Does Building New Housing Cause Displacement?: The Supply and Demand Effects of Construction in San Francisco*

Kate Pennington†

August 9, 2021

Abstract

This paper identifies the causal impact of new construction on nearby rents, displacement, and gentrification in San Francisco by exploiting random variation in construction location induced by serious building fires. I combine parcel-level data on fires and construction with an original dataset of historic Craigslist rents and a panel of individual migration histories that allow me to introduce new, separate measures of displacement at the renter level and gentrification at the parcel level. I find that rents and displacement fall differentially near new market rate projects, while gentrification increases. In contrast, affordable housing does not have spillover effects.

Keywords: Displacement, Gentrification, Housing Supply, Spatial Econometrics
JEL Codes: R130, R230, R310, J1, J60

---

*I would like to thank Brian Asquith and the Upjohn Institute for Employment Research for providing me with a fellowship to use the Infutor data, as well as invaluable discussion. Many thanks to Meredith Fowlie, Jeremy Magruder, Reed Walker, and my PhD cohort for their thoughtful feedback. I am grateful for comments from seminar participants at the NBER Summer Institue in Real Estate and Urban Economics, UC Berkeley's Real Estate and Environmental and Resource Economics seminars, the Online Spatial and Urban Seminar (OSUS) series, the Urban Economics PhD Workshop, and the Urban Economics Association meetings. Robert Collins of the San Francisco Rent Board provided crucial data and information about evictions in San Francisco and Michael Webster of the City Planning Department provided data and context on San Francisco parcel histories. A warm thank you to Pedro Peterson and Joshua Switzky of the Planning Department for sparking this research agenda and for many conversations. This research has been supported by the San Francisco City Planning Department, Fisher Center for Real Estate and Urban Economics, the Upjohn Institute for Employment Research, and the Institute for Research on Labor and Employment at UC Berkeley.

†Department of Agricultural and Resource Economics, University of California, Berkeley. kate.pennington@berkeley.edu

1 Introduction

Cities across the United States are grappling with what to do about rising housing prices. Since the 1980s, the arrival of high-income newcomers has been driving up prices in urban centers and causing displacement and gentrification (Couture et al., 2019). Displacement refers to push migration, where individuals typically move to lower-income neighborhoods with fewer economic opportunities (Mok and Wang, 2020; Bilal and Rossi-Hansberg, 2018; Ding et al., 2016).[1] Gentrification refers to the replacement of lower-income residents with higher-income newcomers (Glass, 1964).[2] Although rising housing prices, displacement, and gentrification often occur together, they can also happen separately. A lack of clarity over the causes and consequences of each of these processes, and their relationship to each other, has complicated policy discussions about how to address them. This paper examines the impact of one obvious but controversial policy lever: the construction of new housing.

Building new housing is controversial because its impact on rents, displacement, and gentrification nearby is ambiguous. New buildings increase the housing supply, but they may also increase demand for nearby housing by improving neighborhood quality. If these demand effects outstrip the supply effects, new construction could accelerate local displacement. Disagreement over the net effect and spatial dynamics has led to contentious policy debate over the role of new construction in addressing neighborhood change.[3]

This debate is really an open empirical question. What is the impact of new housing construction on incumbent residents and neighborhoods? How large is the supply effect compared to any potential demand effect? Is the impact of new market rate housing different from the impact of new affordable[4] housing?

I study these questions in San Francisco, one of the fastest-gentrifying cities in the United States (Gyourko et al., 2013). Concern about housing affordability is nearly universal – 84%

[1]Qualitatively, displacement refers to involuntary mobility, typically forced by rising rents, eviction, landlords or utilities shutting off heat and water, or natural disasters (Grier and Grier, 1980; Desmond and Shollenberger, 2015). Grier and Grier (1980) write that displacement occurs when a household is forced to move away "by conditions which affect the dwelling or immediate surroundings, and which: 1) are beyond the household's reasonable ability to control or prevent; 2) occur despite the household's having met all previously-imposed conditions of occupancy; and 3) make continued occupancy by that household impossible, hazardous, or unaffordable."

[2]In addition to this demographic definition of gentrification, the term gentrification is sometimes used to refer to changes in the physical quality of the neighborhood such as building upgrades or the arrival of upscale businesses. This definition does not specify who lives in the upgrading neighborhood and enjoys its improved quality. Generally, the term 'neighborhood revitalization' refers to quality upgrades when the incumbent residents remain, and 'gentrification' refers to quality upgrades when the incumbent residents are replaced by richer newcomers.

[3]For example, this 2020 article from 48 Hills congratulates activists for successfully changing plans for a market rate development into plans for an affordable development, claiming that "market-rate housing...would drive up prices *(sic)* everyone else in the area and lead to massive displacement." A 2018 piece in San Francisco Magazine is titled, "Is This Oakland Developer Building Sorely Needed Housing–or Dropping Gentrification Bombs?" Monkkonen (2016) and (Zuk and Chapple, 2016) provide additional discussion.

[4]For a one-person household in San Francisco, the qualifying income range was $45,600 - $91,200 for a rental apartment and $66,300 - $107,750 for ownership in 2018[5]

of San Francisco Bay Area residents feel there is a housing crisis.[6] Over my study period from 2003-2017, the average price of a one-bedroom apartment listed on Craigslist increased 97%. Systemic racial inequality means rising housing prices can also drive changes in racial composition (Depro et al., 2015). Between 1990 and 2015, the city's Black population shrank by 45%.[7] Yet the majority of San Franciscans – including renters – oppose new housing in their own neighborhoods, even as they support an increase in the citywide housing supply (Hankinson, 2018).

To identify the impact of new construction on the surrounding neighborhood, this paper overcomes two challenges. The first is an identification problem: developers are more likely to build where prices are already appreciating (Boustan et al., 2019; Green et al., 2005; DiPasquale, 1999). To overcome this endogeneity problem, I exploit exogenous variation in the location of new construction caused by serious building fires. The combination of strict regulation and geography mean that San Francisco cannot grow up or out. As a result, most new construction requires removing an existing building. Serious fires increase the probability of construction on a burned parcel relative to its unburned neighbors by lowering construction costs. I show that severe fires increase the probability of construction on the burned parcel by a factor of 32 compared to unburned parcels. The incidence of serious fires is unrelated to trends in rents, displacement, or gentrification.

The second challenge is to credibly and separately define displacement and gentrification. Separating the measures of displacement and gentrification is crucial. Displacement happens to people; gentrification happens to places. Gentrification may happen without displacement (low-income incumbents willingly move and are replaced by higher-income newcomers), and displacement may happen without gentrification (push movers are replaced by newcomers from the same demographic (Freeman, 2005; Desmond, 2016)). Using spatially aggregated data can mask changes within a smaller spatial unit (Depro et al., 2015; Kinney and Karr, 2017; Ahlfeldt and Maennig, 2010) and blur the distinction between displacement and gentrification (Ding et al., 2016; Zuk and Chapple, 2016).

To quantitatively define displacement and gentrification, I combine data on individual migration histories with proxies for income. Using data from Infutor, I track the address histories of 1.24 million people who lived in San Francisco between 2003 and 2017. I focus on renters by excluding households that filed for an owner-occupation tax benefit. I add median zipcode income from the Internal Revenue Service (IRS) to create measures for displacement and gentrification that capture both individual mobility and income.

I proxy for displacement using moves to poorer zipcodes. Focusing on moves to lower-income zipcodes, rather than the universe of moves, helps to zero in on push migration because renters displaced by rising housing prices tend to move to areas where rents and incomes are lower (2019 Edelman Trust Barometer: Special Report on California, New York

[6] Quinnipiac University poll, 2019.
[7] SF City Planning Department analysis of IPUMS data

City Housing and Vacancy Survey, Puget Sound Regional Council Household Travel Survey Program, Milwaukee Area Renters Study, Couture et al. (2019); Desmond and Shollenberger (2015); Carlson (2020)). Of course, not all moves to lower-income zipcodes are pushed, and some displaced households may move to higher-income zipcodes. As robustness checks, I show that the results are qualitatively the same when I use eviction notices as an alternative measure of displacement and that there is no impact on moves to other types of destination.

To define gentrification, I aggregate these individual address histories to the parcel level. Land parcels are the smallest stable unit of space in San Francisco, typically corresponding to one or more street addresses in the case of condos and large apartment buildings. A parcel gentrifies if the net change in richer residents (the number of arrivers from richer zipcodes minus the number of leavers to richer zipcodes) is larger than the net change in poorer residents (the number of arrivers from poorer zipcodes minus the number of leavers to poorer zipcodes) (Guerrieri et al., 2013). This definition of gentrification improves upon the more common approach of measuring changes in average income within a Census tract or blockgroup (Couture et al., 2019; Zuk and Chapple, 2016), which cannot be differentiated from neighborhood revitalization (an increase in incomes for incumbent residents).

Combining this rich microdata and identification strategy allows me to causally identify and compare the spatial impact of housing construction on rents, displacement, and gentrification. The microdata enable key distinctions between displacement and gentrification, renters and owners, and impacts of market rate versus affordable construction[8] that are not possible with aggregated data.

