability crisis. This backlash ultimately led to Amazon's abrupt withdrawal from the planned Long Island City location on February 14, 2019.

3.2 Data Description

To estimate the effect of the Amazon *HQ2* decision on housing prices, we use several proprietary and public micro data sources, which we now describe. To account for residence and neighborhood heterogeneity, our empirical analyses include covariates that capture factors affecting house prices. These include the type of residence, square footage, and other house attributes, as well as the socioeconomic characteristics of the neighborhood where the property is located (e.g., ZIP code).

To measure house prices, we use confidential data from CoreLogic's *Multiple Listing Services* (*MLS*). The data consists of real estate agent listings and transactions at the unit level, including timing and pricing at listing and closing. These prices are deflated using a personal consumption expenditure index. Our key variables are Close Price and List Price both measured in dollars per square foot, as well as Time on Market (days), which measures market liquidity. The *MLS* data also provides detailed property characteristics, such as Square Footage number of Bedrooms and Bathrooms and the Age of the structure. While most properties in the *MLS* are single-family homes or condominiums, we control for property types to account for apartments, townhouses, and residential income properties (i.e., properties purchased to be rented out) in our empirical analysis.[6]

For the socioeconomic and physical variables, we use a variety of data sources. We use five-year estimates from the *American Community Survey* (ACS) to collect data on ZIP code-level population, homeownership rates, and the percentage of adults with a college degree. Population density (people per square mile) is constructed by dividing the population by land area, which is provided by the *U.S. Census Gazetteer* files. The *Internal Revenue Service's Statistics of Income* (IRS SOI) database is used 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 the closing price-to-earnings

[6] Our sample from the *MLS* was collected in January 2020 and includes data through October 2019, which provides roughly eleven months of transaction data after the first Amazon *HQ2* announcement.

Table 1: Summary Statistics

	Mean	Std. Dev.	Percentile 10%	50%	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
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
Finalist Cities					
Close Price/Sq.Ft	199.84	139.40	81.69	151.38	386.68
List Price/Sq.Ft	224.11	160.84	91.48	169.75	428.00
Time on Market	82.28	63.57	32.00	62.00	161.00
Square Footage	2,068.88	1,071.55	1,034.00	1,800.00	3,437.00
Age	40.85	29.94	5.00	35.00	83.00
Bathrooms	2.44	1.03	1.00	2.00	4.00
Bedrooms	3.29	1.07	2.00	3.00	5.00
Population Density	4,566.37	5,223.06	491.83	2,821.92	10,952.04
College	37.70	17.75	15.82	35.49	63.45
Price/Earnings	1.71	1.81	0.49	1.11	3.39
Ownership Rate	64.53	17.29	39.07	67.43	84.95
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 560 in NY, 2,734 in VA, 1,255,374 in Finalist cities and 2,447,887 in a set of other cities that were not selected by Amazon. We also conduct the analysis with larger samples not including some or all residence attributes. The "Finalist Cities" exclude Crystal City and Long Island City in their statistics.

ratio, or Price/Earnings, for each ZIP code by dividing the average annual transacted house price (as calculated from the *MLS*) by the average income of these top three income classes.

To control for financing costs and macroeconomic conditions in our empirical models, we include the weekly 30-year fixed-rate mortgage rate aggregated by Freddie Mac. This national rate is matched to every transaction that occurred in the same week. During the period of interest, this mortgage rate ranged from 3.65% to 4.81% (bottom and top deciles, respectively) with an average of rate of 4.29%.

In Table 1, we report summary statistics of our sample in Crystal City, Long Island City, the non-winning finalists, and a set of large control metropolitan areas. The selection and characteristics of these control areas are further documented in Section 4.1. Crystal City, VA is a relatively expensive and densely populated market with an average house size of 1,500–1,600 square feet. We find that closed prices average over $348 per square foot. Sellers in both Crystal City and the control cities tend to be over-optimistic throughout the sample period, as average closed prices are consistently lower than list prices. Specifically, closed prices are about 10% lower than list prices in the control cities, compared to 8.8% in our VA sample. Notably, Crystal City's housing market is more liquid than the control cities, with sales taking an average of 53 days on the market rather than 87 days.

We note that New York sample is unique in several dimensions. First, the data are considerably sparser in the New York market, with only approximately 560 observed residential real estate transactions over a two-year period compared to over $3,000$ in Virginia. Second, the New York market is markedly different from our control housing markets, which raises statistical concerns when balancing covariates. For instance, New York's population density is several orders of magnitude higher (33,099 people per square mile) than our non-finalist cities (2,544). The market is also more illiquid, with an average time on the market of 172 days versus 87 days elsewhere, and closing prices amount to three times the average income versus only 1.5 times in our control group.

Comparing the finalist and non-finalist cities as control groups, the mean closing price per square foot is still within a similar order of magnitude, suggesting comparable market conditions ($199.84 versus $151.65). Demographically, the two groups are also broadly alike. The average college attainment rate for finalists is 37.70%, which is only slightly higher than the 32.92% observed in non-finalist cities. The average housing market liquidity is comparable, with finalist cities having a mean of 82 days on the market and non-finalist cities having a similar average of 87 days.

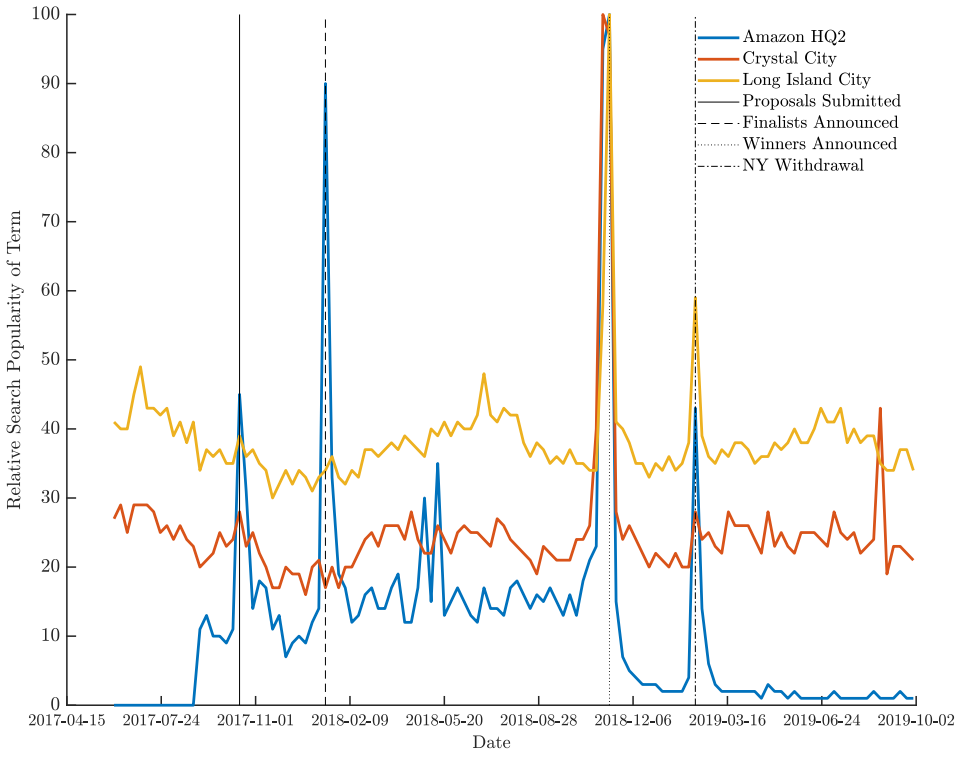
3.3 Amazon Selection Process as a Quasi-Natural Experiment

Amazon successfully kept the identities of the winning cities and its decision to split the new headquarters between two locations a secret from the public until the official announcement. Amazon valued discretion throughout the selection process and signed non-disclosure agreements with local officials to prevent leaks. If this secrecy had been compromised, the public could have acted on the leaked information. Such widespread anticipation would undermine our empirical strategy, which relies on the announcement being exogenous to market participants. We argue that Amazon's *HQ2* announcement amounts to an unanticipated shock, which motivates our use of the

selection process as a quasi-natural experiment. This allows for the causal estimation of the impact of expectation shocks about future housing demand on prices.

To assess whether the public anticipated the decision, we first analyze Google Trends data showing the relative search popularity of the winning locations and the term "Amazon *HQ2*." We plot the relative search popularity time series in Figure 2 and find no preexisting trends that would suggest public anticipation for the winning locations. The search popularity peaked four times, corresponding to the initial search announcement, the unveiling of the 20 finalists, the selection of the winning locations, and the withdrawal from New York. In contrast, the search term "Crystal City" peaked only once, when it was selected as a winning location. "Long Island City" surged in popularity for both the winning and withdrawal announcements.

Figure 2: Google Trends for Amazon HQ2



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

We next analyze the listing behavior of residential properties before and after the announcement. This micro-evidence serves as a complementary way to assess public anticipation. We hypothesize that sellers, anticipating an increase in their homes' value, might have raised the number of listings in Crystal City to capitalize on their gains. However, using MLS data, we find that the number of listings (expressed as a percent change from the previous year) doesn't change

significantly around the *HQ2* announcement date. The time series of listings for the 20 finalists and nearby metropolitan areas are plotted in Figure D.1 in Online Appendix D.

We further qualitatively assess location odds from media outlets and online betting sources. For example, Moody's Analytics placed strong odds on Austin, Atlanta, Philadelphia, Rochester, and New York City. Reviewing other online betting odds and media predictions for the 20 finalists to win the *HQ2* competition corroborates a lack of anticipation. The Irish gambling site Paddy Power identified Atlanta and Austin as the most likely winners, while The New York Times identified Denver, Boston, and D.C. as the strongest contenders.

The lack of public search activity, changes in listing patterns, and a widespread divergence in expert opinion—as seen in betting odds and media speculation—strongly suggests the announcements were unanticipated by markets. In the next Section 4, we formally test whether there were statistically significant price appreciations prior to the winning announcement.

4 Housing Price Impacts of the Amazon HQ2 Announcement

This section analyzes the impact of the Amazon *HQ2* announcement on housing prices. We estimate the causal effects of this shock by comparing the real estate market performance of Crystal City and Long Island City against various control groups, including non-finalist and other finalist cities. Real estate markets are local and relatively illiquid. Consequently, the effects of a market shock like the *HQ2* announcement are not as immediate as they are for financial assets like stocks or bonds; instead, they are delayed and persistent. To account for this, we estimate effects for alternative post-announcement time windows, accounting for the average time it takes to sell a property. We examine transacted (closed) prices, seller expectations (listing prices), and housing liquidity (time on the market) to accurately measure the dynamic reaction of local housing markets to the shock.

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 has 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 dollars per square foot more than prior to the announcement. These increases are economically 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 a 7.5% property price appreciation.

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.[7] In our baseline, this means the treated area is Crystal City and any ZIP codes geographically adjacent to Crystal City.[8] This treated area is 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 sur-

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

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

rounding 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.[9] Despite not being considered by Amazon in the preliminary selection round, our 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*. 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).[10] For robustness, we further estimate *HQ2* effects when using non-winning finalist cities as the control group and find qualitatively and quantitatively similar results.

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 how 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, and Time 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:

$$\begin{aligned} Y_{it} = \alpha \; & + \beta_{\text{Announcement}} \mathbb{1}(t > T) + \beta_{\text{Winner}} \mathbb{1}(m = \text{Winner}) \\ & + \beta_{HQ2} \mathbb{1}(t > T) * \mathbb{1}(m = \text{Winner}) + \lambda_{j(i)} + \mu_{m(i)} + \tau_t \\ & + \gamma X_{it} + \theta W_{z(i)} + \psi Z_t + \varepsilon_{it} \,, \end{aligned} \tag{1}$$

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

[10] *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.

where the outcome variable Y_{it} refers to property i of type $j(i)$ in ZIP code $z(i)$ of metropolitan area $m(i)$ 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. An indicator 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 indicator 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 interactions 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 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 further extend (1) to generate the causal effect of a property being located in a winning location for a given month in our sample. We replace an indicator for the post-announcement period with a monthly indicator and interact this indicator with the winning location indicator, $\mathbb{1}(m = \text{Winner})$. The resulting specification is:

$$\begin{aligned} Y_{it} = \alpha \; &+ \beta_{\text{month}} \mathbb{1}_t + \beta_{\text{Winner}} \mathbb{1}(m = \text{Winner}) \\ &+ \beta_{HQ2,\text{t}} \mathbb{1}_t * \mathbb{1}(m = \text{Winner}) + \lambda_{j(i)} + \mu_{m(i)} + \tau_t \\ &+ \gamma X_{it} + \theta W_{z(i)} + \psi Z_t + \varepsilon_{it} \,, \end{aligned} \tag{2}$$

In our baseline specification, we will interpret β_{HQ2} as the average treatment effect on the treated (ATT) 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 (Abadie, 2005; Angrist and Pischke, 2009,§5.2). We will use (2) to test for significant price pre-trends to diagnose the plausibility of this assumption. The subsequent section implements this estimator and plots the results in Figure 3. We find no evidence of significant price appreciations prior to announcement. Furthermore, Section 5 implements an additional synthetic controls estimator matching prices during the pre-announcement period between the treated and control local housing markets. In that exercise, we find no evidence 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 in Crystal City.

4.3 Crystal City: A Future Full of Opportunity Awaited Us

Using (1) we estimate average treatment effects (ATT) of Amazon's *HQ2* announcement on the closing price of residential housing in Virginia, evaluating the impact 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 results are presented in Table 2.

