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

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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 soon after a new market-rate building is completed.

We use a 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. While we find no effect in the single market model, our submarket approach suggests that lower-priced rental housing close to new construction had 6.6 percent higher rents compared to the comparison group. New construction had the opposite effect on higher-priced housing; rents were 3.2 percent lower close to new construction.

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

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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; Badger, 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 (**?**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 6.6 percent in the lowest rent tercile, had no effect on the middle tercile, and decreased rents by 3.2 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. In our main specification we define housing quality tier using building quality relative to its zip code. These results are more precise than using citywide quality, which suggests that both geographic and quality submarkets are important factors shaping the effects of new construction.

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, 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 (**?**) 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 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 through 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 with five or more units that were constructed 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 by bedroom count, and year constructed.

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, which comprise 80.4 percent of the market-rate, non-student, non-senior units in the CoStar data. 73 percent of all rental units in Minneapolis were one and two-bedroom units in the 2000 census.

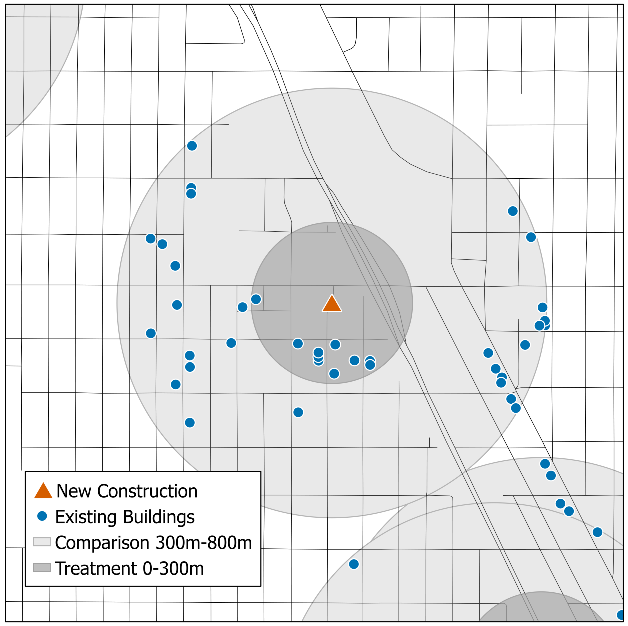
We partition the CoStar data into two analytic datasets; "older" buildings with build dates before Q1 2000 and "new construction" with build dates in 2000 or later. The new construction file identifies the market-rate new construction that we will use to identify treatment and comparison units in the older buildings data. Consistent with previous research, we restrict new construction to 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 set 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 multiple older buildings into our analysis sample. To ensure our older buildings were not constructed immediately before our study period, we require that each index building have a construction date at least ten years after 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 Appendix D. Some older buildings fall in the 300 and 800 meter buffers 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

[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 60 new construction buildings that we are able to use in our estimation sample.

Fig. 1. Treatment vs. Comparison Group Spatial Diagram



if a second new building was constructed within 300 meters. This measurement change can occur if an older building is, for example, 500 meters from a new 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 calculate the percentile rank of each older building within the 2000 rent distribution, by zip code. There are twelve zip codes represented in the CoStar data. This process identifies each older building's place in the geographically specific submarket in which it sits. We calculate these distributional statistics at the zip-code level using the entire dataset of older buildings in 2000, not just those within the 800 meters of an index building. We then define distributional cut points to classify buildings into market tiers (Susin, 2002). This means that our final estimation sample does not have equal numbers of units in each submarket. Our preferred submarket specification splits each zip code market quartiles and defines three quality tiers as low (0-25th percentile), middle (25th-75th percentile), and high (75th-99th percentile). We also repeat all analyses using an above/below median definition for high/low market tier (Appendix E).

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 (see Figure 4).

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:

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

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 interacts $Treat_{it}$ with indicators for whether the unit was in the low or high market tier.

$$ln(rent_{it}) = \delta Treat_{it} + \delta_{high}(Treat_{it} \times high_i) + \delta_{low}(Treat_{it} \times low_i) + \alpha_i + \tau_t + \epsilon_{it} \tag{2}$$

low_i and $high_i$ are indicators for the building-bedroom combination being in the 0-25th percentile/75th-99th percentile of the zip-code specific rent distribution in 2000. δ is the treatment effect in the middle market tier, and the effect in the high and lower market tiers are calculated as (δ + δ_{high}) and (δ + δ_{low}) respectively. Note that there is no intercept term for low_i and $high_i$ because quality submarket is time-invariant and absorbed into α_i in implementation.

After using Equation 2 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. Equation 3 models the distance effect by including 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 comprised of older buildings 401-800 meters from new construction.

$$ln(rent_{it}) = \sum_{d=200,300,400} [\delta^d Treat^d_{it} + \delta^d_{low}(Treat^d_{it} \times low_i) + \delta^d_{high}(Treat^d_{it} \times high_i)] + \alpha_i + \tau_t + \epsilon_{it} \tag{3}$$

The superscripts on the δ terms refer to the mutually exclusive "treatment distance" categories.

[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.

As in Equation 2, low_i and $high_i$ are indicators for the building-bedroom's quality tier.

Equation 3 serves two analytic purposes. First, it allows us to evaluate how the treatment effect varies over distance, an important dimension of heterogeneity that is lost by pooling all treated units together in Equation 1. 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 find a treatment effect in the 300-400 meter distance band, it suggests that these units may not be appropriate to pool together with other comparison buildings 400-800 meters away. Finding no effect in the 300-400 meter band would be evidence that any spillover of the treatment effect into the comparison group does not differ in comparison units closer to and further from the new construction. This type of non-distance dependent "spillover bias" of treatment onto comparison units would attenuate the model's treatment effects toward zero.

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:

$$ln(rent_{it}) = \sum_{k=-20}^{20} [\delta_k\{K_{it} = k\} + \delta_{high,k}(\{K_{it} = k\} \times high_i) + \delta_{low,k}(\{K_{it} = k\} \times low_i)] + \alpha_i + \tau_t + \epsilon_{it} \tag{4}$$

k indexes "event time," or quarters from the construction of the index building. $\{K_{it} = k\}$ 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 middle tier treatment units and all comparison units in event-quarter k. $\delta_{low,k}$ is the differential effect of new construction in the low tier and $(\delta_{low,k} + \delta_k)$ 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 buildings were "treated" (less than 300me from new construction) or "comparison" (between 300m and 800m

Table 1: CoStar Sample Characteristics by Treatment Status at Baseline

	Treated	Comparison	New Construction
Effective Rent ($)	1049.9	915.2	1622.2
	(277.5)	(157.3)	(517.9)
Total Units	46.6	32.8	150.7
	(62.4)	(58.4)	(79.6)
Sample 1br Units (%)	58.9	56.7	65.1
	(20.8)	(19.9)	(16.3)
Vacancy Rate (%)	6.6	4.8	36.3
	(10.7)	(8.9)	(28.8)
Year Built	1937	1943	2012
	(26.5)	(26.1)	(4.6)
N Buildings	138	270	58

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 in constant 2012 dollars. Treated buildings are within 300m of new construction. Comparison buildings are between 300m-800m of new construction.

from new construction). Our final sample contains 138 treatment buildings and 270 comparison buildings, which were brought into the sample by 60 unique index buildings.[3]We calculate average effective rent 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[4]. The average effective rent in treated buildings is $1050 compared to $915 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, 14 more units than comparison buildings but were of similar age. Vacancy rates are 1.8 percentage points higher in treated buildings compared to comparison buildings.