Using an original dataset of historic Craigslist rents, I find that monthly rents fall by \$22.77 - \$43.18 relative to trend, roughly 1.2 - 2.3%, for people living within 500m of a new project. This drop in rents precedes a similar decline in displacement risk. On average, an additional project reduces displacement risk by 17.14% for people living within 500m. Using eviction notices as an alternative measure of displacement, I find that landlords of rent controlled units within 100m are 0.77 percentage points (31.09%) less likely to evict tenants after new housing is built, consistent with a reduction in the opportunity cost of rent-controlled leases. These effects persist for at least four years after the new housing is completed.

Together, these findings suggest that the supply effect outweighs any demand effect at every distance from the new construction project: there is no tradeoff between a reduction in average rents and a hyperlocal increase in rents near new construction. However, the demand effect could still be nonzero. To investigate, I examine changes in the probability of crowd-in of additional new construction. If exogenous new construction creates a positive

[8] A large proportion of new buildings in San Francisco include both market rate units and affordable units, often due to incentives like a density bonus that allows larger developments in exchange for including more affordable units (San Francisco's density bonus program is called Home-SF). I classify all construction that includes market rate units as market rate; only construction that is 100% affordable is designated "affordable housing" here.

demand shock, then a standard supply and demand framework predicts developers will build more endogenous housing nearby. I find that the probability that developers file for a new construction permit more than doubles within 100m of new exogenous construction. Similarly, I find that building renovations and business turnover all increase within 100m. These findings support existing evidence that business turnover increases in gentrifying areas (Li, 2019; Glaeser et al., 2020).

The impact on gentrification follows the same pattern as the demand effect. Parcels within 100m of new market rate construction are 2.5 percentage points (29.5%) more likely to gentrify, that is, to experience a net increase in new richer inhabitants. The effect decays linearly to zero within 700m.

Taken together, these findings suggest a supply effect with a wide radius of at least 1 kilometer and a demand effect with a narrower radius. Demand responses like permitting of new endogenously located construction, residential renovations, and business turnover occur within eyeshot of the new construction. This suggests that building new market rate housing actually benefits incumbent tenants by reducing rents, evictions, and the risk of moves to poorer zipcodes. It also attracts wealthier newcomers and new endogenous construction, slowly gentrifying neighborhoods without displacement.

In contrast, I find that affordable housing does not affect spatial trends in rents, displacement, or gentrification nearby. For policymakers who want to reduce both displacement and gentrification, market rate and affordable housing are complementary: new market rate housing reduces rents and displacement nearby through spillover effects, but invites gentrification over time. New affordable housing only prevents displacement for its inhabitants, but it can preserve long-term income diversity in the neighborhood. In section 6, I discuss how these results can help inform policy discussions about what type of new housing to build.

This work contributes to an active urban economics literature on the causes and consequences of displacement and gentrification. Quantitatively, we can think of displacement as a high-interest tradeoff between the present and future. Location is an asset: it determines people's access to education, job opportunities, social networks, living amenities, and housing costs (Bilal and Rossi-Hansberg, 2018). Borrowers can transfer resources to the present by moving to cheaper areas, trading off short-term reductions in housing cost against long-term opportunity. People who are displaced move to less desirable areas – places with lower earning potential (Bilal and Rossi-Hansberg, 2018; Mok and Wang, 2020), worse schools, higher crime, more job turnover (Qiang et al., 2020), and greater exposure to environmental bads like air pollution (Depro et al., 2015). In this way, displacement intensifies and perpetuates preexisting inequality. This paper expands the literature by distinguishing between displacement and gentrification and exploring how they change in the same context.

While a new literature has begun to explore the supply and demand effects of new construction on local housing prices, this paper introduces a new identification strategy and offers the first estimates of how rent, displacement, and gentrification change together. Li (2019) finds that a 10% increase in New York City housing stock causes rents to decrease 1% within 500 feet. Li also finds evidence of a smaller demand effect, with new high-rises attracting new restaurants. Combining data across metro areas, Asquith et al. (2021) find that new construction decreases rents within 250m relative to 250-600 meters away by \$100-\$150 per month and attracts a more income-diverse group of newcomers. They also find evidence of an overshadowed demand effect: new construction increases in-migration from rich areas, but by less than the increase in supply. Both papers rely on the plausibly exogenous timing of completion conditional upon the timing of approval. My work extends these identification strategies to exploit random variation in location as well as timing.

This work also adds to a growing, diverse spatial economics literature that explicitly considers the spatial dynamics of place-based policy interventions. Ignoring spatial spillover effects can lead to large overestimates (Blattman et al., 2017; Ahlfeldt and Maennig, 2010) or even reverse the sign (Englander, 2020; Glaeser and Gottlieb, 2008) of the total policy impact. In this setting, I explicitly study the spatial spillovers from a quasi-randomly assigned housing supply shock.

Finally, this paper contributes to an urban economics literature on the spatial dynamics of the city. Cities represent large investments in durable goods with a coordination problem. Hornbeck and Keniston (2017) argue that negative spillover effects on property values from outdated neighboring buildings depressed renovation in Boston in the late 1800s. The Great Fire in 1872 unlocked a virtuous cycle of simultaneous reconstruction by removing wide swaths of outdated housing stock. They use a regression discontinuity design to identify a treatment effect gradient over distance from the burned area, showing that proximity to a rebuilt plot increases nearby property values and the probability of renovation. Rossi-Hansberg et al. (2010) and Diamond and McQuade (2019) find evidence of spillover effects from neighborhood revitalization programs on nearby housing values and Ahlfeldt et al. (2015) identify positive spillovers from designated landmarks within 600m. Asquith (2016) finds that San Francisco landlords of rent controlled housing respond to exogenous price increases by increasing eviction. While these papers investigate the impact of unique or sequential treatments, this paper identifies the neighborhood effects of concurrent events within the same city.

The next section discusses the conceptual framework. Section 3 describes the data and section 4 discusses the identification strategy and empirical setup. Section 5 presents results. Section 6 uses the results to compare the effectiveness of market rate and affordable construction for reducing displacement. Section 7 discusses policy implications and concludes.

2 Conceptual framework

This paper aims to identify the causal impact of a local housing supply shock on local rents, displacement, and gentrification. The thought experiment is for a policymaker to impose building more housing at a certain location than would occur endogenously. What are the impacts on people who live nearby? As in Asquith et al. (2021) and Diamond and McQuade (2019), I treat neighborhoods as small closed economies, taking as given all other prices and amenities in the city and ignoring potential impacts on city size. These assumptions seem reasonable given the small quantity of construction over the study period, but this framework and these results do not generalize to large supply shocks which may have significant general equilibrium effects. The benefit is that I am able to clearly identify a causal relationship without concern about the many concurrent changes involved with a larger intervention.

In principle, new construction affects displacement through countervailing supply and demand effects that play out over distance from new construction. It creates supply effects by expanding quantity and demand effects by changing neighborhood quality. Let Θ_{ip} denote person i's risk of displacement from parcel p. The net change in Θ_{ip} is a function of the change in the rent R_p at parcel p, which is determined by the distance $d_{p,n}$ between parcel p and construction parcel n, the change in supply S'_n, and the change in quality Q'_n:

$$\Delta\Theta_{ip} = f_i\big(\Delta R_p(d_{p,n}, S'_n, Q'_n))\big) \tag{2.1}$$

Figure 1 shows three potential scenarios in the familiar supply and demand framework. The supply and demand effects could offset each other, so that supply increases but prices stay the same (panel 1a). The demand effect could outstrip the supply effect, causing both supply and price to increase (panel 1b). Finally, the supply effect could dominate the demand effect, so that supply increases and price falls (panel 1c).

Figure 2 displays four general cases for how these supply and demand shifts might play out over space. For simplicity, I depict linear relationships between price and distance, although the true functional form may be more complex. I also only show one example of each case, although other variations are possible for other rates of decay. The goal of these charts is simply to provide a clear visual for how spatial dynamics might operate.

If the net effect is zero at every distance, then the supply effect and demand effect must have the same slope and intercept (panel 2a). If the demand effect dominates the supply effect, then the net gradient will be positive, kink where the supply effect goes to zero, and then decay to zero (panel 2b). If the supply effect dominates the demand effect, then the net gradient will be negative, kink where the demand effect goes to zero, and then decay to zero (panel 2c). Finally, it is possible that the net effect has an inflection point (panel 2d). This scenario captures the concerns of opponents to housing construction: that prices might increase hyperlocally, even if they decrease on average.

3 Data

To estimate these gradients, I explore two main panel data sets: one at the individual level to study displacement and one at the parcel level to study gentrification. I build both panels by combining data at the address level from several different sources for the years 2003-2017.

3.1 Land Parcels and construction

I first build a comprehensive data set containing information on housing units, year built (and consequently rent control status), and zoning for all land parcels in San Francisco. Of the 160,706 total parcels, 81.7% are zoned to permit residential space. I identify renter- versus owner-occupied units using claims for homeowner's exemptions in annual property tax data.

Next, I add internal Planning Department data on new construction. These data include the address, permit date, date certified for occupancy, number of market rate units, and number of affordable units for each new construction project. Table 1 displays summary statistics and Figure 3 shows photographs of typical large market rate and affordable projects. Since 2003, San Francisco has completed an average of 2,060 new units per year, with a stark drop during the Great Recession. Most of these units are market rate, although the number of affordable units has been rising. In 2017, more than 25% of new units completed were affordable. As shown in Figure 4a, most construction happens in the eastern half of the city, which is zoned for larger residential buildings.