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

	I	II	III	IV	V	VI
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)
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)
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 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), but the results are robust across various specifications. The findings indicate that houses are more expensive in highly dense areas where college graduates tend to live.[11] These educated residents, likely with higher income, favor more expensive neighborhoods, perhaps because of their associated (non-observable) amenities.[12] Indeed, the price effect of the price-to-earnings nearly doubles 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.

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 amounts to a 7.5% appreciation for the average sized property, and 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 and control groups. 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

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

[12] 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 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, VA: Amazon's HQ2 Event Study

	Non-Finalists				Finalists			
	1 Month	**3 Months**	**6 Months**	**12 Months**	**1 Month**	**3 Months**	**6 Months**	**12 Months**
Y = Close Price/Sq.Ft								
HQ2	18.021***	18.627***	26.286***	30.400***	14.547***	19.109***	27.048***	31.560***
	(3.600)	(3.897)	(2.371)	(2.690)	(3.835)	(4.352)	(1.871)	(2.266)
Announcement	-0.200	1.471***	4.291***	5.211***	-1.789***	-0.351	3.384***	4.395***
	(0.307)	(0.365)	(0.285)	(0.278)	(0.504)	(0.384)	(0.304)	(0.295)
Winner	88.190***	88.510***	88.467***	90.741***	50.528***	51.026***	50.866***	51.268***
	(13.257)	(13.220)	(13.259)	(13.278)	(14.211)	(14.194)	(14.186)	(14.188)
R^2	0.615	0.612	0.609	0.607	0.719	0.717	0.715	0.715
Observations	1,345,775	1,498,820	1,828,120	2,533,959	669,243	743,452	904,565	1,258,259
Treated Obs.	1,475	1,687	2,049	2,734	1565	1785	2172	2885
Y = List Price/Sq.Ft								
HQ2	31.102**	11.645**	29.796***	32.589***	27.476*	12.739*	29.857***	32.566***
	(14.586)	(5.153)	(1.633)	(3.303)	(15.838)	(6.784)	(2.652)	(3.891)
Announcement	7.010***	3.388***	-1.024*	-2.369***	7.708***	2.698**	-3.630***	-5.769***
	(0.442)	(0.490)	(0.607)	(0.625)	(1.191)	(1.154)	(1.279)	(1.404)
Winner	89.505***	88.708***	88.937***	90.807***	52.131***	51.372***	51.211***	51.563***
	(14.257)	(14.293)	(14.356)	(14.382)	(14.706)	(14.752)	(14.785)	(14.763)
R^2	0.544	0.541	0.543	0.548	0.536	0.534	0.539	0.540
Observations	1,722,820	1,933,490	2,384,564	2,898,327	971,815	1,085,000	1,350,127	1,648,292
Treated Obs.	1,679	1,834	2,321	2,878	1816	1981	2514	3105
Y = Time on Market								
HQ2	-1.416	2.549	-11.941***	-11.976***	2.716	2.380*	-11.944***	-11.927***
	(2.825)	(3.142)	(2.427)	(2.135)	(1.792)	(1.310)	(1.706)	(1.386)
Announcement	13.675***	22.115***	12.052***	4.117***	16.049***	24.916***	12.323***	4.278***
	(0.324)	(0.283)	(0.207)	(0.206)	(0.421)	(0.354)	(0.296)	(0.313)
Winner	-42.513***	-42.669***	-43.214***	-43.566***	-13.548***	-14.084***	-12.948***	-12.144***
	(4.691)	(4.549)	(4.497)	(4.454)	(3.280)	(3.208)	(3.278)	(3.498)
R^2	0.100	0.113	0.115	0.094	0.094	0.111	0.111	0.089
Observations	1,767,417	1,991,587	2,414,427	3,337,129	961,425	1,085,558	1,315,583	1,832,393
Treated Obs.	1,747	1,979	2,364	3,143	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.

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 immediately reported higher valuations 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, Time on Market drops by nearly 12 days six months after the announcement. This reduction corresponds to a 22% increase in housing liquidity in Crystal City during this period, highlighting how liquidity can adjust even more rapidly than prices in areas with low housing supply elasticity as documented in Famiglietti et al. (2020). Liquidity effects were persistent after six months over our sample when the existing inventory cleared and remained largely unchanged nearly 12 months after the *HQ2* announcement.

Metropolitan areas not selected as finalists for *HQ2* by Amazon is our preferred control group, although it may suffer from sample selection: excluding them from the finalists might be revealing of some fundamental differences across local real estate markets that might be relevant for Amazon's *HQ2* selection. The key econometric issue is whether these differences could bias our estimates of the effect of the *HQ2* announcement on price expectations.

To evaluate this potential shortcoming, the right panel of Table 3 repeats the analysis using the metropolitan areas of 14 finalists cities. The advantage of using the finalist control group, depicted in Figure 1, 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. The disadvantage, however, is that finalist cities are more likely to have experienced anticipatory real estate market effects due to the *HQ2* selection process.

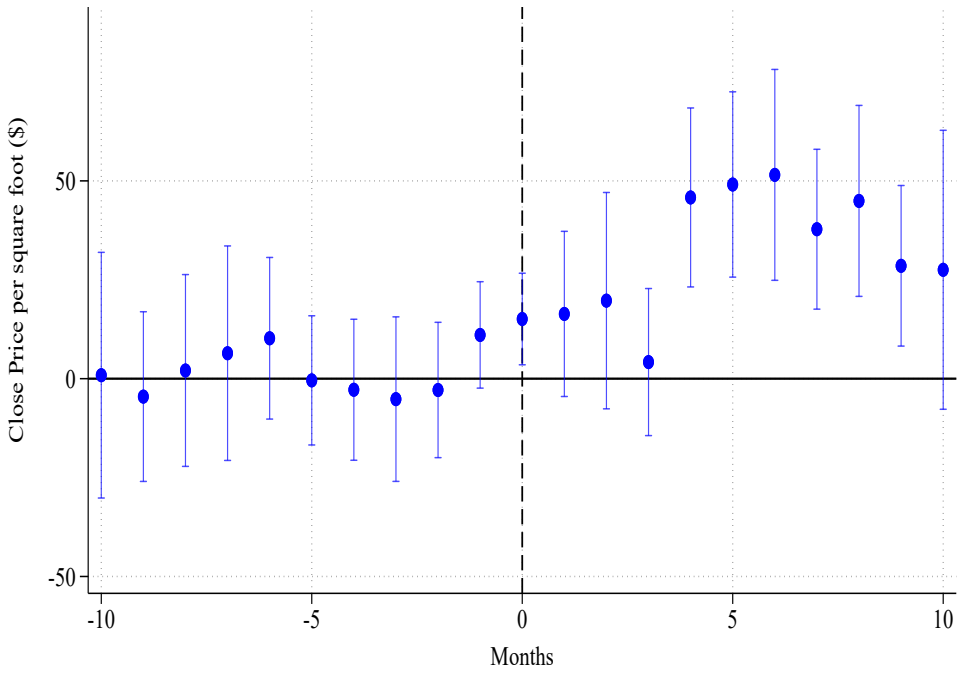
Results indicate that over a window of six months, the *HQ2* announcement increased Close Price/Sq.Ft by $27.05 using finalists vs. $26.29 using non-finalists. The results for other windows are also very similar. When examining List Price/Sq.Ft results, the similarities hold and the point estimate of the ATT with finalists is $29.80 compared to $29.86 with non-finalists. The results for market liquidity, Time on Market, are remarkably similar as well at the six month window with the point estimate using the finalists or non-finalists both being -11.9 days. We therefore

conclude that results are similar across both finalist groups and time windows, indicating that our point estimates are consistent and robust.

Dynamic Treatment of winning announcement: To complement our qualitative analysis of anticipation in Section 3, we formally test whether there were statistically significant price appreciations prior to the HQ2 announcement. We do this by estimating (2), to dynamically report the transacted price changes of properties in Crystal City. This specifications tests if there were any significant price changes before the *HQ2* announcement. Figure 3 presents the estimated coefficients from the Close Price/Sq.Ftspecification, using the non-finalist control group.

Figure 3: Monthly Price Changes, Crystal City, VA

(a) Dynamic Treatment Estimates Close Price/Sq.Ft (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 formally tests the significance of price trends around the announcement. We estimate (1), replacing the post-announcement indicator with a month indicator. The control group used in estimation the non-finalist control group. Details on specification and estimation are found in Section 4. The right panel plots coefficients from $\beta_{\text{month}} \mathbb{1}(t) * \mathbb{1}(m = \text{Winner})$ along with a 95% confidence interval.

The point estimates and their 95% confidence intervals show no statistically significant pre-trends in Close Price/Sq.Ft for Crystal City. This finding is crucial because it supports the assumption that the HQ2 announcement was an unanticipated shock, a key requirement for our empirical strategy. The price difference is significant on impact and at 4 months after the announcement, and remains so for an additional 6 months. This estimator measures the month-to-month impact, which provides a different perspective from an alternative one that measures the cumulative effect at months 1, 3, 6, and 12. Qualitatively, the point estimates are similar to those of Table 3, but show that the per-month impact later in the sample is even higher than $30.

Therefore, the results of Table 3 can be considered as a conservative estimate of the real estate impacts of the *HQ2* announcement in Crystal City.

4.4 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 for causal analysis. 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 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 in NY declined by $37 Close Price/Sq.Ft at a marginally significant 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 Amazon's withdrawal decision inflicted on the housing market, as alternative estimates of *HQ2* on Close Price/Sq.Ft suggests prices dropped by $64 within six months.[13]

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 imply a hypothetical Close Price/Sq.Ft of $613 in NY by mid-May 2019, $42

[13] In the subsequent section we doucment an alternative estimator, *CEM* which more closely balances treatment and control groups. Table D.10 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 implying these estimates may be closer to the true impact.

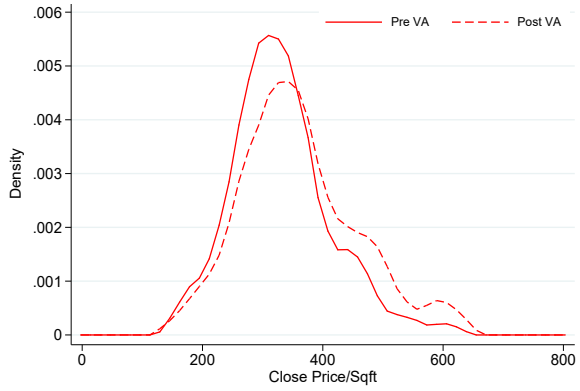
more than by mid-November 2018. The price gap between the actual Close Price/Sq.Ft in NY six months after Amazon's withdrawal amounted to $80 per square feet, i.e., an average effect of $127,000 of the average house property value lost due to the withdrawal using simple back of the envelope calculations when one extrapolates from the price appreciation observed in Crystal City.

4.5 Heterogeneity in Price Distribution Response to an Expectations Shock

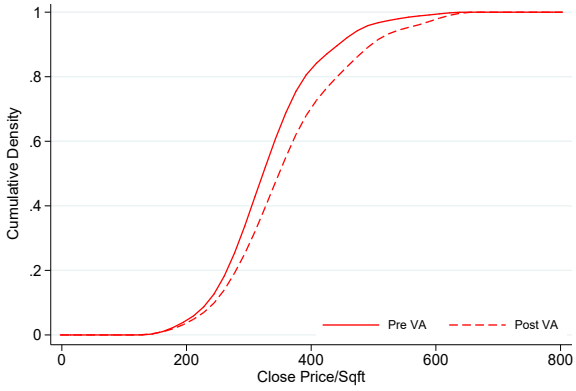
Our empirical analysis thus far has obtained robust *average* effects of the impact of Amazon's decision to locate in Crystal City, VA. By construction, these estimates are averages across all residential homes and ignore that causal effects may be heterogeneous across different real estate market segments. 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.[14]

Figure 4: Pre/Post Close Price Distribution in Crystal City, VA

(a) VA Close Price Density



(b) VA Close Price Distribution



Transaction data allow us to document the shifts in the distribution of prices. Figure 4 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 to the right for Crystal City, and we note that these distributions remain virtually unchanged for the set of other finalist and non-finalist cities.[15]

How significant are these price distribution shifts? Table 4 reports simulation-based, first and second order, Kolmogorov-Smirnov tests of stochastic dominance (Barrett and Donald, 2003). 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.

[14] 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.

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

Table 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**
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).

We reject the null whenever the reported p-values in Table 4 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. 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 as evidenced in Figure 4.

The visually larger shift of the right tails of Figure 4 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.

5 Additional Outcomes: Robustness, Rental Markets, and Mortgage Growth

This section builds on the prior causal analysis by presenting robustness checks for the key finding using different matching techniques for the control groups. It also examines the dynamics of rental yields relative to the treatment groups, whether there were broader regional spillovers from the *HQ2* announcement, and the market drivers of housing appreciation—mortgage-financed versus cash purchases in the selected locations.

5.1 Estimator Robustness: Coarsened Exact Matching and Synthetic Controls

To evaluate the stability of our primary findings, we employ a range of parametric and non-parametric methods to estimate the causal effects of the *HQ2* shock. Specifically, we assess whether these effects on house prices persist when using refined control groups constructed through advanced matching techniques. This analysis ensures the reliability of the ATT by addressing potential selection biases and confirming that treated and control groups are comparable in terms of observable characteristics.

As a first robustness check, we apply *Coarsened Exact Matching* (CEM) to estimate the causal effects on closing prices, listing prices, and liquidity in Crystal City. This non-parametric matching algorithm coarsens continuous variables (e.g., house size and age) into meaningful bins and then performs exact matching on these bins to create a weighted counterfactual control group. The key advantage of this approach is its ability to ensure a high degree of balance in observed characteristics between the treated and control groups, making them nearly identical in key pre-treatment attributes. This balance strengthens our causal identification by minimizing the influence of pre-existing differences. We report the balancing statistics in Table A.1. The CEM results closely align with our main estimates, reinforcing the robustness of our findings. Specifically, it increased closing prices per square foot by \$28.4, listing prices by \$31.1, and reduced time on market (Time on Market) by 10.9 days as reported in Table A.2.