The New Construction column of Table 1 describes the characteristics of the index buildings that brought treated and comparison buildings into the final sample. The baseline characteristics of these buildings are reported as of the first year they appear in the CoStar data. New market-rate construction occurred almost exclusively in the high market tier, priced well above the rent of existing buildings. The average rent in new construction buildings in our sample was $1,622. This is 55 percent higher than the average treated building and 77 percent higher than the average

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

[4]Table 1 reports summary statistics for only 58 of the 60 New Construction buildings because two of the index buildings did not have rent or vacancy data in the CoStar data

Table 2: CoStar Characteristics by Market Tier

Market Tier	N Buildings	N Bld-Br Combo.	Mean Rent 2000	Mean Rent 2018	Pct. Change 2000-2018
Lower	88	116	578.6	851.5	47.9
			(122.7)	(252.5)	(35.5)
Middle	207	316	714	993.2	39.4
			(154.2)	(227.2)	(16)
Upper	113	183	961.9	1,239.8	30.3
			(343.3)	(400.7)	(15.2)
Total	408	615	762.2	1,039.9	38.3
			(263.5)	(325.2)	(21.7)

Note: Standard deviations in parentheses. Rent reported in nominal dollars

comparison building. New construction buildings had more units than existing buildings (due, in part, to our inclusion criteria) and and average vacancy rate of 36 percent in the year of their construction.

Table 2 describes the composition of our estimation sample across market tiers. The sample is not balanced across tiers because we calculate the zip-specific market tiers using the entire CoStar sample, not just buildings within 800 meters of new construction. Our final sample includes 116 low submarket building-bedroom combinations in 88 buildings, 207 middle submarket building-bedroom combinations in 316 buildings, and 113 high submarket building-bedroom combinations in 183 buildings. Appendix C provides choropleth maps showing the spatial variation in rents across market tiers. The average rent in the low market tier units was \$579 in 2000 and \$852 in 2018. The average change in nominal rent for this submarket over the study period was 48 percent. Rent in the middle submarket increased an average of 39 percent during the study period. The most expensive market tier had the lowest rent growth during the study period, from an average of \$962 in 2000 to \$1240 in 2018, or an average relative change of 30 percent. It should be noted that the market tier average rents 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 relative changes in rent reported from our primary models.

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. 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.

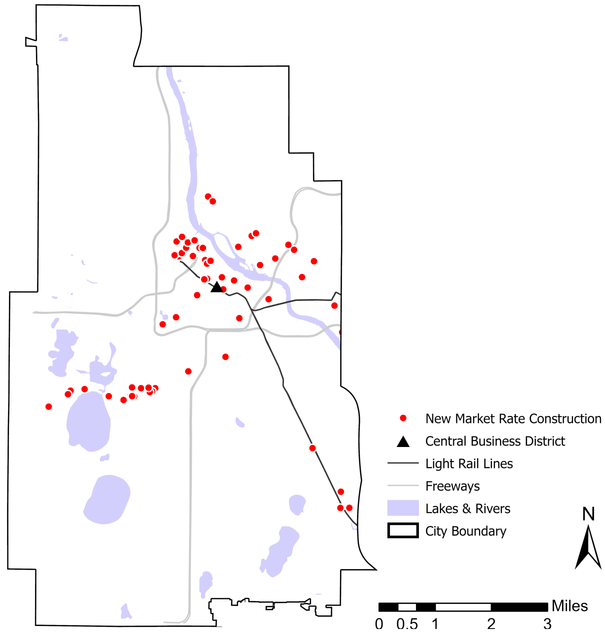
5.2 Neighborhood Characteristics

Our research question is concerned with the effects of new development on individual buildings, but examining differences in neighborhoods receiving new construction helps to contextualize building-level effects. We identified the census tracts that received new construction in our sample and used the Longitudinal Tract Database (LTDB) to create a census tract database using constant 2000 boundaries. We linked the standardized tracts to data from the 2000 Census, 2007-2011 American Community Survey (ACS), and 2014-2018 ACS (Logan et al., 2014) to evaluate the neighborhood characteristics of tracts receiving new construction.

Buildings meeting our definition of "new construction" were built in 23 of Minneapolis' 121 census tracts during our study period (see Table 3). Figure 2 shows the location of these index buildings on a map of Minneapolis. 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.

Table 3 compares demographic characteristics of census tracts that did and did not receive new construction in our sample. At the beginning of our study period in 2000, tracts that received new market rate apartments buildings differed in several ways, including having significantly higher home values, more adults with a college degree, higher shares of renters, lower shares of Black residents and higher rents. Tracts with and without new construction had similar poverty rates, vacancy rates and share of housing stock that consisted of lower tier rental units.

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



Appendix B reports the change in tract-level characteristics over our study period in tracts with and without new construction. In general, tracts that received new construction experienced socio-economic status improvements compared to tracts that did not receive new market rate apartments. From 2000-2018, the change in rent was 22 percent larger in tracts with new construction compared to tracts without new construction. Median household income grew 10 percent faster in tracts with new construction. Strikingly, Poverty rates in tracts with new construction declined by 5.3 percent, while poverty rates in tracts without new construction increased by 11.6 percent. Both types of tracts saw similar growth rates in the share of adults with a college degree.

5.3 Difference-in-Differences Analysis

5.3.1 Effect of New Construction by Market Tier

We find that the effect of new, market-rate construction on older buildings varied across different market tiers. Table 4 presents results for the pooled DID models, with and without market tier interactions, described by Equation 1 and Equation 2. Pooling across all market tiers, older buildings within 300 meters of new construction experienced no significant change in rent relative to buildings 300 to 800 meters away (column 1) (p=0.894). When we include interaction terms for market tier we find that this null effect is the result of offsetting treatment effects for older units above and below zip code median rent in 2000 (column 2). Treated units in below median buildings

Table 3: Census Tract Mean Characteristics by New Construction Status

	With Construction	Without Construction	P-Value
Pct. White	66.6	57.1	0.105
	(23.0)	(28.1)	
Pct. Black	14.2	20.9	0.063
	(13.6)	(18.9)	
Pct. Bachelors	43.4	31.4	0.024
	(21.7)	(19.5)	
Pct. Renter	66.5	45.3	0.001
	(24.0)	(27.3)	
Pct. Poverty	21.7	18.3	0.326
	(14.4)	(13.1)	
Median Value ($)	146,938.1	111,410.5	0.023
	(62,477.1)	(53,365.1)	
Median Rent ($)	592.0	539.4	0.095
	(129.1)	(109.2)	
Vacancy Rate (%)	5.6	4.0	0.165
	(5.1)	(2.3)	
Pct. Low Tier Rentals	37.2	29.9	0.217
	(24.7)	(23.0)	
N	23	98	

Note: Standard deviations in parentheses."Low Tier" rentals defined as units with contract rent less than $600 a month. The average rent in low tier buildings was $579 in 2000. After rounding, $600 was the nearest census bin cutoff.

Table 4: Effects of New Construction by Housing Market Tier

	(1) Pooled	(2) Median	(3) Quartile
Pooled	0.00100		
	(0.00754)		
Below Median		0.0295*	
($p < 50$)		(0.0123)	
Above Median		-0.0137	
($p \geq 50$)		(0.00879)	
Bottom Quartile			0.0642**
($p < 25$)			(0.0214)
Middle 50%			0.00423
($25 \geq p < 75$)			(0.00802)
Top Quartile			-0.0316*
($p \geq 75$)			(0.0127)
Observations	46,740	46,740	46,740

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

Standard errors in parentheses

Note: Table reports treatment effect on ln(rent). All models include building-bedroom and quarter fixed effects. Standard errors clustered at the building level.

had rents that were 3.0 percent higher (p=0.017) following treatment than units in comparison buildings. Treated units in above median buildings experienced a 1.4 percent decrease in rent (p=0.120), but this effect was not statistically different from zero at the 95% confidence level.