3.2 Building fires

I compile information on serious building fires by subsetting the universe of calls for service to the San Francisco Fire Department according to several criteria. First, the call for service must also appear in a separate database of fire incident reports, where it must be classified as an unintentional building fire that required at least 10 units to be dispatched.[9] Second, the incident must appear in Department of Building Inspection complaints or in the description of new construction. Figure 5 shows an example of a serious fire and its damage record, and Table 1 counts the number of these fires in each year. In total, 158 fires serious enough to affect the probability of construction occurred from 2003-2017.

[9] I remove all incidents that the Fire Department categorized as potentially intentional. I select for at least 10 fire units based on a phone conversation on April 19, 2018 with San Francisco Fire Department Chief Information Officer Jesus Mora, who explained that a fire serious enough to impact the probability of redevelopment would require a minimum of 10 fire units.

Combining the fire and construction data yields 47 projects that took place on a burned parcel during the study period. As shown in Figure 4b, these exogenously located projects are distributed over most of the city. To deal with potential selection issues, I limit my sample to the 135,062 parcels that are within 2km of an exogenous construction project. In practice, however, the results are qualitatively unchanged if I use the fulll sample.

3.3 Displacement and gentrification

The heart of this paper relies on individual address histories provided by the consumer data company Infutor.[10] I observe the complete address histories of 1.24 million people who lived in San Francisco at some point during my study period, including their other addresses across the United States. Diamond et al. (2019) show that these data closely match Census tract records, reporting 1.1 adults per adult counted in the Census and performing well within age groups. Adults may be overcounted because Infutor data rely on address change data, which captures moves but not deaths. To address this overreporting issue and to limit my sample to people who are likely to be able to move, I drop individuals with birthyears earlier than 1930. I identify renters as people living at addresses which are not associated with owner occupation tax breaks in the Office of the Assessor-Recorders secured property tax roll.[11]

To define displacement, I use annual zipcode median income data from the Internal Revenue Service to identify moves that are more likely to reflect push migration. I set a displacement dummy equal to one if person i moves to a zipcode with a median income at least 10% lower than their current median zipcode income.[12] I also use this data to proxy for the relative wealth of arrivers and leavers to calculate gentrification as the net change in richer residents. Figure 7 maps the change in the number of residents from richer zipcodes from 2003-2017. Over the course of the study period, one in four parcels gentrified.

Both surveys and research suggest that using moves to poorer zipcodes is an appropriate proxy for displacement. Desmond and Shollenberger (2015) find that renters who report that they did not want to move are more likely to go to poorer neighborhoods than renters who move voluntarily. Surveys from San Francisco, New York, Seattle, and Milwaukee all find that the need for cheaper housing is a primary reason for push migration.[13] More than half of low and moderate income households in San Francisco are rent burdened, that is, spend more than 30% of their monthly income on rent, and most households earning less

[10] I am grateful to the Upjohn Institute for Employment Research for granting me a fellowship to access the Infutor data.

[11] California provides a Homeowners' Property Tax Exemption, which offers a $7,000 reduction in the taxable value of a qualified owner-occupied home.

[12] This cutoff is arbitrary. Results are robust to alternative definitions, such as $\pm$ 1/2 standard deviation. The goal is to make sure that zipcodes with similar incomes are not mechanically classified as either richer or poorer. This approach generates three categories: richer, similar, and poorer.

[13] 2019 Edelman Trust Barometer: Special Report on California, New York City Housing and Vacancy Survey, Puget Sound Regional Council Household Travel Survey Program, Milwaukee Area Renters Study.

than 30% of the Area Median Income (about \$83,000 in 2014) spend over 50% on rent. Figure 6 shows rent burden by income group.[14]

Given the strong correlation between income and housing prices (Couture et al., 2019), this suggests that households who are displaced by high housing prices will move to lower-income areas. It is also consistent with Ding et al. (2016)'s call to focus on the 'quality' of moves rather than the overall mobility rate and Dragan et al. (2019)'s finding that gentrification in New York City predicts moves to lower-quality buildings but not the overall probability of moving. Of course, not all moves to lower-income zipcodes reflect push migration, and some displaced households may move to higher-income zipcodes. I show that the results are qualitatively the same when I use eviction notices as an alternative measure of displacement.[15]Finally, I find evidence supporting the use of median zipcode income to create a gentrification measure in my data. People who move into new affordable housing, which is income restricted, are 23.87 percentage points less likely to come from rich zipcodes (p = 0.00).

3.4 Rental prices

The city of San Francisco does not track rental prices. I construct an original panel of historic rental prices by scraping archived Craigslist ads from 2003-2017. These ads are archived by a nonprofit called the Wayback Machine, which sporadically saves versions of web pages on random dates. I access archived Craigslist search results for housing, scraping information on neighborhood, price, and number of bedrooms. A typical entry reads something like, "\$2995 2BR REMODELED FURNISHED 2BR/1BA Corner of Mission/Potrero/Design Districts." I first construct rents at the neighborhood level and then interpolate them using distance weights to the parcel level. I discuss this procedure in detail in Appendix 12.1. Figure 8 shows the dramatic increase in rental prices over the study period, from an average of \$1,307 for a one bedroom apartment in 2003 to \$2,573 in 2017.

Creating this data has two advantages. First, it allows me to observe changes in prices at a fine geographic scale. Other rental price data are available only at larger spatial scales, such as Census blockgroup or county, and are sometimes averaged over time, as in the American Community Survey. Second, different data sources capture different segments of

[14]SF City Planning Department analysis of American Community Survey 2011-2014 estimates.

[15]This approach is also consistent with extensive work in sociology and urban planning. Carlson (2020) reviews the three most common strategies for measuring displacement: a "population approach" that measures changes in neighborhood demographics over time; an "individual approach" that tracks individual moves; and a "motivational approach" that observes both individual moves and the reasons for those moves. The choice of a proxy is usually determined by data availability, but it has first-order implications for the results. Carlson uses data from the New York City Housing and Vacancy Survey to show that the population approach of measuring demographic change within an aggregated spatial unit, such as an American Community Survey Public Use Microdata Area (PUMA) or a Cenus blockgroup, has almost zero correlation with a motivational measure ($\rho = 0.06$). The individual approach performs better, with a correlation of $\rho = 0.64$.

the housing market. The renters who are most vulnerable to displacement are more likely to use Craigslist than Zillow, which caters to higher-income renters. The average 1 bedroom rent in the Craiglist data is \$2,759 compared to \$3,422 in the Zillow data over the period 2014-2017.[16] Figure 8 shows that the Craigslist data tracks median rent data released by the United States Department of Housing and Urban Development, which combine ACS estimates and data from other sources. It also shows that Zillow rental price data, available beginning in 2011, is consistently higher than the Craigslist rents.

3.5 Other measures of displacement and gentrification

As a robustness check, I compile address-level data on eviction notices from the San Francisco Rent Board as an alternative measure of displacement. In Carlson (2020)'s analysis of the New York City Housing and Vacancy Survey, difficulty paying rent accounted for 59% of push migration and eviction accounted for 8%. This suggests that between my two proxies for displacement, I capture the majority of distress moves. However, it is important to note that these data do not perfectly capture evictions: some landlords evict tenants without going through the formal process (indeed, Carlson (2020) finds that 5% of unwanted moves were driven by harassment by the landlord), and not all eviction notices convert into an actual eviction because tenants have the opportunity to redress the issues cited in the notice.

I will also evaluate changes in the probabilities of other types of moves, including moves to richer zipcodes, moves away from the Bay Area, and any move.

Next, I assemble data that can help capture neighborhood change via demand effects. I observe residential renovations using records from the Department of Building Inspection, property sales from annual Assessors Data, and business turnover using records of business registrations and closures from the Office of the Treasurer-Tax Collector.

4 Research design

The clear identification challenge is that the location and timing of new construction are endogenous: developers prefer to build in areas where rents, displacement, and gentrification are already increasing (Green et al., 2005; Boustan et al., 2019; Asquith et al., 2021). I consider a profit-maximizing developer who wants to build N units in a given neighborhood and year. Because of San Francisco's strict zoning regulation, the choice of neighborhood and N units is joint: in each neighborhood, only a certain size project can be undertaken. The developer then searches across available parcels to find the cheapest location.

I exploit exogenous variation in the location of new construction caused by serious building fires. Regulation and geography make San Francisco one of the most difficult places to build housing in the United States (Albouy and Ehrlich, 2012; Saiz, 2010). Serious fires,

[16] Calculated using publicly-available Zillow data at the zipcode level.

like the one shown in Figure 5, increase the probability of construction on the burned parcel by making it cheaper to build there. Removing incumbent tenants eliminates the need for costly buyouts: under San Francisco just cause eviction law, landlords who want to sell or redevelop must either wait for tenants to voluntarily leave or negotiate a buyout. In 2015, the median buyout per tenant was $18,000 and the maximum was $325,000.[17] Serious fires also streamline the permitting and construction process. Controlling for project size, construction on unburned parcels takes nearly a year longer to complete than projects on burned parcels (p=0.007). During the study period, 27.22% of burned parcels receive new construction compared to 1.11% of unburned parcels. Controlling for microneighborhood and year, this is a 32-fold increase in the probability of construction (p=0.0000).