As a second robustness check, we employ a *Synthetic Control* estimator, a non-parametric method well-suited for cases with a single treated unit. This approach constructs a "synthetic" Crystal City as a weighted average of non-finalist and finalist neighborhoods, with weights chosen to closely match Crystal City's pre-announcement characteristics, such as housing price levels and trends. This method's strength lies in its ability to visually and quantitatively validate the parallel trends assumption, a critical component of our identification strategy. The synthetic control results not only corroborate our main findings but suggest that our difference-in-differences (DID) estimates may have been conservative. We find that the average post-HQ effect on closing prices is a substantial \$41.3 per square foot, with the maximum effect observed approximately five months after the shock[16]. This finding, derived from a method designed to account for pre-treatment trends, further supports the validity of our identification strategy.

[16] These results are reported in Figure A.1.

The consistency of our results across diverse estimation methods and alternative control groups, using counterfactuals based on ex ante or ex post housing market characteristics, provides strong evidence for the robustness of our primary findings.[17]

5.2 Market Mechanisms: Rental Price Responses to Local Economic Shocks

Standard asset pricing models posit that house prices reflect the discounted present value of future rental income, implying a tight, simultaneous link between current prices and expected rents (Poterba, 1984). Under this framework, local labor market shocks or amenity shifts—such as those anticipated from the *HQ2* announcement—should be capitalized into higher rents and house prices alike (Rosen, 1979). However, a well-documented empirical disconnect suggests these markets often respond asynchronously to economic shocks (Shiller, 2007; Garriga et al., 2019). This potential decoupling motivates our analysis: we examine whether the *HQ2* announcement, despite its significant impact on price expectations, generated a corresponding shift in the rental market.

To analyze rental market dynamics, as our primary MLS dataset covers only for-sale transactions, we use monthly, disaggregated data from the Zillow Observed Rent Index (ZORI). ZORI employs a repeat-rent methodology, tracking the same rental units over time to control for changes in rental stock quality, thus providing a more accurate measure of rent inflation than broader government indices, such as the Bureau of Labor Statistics' rent index. As a leading indicator, ZORI captures market dynamics from new listings and recently signed leases. To assess the impact of the *HQ2* announcement on rents, Figure 5 plots the ratio of Crystal City's monthly rental prices to those of the non-finalist control group, showing no noticeable discontinuity or change around the key announcement dates or in leases signed over the subsequent 10 months.

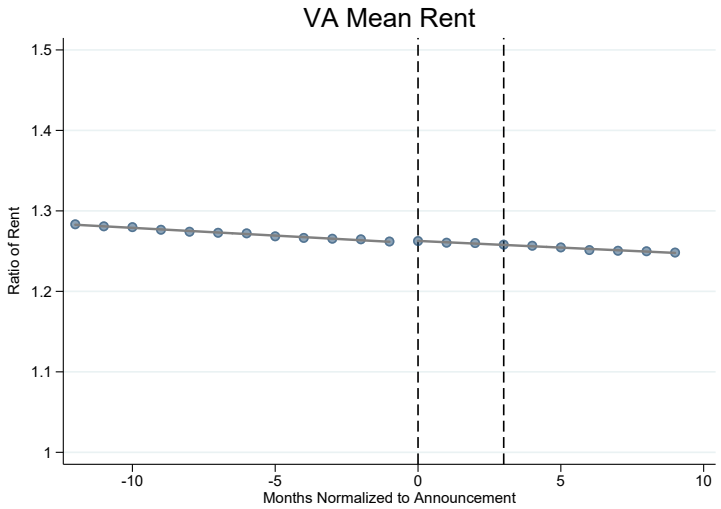
To formally test this observation, we employ the ZORI data in a monthly, ZIP-aggregated *DID* estimator:

$$\begin{aligned} R_{zt} = \alpha \; &+ \beta_{\text{Announcement}} \mathbb{1}(t > T) + \beta_{\text{Winner}} \mathbb{1}(m = \text{Winner}) \\ &+ \beta_{HQ2} \mathbb{1}(t > T) * \mathbb{1}(m = \text{Winner}) + \omega_z + \mu_{m(z)} + \tau_t \\ &+ \gamma X_{j(z)t} + \theta W_z + \psi Z_t + \varepsilon_{jzmt}\,, \end{aligned} \tag{3}$$

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. We aggregate property-level characteristics from MLS micro data to construct the zip-level vector $X_{j(z)t}$ of property covariates. 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 announcement date and estimate the equation using 12 months of data prior to the announcement and six months of post-announcement data. We run two specifications of (3): one where each ZIP code is given an

[17] Detailed results and further discussion are provided in Appendix A.

Figure 5: Rents Index for New Leases Relative to the Control, Crystal City, VA



Notes: The figure shows the monthly rent price ratios relative to the control group around Amazon's *HQ2* announcement dates.

equal weight, and an 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 5.

Table 5: Event Study: Rents in Crystal City, VA

	Six Months after *HQ2* Announcement	
	Unweighted	**Weighted**
Y = Rents		
HQ2	0.598	2.518
	(3.207)	(2.587)
Winner	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.

The results from both specifications consistently show that there was no significant effect on rent appreciation in the treated neighborhoods around the announcement. This finding contrasts sharply with the for-sale housing market, where *listed house prices* increase immediately after the

HQ2 announcement, while *transacted prices* rise between 3 and 12 months post-announcement (Figure 3). Since rents are fixed in nominal contracts, one might expect a discrete jump similar to the response of listed prices, as lease renewals or new leases could reflect increased average earnings in the area following the *HQ2* shock. However, the absence of a rental response suggests a short-term disconnect between the rental and for-sale housing markets, consistent with empirical evidence documenting divergences between prices and rents. Extending the analysis of rent dynamics beyond this 10-month window is not feasible, as it would overlap with the onset of the COVID-19 pandemic in early 2020, introducing confounding effects. Our analysis is conservative, covering data through the end of 2019 to avoid such distortions. This evidence suggests a disconnect between for-sale and rental market that we explore in more detail in Section 6.

5.3 Local Spillovers to the Broader Metro Area

The baseline analysis in Section 4 focuses on how the housing market local to *HQ2* responded to the winning announcement. Crystal City, however, is part of the broader Washington, D.C. metropolitan area. We therefore ask whether the announcement also generated spillovers across the wider region. This distinction is critical for understanding broader housing dynamics: it allows us to determine whether the *HQ2* expectations shock generated a net aggregate increase in regional housing wealth or merely triggered a zero-sum spatial reshuffling of demand within the metro area. Identifying these spillover effects connects our localized findings to the broader literature on spatial equilibrium, agglomeration externalities, and local economic multipliers (Greenstone, Hornbeck and Moretti, 2010; Kline and Moretti, 2014; Ahlfeldt, Redding, Sturm and Wolf, 2015).

To examine this question, we extend the approach from Section 4.2 by defining the treated group as the full Washington, D.C. metropolitan area. Table 6 reports the difference-in-differences estimates from equation (1), providing a test of whether the expectations shock altered housing market conditions systematically across the broader region. The findings indicate that the broader metropolitan area experienced significant price appreciation within six months of the *HQ2* shock. Treating the entire Washington, D.C. MSA yields a highly significant increase in close prices of \$7.45 per square foot, corresponding to a 2.1% rise in realized house prices region-wide. While this average effect is attenuated relative to the 7.5% surge in the immediate vicinity of *HQ2*, its statistical and economic magnitude implies substantial geographic spillovers. This conclusion is reinforced by a straightforward mechanical calculus: we observe only 2,734 housing transactions within Crystal City during this period, compared to 72,543 across the full metro area. Because Crystal City accounts for less than 4% of the MSA transaction volume, an average region-wide appreciation of \$7.45 mechanically necessitates significant price increases in neighborhoods well beyond the immediate *HQ2* footprint.[18]

[18] It is important to note that the large and significant coefficient for the *Winner* indicator in Table 6 reflects the structural baseline price-level differences between the Washington, D.C. metropolitan area and the group of finalist cities (the control group) throughout the sample period. This level shift is orthogonal to the *HQ2* announcement itself, which is captured by the interaction terms.

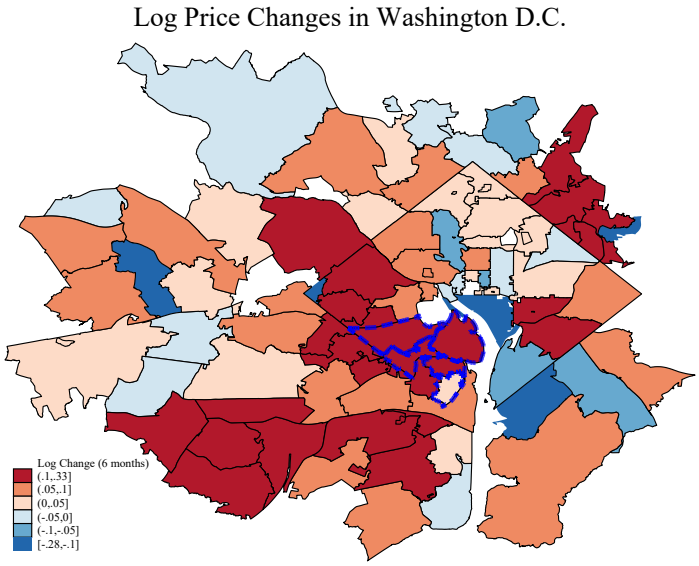
Table 6: Spillovers across Washington D.C.

	Six Months after *HQ2* Announcement	
	List Prices	**Close Prices**
HQ2	2.735*	7.451***
	(1.365)	(2.587)
Winner	44.912***	41.833***
	(7.986)	(6.952)
Announcement	-1.377**	4.324***
	(0.612)	(1.242)
Metro FE	Yes	Yes
Month FE	Yes	Yes
R-Squared	0.754	0.617
Observations	24,462	1,898,614
Treated Observations	98,988	72,543

Notes: Sample includes one year of monthly housing transaction observations prior to Amazon's *HQ2* 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. Appendix Table D.11 reports estimates for all time horizons and outcome variables.

To examine the spatial distribution of these spillovers, Figure 6 maps the raw price changes across the region. The spatial response is pronounced but heterogeneous. Crystal City (outlined in blue) and its adjacent southwestern and central corridors exhibit the sharpest appreciation.

Figure 6: Close Price Changes in Washington D.C.



Notes: The figure shows the log close price per square foot changes in the Washington D.C. metropolitan in the six months after the initial *HQ2* announcement. The area of Crystal City is outlined in blue.

This pattern suggests that the effects of the *HQ2* announcement extended beyond Crystal City, although geography appears to matter. The marked attenuation of the price response across the Potomac River suggests that the capitalization of the expectations shock is constrained by the

perceived geographic boundaries of local labor market linkages; this indicates that agents treat the river as a primary friction to the *HQ2* amenity. Taken together, Table 6 and Figure 6 confirm that while the *HQ2* expectations shock was intensely localized at its epicenter, its propagation served as a net positive wealth shock for the broader Washington, D.C. housing market.

Crucially, this regional appreciation occurred in the absence of a contemporaneous shift in market rents. This decoupling suggests that the *HQ2* announcement functioned as a pure expectations-driven wealth shock, rather than a traditional demand shock for housing services. Under standard frictionless frameworks, an increase in future productivity typically pulls current prices and rents upward in tandem. However, our evidence indicates that while house prices—which are forward-looking assets—jumped to capitalize future news, rents remained anchored to current local economic conditions.

This observation raises a fundamental question: if current income and rents were stagnant, how was the immediate price jump financed? For an expectations shock to be capitalized into transaction prices, the marginal buyer must have access to the liquidity required to bid up the asset today. This suggests a mechanism where the availability of credit allows future news to be 'monetized' into current prices. To explore this, the next section moves beyond price levels to examine the micro-data on mortgage financing and buyer composition.

5.4 Market Mechanisms: Mortgage Financing and Buyer Selection

The short-term disconnect between prices and rents suggests that the market was pricing in a future regime shift. We now investigate the financial architecture that enabled this capitalization. One possibility is a shift in buyer composition: perhaps "deep-pocketed" institutional investors, anticipating long-term appreciation, entered the market with cash, decoupling prices from the credit constraints of typical residents. Alternatively, the price surge may have been sustained by traditional home buyers using mortgage financing to bid against one another based on revised expectations. Distinguishing between these two paths is critical. As Garriga, Gete and Tsouderou (2023) document, while institutional 'Wall Street' landlords have grown in prominence, they still constitute only a small fraction of the aggregate market. If the price jump was driven by a broader set of buyers, we should observe a systematic shift in mortgage behavior.

To test whether the price jump was credit-fueled or cash-driven, we utilize the *Zillow Transaction and Assessment Dataset* (ZTRAX). This allows us to track the evolution of mortgage liens associated with new purchases in high resolution.[19] This dataset captures two primary transaction types: ownership transfers between buyers and sellers, and mortgage lien changes (e.g., refinances) where property ownership is retained. To assess changes in mortgage financing, we calculate the

[19] The ZTRAX dataset contains approximately 90 million property transactions where a mortgage lien was assigned within two weeks of registration. Key variables include sale price, square footage, and loan amount. The dataset offers high coverage, with over 88 million transactions including loan amount and 84 million including sale price. Location identifiers, such as FIPS codes, are always available, while property ZIP codes are missing in only 3.6% of observations.

average mortgage amount per square foot for each biweekly period around the *HQ2* announcement, enabling us to track financing trends relative to house price increases in treated locations.

