Build Baby Build?: Housing Submarkets and the Effects of New Construction on Existing Rents *

Anthony Damiano
University of Minnesota

Chris Frenier
University of Minnesota

March 31, 2020

In the past 20 years, after decades of disinvestment and decline, central cities have seen a reversal of fortune and are experiencing rapid growth, particularly in the urban core. Many large cities are now struggling with housing shortages and affordability problems. There is vigorous debate amongst policy makers, scholars, and activists about the role that new market-rate apartments play in alleviating housing affordability issues. Prior research suggests that new market-rate construction may result in more affordable housing in the long-run, but much less is known about how this type of new development affects neighborhoods in the short-run. This study evaluates how new construction affects rent for nearby apartments in the five years after a new market-rate building is completed. We use a novel panel of building-level rents in Minneapolis, MN from 2000-2018 and a difference-in-differences study design to compare rent trajectories of units within 300 meters of new construction to a comparison group 300 to 800 meters away. We evaluate the impact of new construction using both models that treat nearby units as a single rental market, and "submarket" models that allow for heterogeneous treatment effects based on the units' place in the pre-period rent distribution. While we find no effect in the single market model, our submarket approach suggests that lower-priced rental housing close to new construction had 6.7 percent higher rents compared to the comparison group. New construction had the opposite effect on higher-priced housing; rents were 1.7 percent lower close to new construction than in comparison group buildings, but we cannot rule out a null effect. The size of the effect was larger closer to new construction and the effects persisted for at least two years after the new construction is completed. This study reiterates the importance of understanding housing as a collection of submarkets rather than a singular market. Our findings are important for planners and policymakers who seek to balance growth and protect existing lower-income communities.

Keywords: housing supply, housing submarkets, neighborhood effects, gentrification

*This project made possible with additional funding and support from the Minnesota Housing Partnership. Thank you to Alexander Everhart, Edward Goetz, Gabriella Norton, and Laura Barrie Smith for providing feedback on previous drafts. **Corresponding author** : Anthony Damiano damia025@umn.edu.

1 Introduction

"My landlord does nothing to improve this 3 bedroom apartment we've been living in for 7 years, but every year he raises the rent. This year he tried to raise it twice in a year. I asked him why he was raising it without doing anything; he replied, "have you seen what apartments are going for on Craigslist?" So basically he was just saying 'BECAUSE I CAN'." –Debi (*Tenants Together—Tenant Voices Project*, n.d.)

Since the great recession, the US has witnessed a sharp increase in housing affordability issues, particularly for low-income households (Colburn and Allen, 2018). The Harvard Joint Center on Housing Studies (2019) estimates that 83 percent of households making less than $15,000 per year spend at least half of their income on housing. The report also finds that housing supply has not only failed to keep pace with population growth, but has contracted by four million low-cost (rent < $ 800 a month) rental units since 2011. Stagnant wages, lagging federal support for affordable housing and rising prices for new market-rate housing have also added to the affordable housing crisis (Gould, 2020; Joint Center for Housing Studies of Harvard University, 2019; Weiss and Brown, 2017).

Lagging housing supply and high prices have led to calls from policy makers, advocates, and researchers to focus on adding more rental housing stock to alleviate price pressures (Aurand et al., 2019; Manville et al., 2020; Mogush and Worthington, 2020). New, market-rate housing may alleviate rent pressures on low-income households in the long-run, there are serious concerns, like those voiced in the quote above, that increased supply at the top end of the market may not be an effective mechanism for reducing rent pressures faced by low-income households in the short-run (Jacobus, 2016).

The economic justification for adding housing to reduce price pressures is that construction of additional units, at any price point, creates more competition between property owners to fill their units from a fixed pool of renters. If landlords respond by cutting prices (or at least mediate rent increases), renters will move into new units, creating vacancies and price competition in other parts of the rental market. Some researchers have posited that adding high-end housing may shift both the supply and demand curve if new, modern units affect the desirability of a neighborhood or attract new amenities that put upward pressures on rents (Angotti and Morse, 2017; Couture et al., 2018; Ooi and Le, 2013). Shifts in demand for housing in particular neighborhoods could offset the supply effect, but the relative size of the supply and demand effect are empirical questions that have proven difficult to investigate.

Previous research on the effects of increased housing supply has focused on how new construction affects rents at the regional level (Glaeser et al., 2005; Saks, 2008). Attempts to identify the effects of new construction at smaller geographic scales face two significant empirical challenges. First, developers do not select sites for new rental construction randomly and instead target neighborhoods where their investments will produce the greatest return in the form of higher rental revenue. Second, the introduction of new market-rate units may attract higher-end amenities like restaurants, retail, or affluent residents, that increase demand for surrounding properties (Anenberg and Kung, 2018; Baum-Snow and Hartley, 2017). Both of these concerns make it difficult to disentangle the effect of increased local supply from shifting neighborhood characteristics before and after new construction is completed (Guerrieri et al., 2013).

This study adds to a new and growing body of empirical literature that uses within-neighborhood comparisons of rental prices to estimate how new market-rate buildings affect existing rents. We use building-level data on rental prices in Minneapolis, MN from 2000-2018 to compare the rent trajectories of buildings close to (within 300m) and slightly farther (300 to 800m) from new market-rate apartment construction. We are particularly interested in whether new construction has different effects on existing rents at different points in the pre-construction rent distribution. Drawing on housing submarket theory, we test the hypothesis that the effects of new construction could vary across submarkets.

We employ empirical models that allow us to evaluate heterogeneity in the effect of new construction on existing rents. This effect could differ across three important, related dimensions; the market tier of the nearby housing, the distance from the new construction, and the time since new construction. Our primary analyses use three difference-in-differences (DID) models to address each of these potential sources of heterogeneity. First, we use a DID model with market tier interactions to investigate whether new construction affected submarkets differently. Second, we present models using a categorical distance variable to estimate how the treatment effect changes as one moves away from new construction. Finally, we use an event study approach to trace the effects over time.

Our results suggest that the effects of new construction on rents in older buildings vary depending on the older building's place in the pre-construction rent distribution. We find, overall, that new buildings had no significant effect on rents in nearby units, but this average treatment effect masked meaningful and significant variation across the pre-period price distribution for existing buildings. After dividing the buildings in our data into three submarkets or terciles based on their rent in 2000, we estimate that new construction increased rent by 6.7 percent in the lowest rent tercile, had no effect on the middle tercile, and decreased rents by 1.7 percent in the highest tercile. We also find that these effects are stronger for units located closer to new construction and that the effect of new construction on existing rents persists for up to two years after completion of the new, market-rate building.

We believe this study contributes to the literature on housing supply and rent effects in several ways. First, we produce plausibly causal estimates of the effects of new supply on existing rents, which allow us to address supply and amenity effects at small geographic scales directly. Second, our use of housing submarket theory allows us to test for differential effects between higher and lower quality housing submarkets and furthers our understanding of the dynamics of housing quality submarkets at small geographic scales. Third, we use data from a mid-sized metropolitan area in the Midwest, a type of rental housing market that has received less attention in the literature than large, coastal cities (Asquith et al., 2019; Li, 2019; Singh, 2020).

This research has important implications for public policy. Tenants' rights groups and other place-based community organizations have long viewed luxury construction as driving rent increases and gentrification in lower-income neighborhoods. In places like San Francisco and other high-priced markets, this has put tenant organizers at odds with Yes In My Backyard (YIMBY) activists who have a more positive view of market-rate construction and the role that it can play in lowering housing costs (Schneider, 2020). The conflict has been heated to the point of shouting matches and bitter public conflict between the two sides (Rodriguez, 2018). Our approach allows us to test this hypothesis empirically. If new market-rate apartment construction is leading to higher rents for lower-cost housing, we believe it is crucial to develop policies that both encourage housing supply growth while also protecting existing low-income communities from higher housing costs and displacement.

The remainder of this paper is organized as follows. In Section 1 we summarize a set of recent empirical papers examining how new market-rate construction affects nearby rental prices. This review places our work into direct conversation with other researchers investigating this important question in order to better describe how our results fit with recent findings. We also briefly review the relevant theoretical literature on housing supply, amenity effects, and housing submarkets. Section 2 describes our data and reports building and neighborhood summary statistics. In Section 3, we detail our empirical models and discuss our identification strategy. Section 4 presents the results for our three sets of analyses. In Section 5, we discuss these findings and conclude.

2 Background and Literature Review

2.1 Recent Empirical Papers on the Effects of New Construction

Assessing how new construction affects rent in nearby buildings has proven difficult, in large part, due to sparse data on building-level rents. However, in the past year, there have been several studies using new data sets that provide significant contributions to answering this question. Three recent working papers use building-level (Li, 2019; Singh, 2020) and rental-listing level data (Asquith et al., 2019) to estimate how the construction of new, large, market-rate apartment buildings affects nearby rents. While the results from these papers are mixed, each makes a valuable contribution toward understanding how market-rate construction interacts with local neighborhood conditions to affect the rental market. We briefly summarize the findings of this emergent work below.

Li (2019) uses New York City (NYC) property tax data to measure the effects of new "high-rises" (seven-plus story buildings) on nearby rents in NYC between 2000-2017. Her approach exploits the timing of building permitting and construction completion to measure changes in rents within a 500 foot (150m) buffer around new high rises. Li finds that buildings within 500 feet of new construction saw 1.6 percent lower rents. She also performs a submarket analysis that breaks the rental market into quartiles based on the building's rank in the within-census-tract rent distribution in the last year in her data. The exercise allows Li to calculate the effect of new construction separately for each quartile and she finds that new high rises had a negative effect on nearby rents in the top three quartiles, but an insignificant effect on rents in the lowest quality submarket. While this analysis is, to our knowledge, the first attempt to evaluate treatment effect heterogeneity across submarkets, the findings can be difficult to interpret. Li notes that 74 percent of buildings in the sample underwent some type of renovation between 2003-2013 and that there were buildings that started low in the distribution and subsequently moved up the rent distribution during the study period. Using 2013 market tiers as an indicator makes it challenging to determine whether the re-ordering of buildings within tracts' rent distributions was a differential effect of new construction on lower-tier housing or a secular trend that would have occurred in the absence of new construction.

Asquith et al. (2019) focus on the effects of new market-rate apartments in lower-income neighborhoods, defined by the authors as census tracts with a median household income below the median for the metropolitan area. They use three identification strategies, each pointing to similar findings. The first compares rental listings within 250 meters of new market-rate apartments to listings slightly farther away (250m-600m). The second analysis exploits the timing of new construction and compares rental listing trends in neighborhoods that received new construction in 2015 and 2016 to those that received new construction in 2019 under the assumption that neighborhoods receiving construction in both time periods have similar underlying trends in demand. Lastly, the authors combine both methods into a "triple difference" approach that exploits both timing and distance variation in new construction completion. The results of the three approaches are relatively consistent and find that rental listings close to new construction have prices that are five to seven percent lower than the comparison listings slightly farther away. An important caveat that the authors acknowledge is that the listing data from Zillow may not be representative of the rental market as a whole. The authors find that the listing data in their sample is 53 percent higher than the average census tract median rent in their sample ($1,790 compared to $1,165). Also important to consider is that the authors' main models focus exclusively on new construction effects in lower-income neighborhoods. In a sensitivity analysis, where they include observations from all neighborhood types, they find that new construction has no significant effect on rental listings.

Lastly, a working paper by Singh (2020), also looking at NYC, between 2006-2008, uses the end of a real estate tax program to estimate the effect of new construction on existing rents. In contrast to the previous papers, Singh finds new buildings induced by the tax program were associated with 2.3 percent higher rents in nearby buildings (with 150m/500ft). Singh attributes the rise in rents to the influx of consumption amenities that accompanied the new construction.

We contribute to this emergent body of evidence by adapting the empirical approach used in Asquith et al. (2019) to a different dataset of building-level rents in Minneapolis, MN. We also build on Li's work by formally incorporating submarkets (market tiers) into our modeling to assess whether new construction has differential effects on older units at different pre-construction price points. We discuss how our findings fit in with these recent papers in Section 5.

2.2 Housing Submarkets

The complex nature of housing makes it difficult to model as a standard economic good. Rather than a single good or service, housing can more accurately be thought of as a bundle of goods that include both the unit itself as well as the land beneath it and local spatial amenities. For these reasons, instead of a singular "housing market," it is more advantageous to think of housing as an interconnected set of submarkets segmented by geography, housing type, housing quality, tenure, and neighborhood quality (Galster and Rothenberg, 1991; Galster, 1996; Grigsby, 1963; Piazzesi et al., 2019).

Much of the literature focuses on two types of submarkets: geographic and "structural" or quality submarkets (Watkins, 2001). Geographic submarkets, as the name implies, refers to how different spatial locations have different mixes of amenities which results in quality-adjusted home prices varying across space (Kain and Quigley, 1975). Structural submarkets, first articulated by Grigsby (1963), refer to the close substitutability of housing. For example, a three-bedroom, single-family home is a poor substitute for a studio apartment. As a result, it can reasonably be said that these two types of housing exist in different structural submarkets even if the two buildings are located relatively close together.