While I can use these fires to predict construction location, the limited study window means I cannot use them to predict construction timing. First, I cannot identify post-fire construction projects whose fires occurred before the fire data begins in 2003. Second, the long lag between fires and redevelopment means that not every burned parcel is redeveloped during the study period. On average, 4.8 years pass between the fire and the permit application for new construction (sd 3.6) and 7.2 years before completion (sd 4.2). Figure 11 displays variation in time to construction. Many – perhaps all – of the burned parcels will ultimately be redeveloped, but the study window is too limited to predict the timing with precision.

Instead, I exploit exogenous variation in the timing of project permitting and completion due to bureaucracy and construction management (Mense, 2021; Li, 2019; Asquith et al., 2021). Developers may target year Y_0 for permitting and year Y_1 for completion, but in reality there is wide variation in project completion times (see Figure 11).

This identification strategy relies on within-neighborhood, within-year exogenous variation in proximity to new construction. The thought experiment is for a developer who wants to target a given neighborhood for N units of construction in a given year to randomly select parcel A instead of nearby parcel B. The counterfactual is for the developer to select parcel B instead of parcel A.

To operationalize this identification strategy, I use the incidence of serious fires to identify the subset of new construction whose location is plausibly exogenous within a microneighborhood. I identify 47 parcels that receive market rate projects and 11 parcels that receive affordable projects after a fire.[18] I will estimate the effect of proximity to new construction using only these exogenously located projects and a set of microneighborhood-by-year controls to ensure that I exploit variation within neighborhood-years.[19]

The identifying assumptions are that serious fires 1) increase the likelihood of construc-

[17] San Francisco Open Data, accessed 2 October 2019.

[18] Table 1 reports a total of 60 exogenous projects because one parcel receives multiple projects.

[19] The Appendix includes results for endogenously located projects for comparison. Proximity to endogenously located construction has no differential impact on rents, displacement, or gentrification.

tion on that parcel relative to other parcels within the same 1 km^2 microneighborhood, and 2) are unrelated to trends in rent, displacement, and gentrification. To provide evidence for the second assumption, I conduct a series of balance tests. Table 2 shows baseline characteristics for parcels near burned versus unburned parcels and redeveloped versus not redeveloped parcels, controlling for microneighborhood-year. In the years before the fire, parcels within the 100m neighborhood of the fire parcel are no more likely to see residents move to poorer zipcodes than parcels further away. Similarly, before redevelopment, the burned parcels that will be redeveloped within the study period are no more likely to see residents move to poorer zipcodes than burned parcels that will not be redeveloped yet. Other characteristics are similar as well: there is no significant difference in rents, mean zipcode income, the number of residential units, Infutor population, distance to downtown and train station, building renovations, or eviction notices. The exception is that serious fires are more likely in neighborhoods with older buildings (mean year built = 1927 versus 1933, p = 0.043) and where evictions are more likely (mean eviction notice = 0.010 versus 0.006, p = 0.011). These differences do not exist between redeveloped and undeveloped parcels, which is the time comparison I exploit.

Finally, I verify that the direct impacts of these fires have faded by the time new construction is completed, so that the results reported here are not muddied by direct fire effects. Figure 12 displays results from an event study (the specification is described in detail in the following section), showing 1) there is no relationship between proximity to a future fire and rents; 2) rents fall differentially near fires from year 0 to 3; 3) the differential fades four years after the fire. This result also indicates that the construction on burned parcels should be interpreted as increasing the number of units from 0 to N, rather than from the pre-fire number of units to N.

4.1 Building the empirical specification

The empirical approach in this study differs from other recent work on construction and housing prices in two key ways. First, parcels can be treated more than once as new projects are completed over time. The treatment intensity of parcel p with respect to construction project n varies with time since completion, project size S'_n and change in neighborhood quality Q'_n. Parcel p's total treatment intensity in year t is a function of its exposure over time and distance to all construction projects $n \in N$.

Capturing this complexity requires a new approach that does not rely on static measures of treatment intensity and clear divisions between the pre- and post-periods, as in Asquith et al. (2021), Li (2019), Diamond and McQuade (2019), and Hornbeck and Keniston (2017). Here, I capture treatment exposure using distributed leads and lags in both time and distance. In each year, I count the number of projects and housing units completed in a set of distance bins. Figure 9 shows the construction of these binned treatment measures for an example parcel in an example year. The parcel would have a value of 1 for the number of projects within 0-200m, 0 for projects within 200-400m, and 1 for projects within 400-600m. Similarly, it would have a value of 6 for units within 200m, 0 within 200-400m, and 200 within 400-600m. Figure 10 maps this approach over the city of San Francisco for the years 2015 and 2016. To deal with potential selection issues, I limit the sample to parcels that are within 2km of exogenous construction at some point in the study period, although the results using the universe of parcels are very similar.

The second difference in this empirical setup is that the main outcome of interest is a binary event, rather than a continuous surface of housing prices. Moving is rare: most people never move, or move only once. The average rate of moving is 4.45% per year and the average rate of moves to poorer zipcodes, my proxy for displacement, is only 1.03%. After leaving San Francisco, individuals exit the sample.

Survival models are designed to study rare events like this, where the dependent variable is usually zero and occasionally one, after which the individual exits the study. I build a Cox proportional hazards model with time-varying covariates to study the impact of each explanatory variable on the treatment (proximity to new construction) on the probability of failure (moving to a poorer zipcode). Coefficients are reported as risk factors r, with $r = 1$ indicating no change in risk, and $r < 1$ indicating a reduction of $1 - r$. By construction, these comparisons are made within the same calendar year (analogous to including year fixed effects in a linear specification). I allow the baseline hazard of moving to a poorer zipcode to vary by birth decade and sex within each microneighborhood (analagous to birth decade by sex by cell fixed effects in a linear specification). The results from a linear probability model are similar (Appendix 10.3).

I construct the hazard of moving to a poorer zipcode for person i living in parcel p in microneighborhood c in year t as a function of $\lambda_{0sbc}(t)$, the baseline hazard for a person of sex s born in decade b living in microneighborhood c; X_{ipt}, how long person i has lived at parcel p; and X_p, parcel-level controls including latitude and longitude, rent control status, distance to the financial district and Caltrain station,[20] landuse zoning, 2010 Census tract median income tercile, and a quadratic in residential units; and exposure to new construction.

I begin by exploring the relationship between new construction and displacement using distributed lags and leads in both time and distance. In the next section, I will use these flexible event studies to refine a condensed specification that uses the data more efficiently – including a large set of lags and leads forces me to drop observations of early and late years and reduces power.

In the event study specifications, I include variables to capture the number of market rate and affordable construction projects completed each year in a set of distance bins out to

[20] Caltrain is a train running from San Francisco to Silicon Valley, a second hub for high-paying jobs in the Bay Area.

2km. Each of these variables $mkt200_{p,t,d,k}$ counts the number of completed projects within d meters of parcel p that were completed in calendar year t and event year k. Year $k = 0$ refers to the year in which construction is completed. For example, if parcel p is within 100m of a market rate project completed in 2003 and within 100m of another project completed in 2005, then $mkt100_{p,2003,100,0} = 1$, $mkt100_{p,2003,100,-1} = 0$, and $mkt100_{p,2003,100,-2} = 1$. In the next year, $mkt100_{p,2004,100,1} = 1, mkt100_{p,2004,100,0} = 0, mkt100_{p,2004,100,1} = 1$, and $mkt100_{p,2004,100,2} = 0$.

I include separate counts for new market rate and new affordable construction to allow them to have different effects. To manage the number of spatial leads and lags, I use smaller bins close to the new construction, and larger bins as I move further way. This is consistent with the conceptual framework, which permits a change of sign over very small distances close to the project but predicts a stable sign beyond any potential inflection point (see Figure 2). Distance bins d within 1km of parcel p are 100m wide ($mkt100_{p,t,d,k}$ and $aff100_{p,t,d,k}$); distance bins from 1-2km are 200m wide ($mkt200_{p,t,d,k}$ and $aff200_{p,t,d,k}$). Finally, I restrict the sample to renters who have lived in their current unit for at least one year to avoid selection issues.[21]

The estimating equation is:

$$
\begin{aligned}
move\,poorer_{ipct} = \sum_{k=-2}^{3}\Big(\sum_{d=100}^{1000}&\Big[\alpha_{dt} mkt100_{p,t,d,k} + \beta_{dt}\, aff100_{p,t,d,k}\Big] + \\
\sum_{d=1000}^{2000}&\Big[\alpha_{dt} mkt200_{p,t,d,k} + \beta_{dt}\, aff200_{p,t,d,k}\Big]\Big) + \\
&\lambda_{sbc0}(t) + X_{ipt} + X_p + \epsilon_{pct}
\end{aligned}
\tag{4.1}
$$

To examine the effect of new construction on rents and other parcel-level outcomes, I use a similar ordinary least squares specification on the parcel panel. I include microneighborhood by year fixed effects γ_{ct} and the same set of parcel controls X_p:

$$
\begin{aligned}
rent_{pct} = \sum_{k=-2}^{3}\Big(\sum_{d=100}^{1000}&\Big[\alpha_{dt} mkt100_{p,t,d,k} + \beta_{dt}\, aff100_{p,t,d,k}\Big] + \\
\sum_{d=1000}^{2000}&\Big[\alpha_{dt} mkt200_{p,t,d,k} + \beta_{dt}\, aff200_{p,t,d,k}\Big]\Big) + \\
&\gamma_{ct} + X_p + \upsilon_{pct}
\end{aligned}
\tag{4.2}
$$

In both specifications, I cluster standard errors at the microneighborhood level to address spatial correlation. Alternatively, I can correct standard errors for spatial correlation using randomization inference as described in Appendix 12.2. I report clustered standard errors here because they are more conservative.