Building on this data, we examine whether mortgage financing amounts increased after the *HQ2* shock in line with the observed house price appreciation. A decline in the average loan-to-value (LTV) ratio would indicate a shift toward deep-pocketed investors who rely more on cash or alternative funding sources for new purchases. In this scenario, property values would rise, but loan amounts would grow more slowly than prices, reflecting reduced credit use relative to purchase prices. Conversely, a stable LTV ratio would suggest no significant change in buyer composition, indicating that traditional home buyers, who finance purchases with mortgages at pre-shock levels, continued to dominate the market, with loan amounts rising proportionally to prices.

These trends are illustrated in Figure 7, which shows mortgage-financed transaction patterns in Crystal City before and after the *HQ2* shock. The left panel plots the mortgage amount per square foot for all transactions, including both mortgage-financed and cash purchases, to capture overall financing trends. The right panel focuses on transactions with mortgages, plotting the mortgage amount per square foot. In the panels of Figure 7, the first vertical, dashed line denotes the period of the *HQ2* shock, and the second vertical, dashed line marks eight weeks post-announcement. The eight-week market serves as the starting point for our second trend line measurement. Given an average time on the market of 53 days (7.5 weeks), this time-frame is considered appropriate for assessing any changes in trends.[20] Both measures of mortgage value per square foot remained stable in Crystal City during the 40 weeks preceding the Amazon *HQ2* announcement (the period to the left of the first vertical dashed line), as indicated by the regression trend line.

Figure 7: Evidence Mortgage Use in Transactions



Figure 7 reveals a clear positive shift in mortgage financing trends eight weeks after the *HQ2* announcement (marked by the second dashed line) in both panels. Specifically, the average

[20] Our findings remain robust across different time windows around the announcement (ranging from 4 to 10 weeks).

mortgage amount per square foot increased by approximately 10% for all transactions and for mortgage-financed transactions in the 40 weeks following the announcement (from $342 to roughly $373). This increase closely mirrors the house price appreciation of $28.4 per square foot observed in treated locations relative to the control group, suggesting that announcement was primarily capitalized into mortgage-financed purchases, with no significant shift toward cash-heavy investors.

Our analysis of the Crystal City housing market after the HQ2 announcement shows that mortgage amounts increased alongside house prices. This evidence motivates the theoretical framework in Section 6, in which collateralized mortgage borrowing helps capitalize expected future income into current housing values. The model examines how unsecured borrowing limits and infrequent refinancing constrain households' ability to smooth consumption following news about future income. These frictions allow house prices and rents to respond differently, while mortgages amplify the house price response.

6 Modeling Expectations Shocks

In this section, we develop a general model of endogenous housing prices and rents. The model, as is standard, generates housing prices and rents from a consumption-based model of asset pricing. We introduce financial frictions to break the equivalence of housing prices and rents, which generates the dynamics observed empirically around the *HQ2* announcement. These frictions include collateralizable mortgages, an unsecured asset borrowing constraint ubiquitous in standard incomplete markets models, and a friction which limits consumption out of household wealth.

Using the model, we analytically characterize the impact of the *HQ2* announcement on local housing markets. We model the *HQ2* announcement as the announcement of a permanent increase in future income. Because of frictions in borrowing against future income and converting housing wealth to immediate consumption, prices and rents become disconnected on impact of the announcement. We demonstrate that this requires a sufficiently large shock to permanent income and is amplified by collateralizable mortgages. For analytical results we study a log utility case, but the model is purposefully general and nests the widely used user cost model as another limiting case. All proofs are provided in Appendix C.

6.1 Environment

Time is discrete and runs for infinite periods. We model a representative family of households indexed $i \in [0, 1]$ in a single location in a small open economy. Therefore interest rates are exogenously fixed, but local house prices and rents are endogenous. The representative family discounts at rate β and pools consumption, assets, mortgages, and housing wealth. We assume that in $t = 0$, the economy is in a long-run steady state. The *HQ2* shock will be modeled as the news that income will permanently rise beginning in $t = 1$.

Income and Liquid Savings. In the initial period, all households are endowed with initial income level $\bar{y}$. Beginning in $t = 1$, income will be $y_1 = \bar{y} + \varepsilon$ where $\varepsilon > 0$ is the impact of *HQ2* on local incomes. Therefore the income process is given

$$y_t = \begin{cases} \bar{y}, & t = 0, \\ \bar{y} + \varepsilon, & \forall t \geq 1. \end{cases} \tag{4}$$

The households have access to saving or borrowing in non-contingent, unsecured, one-period bonds, where their next period position is denoted D_{t+1}. The rate of return on this asset is r^D. To facilitate equilibrium analysis, we make the following assumption.

Assumption 1 $\beta(1 + r^D) = 1$

This is a natural assumption as it will imply that both prior to the *HQ2* shock and in the long run, borrowing will reach a steady state. Under this assumption, we will study the *HQ2* shock as a transition between two steady states.

The unsecured risk-free asset is potentially subject to a binding constraint on borrowing,

$$D_{t+1} \geq -\lambda y_t, \quad \lambda > 0. \tag{5}$$

This condition restricts households from borrowing more than a λ fraction of their current income.

Housing Decisions and Mortgages Households both derive utility from housing services while owning houses as an asset (Garriga et al., 2019). Every period, households choose the amount of housing services to purchase h_t, conditional on their previous housing services, h_{t-1}. Housing prices are denoted p_t. To purchase housing, the households can borrow through mortgages, where the value of the loan is B_t which amortizes at rate δ^m, and the mortgage rate is r^M.

Every period, an exogenous share of households in the family, $\nu \in [0, 1]$, cash out and refinance their mortgages after housing decisions are made. This formulation captures infrequent access to mortgage refinancing (Berger, Milbradt, Tourre and Vavra, 2025). A low value of ν limits how quickly households can convert increases in housing wealth into resources available for consumption. This process can also be interpreted as a proxy for delays associated with housing transactions. Given the housing market, the law of motion of mortgages is given

$$B_{t+1} = \underbrace{(1-\nu)(1-\delta^m)}_{\equiv a} B_t + \underbrace{m_{t+1}}_{\text{mortgage originations}} . \tag{6}$$

When households refinance, they can borrow up to a loan-to-value (LTV) ratio of $\phi \in [0, 1]$. Under Calvo process for refinancing new mortgage values are bound by

$$m_{t+1} \leq \nu \phi p_t h_t \, . \tag{7}$$

We impose the following assumption,

Assumption 2 *Mortgage rates satisfy* $r^D > r^M > 0$,

This assumption implies that households optimally borrow up to the mortgage collateral constraint.

Household Problem and Timing Given the defined environment, the continuum of households solve the following problem

$$\max_{c_t, h_t} \sum_{t=0}^{\infty} \beta^t U(c_t, h_t) \tag{8}$$

$$\text{s.t. } c_t + p_t(h_t - h_{t-1}) + D_{t+1} + \left[r_t^M + \nu + (1-\nu)\delta^m\right] B_t = y_t + (1 + r_t^D) D_t + m_{t+1}, \tag{9}$$

$$D_{t+1} \geq -\lambda y_t,$$

$$m_{t+1} \leq \nu \phi p_t h_t \ .$$

Households maximize lifetime utility, where we assume $U(\cdot)$ takes the general form

$$U(C, H) = \frac{\left[(1-\alpha) C^{\frac{\eta-1}{\eta}} + \alpha H^{\frac{\eta-1}{\eta}}\right]^{\frac{\eta(1-\sigma)}{\eta-1}} - 1}{1-\sigma}. \tag{10}$$

Every period, households choose their net housing purchases, liquid savings, and consumption while paying down their mortgages subject to the financial constraints.

Every period, timing in the model is as follows.

1. The households enter the period t with D_t, B_t, and h_{t-1} and receive labor income y_t.
2. Households choose h_t
3. A random draw of ν households refinance their mortgage obligations and new mortgages m_{t+1} are originated
4. Households choose consumption and liquid assets D_{t+1}.

We define equilibrium rent as $R_t = U_h(c_t, h_t)/U_c(c_t, h_t)$, the marginal rate of substitution between housing services and non-housing consumption.

Housing Supply and Housing Market Equilibrium. In the baseline model we restrict the total supply of housing to be fixed,

$$h_t = H \tag{11}$$

A fixed supply equilibrium in this economy is a sequence of allocations $\{c_t, h_t, D_{t+1}, B_{t+1}\}_{t=0}^{\infty}$, prices $\{p_t, R_t\}_{t=0}^{\infty}$ which satisfy household optimization (8) and market clearing (11).

6.2 The HQ2 Shock in Equilibrium

We now study how the *HQ2* affects housing markets in equilibrium. For the analytical results which follow, we make the following functional form assumption over preferences

Assumption 3 $U(c_t, h_t) = \log c_t + \gamma \log h_t$,

which implies that $\eta \to 1$, $\sigma \to 1$ in (10).

Through the lens of the model, the *HQ2* shock is the announcement in $t = 0$ that income will permanently increase in $t = 1$ by amount ε. We make a final assumption over the state of the economy prior to the *HQ2* announcement to facilitate analysis,

Assumption 4 *In pre-history $t = -1$, the economy was in the steady state and liquid assets were in zero net supply.*

Therefore, we will compare the dynamics of the economy to a stationary economy that existed prior to the shock. We define

$$z \equiv \frac{\nu\phi\gamma}{1 - \beta - \nu\phi\bar{\theta}}, \tag{12}$$

where z is the ratio of the value of new mortgage borrowing in the steady state to steady-state consumption.[21] Given these assumptions and definitions, we characterize the impact of the *HQ2* shock on equilibrium price-to-rent ratios.

Proposition 1 (*HQ2* Price-to-Rent Ratios.) *Under Assumptions 1–4 and that $z < 1$, then for a sufficiently large HQ2 shock to future income*

$$\varepsilon > \varepsilon^* \equiv \frac{1 - \bar{\theta}z}{\beta(1 - z)}\lambda\bar{y}, \tag{13}$$

then the equilibrium price-to-rent ratio increases on impact,

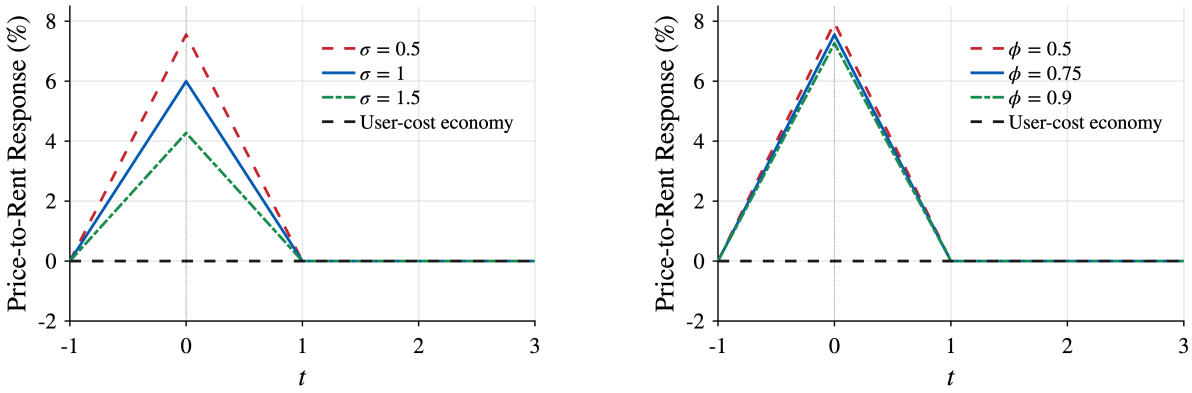
$$\frac{p_0}{R_0} > \frac{\bar{p}}{\bar{R}}\,.$$

Proposition 1 demonstrates that if the *HQ2* announcement is expected to be considerable relative to current income, then the price-to-rent ratio will increase upon announcement. We provide an illustration of price-to-rent ratios around the *HQ2* announcement in Figure 8 for a variety of parameter values. The rationale for this is both intuitive and instructive. The announcement raises expected future housing demand, which increases the value of housing ownership upon announcement. Financial constraint, both borrowing limits and refinancing delays, prevent households from smoothing consumption perfectly to this future income shock.

[21] The term $\bar{\theta}$ is defined in (C.11) and is the stationary Lagrange multiplier on mortgage borrowing from the households' problem (8).

Figure 8: Illustration of price-to-rent ratios around *HQ2* Proposition 1

(a) Alternative σ (b) Alternative ϕ



Notes. Price-to-rent ratio response to *HQ2* announcement for alternative IES levels, σ, and LTV ratios, ϕ. Other illustrative numerical parameters are defined in Table 7.