Similarly, submarkets exist within the same housing type based on cost and housing quality – a new luxury two-bedroom unit does not cater to the same clientele as a two-bedroom unit in a 50-year-old building with a leaky roof. Building on Grigsby (1963), Galster and Rothenberg (1991) hypothesize, "housing submarkets respond to changes in demand and/or supply on their own and in other submarkets in systematic ways, but the pattern and magnitude of the response is not uniform across quality submarkets" (p. 38). We use this insight to motivate our subsequent analysis that explicitly models the effects of new construction across different submarkets.

2.3 Filtering and Supply Effects

Filtering is the primary mechanism through which the addition of expensive, high-quality housing can put downward pressure on housing costs for lower quality housing (Kain and Quigley, 1975; Weicher and Thibodeau, 1988). The filtering process works as follows — as housing ages, it declines in quality. High-income households demand higher quality housing, which is supplied either through the construction of new modern units or through the rehabilitation of older units. Over time, higher-income households will leave aging, lower-quality housing, and move into newly constructed or renovated housing stock. These moves create vacancies in lower quality housing stock that can be filled by lower-income households. Most studies that explore filtering do so at larger geographic scales, most commonly at the metropolitan area scale. In many ways, filtering can be thought of as an extension of housing submarket theory as the movement of housing over time, from higher to lower quality submarkets as the housing ages.

Filtering takes place over several years and may not be able to accommodate sudden shocks to housing demand. Using estimates from Rosenthal (2014), Zuk and Chapple (2016) estimate that new housing built to be affordable for the median household will be affordable at 80 percent of median income in 15 years and affordable at 50 percent of median income in 50 years. Filtering rates are even slower in metro areas with high home price appreciation rates (Rosenthal, 2014). In a well-functioning housing market, filtering should generally be down the quality spectrum over time. However, in cases where demand is far outstripping supply, reverse filtering or the filtering up of lower quality units into higher quality submarkets can occur as well (Somerville and Holmes, 2001). Research has also shown that rental housing filters faster than owner-occupied housing and that the conversion of ownership housing to rental housing is a significant contributor to the rental housing stock (Rosenthal, 2014).

2.4 Amenity Effects of New Construction

The amenity effect refers to how the construction of new housing creates spillover effects that could increase demand to live in the surrounding neighborhood (Bayer et al., 2007; Guerrieri et al., 2013; Schwartz et al., 2006). Examples of amenity spillovers include new restaurants, entertainment, streetscape improvements, or perceived desirability of the area. Depending on the relative magnitude of the amenity effect compared to the local supply effect, new construction could theoretically result in higher rents for the surrounding neighborhood.

Guerrieri et al. (2013) find that citywide demand shocks can result in, what the authors' term, "endogenous gentrification." They find that increased demand from higher-income households raises prices in affluent/higher demand neighborhoods, which creates spillover effects in nearby lower-income neighborhoods. They posit that this is due to the preference for households to live close to higher-income households (Guerrieri et al., 2013). The authors suspect that the increased localized demand is due, in some combination, to lower crime, better schools, better public services, and consumption amenities that often accompany new high-end developments. However, the precise mechanism for the increase in demand is not explicitly modeled. Their empirical results lend support for this theory. They find that housing prices in low-income neighborhoods close to higher-income neighborhoods saw seven percent higher home value appreciation compared to lower-income neighborhoods farther from more affluent neighborhoods. These housing price increases were also coupled with changes associated with neighborhood upgrading including lower poverty rates, higher median household incomes, and higher shares of college-educated adults (Guerrieri et al., 2013). While the authors are modeling demand shocks as opposed to supply shocks, they find that while more elastic housing markets showed less evidence of endogenous gentrification, the results were not statistically significant. Other studies have produced similar evidence that market-rate density creates a localized amenity effect and that more elastic housing markets mediate but do not eliminate the amenity effect (Anenberg and Kung, 2018; Couture et al., 2018).

Diamond & McQuade (2019) find evidence for neighborhood amenity and disamenity effects related to the construction of new Low-Income Housing Tax Credit (LIHTC) properties. LIHTC buildings are privately-built properties that receive federal tax credits in exchange for keeping rents affordable for moderate-income households (households making between 50 percent and 60 percent of Area Median Income). Diamond & McQuade (2019) find that the construction of LIHTC properties has different effects on property values based on the type neighborhood in which the LIHTC properties were built. In high-income neighborhoods, the construction of LIHTC properties is associated with lower property values in the surrounding neighborhood, while LIHTC properties in lower-income neighborhoods are associated with higher local property values. While this study is examining subsidized rather than market-rate housing, it provides direct evidence of neighborhood effects of new construction. In upper-income neighborhoods, LIHTC properties are likely to signal the arrival of relatively lower-income households. Conversely, in lower-income neighborhoods, LIHTC properties represent significant new investment, and units in these buildings often demand rents at or above the neighborhood median (Burge, 2011). LIHTC projects signal new investment and potentially the arrival of households with incomes above the neighborhood median.

3 Data and Sample Definition

We use a novel panel of rental prices for one- and two-bedroom apartments in Minneapolis, Minnesota from 2000-2018. The data were collected by CoStar Group, a commercial real estate analytics firm. CoStar collects data on property availability, pricing, and characteristics and provides these data to clients through search tools, web-based real estate marketplaces, and consulting services. The data for this research were collected by CoStar through quarterly phone inquiries to rental property owners on the rent and characteristics of their units. We acquired the data for this project via a partnership between the Center for Urban and Regional Affairs (CURA) at the University of Minnesota and the Minnesota Housing Partnership, a St. Paul-based coalition of community groups and non-profit developers interested in expanding affordable housing in Minnesota.

The data provided to us for this analysis is at the building-level but includes variables reporting quarterly average rent for different apartment configurations (number of bedrooms) in each building. We use this information to reshape the data so that our unit of observation is the "building-bedroom" – the combination of building and number of bedrooms. For example, one observation in our data reports the average rent in Q1 2000 for one-bedroom units in a particular building.

CoStar used two different approaches to collect data on older and newer buildings. For buildings constructed prior to 2000, CoStar provided a sample of buildings in Minneapolis. We evaluate the representativeness of that sample relative to the city as a whole in Section 5. The CoStar database contains all apartment buildings five units or larger finished after 2000 giving us a complete picture of new market-rate apartment construction in Minneapolis during our study period. In addition to quarterly average rent by bedroom count, the data includes building-level information such as address, number of units, year constructed, and the number of units by bedroom count.

We impose several restrictions on the data used for this analysis. We remove any buildings containing subsidized units from our sample to better capture rental prices that are truly market-rate and not affected by public subsidies. We exclude any buildings that were coded as "student" or "senior" apartments as these are plausibly distinct submarkets outside the scope of our analysis. The CoStar data include observations for larger units (e.g., three or more bedrooms), but the time-intensive nature of extracting and formatting the data necessitated that we narrow our focus to one- and two-bedroom units. Restricting our analysis to one- and two-bedroom units still captures 80.4 percent of the market-rate, non-student, non-senior units in the CoStar data. One and two-bedroom units also accounted for 73 percent of all rental units in Minneapolis, according to the 2000 census.

We partition the CoStar data into two analytic datasets; "older" buildings with build dates before Q1 2000 and "new construction" with build dates in 2000 or later. The new construction file identifies the market-rate new construction that we will use to identify treatment and comparison units in the older buildings data. Consistent with previous research, we restrict new construction to only unsubsidized apartment buildings with at least 50 units (Asquith et al., 2019). Eighty percent (72 of 90) of the buildings constructed in 2000 or later in our data meet this restriction.[1] We do not impose any building size restriction on the dataset of older buildings because we are interested in the effect of new construction on all building types. The panel of older buildings is, by construction, balanced – we observe 76 quarterly rental prices for all older buildings over the 19 years in our panel. The data provided by CoStar did not include any buildings that were demolished or otherwise exited the sample during our study period.

We construct our estimation sample by identifying older buildings that are proximal to new construction buildings. First, we calculate the straight-line distance between each building in the new construction and older building files and select only older buildings within 800 meters of at least one new building. An alternative conception of this step, illustrated in Figure 1, is to draw an 800 meter buffer around all new construction and identify older buildings within that band, noting their straight-line distance from the new construction address. We call each new construction building within 800 meters of an older building an "index building." Each index building can bring in multiple older buildings. To ensure our older buildings were not constructed just before our study period, we require that each index building be at least ten years younger than the older buildings it indexes. This restriction did not eliminate any older buildings from our estimation sample.

Next, we define the difference-in-differences treatment variables for the estimation sample using the timing of index building construction and the distance of the older building from the index building. In our primary specification, the treatment group includes older buildings within 300 meters of an index building. This choice of treatment distance is consistent with the literature that spatial spillover effects of the built environment on housing prices are contained to a relatively small radius (Diamond and McQuade, 2019; Schwartz et al., 2006). Comparison buildings are older buildings between 300 and 800 meters from an index building. We evaluate the implication of alternative treatment distances in Appendices C and F. Some older buildings fall in the 800 meter buffer of multiple new construction buildings. In these cases, we use the first observed new construction building as the index building. Note that we do allow a building to serve as a comparison building in early time periods and become a treatment building in later time periods. This measurement change can occur if an older building is, for example, 500 meters from a new

[1] Seventeen of the remaining new construction buildings do not have any CoStar-sampled older rental buildings within 800 meters meaning we have a total of 55 new construction buildings that we are able to use in our estimation sample.

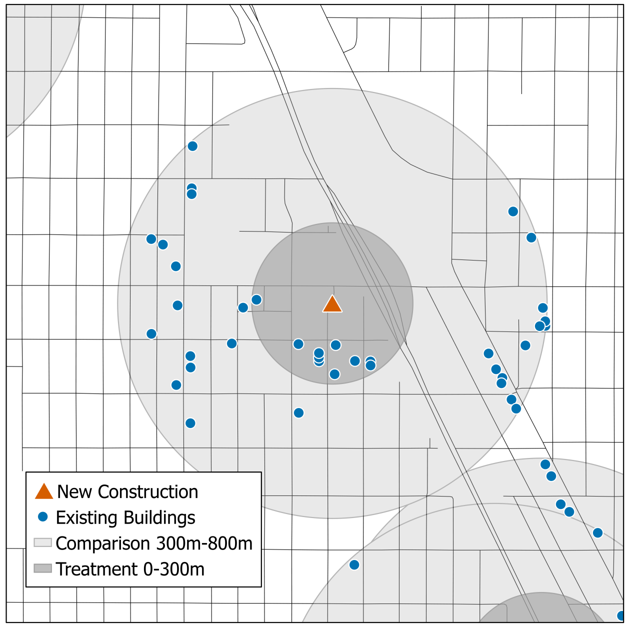


Figure 1: Treatment vs. Comparison Group Spatial Diagram

building constructed in 2005 but 150 meters from a different building constructed in 2010.

Our final data preparation step is to assign each older building to a market tier, a term we use interchangeably with "housing submarket." Consistent with the literature on submarkets, we use the building's position in the rent distribution to define which market tier the building's units are in (Nelson and Vandenbroucke, 1996; Somerville and Holmes, 2001; Susin, 2002). We first calculate building-level rent in 2000 by averaging each building's one- and two-bedroom rental prices over the four quarterly observations in 2000, weighted by the unit composition of the building. We then assign percentile ranks to all buildings and define distributional cut points to classify buildings into market tiers (Susin, 2002). We calculate these distributional statistics using the entire dataset of older buildings in 2000, not just those within the 800 meter buffer described above. This means that our final estimation sample does not have equal numbers of units in each submarket. Our preferred submarket specification splits the Minneapolis rental market into terciles, though we also repeat all analyses using an above/below median definition for high/low market tier (Appendix D).

4 Methods

Our empirical approach is motivated by a hypothesis that new market-rate construction affects the rental price of older buildings differently across market tiers. New, market-rate buildings serve as a plausible substitute for nearby renters in upper-tier apartments and we expect that expanding market-rate housing will reduce rents in the upper market tier. New construction in the top quartile of the housing market is not a realistic option for renters in lower-priced units. We expect that new market-rate construction is a positive demand shock for this submarket because of renters' desire to live near high-income households (Bayer et al., 2007; Guerrieri et al., 2013) and the amenity effect (Albouy and Hanson, 2014)).

Our research design uses proximity from new construction in a difference-in-differences with variable treatment timing framework to estimate the effect of new construction on the rental price of older housing stock. Specifically, we compare how the completion of new, market-rate construction affects the rent paths of buildings located very close to (within 300 meters) and close to (300-800 meters) new construction. We argue that the composition of our treatment and comparison groups help control for the targeted nature of new market-rate development. Units in the treatment group are in neighborhoods with high and rising rents relative to the rest of Minneapolis (see Appendix A) and it is important to select comparison units that are in similar neighborhoods. Rather than relying on administratively drawn neighborhood boundaries that could classify two extremely proximal buildings into different neighborhoods, we use straight-line distance between index buildings and older buildings to identify a sample of units that are plausible geographic substitutes for the index building.

For our approach to produce causal estimates, we must assume that the trends in rent between close and very close buildings would have been the same in the absence of new construction. Our estimates are causal to the extent that the comparison buildings form a valid counterfactual trend for the treatment buildings. We believe this is a plausible assumption for two main reasons.