[21] If some individuals select into the nearby area post-treatment, and they have a different rate of churn, then this selection issue would make it difficult to interpret the displacement results.

4.2 Event study results

Since I expect new construction to affect displacement through housing prices, I begin with event study plots from Equation 4.2. Figure 13 shows that there is no pre-trend in rents during the two years before construction is completed. A clear distance gradient emerges the year before completion, with rents within 100m falling by $100 per new project and decaying to zero over distance and persisting over time.[22] Seeing effects begin the year before completion is consistent both with nuisance effects of construction and with the sale of units before completion. Running the same specification for lags $k \in [4, 9]$ suggests that rents remain differentially lower for at least 9 years.[23]

Next, I plot the impacts of new construction on displacement from Equation 4.1. Figure 14 shows that person i's hazard of moving to a poorer zipcode follows the same pattern: a distance gradient emerges in year $k = -1$, the year that rents begin to fall. Using lags $k \in [4, 9]$, I find that the effects persist for 5 years.

4.3 Main specification

These event plots suggest that the main impact on displacement occurs over the period $k \in [0, 4]$. They also suggest that the distance gradient is approximately linear. Accordingly, I will now condense the event study specification to estimate the average effect of exposure during the effect window $k \in [0, 4]$. This allows me to eliminate the temporal leads and lags, reducing the number of coefficients of interest from more than 200 to 30. This condensed specification is both better-powered and easier to interpret.

To study the average effect of exposure to new construction, I construct a measure of cumulative exposure to new construction during the effect window $k \in [0, 4]$. These new mkt and aff variables capture the sum of construction completed within a rolling five-year window. For example, if parcel p is within 100m of a market rate project completed in 2003 and within 100m of another project completed in 2005, then $mkt100_{p,2003,100} = 1$, $mkt100_{p,2004,100} = 1$, and $mkt100_{p,2005,100} = 2$. The streamlined specification is:

[22] See the Appendix for similar plots for the impact per new unit of housing, rather than per new project.

[23] I run this longer-term specification separately to preserve statistical power: including lags $k \in [-2, 9]$ would limit me to studying projects completed from 2005-2008 because the study window only runs from 2003-2017. These longer-term plots, shown in the Appendix, suggest that rents remain differentially lower for at least 9 years, while impacts on displacement risk decay to zero by $k = 5$.

$$
\begin{aligned}
move\ poorer_{ipct} = &\sum_{d=100}^{1000} \alpha_d mkt100_{p,t,d} + \beta_d aff100_{p,t,d} + \\
&\sum_{d=1000}^{2000} \alpha_d mkt200_{p,t,d} + \beta_d aff200_{p,t,d} + \\
&\lambda_{sbc0}(t) + X_{ipt} + X_p + u_{pct}
\end{aligned} \tag{4.3}
$$

The corresponding OLS specification is:

$$
\begin{aligned}
y_{pct} = &\sum_{d=100}^{1000} \alpha_d mkt100_{p,t,d} + \beta_d aff100_{p,t,d} + \\
&\sum_{d=1000}^{2000} \alpha_d mkt200_{p,t,d} + \beta_d aff200_{p,t,d} + \\
&\gamma_{ct} + X_p + e_{pct}
\end{aligned} \tag{4.4}
$$

5 Results

5.1 Displacement

The results from Equation 4.3 show a clear distance gradient. Figure 15 shows that both rents and the risk of adverse moves plunge for people living near new market rate construction. On average, being within 100m of an additional new project reduces rent by \$28.03. The risk of displacement falls by 17.14%. This effect decays roughly linearly, disappearing completely around 1 kilometer. Figure 16 shows that for each additional housing unit within 100m, rents fall by \$0.20 and displacement risk falls by 0.10%.

Displacement refers to push migration. It is possible that these results reflect a uniform decrease in moving, perhaps through a demand effect: if neighborhood quality improves, people become less likely to want to leave to any sort of destination. If these results are truly capturing a decrease in the risk of displacement, the risk of adverse moves should fall relative to the probability of an advantageous move. Figure 17 compares impacts on moves to different types of destinations. There is no meaningful impact on moves outside the Bay Area, moves to richer zipcodes (at least 10% above current zipcode income), or on the combined probability of any type of move. Proximity to new construction only affects the probability of adverse moves, consistent with the hypothesis that it decreases displacement by lowering nearby housing prices. These findings provide evidence for the existence of a supply effect which decays over distance, and suggest that the supply effect persists for longer than the demand effect.

Alternatively, I can proxy for displacement as the probability of receiving an eviction notice.[24] I find that the probability of an eviction notice at a rent controlled unit drops by 0.77 percentage points (31.09%) within 100m of a new project (Figure 18), while there is no impact on uncontrolled units. The finding that evictions decrease only for rent controlled units is consistent with a supply effect that reduces the opportunity cost of a rent-controlled tenant. While landlords of uncontrolled apartments can change the rent continuously, landlords of rent controlled apartments are limited by the initial rent stated on the lease. They can only meaningfully raise the rent when they sign a new lease with a new tenant. Asquith (2016) finds that landlords respond to exogenous housing price increases by increasing evictions in rent controlled units. I identify the other side of the coin: when prices fall, landlords reduce eviction.

Next, I examine the impact of exogenously located affordable housing. Figure 19 shows that there is no clear change in this spatial pattern after new affordable construction. Rents near new construction are roughly \$50 higher in year 2, but this matches the pattern in year -2. Similarly, Figure 20 shows proximity to affordable projects does not affect moves to poorer zipcodes. This is consistent with the supply effect hypothesis. Theoretically, affordable housing projects may have a demand effect but no supply effect. They do not increase the market rate housing stock, but they do randomly change neighborhood quality by transforming a damaged building into affordable housing. These results show that the net impact of affordable housing is weakly positive, increasing prices insignificantly and leaving displacement risk unchanged.

These results suggest that displacement risk is highly price elastic. When rents fall by roughly 2% (\$40), the risk of moving to a poorer zipcode falls by about 20%, an elasticity of approximately 10.[25] This high price elasticity is consistent with San Francisco's high levels of rent burden, especially among households earning less than the Area Median Income (AMI) as shown in Figure 6. Most households earning less than half of the AMI spend over 30% of their monthly income on rent, and a small reduction could plausibly make the difference between staying in their current housing and being displaced.

5.2 Demand effects

The decrease in rents near new construction indicates that the supply effect dominates, but the demand effect could still be nonzero. I assemble a set of alternative dependent variables to pinpoint changes in demand.

If new construction improves neighborhood quality, then the traditional supply and

[24] It is important to note that eviction notices are not evictions: tenants can redress the issues in a just-cause eviction notice (overcounting) and landlords can pressure tenants to leave outside of the formal eviction process (undercounting). Still, eviction notices provide a useful alternative measure of displacement.

[25] Instrumental variables offers another strategy for identifying this elasticity. The IV recharacterizes the rent results as the first stage and the displacement results as the reduced form, estimating a rent elasticity of displacement of 14.4. However, the exclusion restriction would not hold unless building completion affects displacement only through rents. For a longer discussion, see Appendix 11.

demand framework predicts that supply will increase beyond the initial shock (Figure 1c). To test this, I ask whether developers become more likely to permit new projects near exogenously located construction by running equation 4.4 on a dummy for new permits. I find that the probability of new endogenous construction more than doubles within 100m of new projects (Figure 22).

Residential building upgrades offer another way to test for a demand effect. Hornbeck and Keniston (2017) show that rational building owners internalize positive spillovers by improving their own building quality. Accordingly, I test whether proximity to new construction affects the probability of residential renovations and business turnover. Figure 22 shows large spikes in the probability of a residential renovation (16%) and business turnover (22%) within 100m. The effect drops to zero immediately. These results support Li (2019)'s finding that restaurant openings increase within 500m of new high rises in New York City and Glaeser et al. (2020)'s finding that business turnover increases in gentrifying areas.

5.3 Gentrification

Gentrification refers to demographic change within a small spatial unit. New market rate construction could impact gentrification through a direct effect if the people who move into the new building are richer and through spillover effects that may attract richer newcomers to the surrounding housing stock.