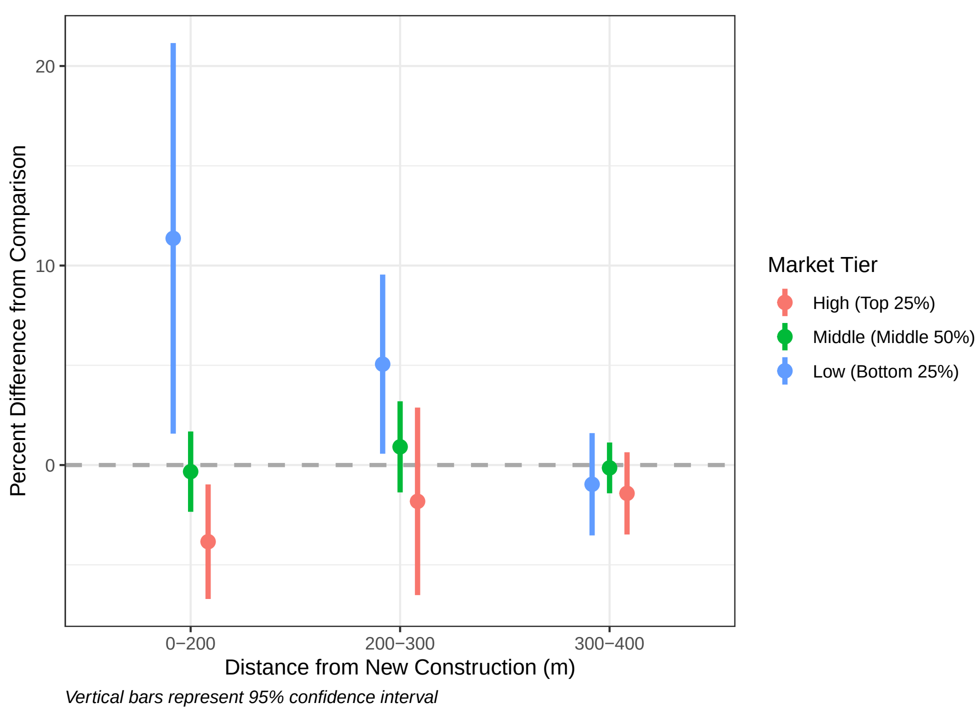
The differentiation of the treatment effect is even stronger when we interact treatment with indicators for being in the low, middle, or high market tier (column 3). Treatment units in the low market tier had rental prices that were 6.6 percent higher (p=0.003) than comparison units. There was no significant effect on rents in the middle market tier (p=0.598). The most expensive market tier of the 2000 rent distribution experienced a rent decrease relative to comparison units of -3.1 percent (p=0.013).

5.3.2 Distance Analysis

Figure 3 shows the results of our model that interacted units' market tier with distance from new construction. The effect of new construction on rent is strongest at very close distances and decays to zero as the distance from new market-rate buildings increases. The comparison group in Figure 3 is comprised of units 400 to 800 meters from new construction.

The pattern of treatment effects in Figure 3 is consistent with our previous previous analysis showing that new market-rate construction is associated with higher rents in the nearby low-rent market tier and lower rents in high market tier buildings. We find that low market tier units within 200 meters of new construction had post-period rents that were 11.4 percent higher than comparison units (p=0.028). This effect is significant but imprecisely measured, in part due to a limited number of low market tier buildings in this distance band. Low tier units 200 to 300 meters away from new construction had rental prices that were 5.1 percent higher than comparison buildings (p=0.030), roughly the same effect we found in the quality-interacted model in Table 4. We did not identify a significant difference in rent in any market tier between units 300-400 meters from new construction and comparison units 400-800 meters away.

Fig. 3. Effects of New Construction by Distance from New Construction and Market Tier



In the middle market tier, the treatment effect was -0.3 percent for units within 200 meters (p=0.747), 0.9 percent between 200-300 meters (p=0.433), and -0.1 percent between 300-400 meters (p=0.826), 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 market units, the effect is smaller in magnitude than in the low market tier.

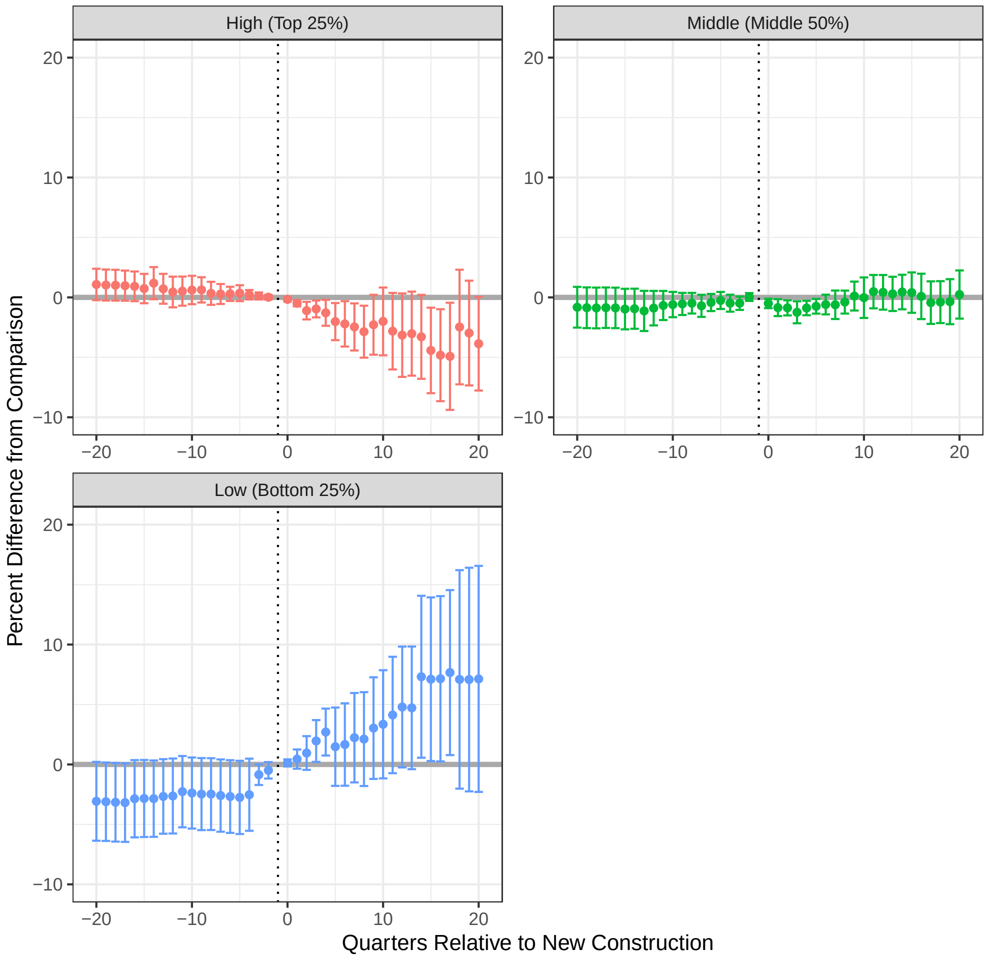
The distance gradient of our treatment effect was less pronounced in the high market tier. The closest units had rents that were significantly lower than the comparison group by 3.8 percent (p=0.007). The effect in the middle band was a difference of -1.8 percent (p=0.439) and the effect in the 300-400 meter band was -1.4 percent (p=0.172), but these estimates are also not distinguishable from zero at the 95% confidence level.