First, by restricting our analysis to units within 800 meters of new construction, we are capturing comparison units that are in the same geographic submarkets as the treated units. Our restriction means that we only analyze units in or very near neighborhoods that developers have deemed profitable for new development. Though the choice of 800 meters is ultimately arbitrary, we selected this buffer to ensure that each unit in our sample could reasonably be considered in the same geographic submarket as the new construction. For context, 800 meters is a distance of less than a half-mile, and Google Maps reports that an adult of reasonable physical ability could walk from the index building to a comparison unit 800 meters away in about ten minutes.

The farthest a treatment and control unit with the same index building could be from each other is 1100 meters. This could occur if a treatment building is located 300 meters east of the index building, and the comparison building is located 800 meters west of the same index building. Though these two units are less proximal to each other, we argue that the distance is still small enough that they are can reasonably be considered in the same neighborhood. 1100 meters is a distance of about 0.7 miles and is less than the straight-line east-west or north-south size of most of Minneapolis' administratively-defined neighborhoods.

Second, the granularity of our data allows us to use building-bedroom fixed effects in all models so that the treatment effect is identified using the within-unit changes in rent rather than comparisons of rent across units. The fixed effects control for unobserved, time-invariant differences in building quality, amenities, and consumer preferences across buildings. This approach flexibly accounts for unobserved differences in quality that could bias naïve estimates. Even after controlling for differences in rent levels across buildings, we must still assume that treatment and comparison buildings would have parallel trends in rent in the absence of treatment. While this is untestable, we find no evidence that the rent trajectory of treated units differed from control units prior to new construction.

Finally, we believe that our DID design is a conservative approach to estimating the effect of new construction on rent in older units. It is highly plausible that the treatment effect of new construction extends beyond the 300 meters treatment distance we use in our primary specifications. If this is true, treatment "spills over" onto comparison units, and our effects will be attenuated toward zero. To the extent that new market-rate construction affects rents in the entire neighborhood, not just within 300m, our models allow us to identify the differential impact of being located very close to new construction. Understanding how new market-rate construction affects rents relative to similar areas without any new construction is certainly a research question of considerable academic and policy interest, but our setting and data are not amenable to producing unbiased estimates of this effect.

4.1 difference-in-differences Models

Our primary modeling equations are linear multi-period difference-in-differences equations (Bertrand et al., 2004; Meyer, 1995). Our first modeling approach estimates an effect that is pooled across quality submarkets.[2] Specifically, we estimate:

[2]Previous literature identified this effect as the Average Treatment Effect on the Treated (ATET). Recent work by Goodman-Bacon (2018) and others demonstrate that the effect identified in two-way fixed effects DID models is a weighted average of pairwise comparisons between treated and untreated units.

$$ln(rent_{it}) = \delta Treat_{it} + \alpha_i + \tau_t + \epsilon_{it} \quad (1)$$

Where i indexes building-bedroom combinations and t indexes time in quarters (e.g., Q1 2005). $ln(rent_{it})$ is the natural log of rent for building i in period t, $Treat_{it}$ is our treatment indicator that takes a value of one if building-bedroom i has been exposed to new construction within 300 meters at time t or earlier, and the α_i and τ_t terms are building-bedroom and time fixed effects, respectively. δ is the parameter of interest in this model and represents the difference in $ln(rent)$ in the post-period between the treatment and comparison groups.

To investigate whether the effects of new construction differ across market tiers, we estimate a version of Equation 1 that decomposes the aggregate effect captured in δ into separate effects for low, middle and high market tiers (the middle tier is the omitted category):

$$\begin{aligned} ln(rent_{it}) = \delta Treat_{it} + \delta_{high}(Treat_{it} * high.tier_i) + \delta_{low}(Treat_{it} * low.tier_i) + \\ \alpha_i + \tau_t + \epsilon_{it} \end{aligned} \quad (2)$$

$low.tier_i$ and $high.tier_i$ are indicators for the building-bedroom combination being in the first/third tercile of the 2000 rent distribution. Note that there is no intercept term for $low.tier_i$ and $high.tier_i$ because quality submarket is time-invariant and absorbed into α_i in implementation.

After using the first set of models to test our hypothesis that the effect of new construction on rent in older buildings differs across housing submarkets, we perform a similar DID analysis that tests whether the treatment effect differs across the combination of tier and distance from new construction. We model the distance effect using Equation 3 that includes categorical treatment variables for being within 0-200 meters, 201-300 meters, and 301-400 meters from new construction. The comparison group for this model is smaller than in Equations 1 and 2 and includes buildings 401-800 meters from new construction. This analysis serves two purposes. First, it allows us to evaluate how the treatment effect varies over distance. Second, it serves as a type of sensitivity test for the specification of our comparison group. Our empirical approach assumes that new construction affects buildings within 300 meters differently than buildings in the 300-800 meter distance band. If we observe significant treatment effects in the 300-400 meters distance group, it would be suggestive that new construction has differential spillover effects onto the comparison group. While a null finding for the 301-400 meters group does not address the concern that construction affects rents further than 300 meters away, it does provide evidence that these effects are constant throughout the comparison group and any bias this introduces should attenuate our effects toward zero. We implement this test using Equation 3:

$$\begin{aligned} ln(rent_{it}) = {} & \delta^{200} Treat^{200}_{it} + \delta^{200}_{high}(Treat^{200}_{it} * high.tier_i) + \delta^{200}_{low}(Treat^{200}_{it} * low.tier_i) + \\ & \delta^{300} Treat^{300}_{it} + \delta^{300}_{high}(Treat^{300}_{it} * high.tier_i) + \delta^{300}_{low}(Treat^{300}_{it} * low.tier_i) + \\ & \delta^{400} Treat^{400}_{it} + \delta^{400}_{high}(Treat^{400}_{it} * high.tier_i) + \delta^{400}_{low}(Treat^{400}_{it} * low.tier_i) + \\ & \alpha_i + \tau_t + \epsilon_{it} \end{aligned} \tag{3}$$

The superscripts on the δ terms refer to the mutually exclusive "treatment distance" categories. We specify Equation 3 such that the treatment effect for a building in a given distance band and market tier is the sum of the uninteracted, distance-specific δ and the corresponding tier-specific δ. For example, the treatment effect for low market tier buildings 301-400 meters away is $\delta^{400} + \delta^{400}_{low}$ and similarly for other distance/tier combinations. The distance model also uses unit and quarter fixed effects to flexibly account for time-invariant differences between units and common time trends in Minneapolis.

Our final analysis examines differences in the treatment effect over time. We use event study models to trace out the rent difference between treatment and comparison buildings, by submarket, over time relative to the completion of new construction. Equation four describes our submarket specific event study approach:

$$\begin{aligned} ln(rent_{it}) = \sum_{k=-20}^{20} \delta_k \{K_{it} = k\} + & \sum_{k=-20}^{20} \delta_{high,k}(\{K_{it} = k\} * high.tier_i) + \\ & \sum_{k=-20}^{20} \delta_{low,k}(\{K_{it} = k\} * low.tier_i) \end{aligned} \tag{4}$$

k indexes "event time," or quarters from the construction of the index building. $\{K_i t = k\}$ is the indicator function that creates a binary variable for each event time period, excluding the one quarter before new construction as the omitted category. Each δ_k coefficient represents the difference in rent between treatment and comparison units in event-quarter k across all tiers. $\delta_{low,k}$ is the differential effect of new construction in the low tier and $\delta_{low,k} + \delta_t$ is the treatment effect in event time k for the low tier and similarly for the high tier.

5 Results

5.1 Summary Statistics for the CoStar Sample

Table 1 shows summary statistics for older buildings broken out by whether the older buildings were "treated" (less than 300me from new construction) or "comparison" (between 300m and 800m from new construction). We calculate average effective rent[3]for the treatment and comparison buildings by averaging the unit-level quarterly rental observations for each building in 2000, weighted by the percentage of the building's units that are one/two-bedroom. The average effective rent in treated buildings is $805 compared to $702 in comparison buildings, a difference of $103 or about 14 percent of the average effective rent for the entire sample. Treated buildings contained, on average, 16 more units than comparison buildings but were of similar age. Vacancy rates are 3.5 percentage points higher in treated buildings compared to control buildings.

Table 1: CoStar Sample Characteristics by Treatment Status

	Treated	Comparison	New Construction
Effective Rent ($)	804.86	701.5	1,726.98
	(288.56)	(161.02)	(546.55)
Total Units	46.19	33.24	151.58
	(62.47)	(59.17)	(79.96)
Sample 1br Units (%)	71.42	72.63	64.75
	(32.49)	(30.07)	(16.19)
Vacancy Rate (%)	6.66	4.69	37.59
	(10.78)	(8.92)	(28.89)
Year Built	1937	1942	2012
	(26.61)	(26.11)	(3.81)
N Buildings	136	265	55
N Observations	15,631	31,031	2,680

Note: Standard deviations in parentheses. Baseline observations in the year 2000 for existing buildings and first year of observation for new construction. Effective rent is gross rent less any landlord concessions. Treated buildings are within 300m of new construction. Comparison buildings are between 300m-800m of new construction.

Though our analysis is restricted to older buildings, we find it useful to evaluate the characteristics of the index buildings that are serving as treatment in our models. Table 1 confirms that new market-rate construction occurs almost exclusively in the high market tier, priced well above the median rent of existing buildings. The average rent in new construction buildings in our sample

[3]"Effective rent" is defined as contract rent less any developer/landlord discounts.

was $1,727, which is 47 percent and 69 percent higher than existing treatment and comparison buildings, respectively. We calculate this average rent using the rent in the first year that the new building appears in the CoStar data. New construction buildings have more units than existing buildings (due, in part, to our inclusion criteria) and higher vacancy rates than either treated or comparison buildings.

As noted in Section 3, the estimation sample does not include equal numbers of first, second, and third tercile buildings because we calculate these market tiers using the entire Minneapolis sample. Table 2 reports the sample size and average rent at baseline for each market tier. Our final file includes 136 first tercile unique building-bedroom combinations in 99 buildings, 194 second tercile combinations in 133 buildings, and 276 third tercile combinations in 169 buildings. The distribution of building market tier within the 800 meter buffer of index buildings suggests that new construction was more likely to occur near older buildings with higher rents at baseline.

The average rent in the lowest market tier units was $553 in 2000 and grew 43 percent to $787 in 2018. Rent growth in the middle market tier was comparable to the lowest market tier – from $662 in 2000 to $924 in 2018 for a change of about 40 percent. The most expensive tercile of older buildings had an average rent of $903 in 2000 and grew 33 percent to $1,193 in 2018. It should be noted that these average rents are nominal and include a mixture of treatment and control units. We present the nominal change in rent over our study period to provide more context for the Minneapolis rental market and to contextualize the percent changes in rent reported from our primary models.

Table 2: CoStar Characteristics by Market Tier

Tier	N Buildings	N Bld-Br Combo.	Mean Rent 2000	Mean Rent 2018	Pct. Change 2000-2018
Low	99	136	552.5	786.8	43.1
			(58.8)	(155.6)	(30.2)
Middle	133	194	661.6	923.9	39.7
			(28.2)	(98.7)	(14.3)
High	169	276	903.3	1,192.5	32.9
			(241.5)	(300.1)	(15.1)
Total	401	606	736.6	1,003.3	37.7
			(218.2)	(275.3)	(20.1)

Note: Standard deviations in parentheses

The older buildings in the CoStar sample are representative of the rental housing market in Minneapolis. We compared rental prices in the CoStar sample to data on contract rent from the 2000 Census (see Appendix A1). The CoStar data over-samples the middle and upper parts of the rent distribution and under-samples units with rents less than $500 a month. This oversampling is in part because the CoStar sample is limited to market-rate units, while the census data includes both market-rate units and subsidized units. That being said, the CoStar sample reaches as low as the 25th percentile for all rentals in the Minneapolis rental distribution in 2000 according to the census, which we believe is low enough to adequately estimate the effects of new construction across multiple submarket tiers. the We also find that the units in the CoStar data are geographically representative of multi-family housing in the city of Minneapolis in 2000. Appendix Figure A2 plots the units in the CoStar sample (prior to our distance restrictions) alongside the locations of all multi-family parcels in the city. The CoStar units are more concentrated in the central city and the lack of sample north and northeast of downtown is largely due to our exclusion of student and subsidized housing from the sample.

5.2 Neighborhood Characteristics

Our research question is concerned with the effects of new development on individual buildings, but understanding differences in neighborhoods receiving new construction can help contextualize building-level effects. To place the CoStar sample into context, we identified the census tracts that received new construction in our sample. Using the Longitudinal Tract Database (LTDB), we created a census tract database using constant 2000 boundaries and data from the 2000 Census, 2007-2011 American Community Survey (ACS), and 2013-2017 ACS (Logan et al., 2014). These data allow us to evaluate the neighborhood characteristics of tracts receiving new construction by plotting the new construction buildings in the CoStar data onto a constant set of tract characteristics from the ACS.