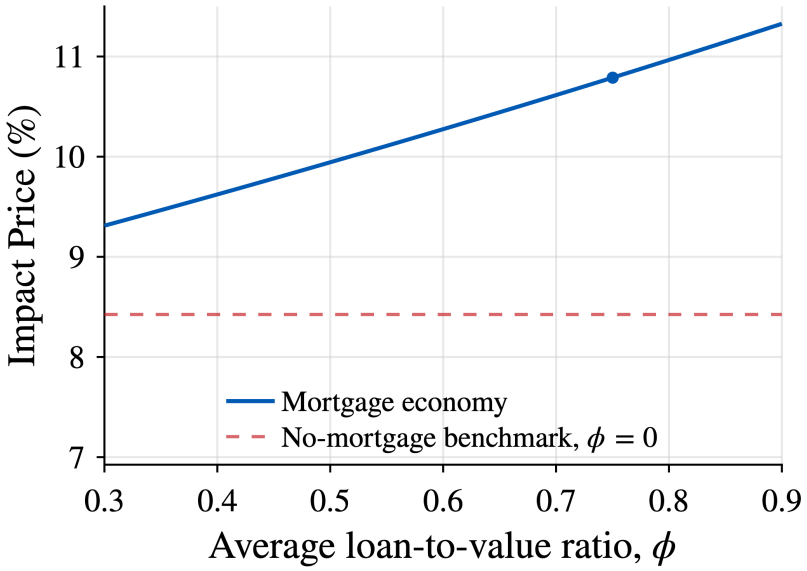
Aiding the price-to-rent dynamics around the *HQ2* announcement is the presence of collateralizable mortgages. Mortgages amplify the price responses because higher house prices relax collateral constraints and expand credit available to households. As a result, house prices reflect both the value future housing service as well as the value of additional credit. On the other hand, rents reflect the ratio of marginal housing utility to consumption, which can become constrained if households cannot bring enough income forward to smooth consumption. Therefore, higher LTV ratios, ϕ, increase contemporaneous resources available to households, relaxes liquidity constraints, and generates additional demand for housing services. Our next proposition formalizes that mortgages amplify price changes.

Proposition 2 (Mortgage Amplification) *Suppose Assumptions 1–4 hold, $z < 1$, the HQ2 shock is sufficiently large, $\varepsilon > \varepsilon^*$. Let M denote the baseline mortgage economy and N the no-mortgage economy, with $\phi = 0$. Then*

$$\frac{p_0^M}{\bar{p}^M} > \frac{p_0^N}{\bar{p}^N}.$$

Proposition 2 demonstrates that mortgages are a critical component of house price amplification for the *HQ2* shock. Relative to an economy where there were no mortgages $\phi = 0$, house prices increase more on impact. This occurs because mortgages provide credit to households, and additional credit increases the value of housing. We plot the implications of increasing LTV ratios in Figure 9. Increasing ϕ results in amplified price changes through increased housing demand and increasing household consumption.

Figure 9: On Impact Price Increases, Proposition 1



Notes. Price-to-rent ratio response to *HQ2* announcement. Illustrative numerical parameters are defined in Table 7.

Comparison to the User Cost Model. As a special case, our model nests the commonly used *user cost* model of housing when there are no mortgages ($\phi = 0$) and borrowing constraints do not bind ($\lambda \to \infty$). To distinguish our approach, and its novelty, from this model, we demonstrate that collateralized mortgages and financial frictions are sufficient conditions to generate *HQ2* dynamics. This is demonstrated numerically in Figure 8, where the user-cost case shows no price-to-rent ratio changes around the *HQ2* announcement. This result, which the next proposition formalizes, occurs because households without financial constraints can perfectly smooth consumption in response to a permanent income shock.

Proposition 3 (Comparison to User Cost.) *Suppose Assumptions 1–4 hold and that* $z < 1$.

A user-cost economy is defined as one where $\lambda \to \infty$ *and* $\phi = 0$ *(UC). Then under a sufficiently large HQ2 shock,* $\varepsilon \geq \varepsilon^*$*, the user-cost price-to-rent ratio does not change and is therefore smaller than the price-to-rent ratio on impact in the mortgage economy.*

6.3 Numerical Illustration

We provide a numerical calibration of the general model to illustrate the propositions and provide insight into the role of the various frictions we have introduced in the model. The illustrative parameters are defined in Table 7. We maintain most of the assumptions made previously, but relax the log utility limit case, and instead make consumption and housing complementary for

quantitative purposes. We do not allow households to borrow against income, $\lambda = 0$, as a natural limiting case of the model.

Table 7: Illustrative Numerical Calibration

Parameter	Description	Value
Preferences and Endowments		
β	Discount factor	0.95
σ	CRRA coefficient over the CES composite	0.5
α	Housing weight in the CES composite	0.20
η	Elasticity of substitution: consumption and housing	0.40
$\bar{y}$	Pre-HQ2 income	1.00
$\bar{H}$	Fixed aggregate housing supply	1.00
Credit markets		
λ	Unsecured borrowing-limit coefficient	0.00
ν	Annual Refinancing probability	0.03
ϕ	Average loan-to-value ratio	0.75
δ_m	Mortgage amortization rate	0.035
r^M	Mortgage interest rate	0.03
HQ2 experiment		
ϵ	Permanent increase in income	0.055

Quantitatively, we characterize the roles of the various financial frictions affecting price-to-rent dynamics in Figure 10. Panels (a) and (c) consider varying the refinancing rate, ν,. Increasing refinancing rates allows for households to use their housing gains from future income to increase consumption today, implying that after $\nu > 0.10$, the refinancing gains are enough to smooth consumption. Panels (b) and (d) change the unsecured borrowing limit, λ. In general, a higher borrowing limit allows for the households to smooth consumption and borrow against the future, permanently higher labor income due to the *HQ2* shock. If the borrowing limit is sufficiently tight, then the price-to-rent ratio must increase as households are constrained from bringing future income gains forward in time by credit frictions. These findings are qualitatively similar across various values of risk aversion or mortgage LTV ratios.

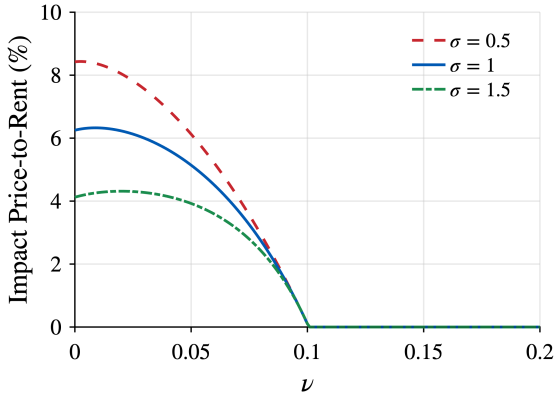
6.4 Discussion

This section has developed a general equilibrium model of housing prices and rents by incorporating mortgages into a standard incomplete markets model with fixed housing supply. By modeling interest rates as in a small open economy, the model represents a local housing and labor market environment.

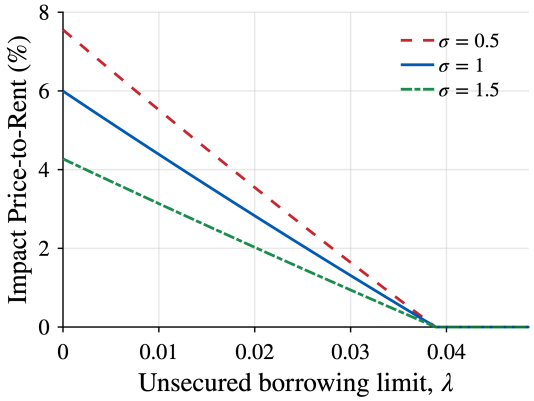
By studying a parametrization of the economy which was an initial steady state with constant savings, and log utility, we can analytically characterize the dynamic path of the economy when the *HQ2* announcement occurs. We model the *HQ2* shock as the arrival of news in $t = 0$

Figure 10: The Role of Frictions: Price-to-Rent Ratios on Impact

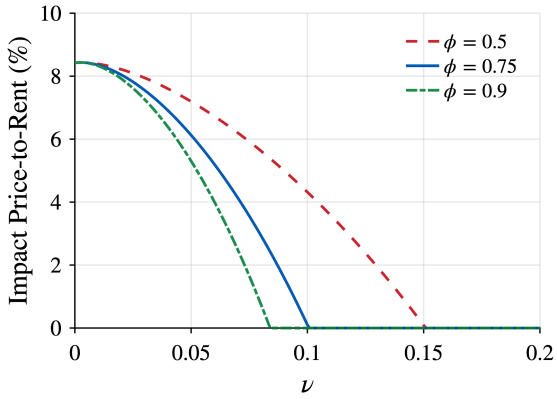
(a) Varying ν and σ



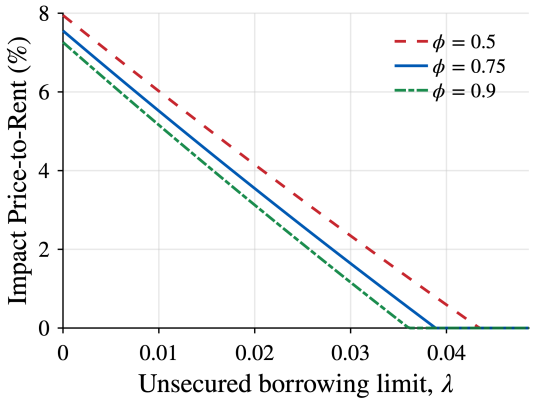
(b) Varying λ and σ



(c) Varying ν and ϕ



(d) Varying λ and ϕ



Notes. Price-to-rent ratios on announcement of *HQ2* for various magnitudes of financial frictions. Panel (a) displays on-impact responses when varying the frequency of cash-out refinances, ν. Panel (b) displays on-impact responses when varying the unsecured borrowing limit, λ. Baseline parameters are defined in Table 7.

that labor income will permanently increase in $t = 1$. If the expected labor income is sufficiently large, mortgage frictions will raise the price of housing services, but standard financial frictions in incomplete markets models prevent households from smoothing consumption. The result is that prices will increase, amplified by collateralized mortgages. Meanwhile, rent increases will be dampened, and price-to-rent ratios will rise.

This analytical characterization is an important contribution to the literature. While many papers have studied the quantitative implications of credit market or expectations shocks on local housing markets, our exercises provide an anatomy of expectations shocks and yield new insights. Specifically, the presence of financial frictions, beyond being a source of housing dynamics, are a critical feature of housing markets which are necessary to generate separation in price-to-rent ratio paths. We explicitly compare our frictional housing model to the widely used user cost specification

and demonstrate that, without financial frictions, households can always smooth their consumption to accommodate changes in future income which result in flat housing dynamics. In contrast, when shocks to future income are large, financial frictions prevent this intertemporal smoothing and generate the empirically observed *HQ2* dynamics.

7 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 for macro-housing models of expectations shocks. We develop a tractable general equilibrium model with fixed housing supply, collateralized mortgages, an unsecured borrowing limit, and infrequent refinancing. The model provides analytical conditions under which news about higher future income raises house prices relative to rents on announcement. Mortgages amplify the house price response, while borrowing limits and refinancing frictions constrain the contemporaneous adjustment of consumption and rents. The comparison with an unconstrained user-cost benchmark demonstrates the role of these financial frictions in generating different responses of prices and rents to an expectations shock.

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 describe the alternative methods used too examine the robustness of our empirical results of Section 4. These alternative estimators match either ex ante or ex post observable information to generate counterfactuals.

A.1 Coarsened Exact Matching

Coarsened Exact Matching (*CEM*) is a complementary approach to the standard *DID* estimates reported. 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.[22] For identification we assume outcomes differences in outcomes between treated and matched counterfactual observations are driven by the *HQ2* announcement.

Table A.1: Crystal City, VA – 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

[22] 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).

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.[23]

Table A.1 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 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.2 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.[24]

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 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.2 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

[23] 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.

[24] 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.2 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.

Table A.2: Crystal City, VA: 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.

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.[25] 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

[25] 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

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.

Table A.3: Crystal City, 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

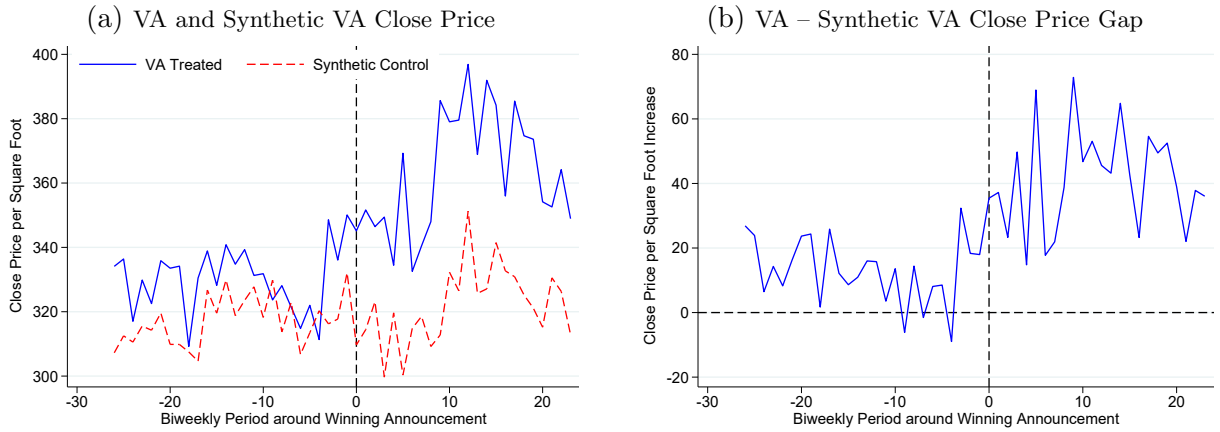
Notes: Variable means over the twelve months period leading to Amazon's *HQ2* announcement date. Time t is measured in biweekly periods.

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 mean squared prediction error of Close Price/Sq.Ft to define the synthetic control. Table A.3 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.

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



Notes: Figures show bi-weekly average close price per square foot, CLOSE PRICE/SQ.FT, in VA relative to Amazon's *HQ2* announcement date.

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 *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 4.4, 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 using the non-finalist control sample.[26] 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.1.[27] 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.[28]

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 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

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

[27] We report *CEM* balance statistics in Table D.10.

[28] 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.5-D.9 in Online Appendix D.