5.3.3 Event Study Analysis

The quality and distance analysis suggest that new construction has heterogeneous effects across both market tiers and distance, but it is also important to understand the time path of the hypothesized treatment effect. By plotting the treatment effect 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, middle, and bottom market tiers. This figure serves two purposes. First, 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, the post-period coefficients decompose the pooled effects reported in Table 3 into year-specific effects to help us better understand the time path of the treatment effect. The assumption that our comparison group is a valid counterfactual cannot be empirically proven, but the pre-period event study results do not show strong evidence that rent trends differed across the treatment and comparison groups. Figure 4 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 building-bedroom fixed effects. There was no significant difference in low tier treated and pooled control buildings in any specific pre-period quarter, but averaging the pre-period coefficients suggests an average rent difference of about -2.5 percent between low tier treated units and control units (p<0.000). There was no significant difference between pre-period treatment and control units in the middle (p=0.227). Pre-period rents in treated units in the top market tier were an average of 0.6 percent higher than control units (p=0.061).

We observe a sharp increase in rent in the low market tier treated units in the three quarters prior to the availability of units in new, market-rate buildings. This could be an anticipatory effect

Fig. 4. Event Studies of Effect of New Construction by Market Tier (Unbalanced Panel)



Vertical bars represent 95% confidence interval

– 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 deferentially 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 ($t >= 4$) coefficients in Figure 4 show that the effects of new construction are evident within two quarters of the completion of new market-rate buildings and persist for the entire post-period. Two quarters after treatment, high tier treatment buildings had rents that were significantly lower than comparison buildings by about 1.1 percent (p=0.003). The low tier submarket had rents 1.0 percent higher than the comparison buildings (p=0.185) at $t = 2$. After one year ($t = 4$), rents in high tier treated units were 1.3 percent lower (p=.019) than comparison units. In the low market tier, rents were 2.7 percent higher (p=0.008) than comparison buildings. By year two ($t = 8$), the effect in the low market tier was 2.1 percent (p=0.290) and -2.9 percent (p=0.009) in the higher market tier.

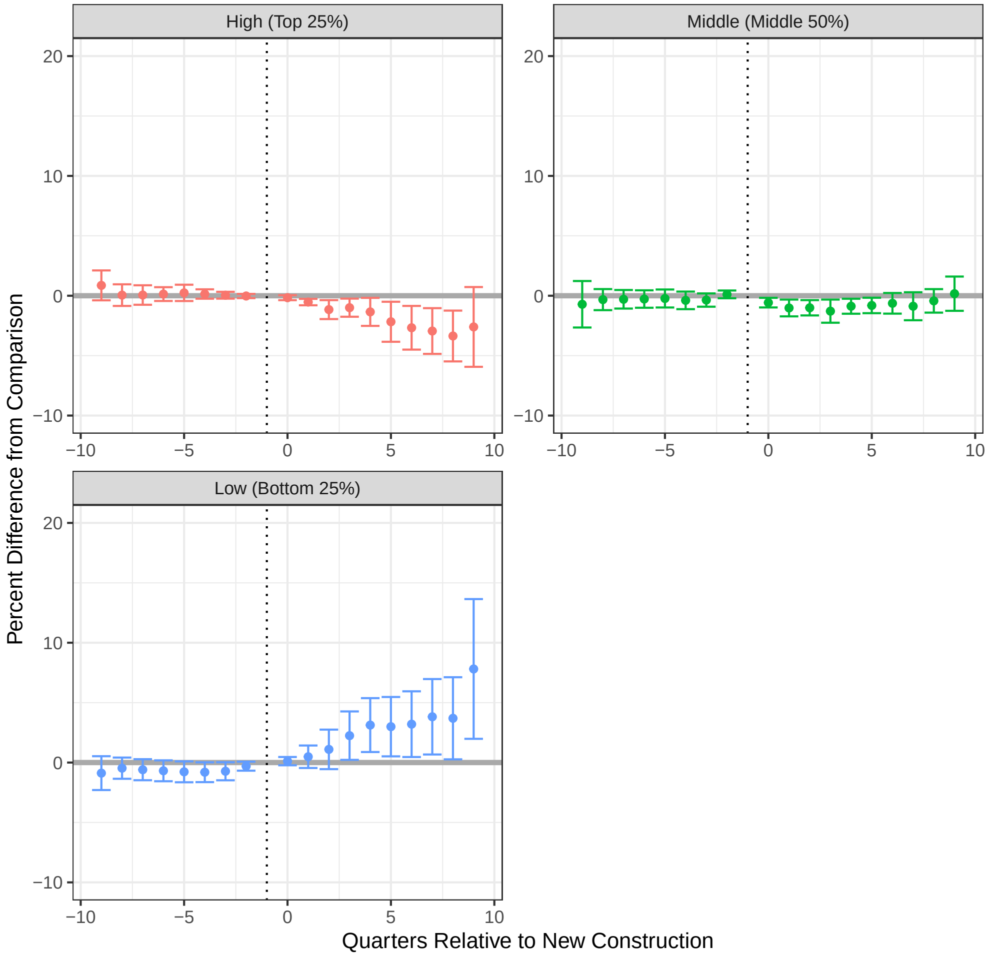
Our model finds that the effect of new construction on rent in the low market tier grew over the interval $t = 8$ through $t = 20$, or two to five years after new construction. Three years following the completion of a new building, low market tier rents were 4.8 percent higher than control units (p=0.074), growing to 7.1 percent at year four (p=0.046) and 7.1 percent at year five (p=0.143). It should be noted that these later event time periods are not available for all units in our sample due to differential timing of new construction - the sample composition is changing during this period. We replicate our analysis using a balanced panel of buildings in the following section.

5.3.4 Robustness

While the results in Figure 4 confirm the findings from the multi-period DID models in Table 4, one concern may be that the panel of treated buildings is changing across the range of event time. The sample in Figure 4 is "unbalanced" because treatment units spend differing amounts of time in the pre-treatment and post-treatment conditions based on when new construction was completed. For example, older buildings with index buildings built in 2016 are only in the post-period sample until $t = 8$. 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 largely confirms the market tier findings in the unbalanced event study. The pre-period coefficients in the low market tier suggest that rent was about 0.6 percent lower in the low tier treated units than comparison units (p=0.021) and the lowest tier also exhibits increasing rent relative to the comparison group in the three quarters before the first observation of a completed new building in the CoStar data. The pre-period coefficients were jointly non-significant in high market tier (p=0.347) and middle market tier (p=0.248).

Fig. 5. Event Studies of Effect of New Construction by Market Tier (Balanced Panel)



Vertical bars represent 95% confidence interval

The post-construction coefficients in the lowest market tier grew from 0.5 percent in the first quarter after new construction (p=0.312) to 3.1 percent in the fourth quarter (p=0.007) and 3.7 percent in the eighth quarter (p=0.037) after treatment. The treatment effects in the highest market tier showed a persistent and growing negative effect. One quarter after new construction, rents were 0.5 percent lower (p=0.000) than the comparison group and this effect grew to -1.3 percent (p=0.023) at one year and -3.4 percent (p=0.002) at two years.

Our choice of a 300 meter treatment distance, a 301 to 800 meter comparison distance, was 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 definitions of our treatment group in Appendix D. The results of this exercise are consistent with the findings from our distance analysis. Using the three-tier market definitions, focusing on treated units within 200 meters of new construction results in a treatment effect of 11.0 percent (p=0.035) in the low market tier and -3.7 percent in the high market tier (p=0.009).