New market-rate apartment construction with 50 or more units occurred in 21 of Minneapolis' 121 census tracts during our study period. Figure 2 shows the location of the new construction in the CoStar data that met the inclusion criteria to be considered an index building. The majority of tracts receiving new construction were in core urban areas in and adjacent to downtown Minneapolis. There was also new construction in the southern part of the city near light rail transit stations.

At the beginning of our study period in 2000, tracts with and without new construction had

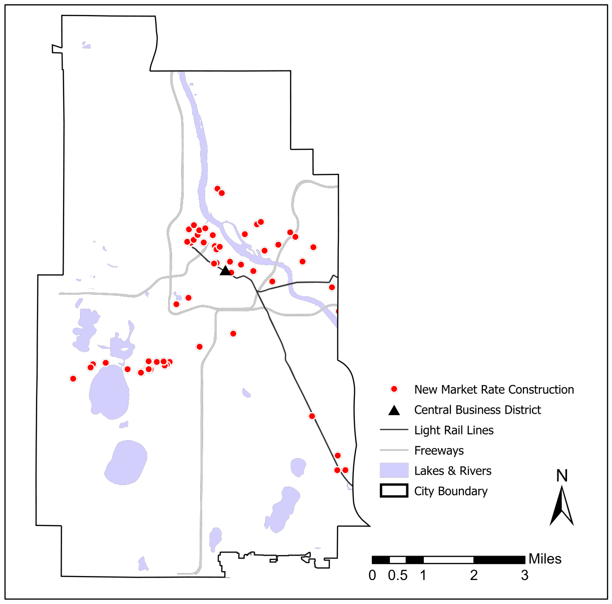


Figure 2: Market-Rate Apartment Buildings Over 50 Units Constructed in Minneapolis, MN 2000-2018

similar poverty rates, median household incomes, median rents, and vacancy rates. Construction was concentrated in tracts with higher median home values, higher percentages of residents with college degrees, and tracts that had more white and fewer black residents (see Table 3). Despite having higher home values, on average, tracts receiving new construction contained significant amounts of lower-tier rental housing, which could potentially be effected by new market-rate construction.

Appendix B reports the change in tract characteristics over our study period for tracts that do and do not contain new construction from the CoStar sample. From 2000-2018, rent rose twice as fast in areas with new construction (36 percent) compared to tracts without new construction (17 percent). Median household income grew 84 percent faster in tracts with new construction. While new construction tracts had a higher percentage of residents with college degrees in 2000 (44 percent vs 32 percent), this gap widened during our study period. Construction was concentrated in tracts with a higher percentage of white residents in 2000 and experienced little change in their racial composition from 2000-2018. Areas without new construction saw growth in the percentage of black, indigenous, and people of color.

Table 3: Census Tract Characteristics by New Construction Status

	With Construction	Without Construction	T-Stat	p
Pct. White	66.63	76.91	1.840	0.080
	(23.56)	(23.40)		
Pct. Black	13.47	10.09	-1.053	0.304
	(13.47)	(14.37)		
Pct. Bachelors	44.02	36.25	-1.551	0.137
	(21.36)	(17.47)		
Pct. Renter	64.61	32.11	-5.556	0.000
	(24.65)	(24.90)		
Pct. Poverty	21.56	9.11	-3.655	0.002
	(14.59)	(10.69)		
Median Value ($)	145,561.11	145,823.19	0.017	0.986
	(61,981.10)	(60,567.14)		
Median Rent ($)	587.33	663.59	2.445	0.023
	(123.65)	(184.04)		
Vacancy Rate (%)	4.69	2.79	-2.446	0.024
	(3.32)	(2.42)		
Pct. Built in Last 20 yrs	16.70	21.72	1.178	0.251
	(17.48)	(24.37)		
N	20	672		

Note: Standard deviations in parentheses

5.3 Difference-in-Differences Models

5.3.1 Pooled Models

We find that the effect of new, market-rate construction on older buildings varies by market tier. Table 3 presents results for the pooled DID model and two different implementations of models that present heterogeneous treatment effects by submarket. On average, older buildings within 300 meters of new construction experienced no change in rent relative to buildings 300 to 800 meters away (column 1). This average null effect, however, masks significant treatment effects on rents in the low market tier. Units in treated buildings with rents below the sample median in Q1 2000 had rents that were 4.0 percent higher than similar low market tier units in the comparison group. This four percent difference is equivalent to monthly rent that is $34 higher than comparable units in the post-period.

Table 4: Effects of New Construction by Housing Market Tier

	Pooled (1)	Median (2)	Terciles (3)
Pooled	0.0018		
	(0.0079)		
Low Tier		0.0402*	0.0671**
		(0.0169)	(0.0249)
Middle Tier			0.0119
			(0.0092)
High Tier		-0.0095	-0.0172
		(0.0083)	(0.0099)
Observations	46,662	46,662	46,662

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors in parentheses

Note: All models include building-bedroom and quarter fixed effects. Standard errors clustered at the building level.

These effects are even more apparent when market tiers are defined according to terciles of the 2000 rent distribution. Treatment units in the first tercile had rental prices that were 6.7 percent higher than comparison units. There was no significant effect on rents in the middle tercile. The most expensive tercile of the 2000 rent distribution experienced a small, but statistically insignificant rent decrease relative to comparison units of -1.7 percent.

5.3.2 Distance Models

The effect of new construction is strongest at very close distances and decays to zero as the distance from new market-rate buildings increases. Figure 3 presents the results from our distance- and submarket-differentiated DID model. We categorize all older buildings as 0-200 meters, 200-300 meters, 300-400 meters, or 400-800 meters from their index building. We interact this categorical distance variable with the buildings tercile in the 2000 rent distribution to produce distance-by-tier specific treatment parameters. The comparison group for this model is units 400 to 800 meters from new construction, and we conceive of the three closer distance groups as progressively weaker treatments.

The pattern of treatment effects in Figure 3 is consistent with the previous finding that new market-rate construction is associated with higher rents in the nearby low-rent market tier. We find that first tercile units within 200 meters of new construction had 14.2 percent higher rent relative to comparison units. This effect is significant but imprecisely measured, in part due to a limited number of first tercile buildings in this distance band. Lower-tier units 200 to 300 meters away from new construction had rental prices that were 5.0 percent higher than comparison buildings. There was no significant difference between the 300-400 meter distance band and the comparison group in the first tercile.

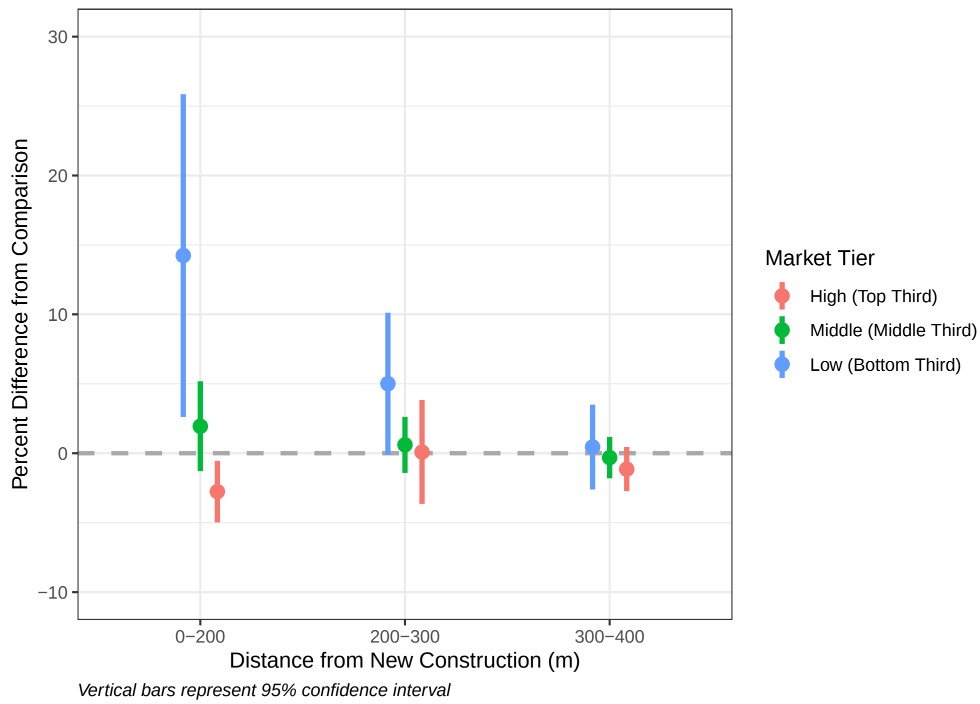


Figure 3: Effects of New Construction by Distance from New Construction and Market Tier

In the second tercile, the treatment effect was 1.9 percent for units within 200 meters, 0.1 percent between 200-300 meters, and -0.3 percent between 300-400 meters, but none of these parameters are statistically distinguishable from zero. The confidence intervals in Figure 2 suggest that even if new construction did affect middle tercile units, the effect is smaller in magnitude than in the first tercile. The distance gradient was less strong in the top tercile. The closest units had rents that were significantly lower than the comparison group by 2.7 percent. The effect in the middle band was a difference of 0.1 percent and the effect in the 300-400 meter band was 1.2

percent, but these estimates are also not distinguishable from zero.

5.3.3 Event Study Models

Figure 4 shows results for the event study analysis. By plotting the effect of new construction in event time (quarters relative to the completion of new construction), we can trace out the differential effect of treatment over time in the top and bottom rent terciles. This figure serves two analytic purposes. The coefficients on the x-axis to the left of zero (quarters prior to new construction) serve as a test of the parallel trends assumption discussed above. Second, it decomposes the pooled effects reported in Table 3 into year-specific coefficients to help us better understand the time path of the treatment effect. The assumption that our comparison group is

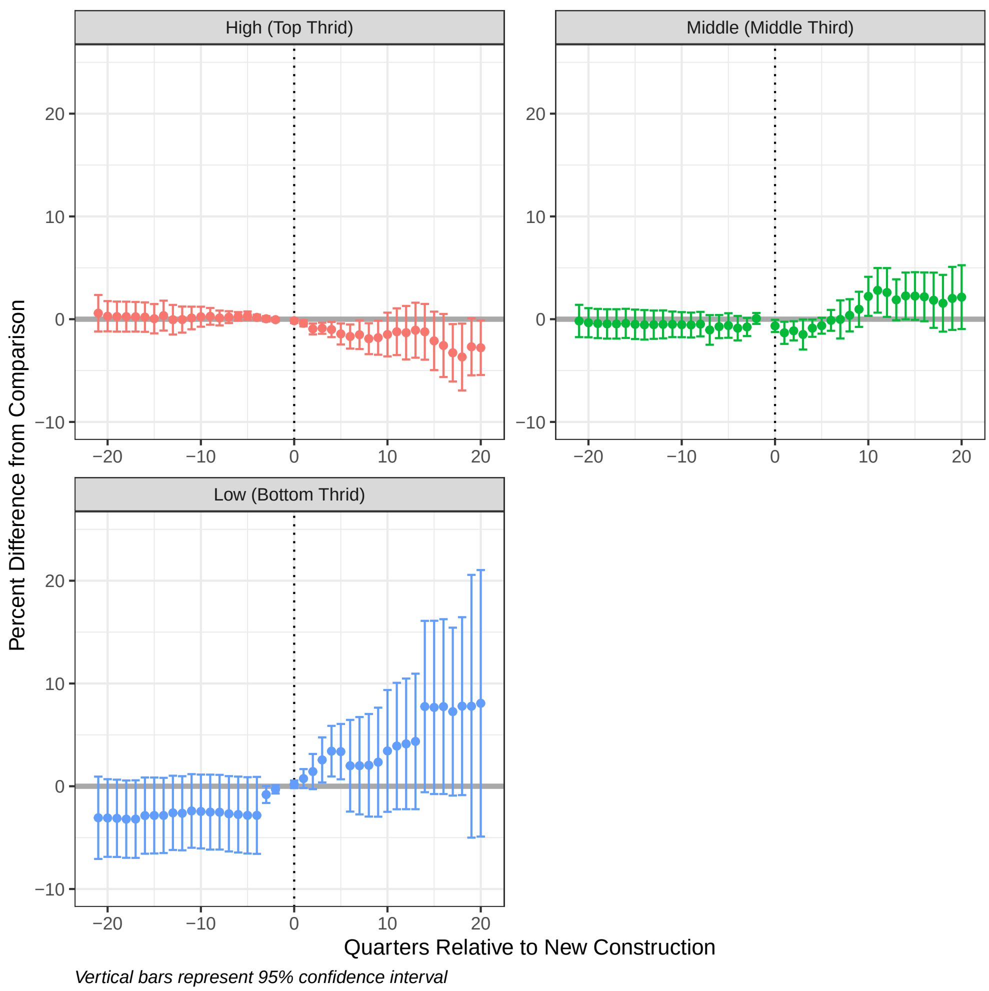


Figure 4: Event Studies of Effect of New Construction by Market Tier (Unbalanced Panel)

a valid counterfactual cannot be empirically proven, but the pre-period event study results do not show strong evidence to disprove the identifying assumptions. Figure 3 suggests that treated low tier buildings had lower rents than the comparison group by about two percent, even after accounting for time-invariant differences in buildings using the building-bedroom fixed effects. This difference was not significant but is constant across all pre-period quarters until one year prior to new construction. There was no significant difference between pre-period treatment and control units in the middle and top terciles.