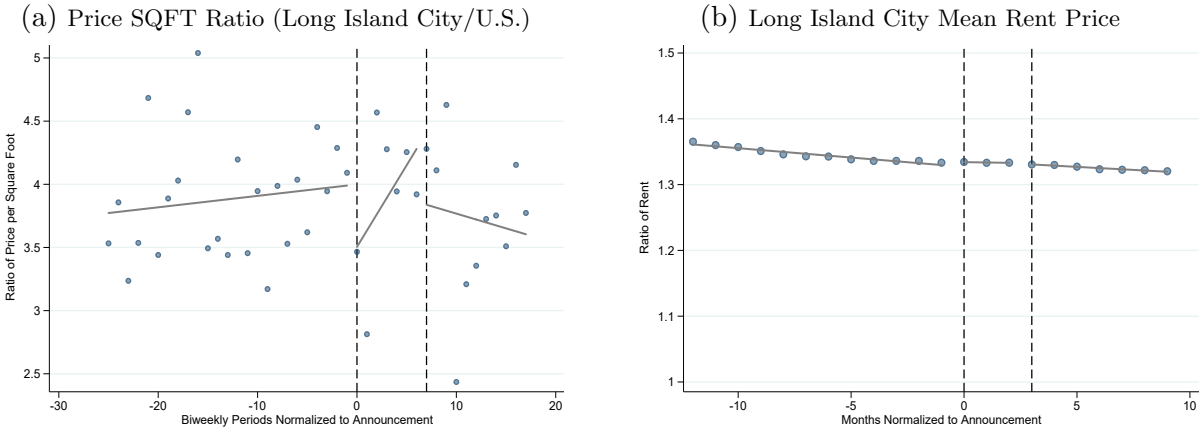
Table B.1: Summary Statistics for Long Island City, NY

	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.

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



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 5.2, 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

Table B.2: Long Island City, NY: 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.

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 3 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 Table 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

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

	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.

$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 5.2 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 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.

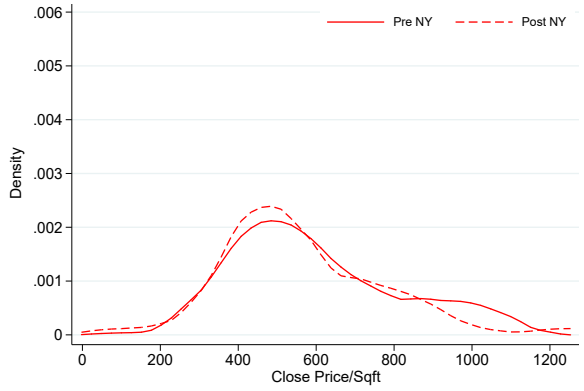
Table B.4: Stochastic Dominance Tests

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

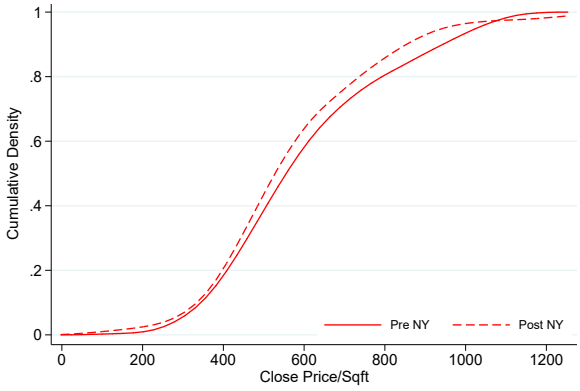
Notes: Test reports p-values of the Kolmogorov-Smirnov test of stochastic dominance obtained by simulating the maximal difference of two cumulative distribution functions over an evenly spaced grid of 100 points covering the whole support of each simulated distribution of Close Price/Sq.Ft using 1000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

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

(a) NY Close Price Density



(b) NY Close Price Distribution



C Model Derivations

This appendix contains model derivations and proofs that do not appear in the main text.

C.1 Household Optimality Conditions

Consider the household problem defined in (8). The consolidated household budget constraint can be written as

$$c_t + p_t(h_t - h_{t-1}) + D_{t+1} + (1 + r^M)B_t = y_t + (1 + r^D)D_t + B_{t+1}, \tag{C.1}$$

and mortgage balances are constrained to follow

$$B_{t+1} \leq aB_t + \nu\phi p_t h_t \ .$$

Denote the Lagrange multiplier on the budget constraint q_t, the Lagrange multiplier on the liquid asset borrowing constraint μ_t, and the Lagrange multiplier on the consolidated mortgage

constraint defined above as ξ_t. Under Assumptions 1–3 the first order conditions for the model are given

$$[D_{t+1}]: \quad \frac{1}{c_t} = \frac{1}{c_{t+1}} + \mu_t, \tag{C.2}$$

$$[B_{t+1}] \quad \xi_t = \frac{1}{c_t} - \beta(1 + r^M)q_{t+1} + \beta a \xi_{t+1} \ , \tag{C.3}$$

$$[h_t]: \quad \frac{\gamma}{h_t} - q_t p_t + \nu\phi p_t \xi_t + \beta q_{t+1} p_{t+1} = 0 \ . \tag{C.4}$$

We note that $q_t = \frac{1}{c_t}$ and define

$$\theta_t \equiv \frac{\xi_t}{q_t} = \xi_t c_t, \tag{C.5}$$

which we use to rewrite the Euler equation for mortgages as

$$\theta_t = 1 - \beta(1 + r_t^m)\frac{c_t}{c_{t+1}} + \beta a \frac{c_t}{c_{t+1}}\theta_{t+1} \ . \tag{C.6}$$

We also note that because $r_r^D > r_t^m$ and the Lagrange multipliers weakly positive, we can rewrite the mortgage law of motion and mortgage originations as

$$B_{t+1} = aB_t + \nu\phi p_t h_t, \tag{C.7}$$

$$m_{t+1} = \nu\phi p_t h_t. \tag{C.8}$$

Using (C.4) we can write equilibrium rents as

$$R_t = \gamma \frac{c_t}{H}. \tag{C.9}$$

Furthermore, we can normalize (C.4) by dividing by q_t to derive the following price and rent relation,

$$p_t(1 - \nu\phi\theta_t) = R_t + \beta \frac{c_t}{c_{t+1}} p_{t+1}. \tag{C.10}$$

C.2 Steady State with Zero Liquid Assets

In this Appendix we characterize the steady state of the model when equilibrium liquid assets are in zero net supply. For analytical convenience, we assume this was the initial pre-history of the model prior to $t = 0$.

The stationary mortgage multiplier $\bar{\theta}$ derived from (C.6) is derived

$$\bar{\theta} = \frac{r^D - r^m}{1 + r^D - a}, \tag{C.11}$$

where $a \equiv (1-\nu)(1-\delta^m)$. Similarly, the steady state rent is given

$$\bar{R} = \bar{p}(1 - \beta - \nu\phi\bar{\theta}). \tag{C.12}$$

We denote the steady state price-to-rent ratio as $K \equiv \frac{\bar{p}}{\bar{R}}$, which can then be written as

$$K = \frac{1}{1 - \beta - \nu\phi\bar{\theta}}\,. \tag{C.13}$$

A useful object to define for equilibrium analysis is $z \equiv \nu\phi\gamma K$. This term helps define steady state consumption as

$$\bar{c} = \frac{\bar{y}}{1 + \frac{r^m z}{1-a}}\,. \tag{C.14}$$

C.3 Proofs

Proof of Proposition 1. PROOF: The proof proceeds in multiple steps.

Throughout, we restrict attention to equilibria satisfying the standard transversality and no-bubble conditions,

$$\lim_{T\to\infty} \beta^T \frac{D_{T+1}}{c_T} = 0,$$
$$\lim_{T\to\infty} \beta^T \frac{p_T H}{c_T} = 0.$$

Unconstrained allocation. First, assume the unsecured borrowing constraint is not binding in any period. Then $\mu_t = 0$ for every t. Assumption 1 and the liquid-asset Euler equation imply

$$\frac{1}{c_t^u} = \frac{1}{c_{t+1}^u},$$

so consumption is constant: $c_t^u = c^u$ for every t. Since housing supply is fixed, rent is also constant, and the housing Euler equation implies

$$R^u = \gamma\frac{c^u}{H}, \qquad p^u = K\gamma\frac{c^u}{H}.$$

Define deviations from the steady state which holds initially as

$$x^u \equiv c^u - \bar{c}, \qquad b_t^u \equiv B_t^u - \bar{B}.$$

Because the mortgage constraint binds under Assumption 2, mortgage debt evolves according to

$$b^u_{t+1} = ab^u_t + zx^u, \qquad b^u_0 = 0,$$

and therefore

$$b^u_t = \frac{zx^u}{1-a}(1-a^t).$$

Using the household budget constraint and the liquid-asset transversality condition, the present-value budget constraint is

$$\frac{x^u}{1-\beta} + (r^M - r^D)\sum_{t=0}^{\infty}\beta^t b^u_t = \frac{\beta\varepsilon}{1-\beta}.$$

Moreover the present value of mortgage payments is defined as,

$$\sum_{t=0}^{\infty}\beta^t b^u_t = \frac{\beta z x^u}{(1-\beta)(1-\beta a)}$$

and we define the parameters,

$$\bar{\theta} = \frac{\beta(r^D - r^M)}{1-\beta a}.$$

It follows that consumption dynamics out of the steady state are defined as

$$(1-\bar{\theta}z)x^u = \beta\varepsilon, \qquad x^u = \frac{\beta\varepsilon}{1-\bar{\theta}z}.$$

At $t = 0$, $b^u_1 = zx^u$. The period-0 budget constraint then gives

$$D^u_1 = b^u_1 - x^u = -(1-z)x^u = -\frac{\beta(1-z)}{1-\bar{\theta}z}\varepsilon.$$

Consequently, the unconstrained allocation violates $D_1 \geq -\lambda\bar{y}$ if and only if

$$\varepsilon > \frac{1-\bar{\theta}z}{\beta(1-z)}\lambda\bar{y} \equiv \varepsilon^*.$$

Thus, when $\varepsilon > \varepsilon^*$, the period-0 unsecured borrowing constraint binds:

$$D_1 = -\lambda\bar{y}, \qquad \mu_0 > 0.$$

Guess the Continuation Path. Guess that the unsecured borrowing constraint is slack for every $t \geq 1$. Because the constraint binds at $t = 0$, the liquid-asset Euler equation gives

$$\frac{1}{c_0} = \frac{1}{c_1} + \mu_0,$$

and hence

$$c_1 > c_0.$$

Continuation path. Under the guess, $\mu_t = 0$ for every $t \geq 1$. Assumption 1 therefore implies

$$c_t = c_1, \qquad \theta_t = \bar{\theta}, \qquad R_t = R_1, \qquad p_t = p_1 = KR_1 \quad \text{for every } t \geq 1.$$

On-impact price-to-rent ratio. Define

$$s \equiv \frac{c_0}{c_1} \in (0, 1).$$

The mortgage Euler equation at $t = 0$ gives

$$\begin{aligned} \theta_0 &= 1 - \beta(1 + r^M)s + \beta a s \bar{\theta} \\ &= 1 - (1 - \bar{\theta})s, \end{aligned}$$

where the second equality uses $1 - \bar{\theta} = \beta[(1 + r^M) - a\bar{\theta}]$. Therefore,

$$\theta_0 - \bar{\theta} = (1 - \bar{\theta})(1 - s) > 0.$$

The housing Euler equation at $t = 0$ is

$$p_0(1 - \nu\phi\theta_0) = R_0 + \beta s p_1.$$

Because $R_0 = sR_1$ and $p_1 = KR_1$, the initial price-to-rent ratio is characterized by

$$\frac{p_0}{R_0}(1 - \nu\phi\theta_0) = 1 + \beta K.$$

The definition

$$K = \frac{1}{1 - \beta - \nu\phi\bar{\theta}}$$

implies $1 + \beta K = K(1 - \nu\phi\bar{\theta})$. Hence the price-to-rent ratio on impact is defined as

$$\frac{p_0}{R_0} = K\frac{1 - \nu\phi\bar{\theta}}{1 - \nu\phi\theta_0} > K = \frac{\bar{p}}{\bar{R}},$$

where the strict inequality follows from $\theta_0 > \bar{\theta}$ and $\nu\phi > 0$.