Appendix Figure D.1 reports the results of the event study analysis using a 200 meter treatment definition. The time pattern of the treatment effect is similar under this alternative specification, with rent increases relative to the comparison group beginning in the three quarters prior to new construction and persisting through the post period in the low market tier. At one year ($t = 4$) we find that rent was 3.1 percent higher in the low market tier (p=0.047), 0.6 percent lower in the middle market tier (p=0.099), and 1.3 percent lower in the high market tier (p=0.056). The low and high market tier effects grew to 6.2 percent (p=0.042) and -3.6 percent (p=0.007) at two-years post construction respectively. Appendix Figure D.1b shows these event studies using a balanced panel of buildings over two years before and after new construction.

Appendix E recreates the event study models using a 300 meter treatment distance and the two-tier definition of quality divided at zip-code median rent. This alternative specification is consistent with a positive treatment effect in the low market tier and a negative treatment effect in the high market tier. Table E.1 recreates Table 2 using the two-market quality definition and Figure E.1 shows the event study (unbalanced and balanced) by above and below zip code median.

In Appendix F, we recreate the main models using city-wide quality as opposed to within zip code quality. This means that we assign buildings to market tiers according to their place in the 2000 rent distribution of Minneapolis, rather than their zip code specific rent distribution. Table F.1 summarizes the market tiers using this alternative definition and Table F.2 shows the results of the quality-stratified DID model using the city-wide rent distribution. This alternative specification yields a treatment effect on low tier units of -6.6 percent (p=0.009) and a high tier treatment effect of -1.9 percent (p=0.069). The distance models in Figure F.1 show a comparable distance gradient across quality tiers and the event studies in F.2 are similar to our primary models as well.

Finally, a reasonable objection to our approach is that using a comparison that contains units of all quality tiers is not appropriate, even after including unit fixed effects to control for differences in levels of rent. Appendix G addresses this by estimating the pooled and event study DID models on subsamples of our full analytic sample, partitioned by market tier. This means that the treatment and comparison groups in each stratified model include only buildings in the same market tier in 2000.

In the stratified pooled models (Table G.1), we find treatment effects of smaller magnitude for both low and high market tier units. The effect in the low market tier was 3.8 percent (p=0.123), compared to 6.6 percent in our preferred specification. Similarly, the effect in the high market tier was -0.5 (p=0.673) in the stratified analysis compared to -3.1 percent in our main analysis. The balanced and unbalanced event studies are noisier due to the reduced sample size, but the overall path of the treatment effect in all market tiers was comparable to the event studies in Figure 4.

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 6.6 percent higher than comparison units following the completion of new buildings. High tier housing close to new construction had rents that were about 3.2 percent lower than comparison buildings. We found that the effects on new buildings in both high and low market tiers were concentrated within 300 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 are consistent with an effect that is persistent beyond the two-year period in our balanced sample.

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 fact that our models using zip code measures of housing quality speak to the importance of incorporating information about both quality and geographic submarkets in housing supply analysis.

The higher rent growth we observe in low market tier buildings close to new construction could be evidence of a type 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 finds 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 new 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 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 (see Appendix G). 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. Our analytic approach attempts to address the selection issues related to developers building in neighborhoods with high or increasing rent, but the identifying assumptions of our difference-in-differences models are ultimately unstable. There may be important ways in which buildings 301-800 meters from new construction are not a good counterfactual for closer buildings. 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.

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A Costar Sample Characteristics

Fig. A.1. Spatial Comparison of CoStar Sample Buildings and Minneapolis Mulit-Family Parcels

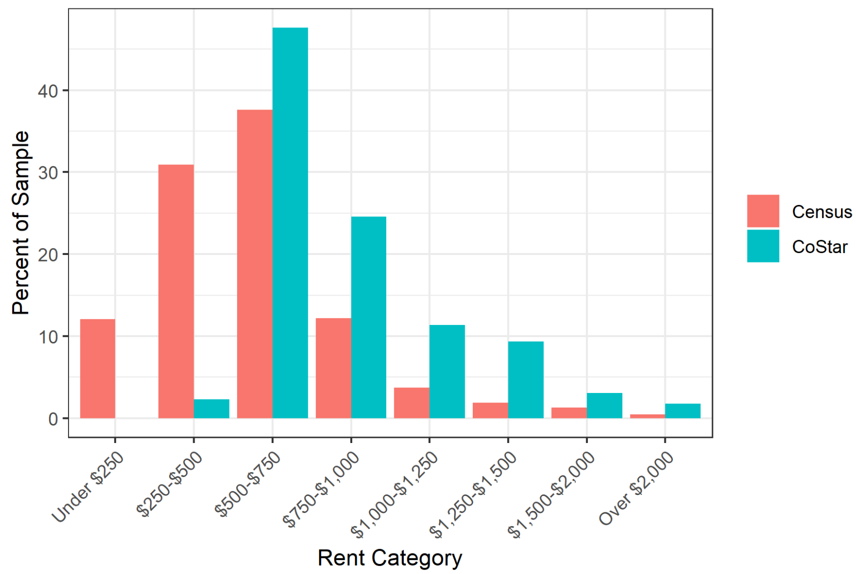
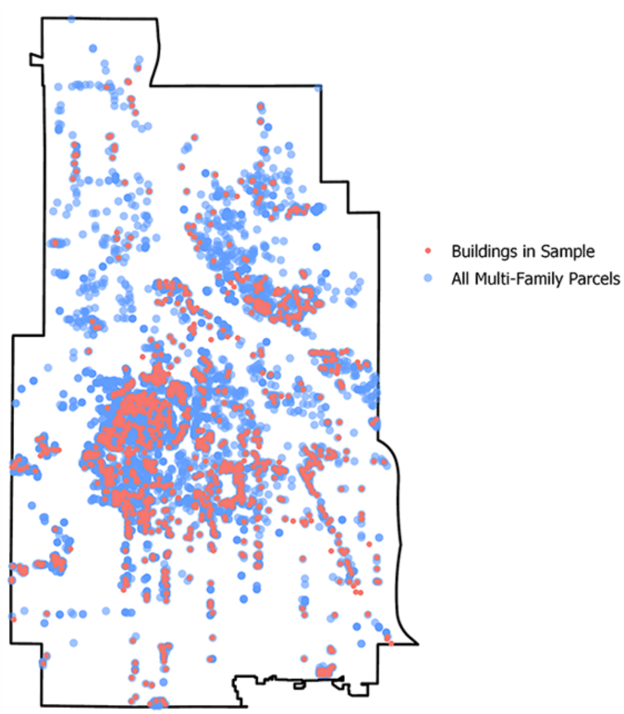


Fig. A.2. Distribution of Rents in CoStar Sample Compared to 2000 Census Minneapolis, MN