I begin by exploring direct effects: who moves into the new buildings? I identify 22,730 people who move into newly constructed housing units during the study period, of whom 9,696 moved into exogenously located construction. I construct a dummy variable equal to 1 if person i came from a richer zipcode. Then I run a descriptive, cross-sectional regression to explore whether people who move into exogenously located new market rate or new affordable housing are more likely to be from richer zipcodes:

$$from\, richer_{ip} = \alpha + \beta exog_p + \gamma aff_p + \delta exog_p \times aff_p + e_i \tag{5.1}$$

Next, I explore impacts in the panel. I limit the sample to all arrivers in their year of arrival, including those who move into existing housing as well as new construction. I include the familiar set of microneighborhood by year, individual, and parcel controls:

$$from\, richer_{ipct} = \tilde{\beta} exog_p + \tilde{\gamma} aff_p + \tilde{\delta} exog_p \times aff_p + X_i + X_p + \gamma_{ct} + \tilde{e}_i \tag{5.2}$$

Table 3 compares the results from the cross-sectional and panel regressions. In the cross-section, I find that arrivers to exogenous market rate construction are 3.54 percentage points more likely to come from richer zipcodes, while arrivers to exogenous affordable construction are 23.87 percentage points less likely to come from richer zipcodes. In the panel, I find that arrivers to exogenously located market rate housing are 9.6 percentage points more likely to come from a richer zipcode, compared to arrivers to other types of housing. Arrivers to exogenous affordable housing are 12.2 percentage points less likely to come from richer sending zipcodes. New market rate construction attracts richer newcomers, while new affordable construction houses lower-income newcomers.

Next, I test for neighborhood spillover effects on gentrification. First, I aggregate individual address histories to the parcel level. In each parcel-year, I calculate the net number of nricher arrivers as $(arrivers\_richer_{pt} - movers\_richer_{pt}) - (arrivers\_poorer_{pt} - movers\_poorer_{pt})$.

The previous section showed that exogenous market rate construction reduces displacement. However, gentrification can occur without displacement, if willing movers are replaced by higher income arrivers. The identification of a demand effect within 100m suggests that, even though the rate of adverse moves has fallen, newcomers may be different.

This is precisely what I find. Figure 23 shows that parcels within 100m of market rate construction receive one additional richer arriver per new project, corresponding to a 29.5% increase in the probability of gentrification. The effect decays to zero within 700m. Exogenously located affordable construction does not have a significant differential impact on gentrification.

What is driving this increase in gentrification? I decompose the gentrification definition by studying each term separately. Figure 24 plots results for richer arrivers, richer leavers, poorer arriver, and poorer leavers. The gentrification effect is driven by a net increase in arrivers from richer areas.

6 Policy implications

This paper shows that there is no tradeoff between local price increases and average price decreases from building new housing. New market rate housing benefits all nearby renters through its price effects, and new affordable housing benefits its occupants. A natural next question is: should cities with limited resources build more market rate or affordable housing?

This paper can offer suggestive insight, with an important caveat. The identification strategy is based on local effects from small events. The drawback is that it cannot identify general equilibrium effects of large construction projects. If policymakers dramatically increased housing production, local effects might be very different – for example, it might attract newcomers who would not otherwise have come. The benefit of the identification strategy is clear causal identification without the confounders of concurrent changes that come with large-scale interventions.

The results from studying small housing shocks suggest that market rate and affordable housing are complementary policy levers. Market rate housing has meaningful spillover effects on nearby rents, displacement, and gentrification, while affordable housing does not. However, a full comparison of the neighborhood impact of market rate and affordable housing must also consider their direct effects. The market rate housing studied here has no direct displacement effects because it is built on parcels with no residents. It directly increases gentrification because the people who move in tend to be richer and indirectly increases gentrification through spillover effects on the surrounding neighborhood. Affordable housing directly reduces the displacement risk of the people who live there and prevents gentrification by reserving units for low-income households. What do these results mean for policymakers who want to reduce displacement and gentrification?

Two approaches to answering this question are welfare calculations and estimates of prevented displacement events. I focus on counting prevented displacements because they directly capture the outcome that policymakers care about, although welfare calculations are included in Appendix 12.

I begin by comparing the displacement impacts of market rate and affordable construction. I make a back-of-the-envelope calculation of the number of moves to poorer zipcodes prevented by the spillover effects of market rate construction by multiplying the average effect of exposure over the study period times the number of renters: $\sum_0^{2000} \widehat{\alpha_d} * \overline{mkt100}_{p,d,t} \times n$. Next, I make a generous estimate of the direct effect of affordable housing as the number of units built from 2003-2017 times four people per unit. Since many of these units are studios and one-bedrooms, this should yield an overestimate.

This exercise suggests that roughly 56,000 moves to poorer zipcodes were prevented by the spillover effects of market rate construction, compared to 36,000 prevented by the direct effect of affordable construction. Next, I divide each estimate by the number of housing units built. I find that new market rate construction prevented 14.27 moves to poorer zipcodes per new unit, compared to 4 moves to poorer zipcodes per unit of affordable housing (by construction).

There are three important caveats. First, the people who select into affordable versus market rate housing are different, so I am necessarily comparing prevented moves among two different groups. Second, the effectiveness of market rate construction rent spillovers depends on the city's current income distribution and rental price level. As the city's demographics change, so will the magnitude of these spillover effects. Third, this analysis abstracts from general equilibrium effects which might become important if the city scaled up construction significantly.

Next, I compare impacts on gentrification. Although market rate construction prevents more moves to poorer zipcodes per unit under current conditions, it is less effective than affordable housing at targeting and preserving long-term income diversity. Its spillovers accrue to anyone living nearby, regardless of their displacement risk. As neighborhoods gentrify, the beneficiaries of these lower rents will be less and less in need of support. In contrast, affordable housing targets people at a high displacement risk by basing eligibility on income. It can also achieve long-term income diversity by retaining lower-income people permanently, while market rate housing contributes to gradual gentrification. For policymakers interested in reducing both displacement and gentrification, market rate and affordable housing are complementary.

7 Conclusion

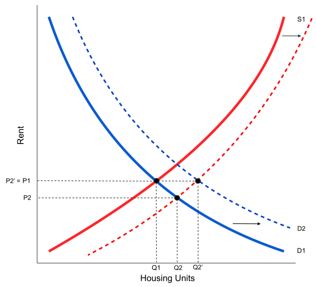
This paper explores the spillover effects of new housing construction in San Francisco from 2003-2017. Like many gentrifying cities across the United States, San Francisco is locked in a policy debate over how to achieve housing affordability. New market rate construction has become politically divisive as advocates debate whether its aggregate supply effects outweigh its potential local demand effects.

This paper provides evidence that there is no tradeoff between aggregate and local effects: the supply effect is larger than the demand effect at every distance from the new construction. However, a hyperlocal demand effect exists within a narrow radius of 100m, i.e., within eyeshot of the new construction. Within this narrow band, building renovations and business turnover increase. The upgrade in neighborhood quality[26] attracts higher-income newcomers, so that when incumbents move out, they are more likely to be replaced by wealthier newcomers. In San Francisco, new market rate housing increases gentrification and reduces displacement.

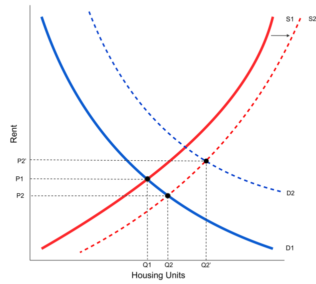
These findings highlight that market rate and affordable housing construction are complementary. Building more market rate housing benefits all San Francisco renters through spillover effects on rents. However, these spillover effects do not reduce gentrification and they may not continue to reduce displacement in the long term. As the city gentrifies over time, these reduced rents will become less effective at retaining lower-income people because there will be fewer low-income people to retain. Affordable housing can effectively reduce both displacement and gentrification by targeting people at higher risk of displacement and preserving housing for low-income people. The high rent elasticity of displacement also suggests that policies like rental assistance or a universal basic income (UBI) could be efficient, cost-effective ways to meaningfully reduce displacement and preserve income diversity in the short term while more housing is built.

[26]Neighborhood quality here could mean either the upgrade of physical housing stock, or the arrival of higher-income residents to the new building. Guerrieri et al. (2013) find that universal preferences for wealthy neighbors can drive gentrification.

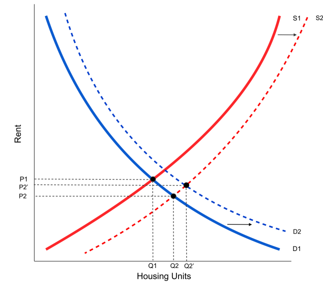
8 Figures



(a) Net Neutral



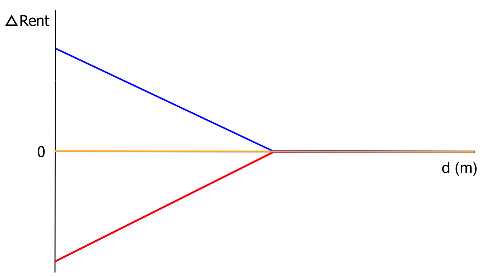
(b) Net Positive



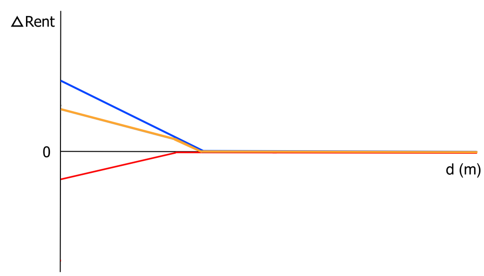
(c) Net Negative

Figure 1: Supply and Demand Scenarios

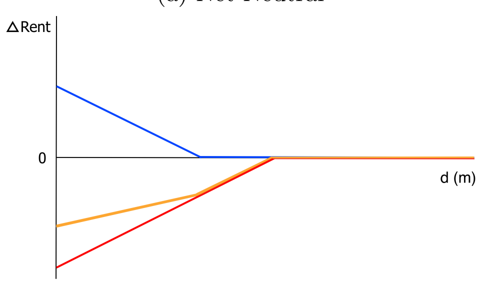
Note: These plots show three theoretically possible scenarios for the supply and demand effects of new construction. The goal of this paper is to identify which of these theoretical scenarios actually occurred.