We observe a sharp increase in rent in the low tier (first tercile) treated units in the four quarters prior to the availability of units in new, market-rate buildings. This could be an anticipatory effect – the new building is presumably under construction during this period because $t = 0$ represents the first quarter of unit-average rents for the new building in the CoStar data. It is plausible that landlords in nearby buildings react to the initiation of construction rather than the availability of new units as a signal to raise rents. A more worrying explanation for the change in trend at $t = -4$ would be that developers are locating buildings in areas with rapidly rising rents. We find this implausible because the site selection, acquisition, financing, permitting, and construction process for new market-rate buildings is longer than one year (Enterprise Foundation, 1999). If developers were to respond to differentially increasing demand in the low market tier, we would expect to see the change in rent occur earlier in the event study. The post-period (right of zero) coefficients in Figure 4 show that the effects of new construction occur within two quarters of the completion of new market-rate buildings and persist for the entire post-period. Six months after treatment, high-tier treatment buildings had rents that were significantly lower (p=.0001) than comparison buildings by about 0.9 percent. The low tier submarket had rents 1.4 percent higher than the comparison buildings (p=.103). After one year, rents in high-tier treated units were 1.0 percent lower (p=.008) than comparison units. In the low market tier, rents were 3.4 percent higher (p=.007) than comparison buildings. By year two (t=8), the effect in the first tercile was 2.0 percent (p=.423) and -1.9 percent (p=0.012) in the third tercile. We observe large but insignificant effects in the low-tier treated units beyond year two.

5.3.4 Robustness

The results in Figure 4 are somewhat challenging to interpret in practical terms. The sample for this analysis is "unbalanced" in that the composition of the treatment group is changing through the post-period due to treatment units having different numbers of post-period observations. For example, older buildings with index buildings built in 2016 are only in the post-period sample

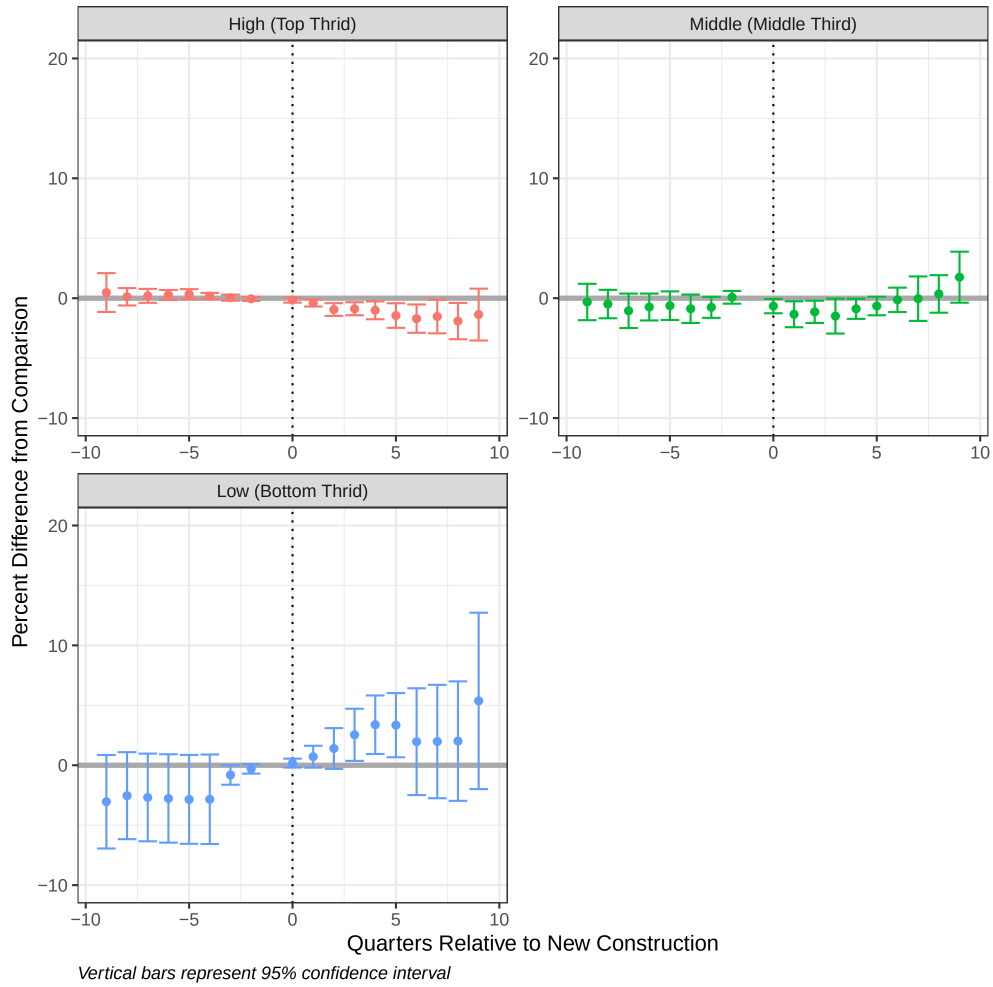


Figure 5: Event Studies of Effect of New Construction by Market Tier (Balanced Panel)

until $t = 8$ on the x-axis. A large proportion of new construction that effects lower-tier buildings occurred in 2016 or later, meaning that we lack large sample sizes for the post-period beyond eight quarters. To address this, we repeated the analysis in Figure 4 but included coefficients for eight quarters (two years) before and after treatment. This reduces the time scope of our estimates but ensures that the treatment group composition remains constant throughout the entire event study. The results for this analysis are reported in Figure 5. The balanced panel confirms the market tier findings in the unbalanced event study. The pre-treatment coefficients are not significantly different from zero in any market tier, but the lowest tier also exhibits increasing rent relative to the comparison group in the four quarters before new construction. The post-construction coefficients in the lowest market tier grew from 0.7 percent in the first quarter after new construction to a 3.3 percent in the fourth (p=0.007) and fifth (p=0.016) quarters after treatment. The findings from the middle and high market tiers are also similar in the balanced panel analysis.

Our choice of a 300 meter treatment distance, a 301 to 800 meter comparison distance, and tercile definition of market tier are motivated by the recent work reviewed in Section 2 and our evaluation of the trade-off between precision in our models and the degree of potential selection in the treatment and comparison groups. We evaluate the sensitivity of our results to alternative specifications in Appendices C and D.

In Appendix C, we re-create the pooled and event study analysis using an alternative treatment definition of being within 200 meters of new construction compared to units 201 to 800 meters from new construction. Our findings using this specification confirm the distance gradient observed in the distance analysis. In our pooled models, treated units in the low market tier have rents 12.5 percent higher in the post-period (p=0.027) relative to the comparison group. The effects for the middle and high market tier are 1.9 percent (p=0.245) and -2.8 percent (p=0.13). We conduct a similar set of robustness tests for our event study models. Appendix C Figure C.1 reports event study plots for all the balanced and unbalanced panel of units under 200 meter treatment distance specifications and using baseline rent terciles.

In Appendix D, we perform event study models but adjust our submarket definition to two groups (above or below the median). The time-specific treatment effects vary in size and significance across these different specifications, but all versions of the event studies suggest that low market tier units near new construction experience post-period rent increases of three to ten percent relative to the comparison group. Most specifications also suggest a negative effect of between two and six percent in the high market tier and no significant difference in rent in the middle market tier.

The comparison group for our primary specification contains more high- and middle-tier units than low-tier units (see Table 2). In Appendices E and F, we evaluate the implications of this imbalance by conducting pooled and event study subgroup analyses that compare rent outcomes between treated units and comparison units in the same market tier. Dividing our sample in this fashion reduces the precision of our estimates considerably but the treatment effects from these portioned models are similar in direction and magnitude to the combined analysis.

6 Discussion & Conclusion

This study provides new evidence on how new, market-rate apartment construction affects rental prices in nearby areas. Many current policy proposals to improve housing affordability focus on expanding the supply of market-rate housing to promote slower price growth throughout the rent price distribution. Our findings provide credible estimates of how adding housing stock to the most expensive part of the housing market affects rents in the same neighborhood and how these effects differ across housing submarkets. A key takeaway from our study is that the effects of new construction vary meaningfully across housing submarkets. We find that rents in lower-tier rental units close to new market-rate development were about 6.7 percent higher than comparison units following the completion of new buildings. High-tier housing close to new construction had rents that were about 1.7 percent lower than comparison buildings. We found that the effects on new buildings in both high and low market tiers were concentrated within 400 meters from the new building when using a comparison group of other nearby apartments. Our event study results suggest that the effect of construction is observable in the year prior to the new building accepting tenants and persist for at least two years post-completion. Our estimates grow imprecise after two years, but we cannot rule out the possibility that the effects last at least five years after new construction.

The findings from this analysis are important for understanding how housing markets function and also provide further support for recent empirical developments in the housing literature. As we describe in Section 2, there is theoretical support for the idea that new market rate construction could increase or decrease nearby rents. We interpret our submarket differentiated effects as supporting both the supply effect and amenity effect hypotheses. For existing buildings in the same market tier as new construction, the new buildings serve as plausible substitutes for renters and inject more price competition into the neighborhood. This results in slower rent growth in the upper market tier in the immediate vicinity of the new building, because these are the units for which the new construction is the best geographic and price substitute.

The higher rent growth we observe in low market tier buildings close to new construction can be interpreted as a kind of amenity effect. It is plausible that new market-rate apartments serve as signal to landlords that demand for their units may be increasing or higher than before and property owners respond to this signal by increasing rent more rapidly. We should note that without data on neighborhood amenities like restaurants, shopping, or transit, we are unable to identify whether the new buildings precede amenities or visa versa. Evidence from Li (2019) and Singh (2020) suggest that new construction does produce new amenities; however, their results differ as to whether and how the new amenities affect rents. Our analysis does find that the price effect is strongest closer to new apartment construction which suggests that new apartment buildings may be driving some of the effect. It is also not implausible that new construction has some supply effect on lower-tier apartments, but expensive housing is a poor substitute for affordable housing, so we expect the supply effect to be much weaker in the low market tier. Our treatment effects indicate that, to the extent that there is a price effect, is swamped by the amenity (or amenity-like) effect discussed here.

This study adds to the burgeoning literature of difference-in-differences studies interested in better understanding of how new construction affects rental housing prices. We use a novel dataset that had previously been unavailable to researchers and we feel that high-quality data on Minneapolis, in particular, will be important as housing researchers investigate the effects of the recent zoning policy changes in the Minneapolis 2040 comprehensive plan. Our analysis suggests that data collected by CoStar are of high quality and representative of the Minneapolis rental market and that these data could be useful for investigating a range of empirical questions. Our neighborhood-level analysis shows that in Minneapolis, much of the new construction has taken place in neighborhoods with significant amounts of low-cost rental housing which could see higher rents as new large apartments are completed.

In addition to using a novel dataset, we contribute to the literature by combining the distance-based DID design used by Asquith et al. (2019) with submarket specifications in the spirit of those employed by Li (2019). A crucial difference between our implementation of submarkets and the method used by Li is whether the market tiers are defined at the beginning or end of the study period. The two approaches have substantial trade-offs. Our approach risks including mean reversion or other differential, secular time trends across the rent distribution as part of the treatment effect. Our subgroup analysis that restricts treatment and comparison units to be of the same market tier suggest that the entire treatment effect is unlikely to be mean reversion, but the smaller positive treatment effects in the low market tier lend support to the concern that rents may have increased faster in these units even in the absence of new construction. On the other hand, defining submarkets at the end of the study period could obfuscate differential treatment effects if new construction causes re-ordering of units in the rent distribution and causes some units to move across market tier boundaries throughout the study period. Further refinement of submarket analysis in the empirical literature is an important next step toward better understanding the local effects of new housing.

In addition to ambiguity around the proper way to operationalize submarkets, there are several other important limitations to our study. First, it is of only one city, and the dynamics of housing markets in Minneapolis may differ significantly from other cities. Additionally, the sample of buildings available to use means that we are unable to accurately measure the effects of new construction past five years. It is also possible that, with enough new supply in a limited area, the amenity effect diminishes over time as the market for high-end consumption becomes saturated and the supply effect predominates. We believe that our study is a crucial first step, but that much more research in this area is required.

Research shows that new housing supply at all affordability levels is an important step toward ensuring housing is affordable to urban residents (Rosenthal, 2014). This belief does not contradict the possibility that new, expensive housing development can produce rent increases or other undesirable outcomes in the short- and medium-term. We feel the most important conclusion from our study is that future research on the effects of new construction needs to meaningfully engage with the idea that supply effects likely differ across quality submarkets, and these differences should be formally incorporated into future work. Understanding how new market-rate construction affects the housing stock available to low-income and marginalized urban residents ought to be a central goal of future housing policy analysis and research.

References

Albouy, D. and Hanson, A. (2014). Are Houses Too Big or In the Wrong Place? Tax Benefits to Housing and Inefficiencies in Location and Consumption. *Tax Policy and the Economy*, 28(1):63–96.