B Neighborhood Trajectories

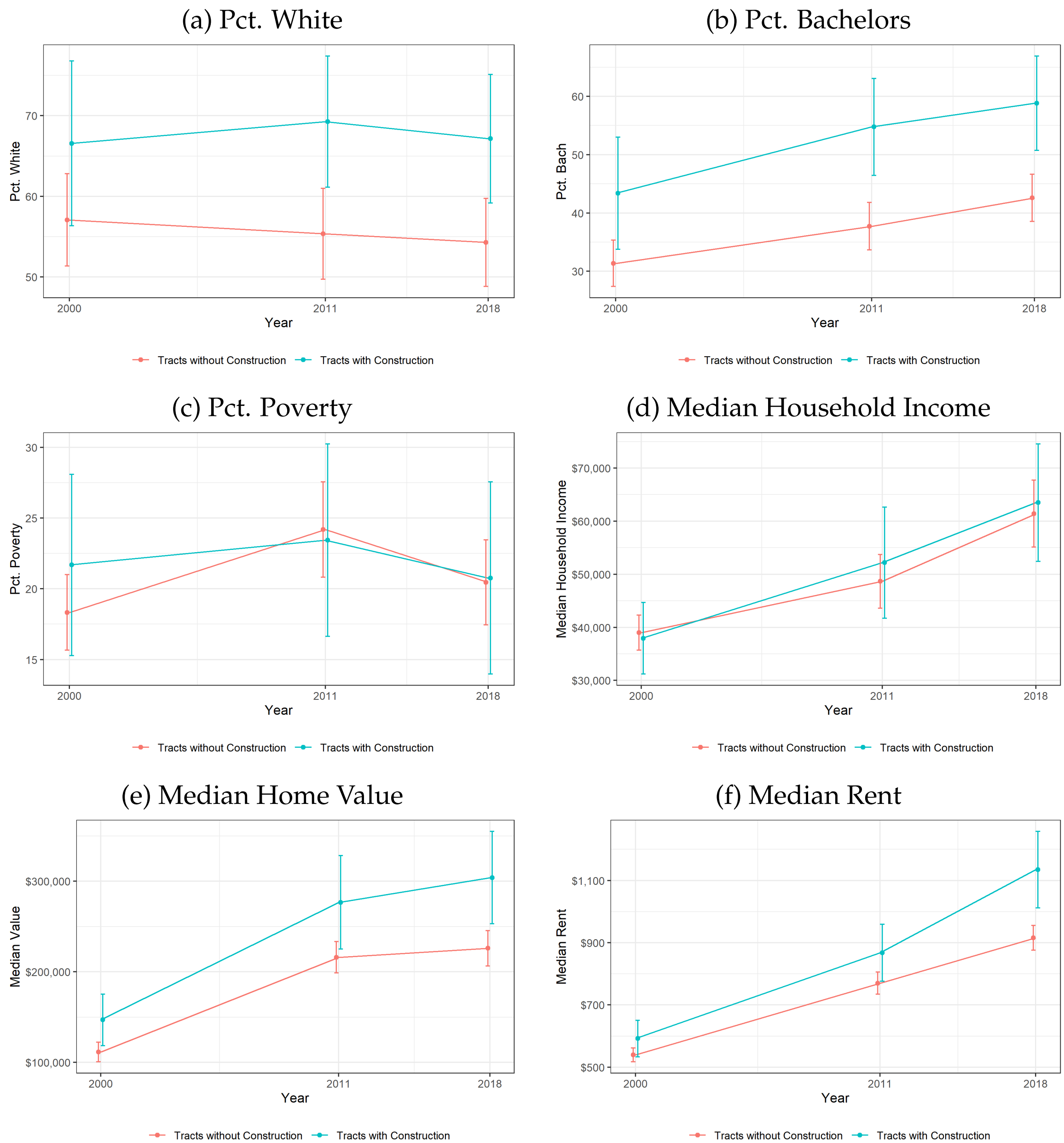


Fig. B.1. Neighborhood Trajectories

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 Rent Tier Threhsolds by Zipcode in 2000

Fig. C.1. Average Rent by Zipcode - Low (Bottom 25%)

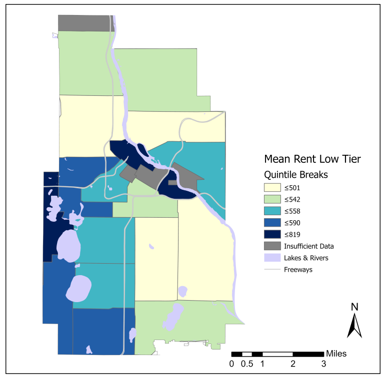


Fig. C.2. Average Rent by Zipcode - Middle (Middle 50%)

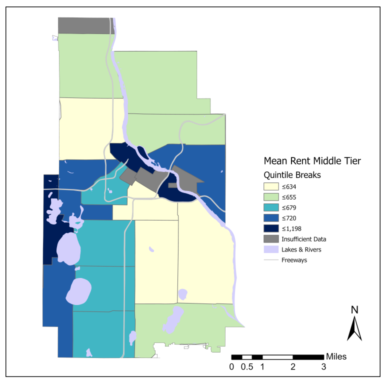
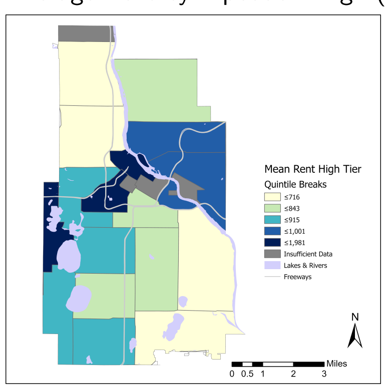


Fig. C.3. Average Rent by Zipcode - High (Top 25%)



D Results Using 200m Treatment Distance

Table D.1: Effects of New Construction by Housing Market Tier - 200m Treatment Distance

	Pooled	Median	Quart
Pooled	-0.0137		
	(0.0102)		
Below Median		0.0450*	
($p < 50$)		(0.0229)	
Above Median		-0.0296**	
($p \geq 50$)		(0.0101)	
Bottom Quartile			0.104*
($p < 25$)			(0.0493)
Middle 50%			-0.00420
($25 \geq p < 75$)			(0.00972)
Top Quartile			-0.0376**
($p \geq 75$)			(0.0143)
Observations	46,740	46,740	46,740

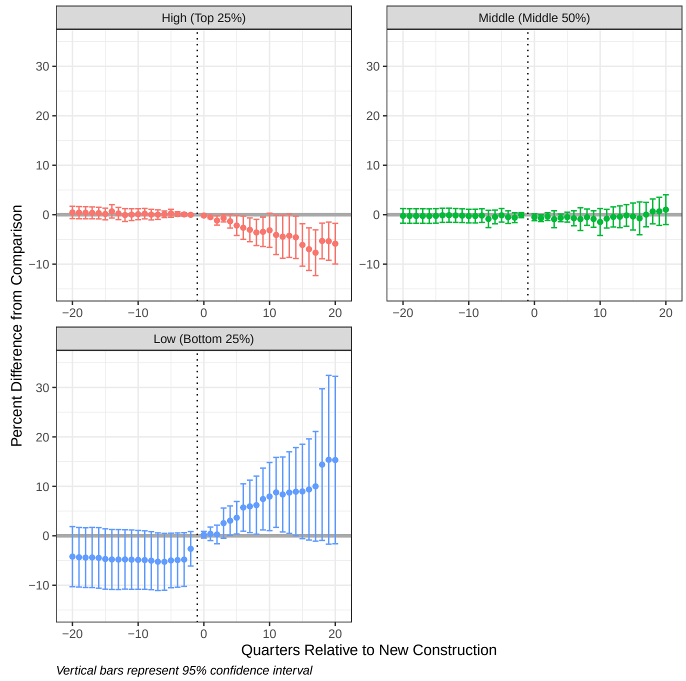
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors in parentheses

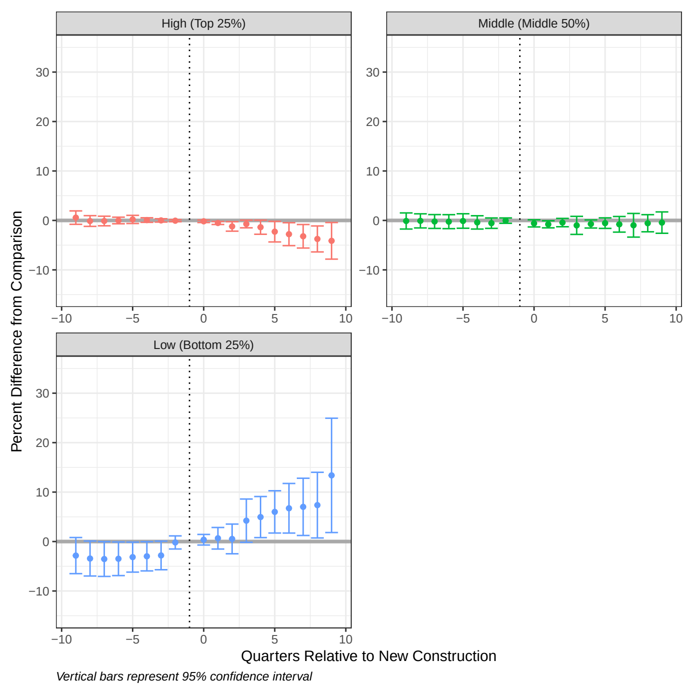
Note: Table reports treatment effect on ln(rent). All models include building-bedroom and quarter fixed effects. Standard errors clustered at the building level.