Verification of the continuation guess. It remains to verify that the candidate path of Lagrange multipliers holds: $\mu_t = 0$ for every $t \geq 1$. Define the long-run mortgage balance associated with c_1 by

$$B^* \equiv \frac{zc_1}{1-a}.$$

For $t \geq 1$, let

$$b_t \equiv B_t - B^*, \qquad d_t \equiv D_t - D^*,$$

where D^* denotes the long-run liquid-asset position. Mortgage dynamics and the household budget constraint imply

$$b_{t+1} = ab_t,$$

where recall $(1-\nu)(1-\delta^m)$. We then write mortgage and liquid asset dynamics jointly as

$$d_{t+1} + (1+r^M)b_t = (1+r^D)d_t + b_{t+1}.$$

Using the parameter definition $1-\bar{\theta} = \frac{1+r^M-a}{1+r^D-a}$, we define liquid assets net of mortgage balances as

$$e_t \equiv d_t - (1-\bar{\theta})b_t.$$

Combining the preceding equations implies

$$e_{t+1} = (1+r^D)e_t.$$

Because $\beta(1+r^D) = 1$ by assumption, the liquid-asset transversality condition requires $e_1 = 0$. Thus,

$$D_t - D^* = (1-\bar{\theta})(B_t - B^*) \quad \text{for every } t \geq 1.$$

We must next establish that $c_1 > \bar{c}$. The post-shock long-run budget constraint, the preceding relation evaluated at $t = 1$, and $D_1 = -\lambda\bar{y}$ imply initial dynamics as,

$$(1-\bar{\theta}z)(c_1 - \bar{c}) + r^D(1-\bar{\theta})(B_1 - \bar{B}) = \varepsilon - r^D\lambda\bar{y}.$$

The period-0 budget constraint gives

$$B_1 - \bar{B} = c_0 - \bar{c} - \lambda\bar{y}.$$

Combining these equations yields

$$(1-\bar{\theta}z)(c_1-\bar{c})+r^D(1-\bar{\theta})(c_0-\bar{c})=\varepsilon-r^D\bar{\theta}\lambda\bar{y}.$$

Furthermore we can demonstrate that the following must hold,

$$\frac{\varepsilon^*}{\lambda\bar{y}}-r^D\bar{\theta}=\frac{1+r^D(1-\bar{\theta})-\bar{\theta}z}{1-z}>0.$$

If $c_1\leq\bar{c}$, then $c_0<c_1\leq\bar{c}$, which implies the left-hand side of the preceding consumption equation is non-positive, while its right-hand side is positive because $\varepsilon>\varepsilon^*$. This is a contradiction. Therefore the guess is verified and,

$$c_1>\bar{c},\qquad B^*>\bar{B}.$$

Characterizing the price-to-rent ratio, the on-impact housing price equation is given

$$\frac{p_0}{p_1}=s\frac{1-\nu\phi\bar{\theta}}{1-\nu\phi+\nu\phi(1-\bar{\theta})s}.$$

The denominator is less than the numerator $(1-\nu\phi)(1-s)\geq 0$ under parameter restrictions. Therefore, $p_0\leq p_1$. It follows that

$$B_1-B^*=a(\bar{B}-B^*)+\nu\phi H(p_0-p_1)\leq 0.$$

Since $B_{t+1}-B^*=a(B_t-B^*)$ and $a<1$,

$$B_{t+1}-B_t=(a-1)(B_t-B^*)\geq 0\quad\text{for every } t\geq 1.$$

The relation between liquid assets and mortgage debt then implies the stock of liquid assets evolves as

$$D_{t+1}-D_t=(1-\bar{\theta})(B_{t+1}-B_t)\geq 0.$$

Finally, for every $t\geq 1$,

$$D_{t+1}\geq D_1=-\lambda\bar{y}>-\lambda(\bar{y}+\varepsilon)=-\lambda y_t.$$

Thus, the unsecured borrowing constraint is slack for every $t\geq 1$, which verifies the continuation guess and completes the proof. ∎

Proof of Proposition 2. PROOF: Because

$$\varepsilon^*=\frac{1-\bar{\theta}z}{1-z}\frac{\lambda\bar{y}}{\beta}>\frac{\lambda\bar{y}}{\beta},$$

the condition $\varepsilon > \varepsilon^*$ implies that the liquid borrowing constraint binds on impact in both the mortgage and no-mortgage economies as proved in Proposition 1.

No-Mortgage Economy. Under the economy with no mortgages (N), $\phi = 0$ which implies $B_t = 0$ for all periods. Under the *HQ2* income process, $y_1 = y_t = \bar{y} + \varepsilon$, $\forall t \geq 1$. Under $\varepsilon \geq \varepsilon^*$, the liquid asset borrowing constraint binds as $D_1 = -\lambda\bar{y}$. Therefore, the borrowing constraint in $t = 0$ gives

$$c_0^N = (1 + \lambda)\bar{y} \, .$$

In the no-mortgage initial steady state, $\bar{y} = \bar{c}^N$ under the assumption that $\beta(1 + r^D) = 1$, and $D_{-1}^N = 0$. Therefore $\frac{c_0^N}{\bar{c}^N} = 1 + \lambda$.

We now prove that the price-to-rent ratio does not move in the no-mortgage economy. For $t \geq 1$, income is constant and budget constraints are given

$$c_t^N + D_{t+1}^N = \bar{y} + \varepsilon + (1 + r^D)D_t^N.$$

Guess that future borrowing constraints are slack, $\mu_t^N = 0$, $\forall t \geq 1$. Then the consumption Euler equation implies that $c_{t+1}^N = c_t^N = c^*$. The budget constraint will then imply,

$$D_{t+1}^N = (1 + r^d)D_t^N + \bar{y} + \varepsilon - c^*,$$

which can be solved forward to derive

$$D_t^N = D^* + (1 + r^d)(D_1^N - D^*),$$

where $c^* = Y + r^D D^*$. The transversality condition rules out an explosion in $D_t^N - D^*$, so $D_1^N = D^*$. This then gives the consumption path as $c_t^N = \bar{y} + \varepsilon + r^D D_1^N$ for all $t \geq 1$.

To verify feasibility, the future borrowing limit under *HQ2* is $D_{t+1}^N \geq -\lambda(\bar{y} + \varepsilon)$. Because the initial borrowing limit is against current income and it is binding in $t = 0$ with $\varepsilon \geq \varepsilon^*$, it is the case that

$$D_1^N = -\lambda\bar{y} > -\lambda(\bar{y} + \varepsilon).$$

This implies that for all $t \geq 1$, future borrowing constraints are feasible and slack, and therefore optimal under the consumption Euler equation. This proves that $c_t^N = c_1^N$ and $R_1^N = R_t^N$.

Under $\phi = 0$, the housing price equation is derived as

$$p_t^N = R_t^N + \beta \frac{c_t^N}{c_{t+1}^N} p_{t+1}^N.$$

For $t \geq 1$, $p_t^N = R_1^N + \beta p_{t+1}^N$. Under the assumption that there cannot be housing bubbles,

$$p_t^N = \frac{R_1^N}{1-\beta},$$

and it immediately follows that the price to rent ratio is given $\frac{p_t^N}{R_t^N} = \frac{1}{1-\beta}$. On the impact of *HQ2* at $t = 0$, housing prices are defined as

$$p_0^N = R_0^N + \beta \frac{c_0^N}{c_1^N} p_1^N,$$

which after dividing by R_1^N and using that $R_t^N = \gamma \frac{c_t^N}{H}$,

$$\frac{p_0^N}{R_0^N} = 1 + \beta \frac{c_0^N}{c_1^N} \frac{1}{1-\beta} \frac{c_1^N}{c_0^N} = \frac{1}{1-\beta}\ .$$

Therefore, in the no-mortgage economy $\frac{p_0^N}{R_0^N} = \frac{\bar{p}^N}{\bar{R}^N}$, so the price-to-rent ratio is constant. Because $R \propto c$, this gives the result that

$$\frac{p_0^N}{R_0^N} = \frac{\bar{p_0}^N}{\bar{R}_0^N} = \frac{1}{1-\beta}, \quad \frac{p_0^N}{\bar{p}^N} = \frac{c_0^N}{\bar{c}^N} = 1 + \lambda$$

Mortgage Economy. In the mortgage economy, $\phi > 0$. By Proposition 1, $\frac{p_0^N / R_0^M}{\bar{p}^M / \bar{R}^M} > 1$. Using that $R_t^M = \gamma \frac{c_t^M}{H}$, this implies that

$$\frac{p_0^M / \bar{p}^M}{c_0^M / \bar{c}^M} > 1.$$

Because $\varepsilon \geq \varepsilon^*$, the $t = 0$ budget constraint implies

$$c_0^M - \lambda \bar{y} + (1 + r^M)\bar{B} = \bar{y} + B_t,$$

and subtracting the steady-state budget constraint and using the mortgage law of motion under the steady state of mortgages $\bar{B} = a\bar{B} + \nu\phi\bar{p}^M H$ yields

$$c_0^M = \bar{c}^M + \lambda \bar{y} + \nu\phi H(p_0^M - \bar{p}^M).$$

We can rewrite to be

$$\frac{c_0^M}{\bar{c}^M} = 1 + \frac{\lambda \bar{y}}{\bar{c}^M} + \frac{\nu\phi\bar{p}^M H}{\bar{c}^M} \left(\frac{p_0^M}{\bar{p}^M} - 1 \right),$$

where Proposition 1 guarantees that $\frac{p_0^M}{\bar{p}^M} > \frac{c_0^M}{\bar{c}^M}$. Therefore,

$$\frac{p_0^M}{\bar{p}^M} = 1 + \frac{\lambda \bar{y}}{\bar{c}^M} + \frac{\nu \phi \bar{p}^M H}{\bar{c}^M} \left(\frac{p_0^M}{\bar{p}^M} - 1 \right),$$

which implies

$$\left(1 - \frac{\nu \phi \bar{p}^M H}{\bar{c}^M} \right) \left(\frac{p_0^M}{\bar{p}^M} - 1 \right) > \frac{\lambda \bar{y}}{\bar{c}^M}.$$

Under the maintained condition that $z \equiv \frac{\nu \phi \bar{p}^M H}{\bar{c}^M} < 1$, it must be true that

$$\frac{p_0^M}{\bar{p}^M} > 1 + \frac{\frac{\lambda \bar{y}}{\bar{c}^M}}{1 - z} > 1 + \frac{\lambda \bar{y}}{\bar{c}^M} .$$

The steady-state budget constraint implies $\bar{c}^M + r^M \bar{B} = \bar{y}$, and therefore the initial consumption path was $\bar{c}^M \leq \bar{y}$ as $\bar{B}$ is positive.[29] This then implies that

$$\frac{\lambda \bar{y}}{\bar{c}^M} \geq \lambda,$$

and therefore, $\frac{p_0^M}{\bar{p}^M} > 1 + \lambda$.

Therefore, $\frac{p_0^M}{\bar{p}^M} - 1 > \lambda = \frac{p_0^N}{\bar{p}^N} - 1$, which completes the proof. ∎

Proof of Proposition 3. Proof: Under Proposition 1, the price-to-rent ratio rises in the mortgage economy.

In the user-cost model, the Euler equations hold without binding Lagrange multipliers. Therefore,

$$\frac{p_0^{UC}}{R_0^C} = \frac{1}{1 - \beta} .$$

This is strictly smaller than the price-to-rent ratio in the mortgage economy,

$$\frac{\bar{p}^M}{\bar{R}^M} = \frac{1}{1 - \beta - \nu \phi \theta} > \frac{1}{1 - \beta} = \frac{p^{UC}}{R^{UC}} .$$

∎

[29] See Appendix C.2.

Online Appendix

D Additional Evidence

Table D.1: Amazon HQ2 Announcement Treated ZIP Codes

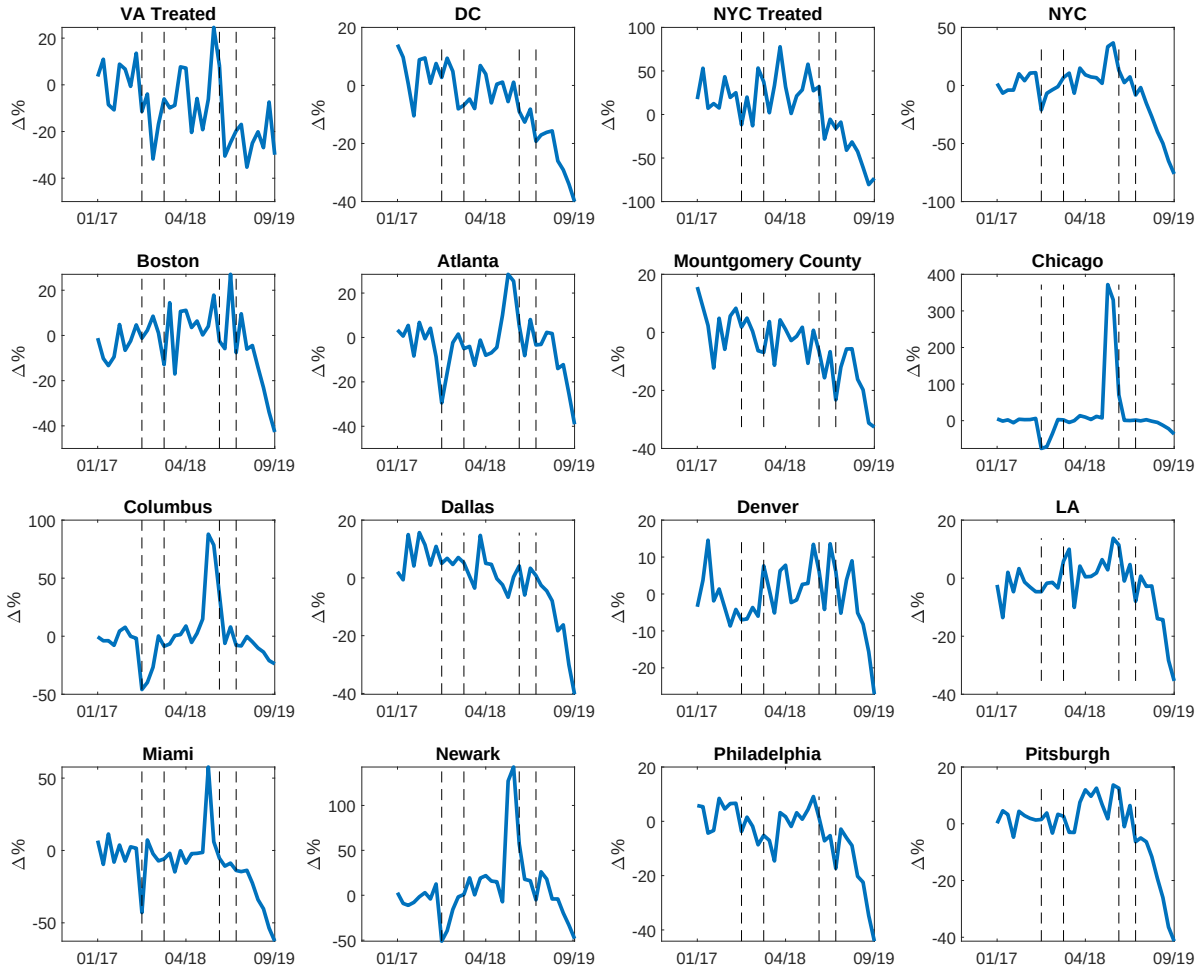
Region	ZIP Codes
Crystal City	22202, 22211, 22305, 22206, 22204, 22301
Long Island City	11101, 11102, 11103, 11104, 11105, 11106, 11109, 11120, 11222 11378, 11377

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.