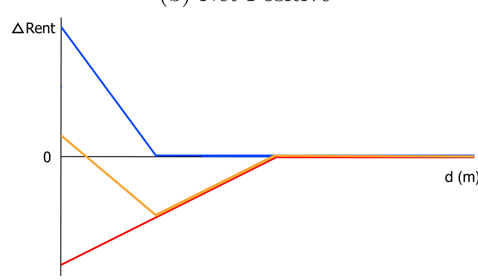
(a) Net Neutral



(b) Net Positive



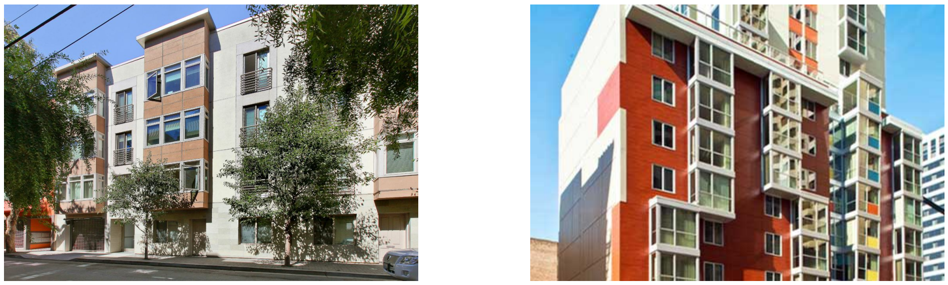
(c) Net Negative



(d) Inflection Point

Figure 2: Spatial Supply and Demand Scenarios

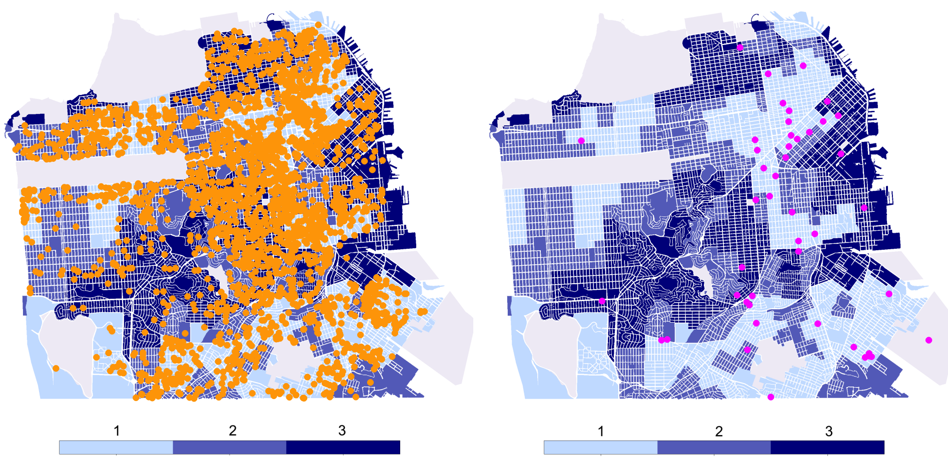
Note: These plots show examples of four theoretically possible cases for combinations of supply and demand effects over space. The demand effect is shown in blue, the supply effect in red, and the net effect in gray. The goal of this paper is to identify which of these theoretical scenarios actually occurred.



(a) Market Rate (b) Affordable

Figure 3: Examples of New Construction

Note: The first picture shows 329 Bay St, a 21-unit market rate building completed in 2007. The second picture shows 125 Mason St, an 81-unit affordable housing building completed in 2008.



(a) Endogenously Located Construction (b) Exogenously Located Construction

Figure 4: New Construction and Income Tercile

Note: These figures map construction against 2010 income terciles by Census tract. Panel (a) plots endogenously located construction in orange and panel (b) plots exogenously located construction in pink. Gray areas are parks and lakes, the large former military base neighborhood called the Presidio (northwest), and Hunters Point Shipyard (southeast).

(a) Five-Alarm Fire at 1502 Golden Gate Ave, 2011

(b) Record of Fire Damage Complaint

Figure 5: Example of a Serious Building Fire

Note: These figures given an example of a serious building fire and its damage record from 2011. Serious building fires are identified by crossreferencing the San Francisco Fire Department's calls for service with incident reports and Department of Building Inspection complaints. To qualify, the incident must be classified as an unintentional building fire requiring at least 10 units to be dispatched and to appear in records of building complaints or in the description of new construction.

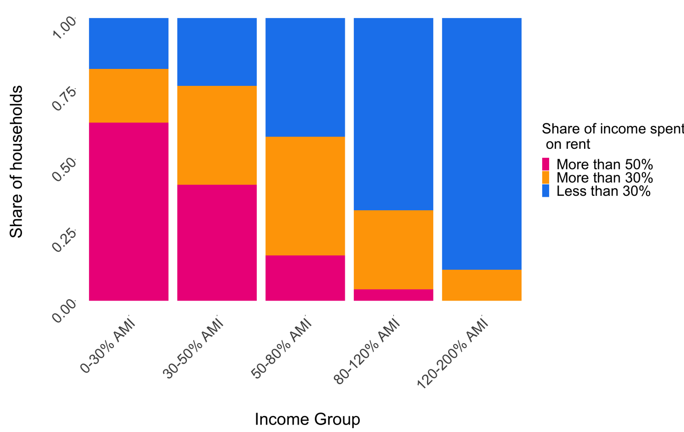


Figure 6: Rent Burden and Income

Note: This figure shows the share of households in five income groups who are 'rent burdened.' The data come from a San Francisco City Planning Department analysis of 2011-2014 American Community Survey estimates.

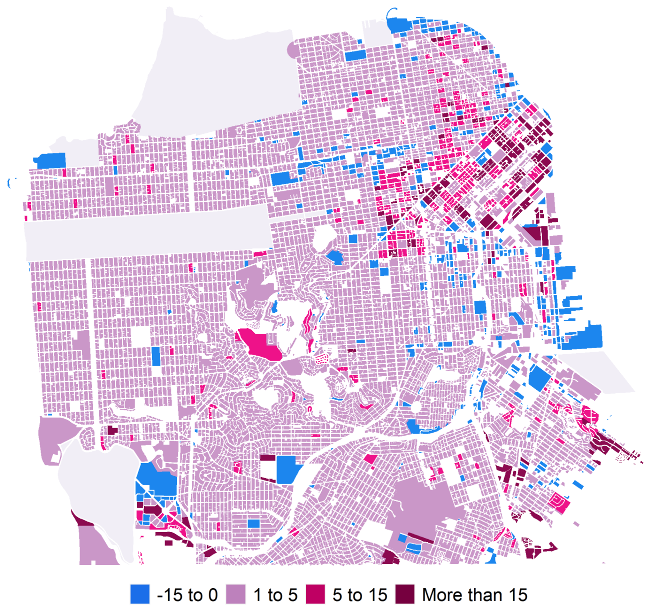
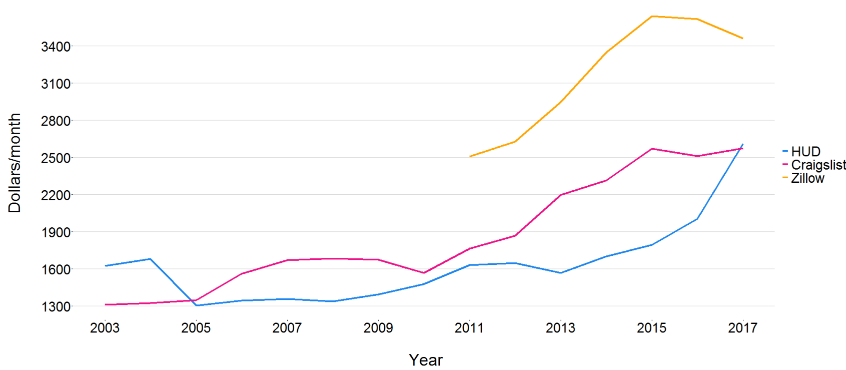


Figure 7: Change in Residents from Richer Zipcodes, 2003-2017

Note: This figure maps the difference in the 2003 and 2017 count of residents whose previous address was in a richer zipcode. For visibility, the colors display the maximum count per block rather than the count per parcel.

Figure 8: 1BR Rents from Craigslist, HUD, and Zillow



Note: This plot compares the median 1BR Craigslist rent from my data collection process with median estimates from the Department of Housing and Urban Development (HUD) and mean estimates from Zillow (Zillow does not provide median rent). HUD estimates combine gross rent data from the U.S. Census Bureau, gross rent data from the American Housing Survey, and yearly telephone surveys. Zillow's methodology is discussed here.