Fig. D.1. Event Studies of Effect of New Construction by Market Tier - 200m Treatment Distance

(a) Unbalanced



(b) Balanced



E Results Using Medians as Market Tier Threshold

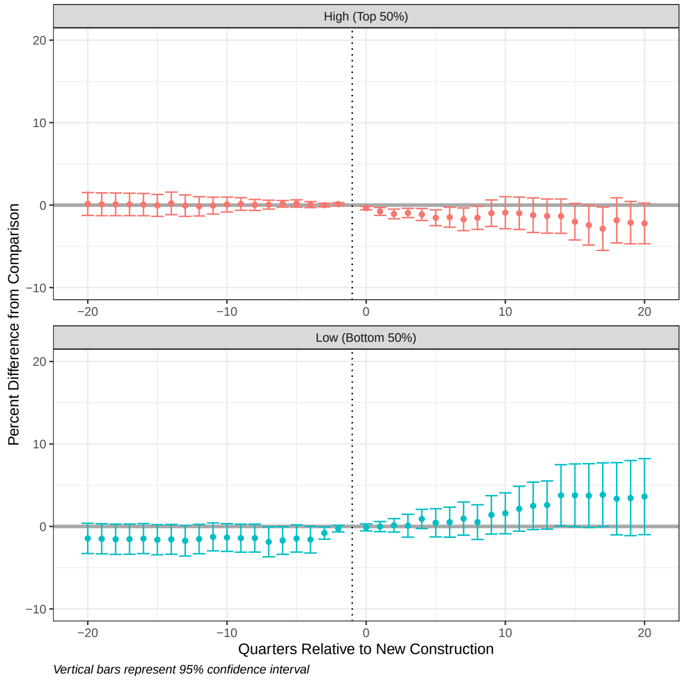
Table E.1: CoStar Characteristics by Market Tier (Medians)

Market Tier	N Buildings	N Bld-Br Combo.	Mean Rent 2000	Mean Rent 2018	Pct. Change 2000-2018
Above Median	220	351	864.6	1,145.9	33.9
			(292.5)	(348.7)	(15.4)
Below Median	188	264	626.1	899	44.1
			(125)	(224)	(27)
Total	408	615	762.2	1,039.9	38.3
			(263.5)	(325.2)	(21.7)

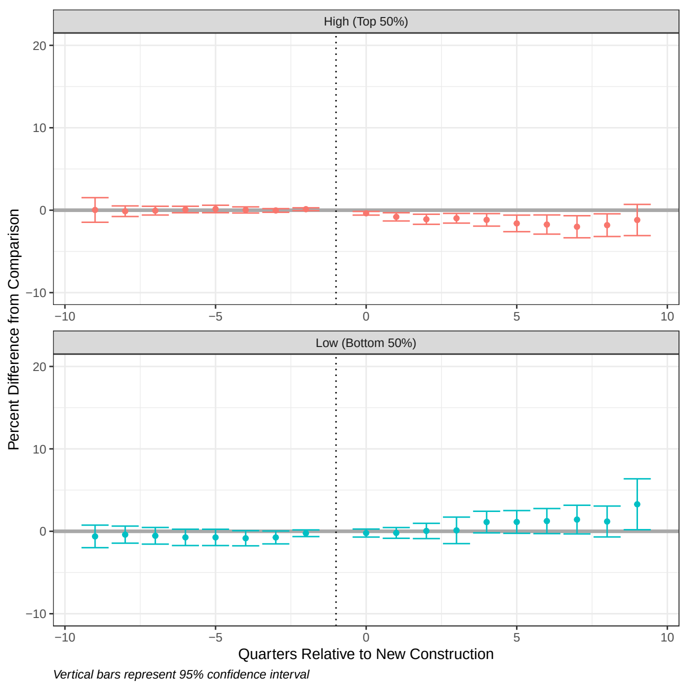
Note: Standard deviations in parentheses

Fig. E.1. Event Studies of Effect of New Construction by Market Tier

(a) Unbalanced



(b) Balanced



F Results Using Citywide Rent Distribution

Table F.1: CoStar Characteristics by Market Tier (Citywide Quality Distribtuion)

Market Tier	N Buildings	N Bld-Br Combo.	Mean Rent 2000	Mean Rent 2018	Pct. Change 2000-2018
Lower	73	94	545.7	797.3	47.4
			(94.8)	(208.2)	(37.5)
Middle	197	297	679.6	949.4	39.8
			(92.8)	(159)	(15.3)
Upper	138	224	962.7	12,61.7	32.5
			(326.7)	(393)	(18.2)
Total	408	615	762.2	1,039.9	38.3
			(263.5)	(325.2)	(21.7)

Note: Standard deviations in parentheses

Table F.2: Effects of New Construction by Housing Market Tier - Citywide Quality Distribution