Anenberg, E. and Kung, E. (2018). Can More Housing Supply Solve the Affordability Crisis? Evidence from a Neighborhood Choice Model. *Finance and Economics Discussion Series*, 2018(035).

Angotti, T. and Morse, S., editors (2017). *Zoned Out! Race, Displacement, and City Planning in New York City*, volume 7 of *Urban Research*. Terriform.

Asquith, B., Mast, E., and Reed, D. (2019). Supply Shock Versus Demand Shock: The Local Effects of New Housing in Low-Income Areas. *Upjohn Institute Working Papers*.

Aurand, A., Emmanuel, D., Errico, E., Pinsky, D., and Yentel, D. (2019). The Gap: A Shortage of Affordable Homes. Technical report, National Low Income Housing Coalition.

Baum-Snow, N. and Hartley, D. (2017). Accounting for Central Neighborhood Change, 1980-2010;. Technical report, Federal Reserve Bank of Chicago.

Bayer, P., Ferreira, F., and McMillan, R. (2007). A Unified Framework for Measuring Preferences for Schools and Neighborhoods. *Journal of Political Economy*, 115(4):588–638.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly journal of economics*, 119(1):249–275.

Burge, G. S. (2011). Do Tenants Capture the Benefits from the Low-Income Housing Tax Credit Program?: Do Tenants Capture the Benefits from the Low-Income Housing Tax Credit Program? *Real Estate Economics*, 39(1):71–96.

Colburn, G. and Allen, R. (2018). Rent burden and the Great Recession in the USA. *Urban Studies*, 55(1):226–243.

Couture, V., Gaubert, C., Handbury, J., and Hurst, E. (2018). Income Growth and the Distributional Effects of Urban Spatial Sorting. *Working Paper*.

Diamond, R. and McQuade, T. (2019). Who Wants Affordable Housing in Their Backyard? An Equilibrium Analysis of Low-Income Property Development. *Journal of Political Economy*, 127(3):55.

Galster, G. (1996). William Grigsby and the Analysis of Housing Sub-markets and Filtering. *Urban Studies*, 33(10):1797–1805.

Galster, G. and Rothenberg, J. (1991). Filtering in Urban Housing: A Graphical Analysis of a Quality-Segmented Market. *Journal of Planning Education and Research*, 11(1):37–50.

Glaeser, E. L., Gyourko, J., and Saks, R. (2005). Why Is Manhattan So Expensive? Regulation and the Rise in Housing Prices. *The Journal of Law and Economics*, 48(2):331–369.

Goodman-Bacon, A. (2018). Difference-in-Differences with Variation in Treatment Timing. Working Paper 25018, National Bureau of Economic Research. Series: Working Paper Series.

Gould, E. (2020). State of Working America Wages 2019: A story of slow, uneven, and unequal wage growth over the last 40 years.

Grigsby, W. G. (1963). *Housing Markets and Public Policy*. University of Pennsylvania Press.

Guerrieri, V., Hartley, D., and Hurst, E. (2013). Endogenous gentrification and housing price dynamics. *Journal of Public Economics*, 100:45–60.

Jacobus, R. (2016). Why We Must Build.

Joint Center for Housing Studies of Harvard University (2019). The State of the Nation's Housing 2019. Technical report, Harvard University.

Kain, J. F. and Quigley, J. M. (1975). *Housing Markets and Racial Discrimination: A Microeconomic Analysis*. Number no. 3 in Urban and Regional Studies. National Bureau of Economic Research : distributed by Columbia University Press, New York. OCLC: 1502326.

Li, X. (2019). Do New Housing Units in Your Backyard Raise Your Rents? *Working Paper*, page 58.

Logan, J. R., Xu, Z., and Stults, B. (2014). Interpolating U.S. Decennial Census Tract Data from as Early as 1970 to 2010: A Longtitudinal Tract Database. *The Professional geographer : the journal of the Association of American Geographers*, 66(3):412–420.

Manville, M., Monkkonen, P., and Lens, M. (2020). It's Time to End Single-Family Zoning. *Journal of the American Planning Association*, 86(1):106–112.

Meyer, B. D. (1995). Natural and Quasi-Experiments in Economics. *Journal of Business & Economic Statistics*, 13(2):151–161.

Mogush, P. and Worthington, H. (2020). The View From Minneapolis: Comments on "Death to Single-Family Zoning" and "It's Time to End Single-Family Zoning". *Journal of the American Planning Association*, 86(1):120–120.

Nelson, K. P. and Vandenbroucke, D. A. (1996). Affordable Rental Housing: Lost, Stolen, or Strayed. *US Department of Housing and Urban Development Office of Policy Development and Research. Washington, DC. Mimeographed*.

Ooi, J. T. and Le, T. T. (2013). The spillover effects of infill developments on local housing prices. *Regional Science and Urban Economics*, 43(6):850–861.

Piazzesi, M., Schneider, M., and Stroebel, J. (2019). Segmented Housing Search. *NBER Working Paper*, page 86.

Rodriguez, J. F. (2018). SB 827 rallies end with YIMBYs shouting down protesters of color. *The San Francisco Examiner*.

Rosenthal, S. S. (2014). Are Private Markets and Filtering a Viable Source of Low-Income Housing? Estimates from a "Repeat Income" Model. *American Economic Review*, 104(2):687–706.

Saks, R. E. (2008). Job creation and housing construction: Constraints on metropolitan area employment growth. *Journal of Urban Economics*, 64(1):178–195.

Schwartz, A. E., Ellen, I. G., Voicu, I., and Schill, M. H. (2006). The external effects of place-based subsidized housing. *Regional Science and Urban Economics*, 36(6):679–707.

Singh, D. (2020). Do Property Tax Incentives for New Construction Spur Gentrification? Evidence from New York City. *Working Paper*, page 109.

Somerville, C. T. and Holmes, C. (2001). Dynamics of the Affordable Housing Stock: Microdata Analysis of Filtering. *Journal of Housing Research*, 12(1):27.

Susin, S. (2002). Rent vouchers and the price of low-income housing. *Journal of Public Economics*, 83(1):109–152.

Watkins, C. A. (2001). The Definition and Identification of Housing Submarkets. *Environment and Planning A: Economy and Space*, 33(12):2235–2253.

Weicher, J. C. and Thibodeau, T. G. (1988). Filtering and housing markets: An empirical analysis. *Journal of Urban Economics*, 23(1):21–40.

Weiss, E. and Brown, N. (2017). A Place to Call Home. Technical report, Campaign for Housing and Community Development Funding (CHCDF), Washington D.C.

Zuk, M. and Chapple, K. (2016). Housing Production, Filtering and Displacement: Untangling the Relationships. Research Brief, Institute of Governmental Studies, University of California Berkeley.

A Costar Sample Characteristics

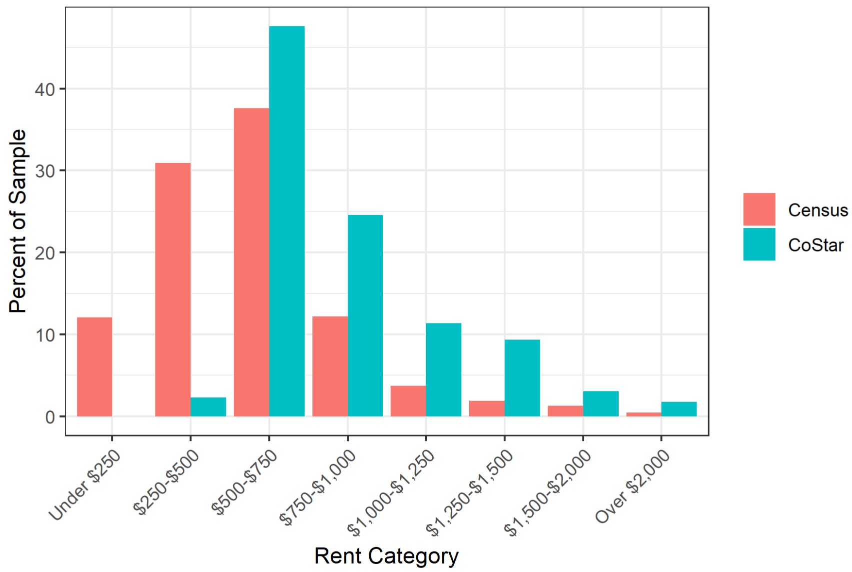


Figure A.1: Distribution of Rents in CoStar Sample Compared to 2000 Census Minneapolis, MN

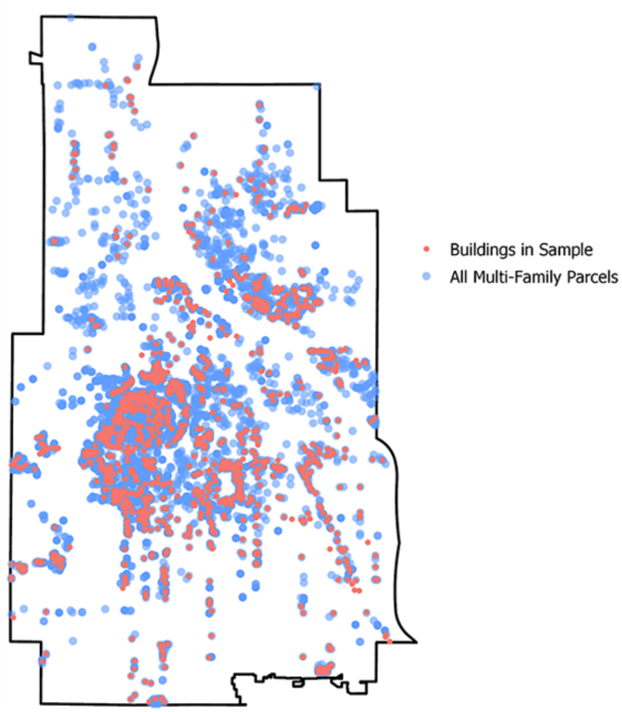
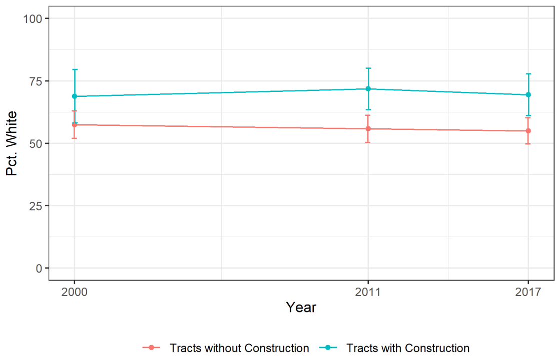
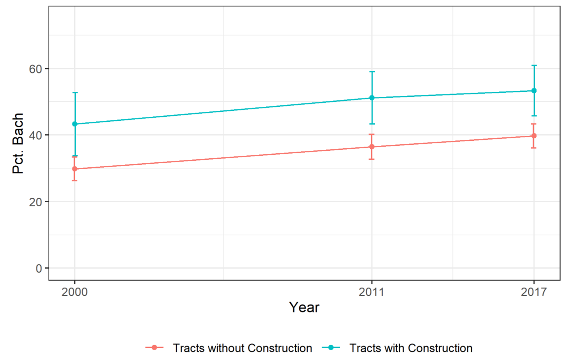


Figure A.2: Spatial Comparison of CoStar Sample Buildings and Minneapolis Mulit-Family Parcels

B Neighborhood Trajectories

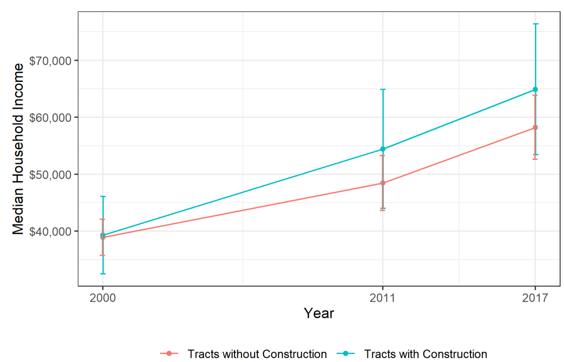


(a) Pct. White

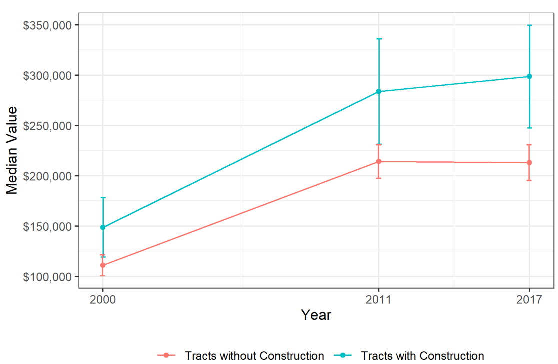


(b) Pct. Bachelors

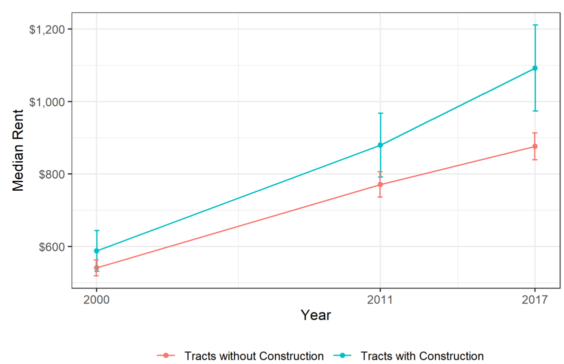
(c) Pct. Poverty



(d) Median Household Income



(e) Median Homve Value



(f) Median Rent

Figure B.1: Neighborhood Trajectories

Note: Data from from the 2000 Census, 2007-2011 American Community Survey (ACS), and 2013-2017 ACS. Census data normalized to 2000 boundaries. Dots represent the average value of census tracts that received new construction compared to those that did not during the study period. Vertical lines represent 95 percent confidence intervals