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

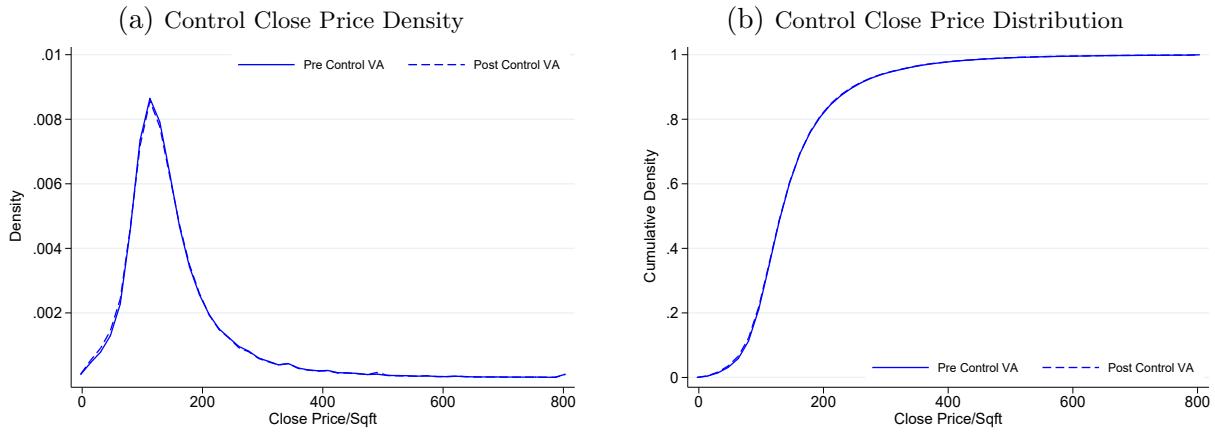


Table D.3: Crystal City: Amazon's HQ2 and Listing Price

	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.4: 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.5: 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.6: 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.7: 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.8: 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.9: 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.

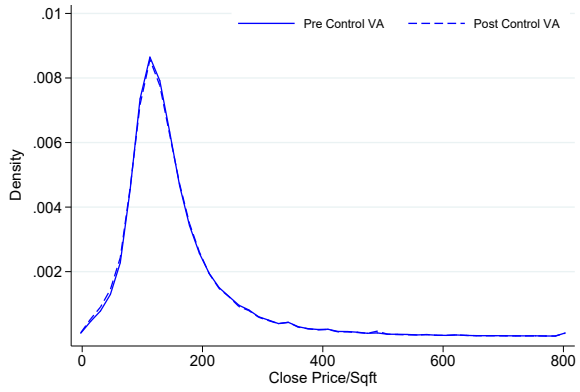
Table D.10: Long Island City: CEM Balance Analysis

	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

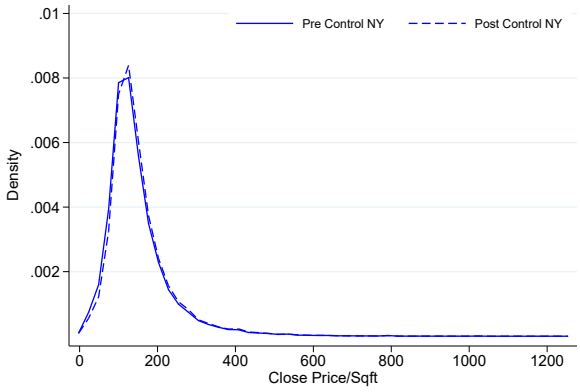
Notes: Sample means for Long Island City, NY. Treated observations in the *CEM* sample receive weight of 1. Control observations in the *CEM* sample receive a weight equal to the ratio of the number of treated and control observations in their specific stratum multiplied by the ratio of the total number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

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

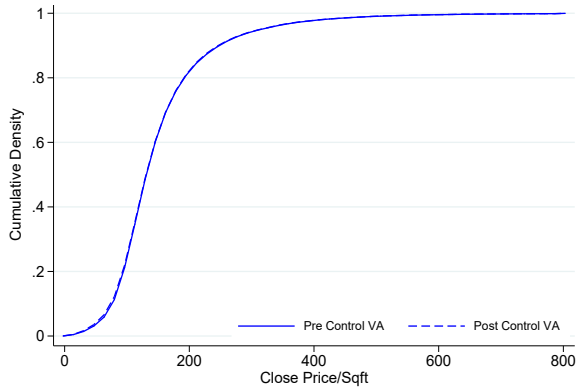
(a) Crystal City Control Close Price Density



(b) Long Island City Control Close Price Density



(c) Crystal City Control Close Price Distribution



(d) Long Island City Control Close Price Distribution

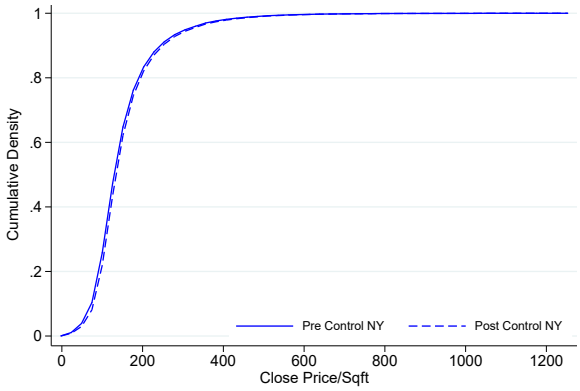


Table D.11: Washington D.C.: Amazon's HQ2 Event Study

	Non-Finalists			
	1 Month	**3 Months**	**6 Months**	**12 Months**
Y = Close Price/Sq.Ft				
HQ2	6.805***	7.672***	7.451***	5.096***
	(1.693)	(1.516)	(1.201)	(1.242)
Announcement	-0.265	1.466***	4.324***	5.321***
	(0.307)	(0.361)	(0.283)	(0.272)
Winner	42.036***	41.953***	41.833***	42.949***
	(7.043)	(6.977)	(7.009)	(6.952)
R^2	0.622	0.619	0.617	0.614
Observations	1,394,446	1,554,516	1,898,614	2,635,034
Treated Obs.	50,146	57,383	72,543	103809
Y = List Price/Sq.Ft				
HQ2	3.132	3.182*	2.735*	3.616***
	(2.428)	(1.784)	(1.426)	(1.365)
Announcement	7.017***	3.213***	-1.377**	-2.814***
	(0.441)	(0.483)	(0.594)	(0.612)
Winner	44.650***	44.462***	44.912***	46.029***
	(7.960)	(8.007)	(8.016)	(7.986)
R^2	0.553	0.550	0.552	0.557
Observations	1,789,146	2,007,712	2,481,231	3,018,183
Treated Obs.	68,005	76,056	98,988	122,734
Y = Time on Market				
HQ2	7.725***	11.485***	6.941***	7.737***
	(1.037)	(0.869)	(0.871)	(0.876)
Announcement	13.748***	22.213***	12.052***	4.057***
	(0.322)	(0.282)	(0.206)	(0.205)
Winner	-49.423***	-48.958***	-49.987***	-50.225***
	(2.202)	(2.147)	(2.160)	(2.067)
R^2	0.098	0.112	0.113	0.092
Observations	1,830,741	2,064,140	2,504,974	3,467,784
Treated Obs.	65,071	74,532	92,911	133,798

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.

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.

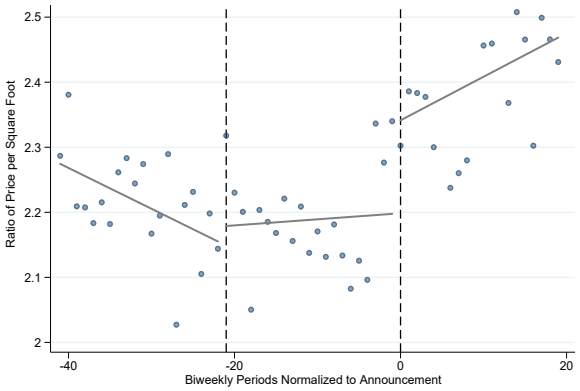
The most natural candidates to experience a significant change in housing conditions as a result of the finalist announcement are the locations that became the winning location. The online oddsmaker Bovada set odds for which area would be chosen as the *HQ2* prior to the winning announcement. The winning 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 3 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.

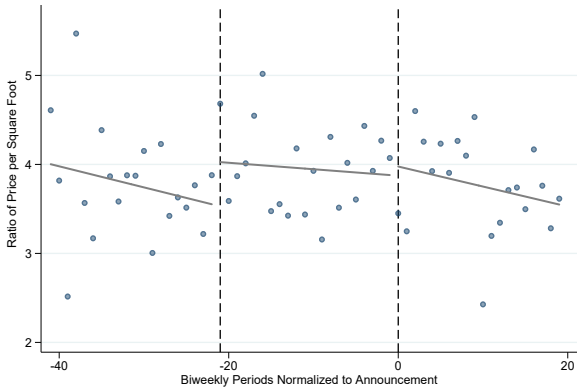
Despite the lack of graphical evidence of a change in price trend around the finalist announcement, we estimate a modification of our baseline difference-in-differences specification to formally test whether the finalist announcement generated significant treatment effects for close prices, list prices, 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.

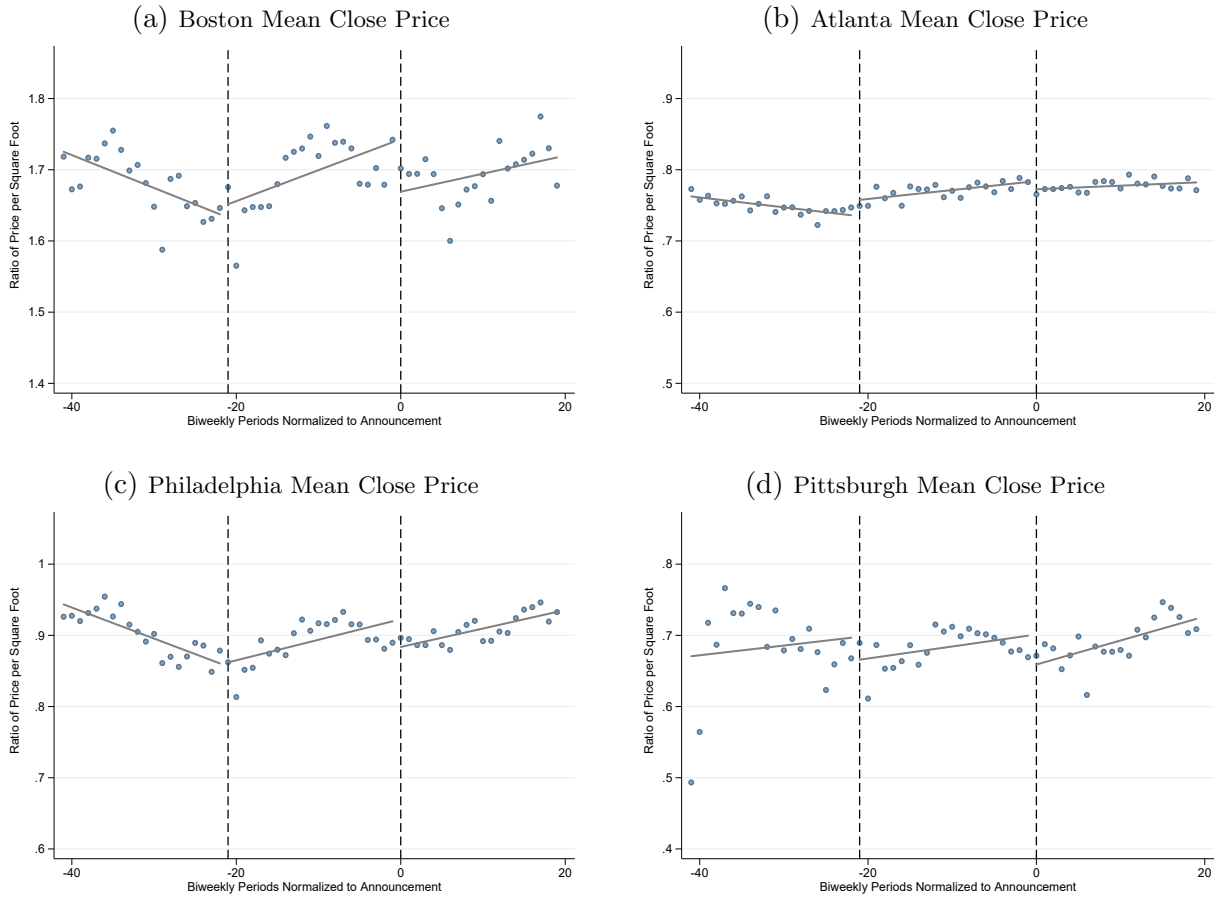
One important note is that although we know the eventual neighborhoods for Northern Virginia and New York City, Amazon never publicly disclosed specific locations within its chosen finalists for the eventual building site of *HQ2* . Therefore, we use the entirety of the finalist metros in our analysis 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.