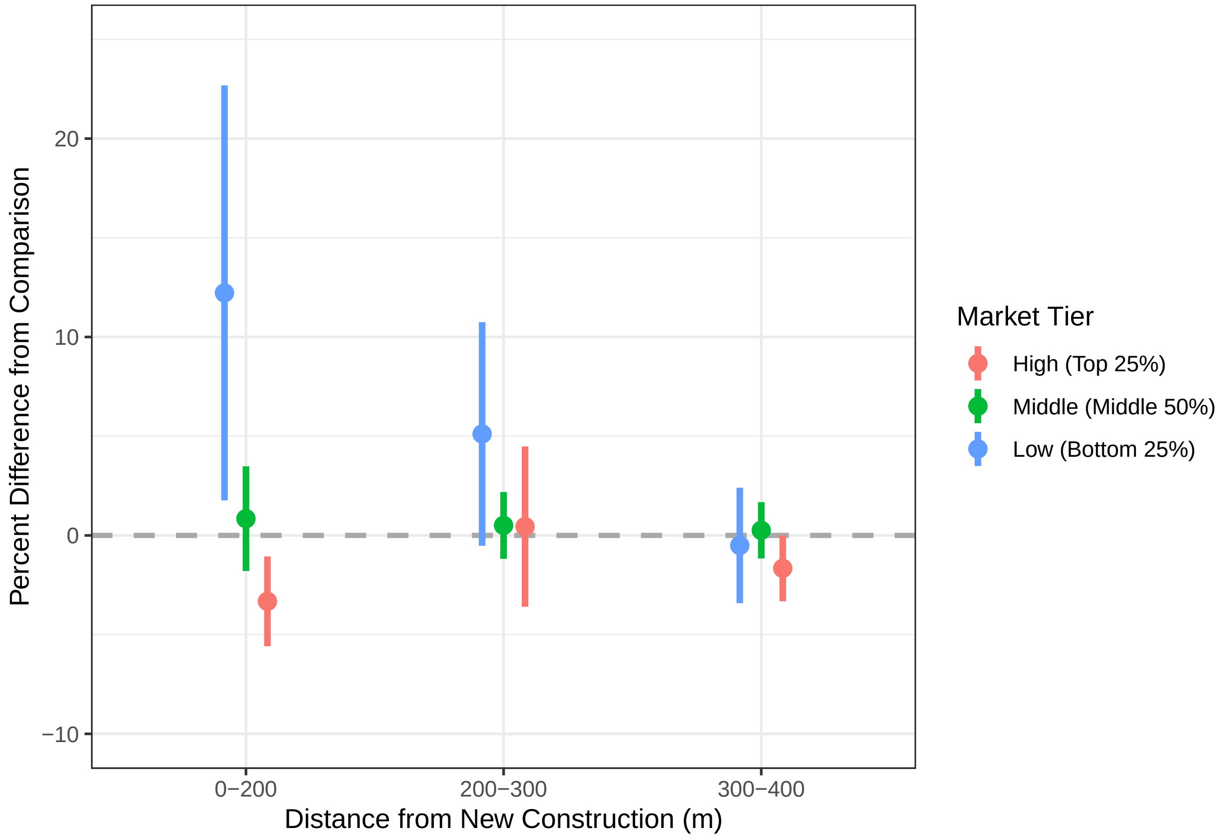
	Pooled	Median	Quart
Pooled	0.00100		
	(0.00754)		
Below Median		0.0387*	
($p < 50$)		(0.0162)	
Above Median		-0.0105	
($p \geq 50$)		(0.00795)	
Bottom Quartile			0.0681**
($p < 25$)			(0.0259)
Middle 50%			0.00718
($25 \geq p < 75$)			(0.00765)
Top Quartile			-0.0192
($p \geq 75$)			(0.0105)
Observations	46,740	46,740	46,740

Standard errors in parentheses

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

Note: Table reports treatment effect on ln(rent). All models include building-bedroom and quarter fixed effects. Standard errors clustered at the building level.

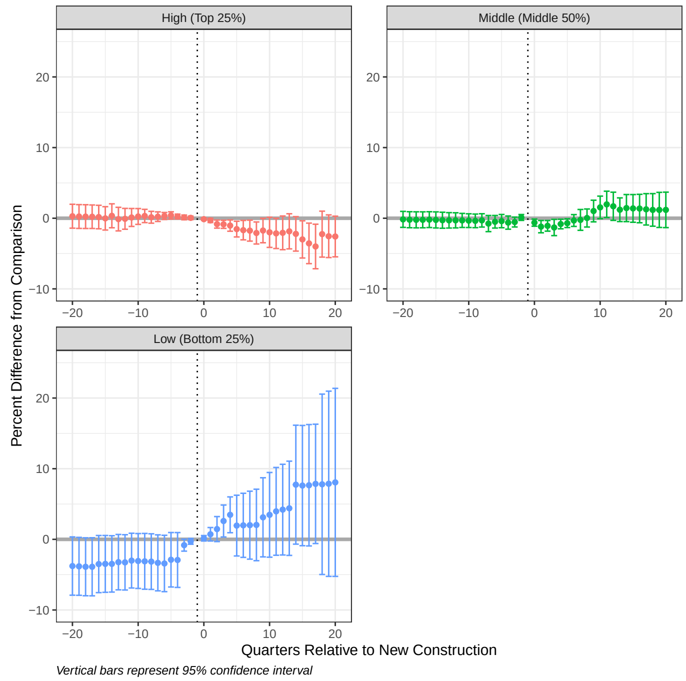
Fig. F.1. Effects of New Construction by Distance from New Construction and Market Tier (Citywide Quality Distribution)



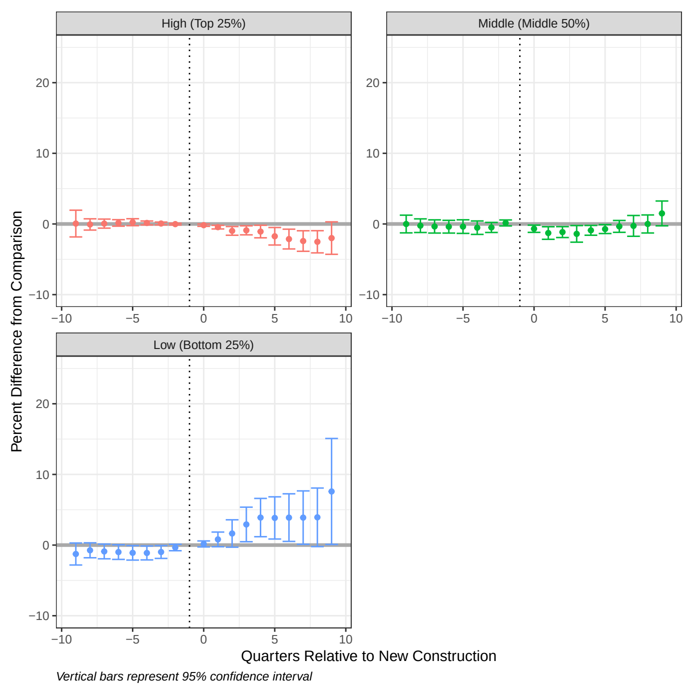
Vertical bars represent 95% confidence interval

Fig. F.2. Event Studies of Effect of New Construction by Market Tier - Citywide Rent Distribution

(a) Unbalanced



(b) Balanced



G Results Using Stratified Sample

Table G.1: Effects of New Construction by Housing Market Tier - Partitioned Model

	Low	Middle	High
Treatment	0.0386	-0.00109	-0.00496
	(0.0248)	(0.00770)	(0.0117)
Observations	8,816	24,016	13,908

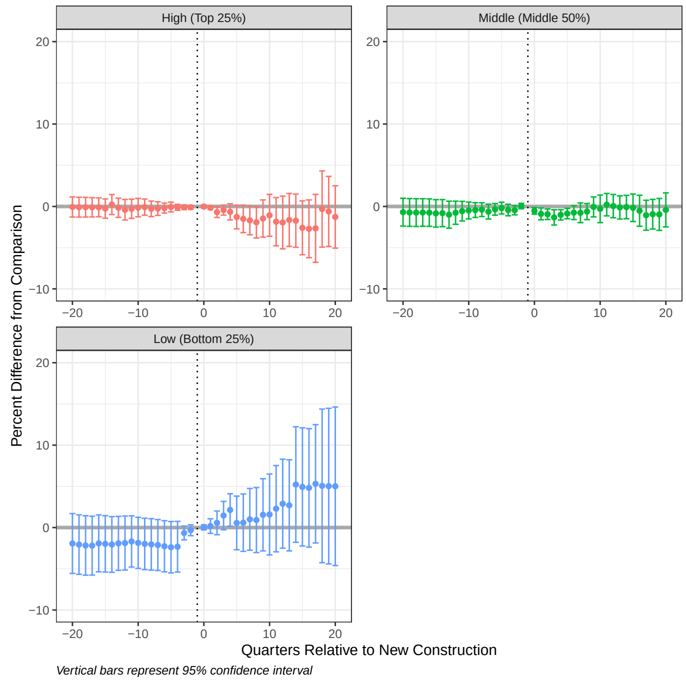
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors in parentheses

Note: Table reports treatment effect on ln(rent). All models include building-bedroom and quarter fixed effects. Standard errors clustered at the building level.

Fig. G.1. Event Studies of Effect of New Construction by Market Tier

(a) Unbalanced



(b) Balanced