C Results Using 200m Treatment Distance

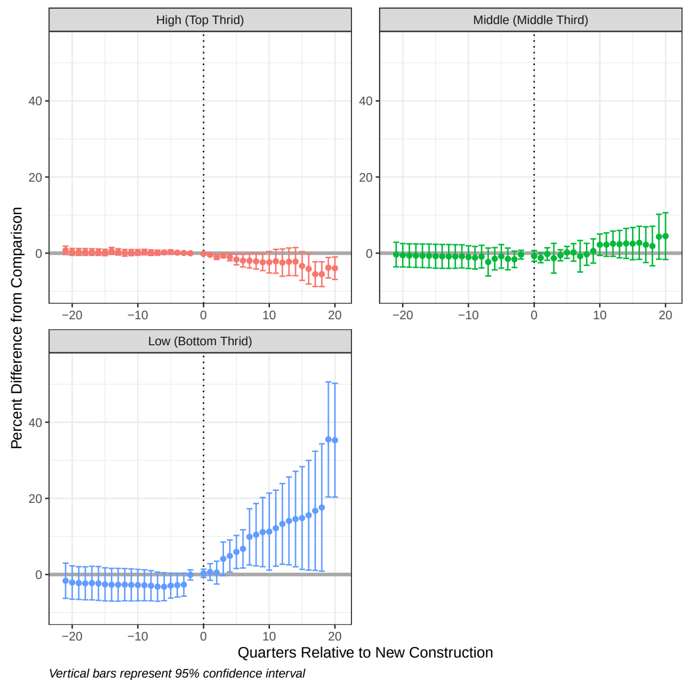
Table C.1: Effects of New Construction by Housing Market Tier - 200m Treatment Distance (replace t with se)

	Pooled (1)	Median (2)	Terciles (3)
Pooled	-0.0110 (0.22)		
Low Tier		0.0664 (2.37)	0.125* (2.69)
Middle Tier			0.0186 (1.30)
High Tier		-0.0200 (-1.14)	-0.0278* (-1.74)
Observations	46,662	46,662	46,662

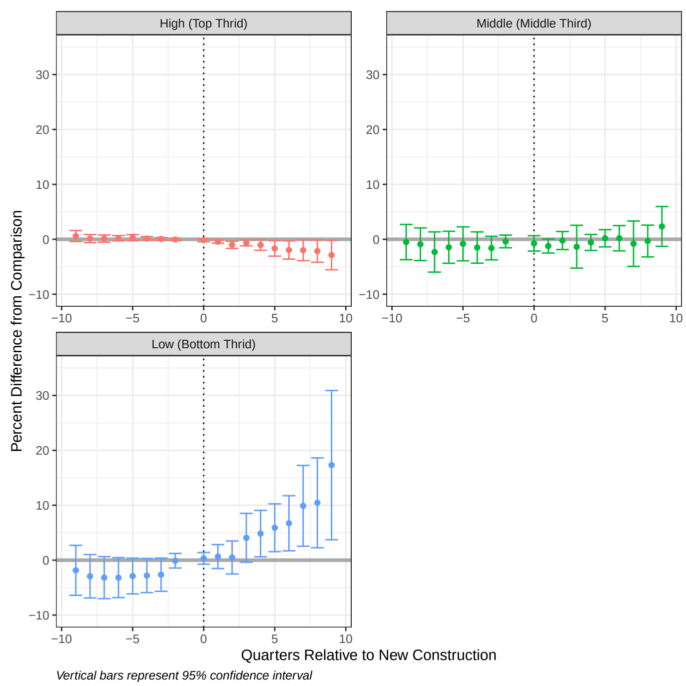
t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: All models include building-bedroom and quarter fixed effects. Standard errors clustered at the building level.



(a) Unbalanced



(b) Balanced

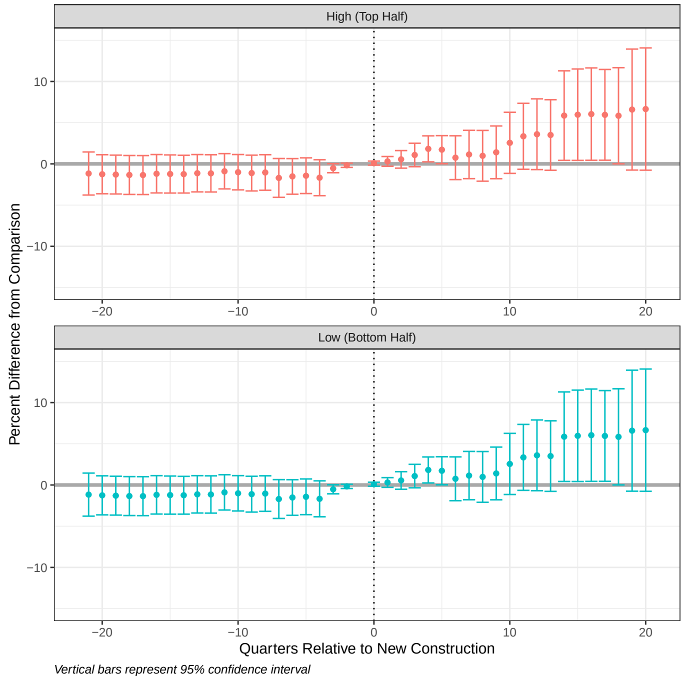
Figure C.1: Event Studies of Effect of New Construction by Market Tier - 200m Treatment Distance

D Alternative Submarket Specification - Above/Below Median

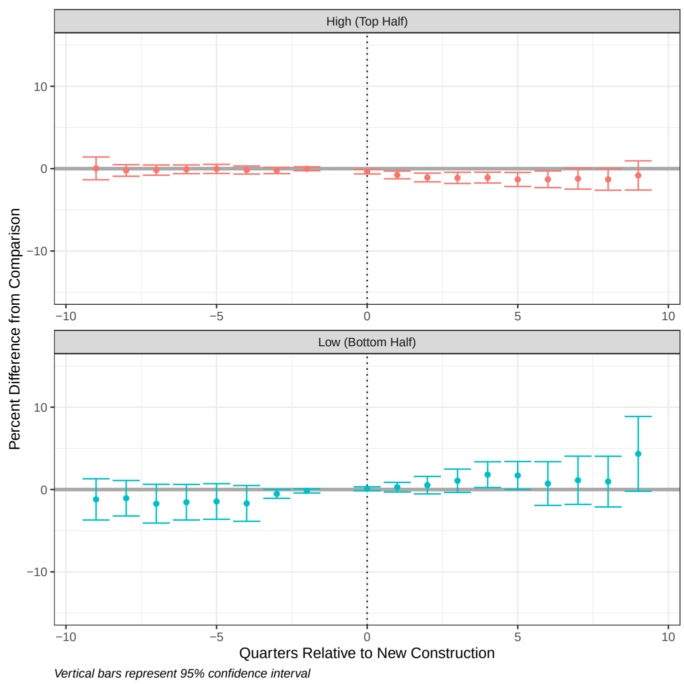
Table D.1: CoStar Characteristics by Market Tier - (Above/Below Median)

Quality	N Buildings	Bld-Br Combo.	Mean Rent 2000	Mean Rent 2018	Pct. Change 2000-2018
Low	164	227	585.8	829.2	42.0
			(62.0)	(145.9)	(25.5)
High	237	379	840.9	1,123.7	34.6
			(226.6)	(279.5)	(14.7)
Total	401	606	736.6	1,003.3	37.7
			(218.2)	(275.3)	(20.1)

Note: Standard deviations in parentheses

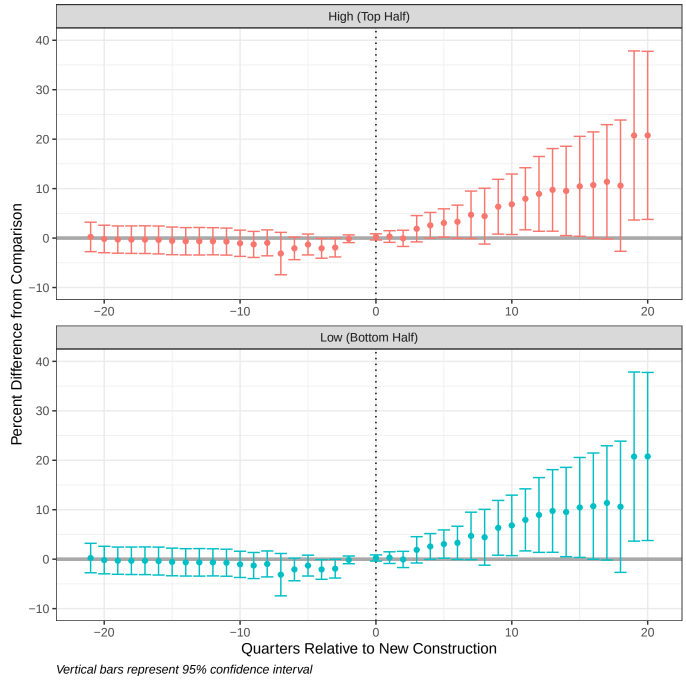


(a) Unbalanced

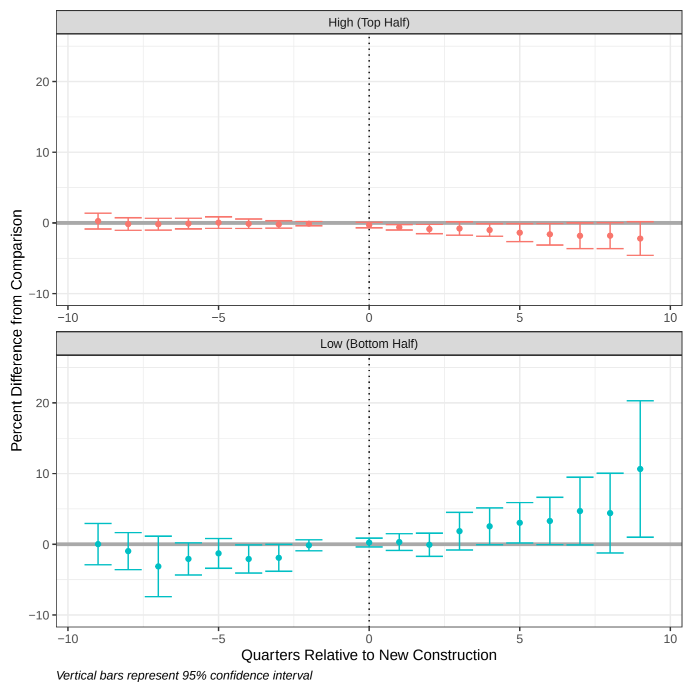


(b) Balanced

Figure D.1: Event Studies of Effect of New Construction by Market Tier - 300m Treatment Distance



(a) Unbalanced



(b) Balanced

Figure D.2: Event Studies of Effect of New Construction by Market Tier - 200m Treatment Distance

E Partitioned Submarket Models 300m Treatment Distance

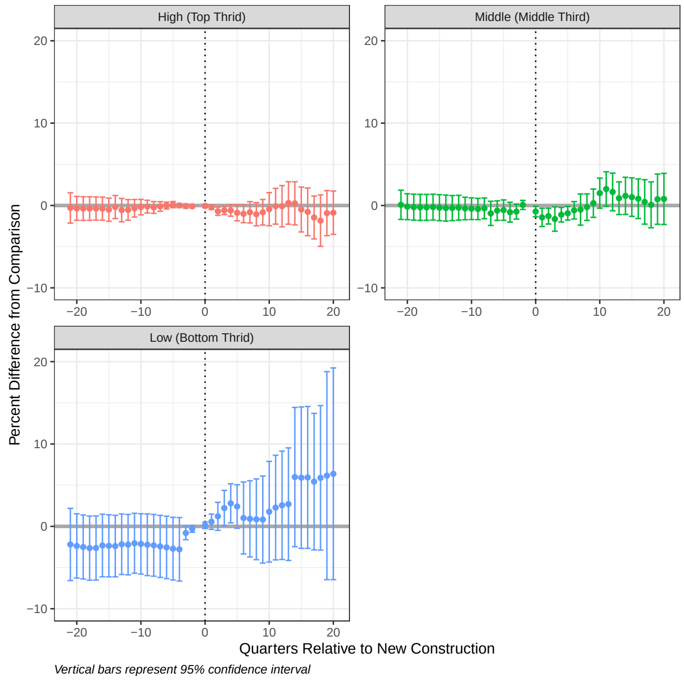
Table E.1: Effects of New Construction Partitioned Model 300m Treatment Distance

	Pooled	Median Tiers		Tercile Tiers		
		Low	High	Low	Mid	High
Treatment Effect	0.0018	0.0207	0.0031	0.0477	0.0013	0.0018
	(0.0079)	(0.0177)	(0.0078)	(0.0265)	(0.0094)	(0.0092)
Observations	46,662	17,479	29,183	10,472	14,938	21,252
Unique buildings	401	164	237	99	133	169

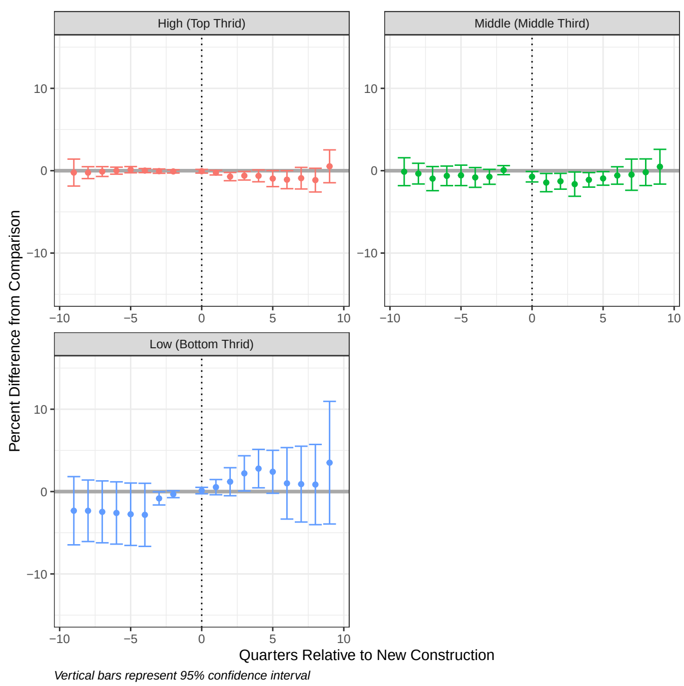
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Robust standard errors in parentheses. All models include building-bedroom and quarter fixed effects. Data is first partitioned by market tier and each model run separately (E.g. Lower-tier buildings treated buildings are only compared with lower-tier comparison buildings).

E.1 Tercile Submarket Event Studies



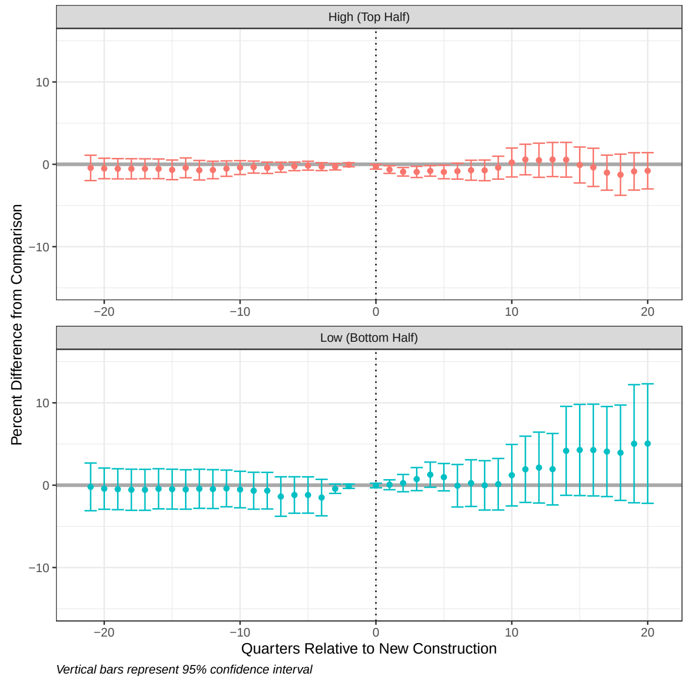
(a) Unbalanced



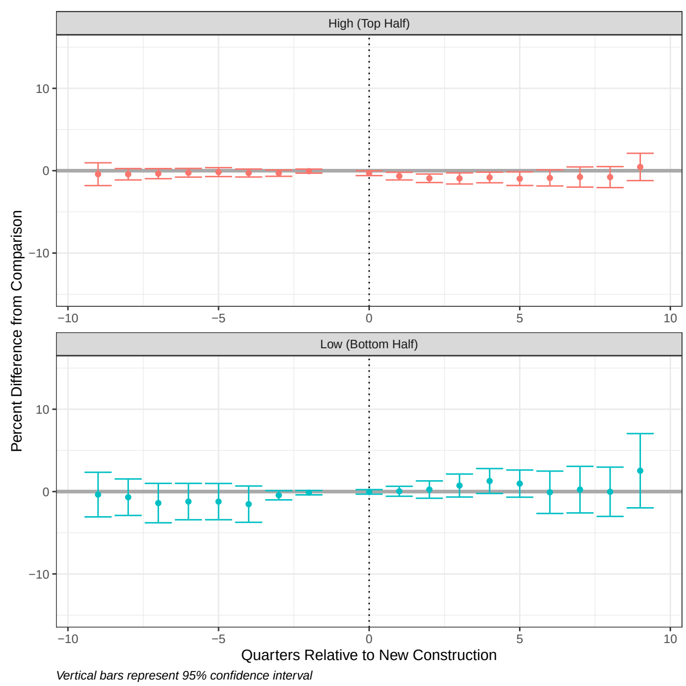
(b) Balanced

Figure E.1: Event Studies of Effect of New Construction by Market Tier - 300m Treatment Distance Partitioned

E.2 Median Submarket Event Studies



(a) Unbalanced



(b) Balanced

Figure E.2: Event Studies of Effect of New Construction by Market Tier - 300m Treatment Distance Partitioned

F Partitioned Submarket Models 200m Treatment Distance

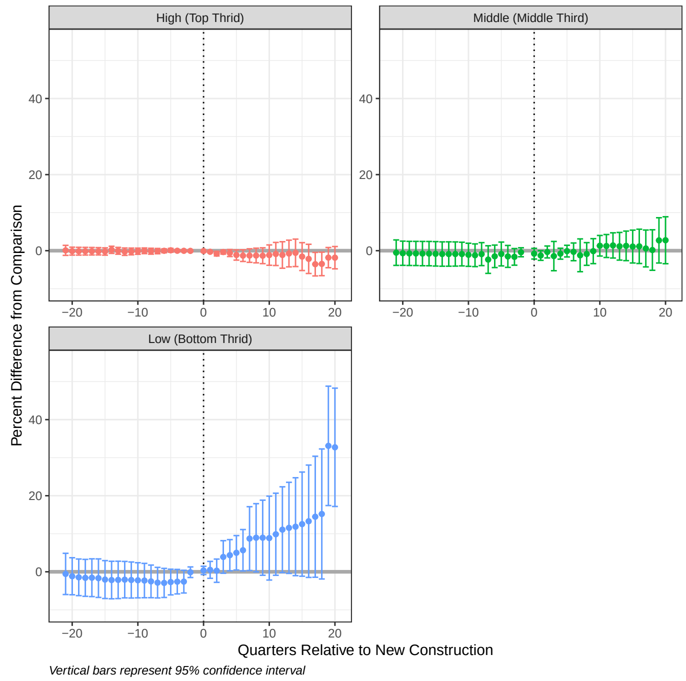
Table F.1: Effects of New Construction Partitioned Model 200m Treatment Distance

	Pooled	Median Tiers		Tercile Tiers		
		Low	High	Low	Mid	High
Treatment Effect	-0.0110	0.0449	-0.0075	0.1020	0.0095	-0.0089
	(0.0108)	(0.0410)	(0.0101)	(0.0584)	(0.0164)	(0.0111)
Observations	46,662	17,479	29,183	10,472	14,938	21,252
Unique buildings	401	164	237	99	133	169

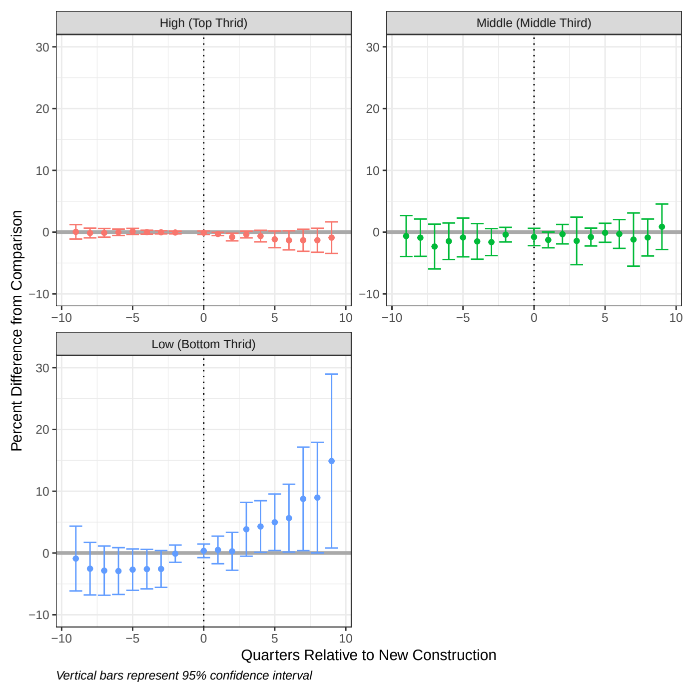
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Robust standard errors in parentheses. All models include building-bedroom and quarter fixed effects. Data is first partitioned by market tier and each model run separately (E.g. Lower-tier buildings treated buildings are only compared with lower-tier comparison buildings)

F.1 Tercile Submarket Event Studies



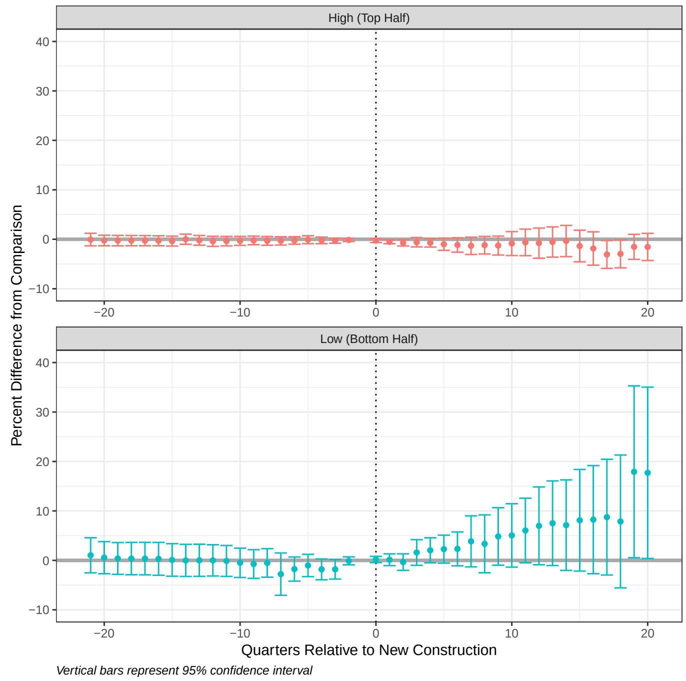
(a) Unbalanced



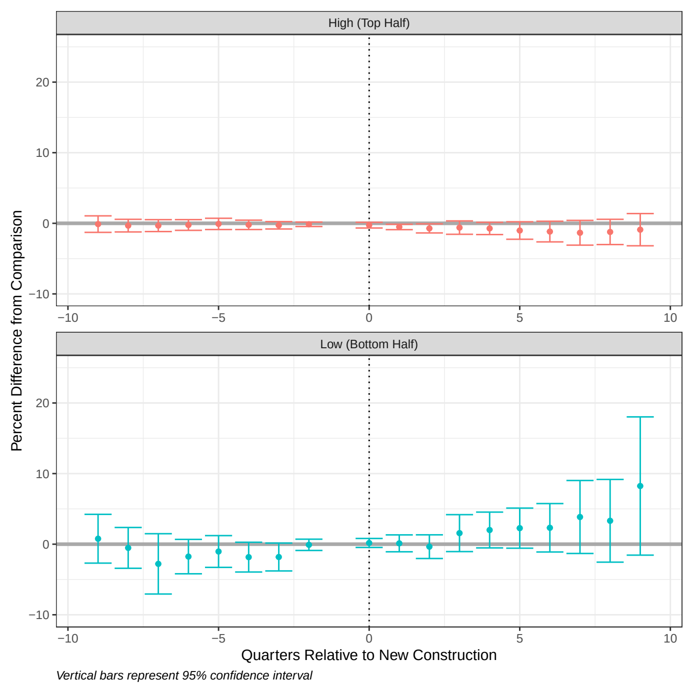
(b) Balanced

Figure F.1: Event Studies of Effect of New Construction by Market Tier - 200m Treatment Distance Partitioned

F.2 Median Submarket Event Studies



(a) Unbalanced



(b) Balanced

Figure F.2: Event Studies of Effect of New Construction by Market Tier - 200m Treatment Distance Partitioned