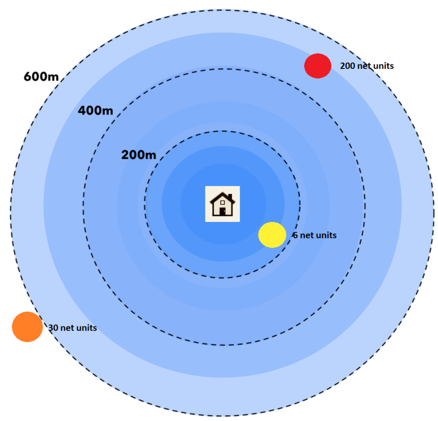


Figure 9: Measuring Treatment Exposure

Note: This figure shows shows the construction of treatment measures for an example parcel in an example year. This observation would have a value of 1 for the number of projects within 0-200m, 0 for projects within 200-400m, and 1 for projects within 400-600m. Similarly, it would have a value of 6 for netunits within 200m, 0 within 200-400m, and 200 within 400-600m.

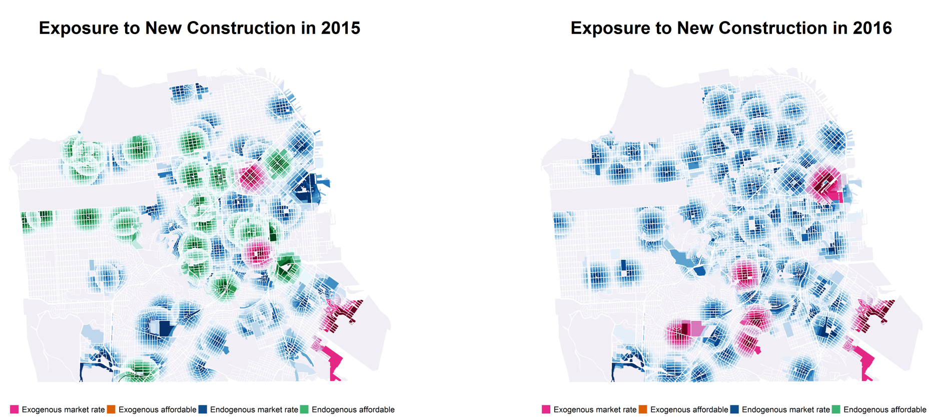
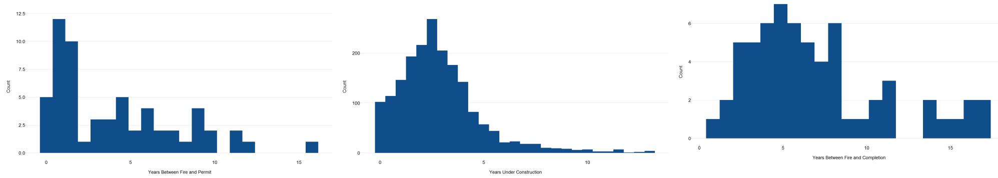


Figure 10: Variation in Treatment Exposure by Distance Bin

Note: These figures visualize treatment exposure measured by counting completed projects within distance bins. For visual clarity, the bins pictured here begin at 50m and end at 600m. My specifications include distance bins out to 2km. These figures show that many parcels are exposed to more than one construction project at a time – parcel p might have $bin_{50,p,t} = 2, bin_{200,p,t} = 1$, and $bin_{550,p,t} = 1$, for example.



(a) Years from Fire to Permit (b) Years Under Construction (c) Years from Fire to Completion

Figure 11: Project Duration

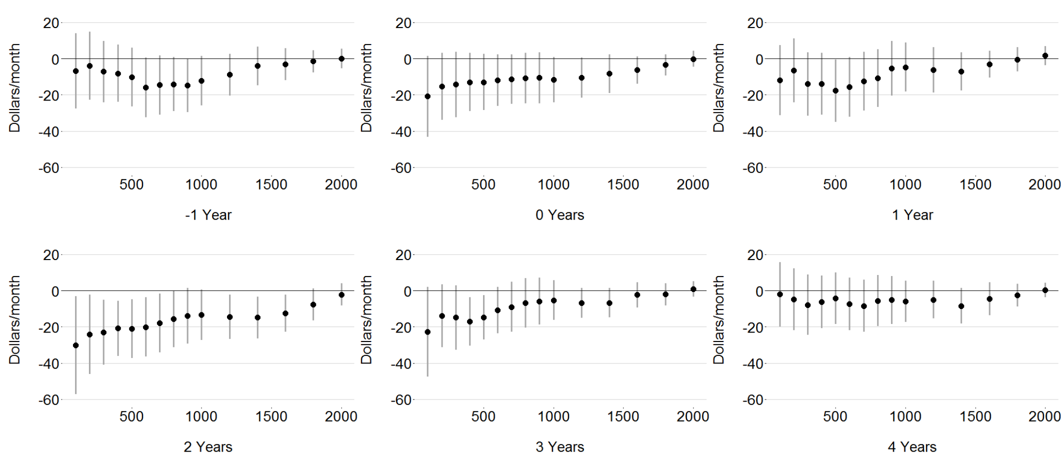


Figure 12: Event Study: Impact of serious building fires on rents

Note: This figure plots event-study style coefficients from running Equation 4.2 on Craigslist rents for one-bedroom apartments, using exposure to serious building fires as the treatment measure.

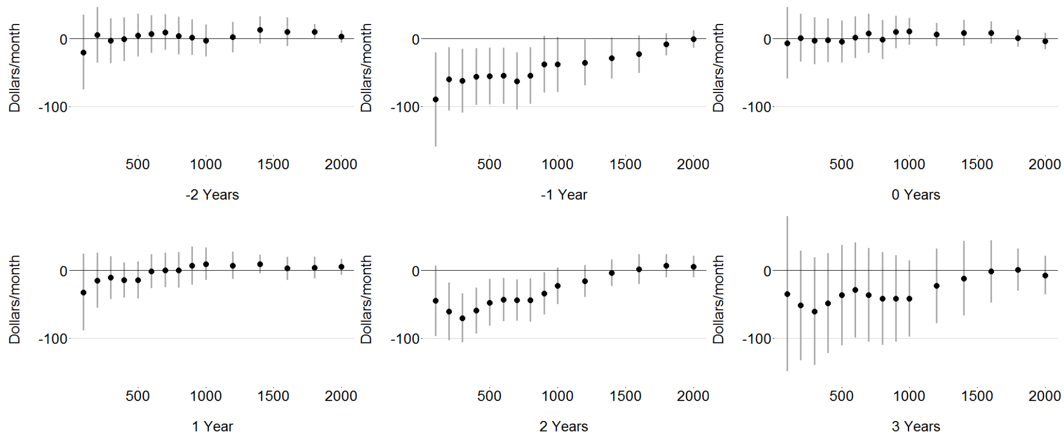


Figure 13: Event Study: Impact of new market rate projects on rents

Note: This figure plots event-study style coefficients from running Equation 4.2 on Craigslist rents for one-bedroom apartments, using exposure to new market rate housing projects as the treatment measure.

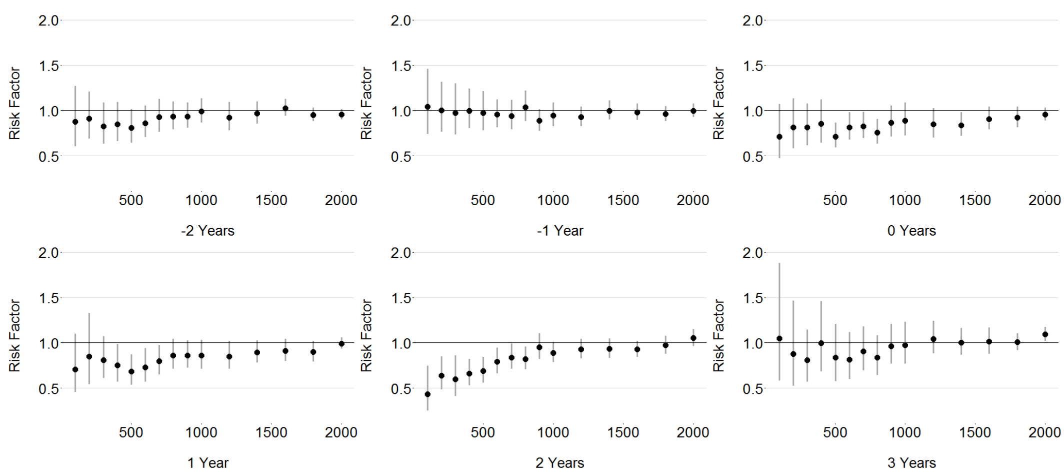
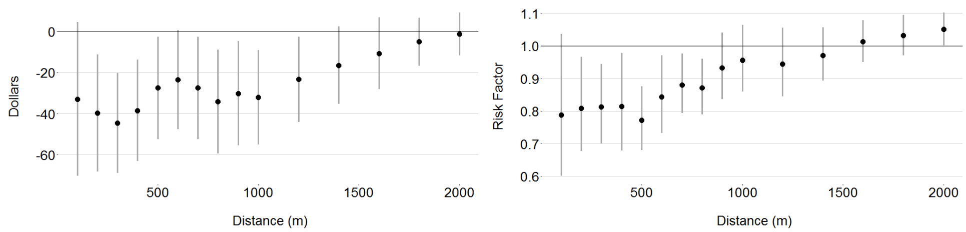


Figure 14: Event Study: Impact of new market rate projects on moves to poorer zipcodes

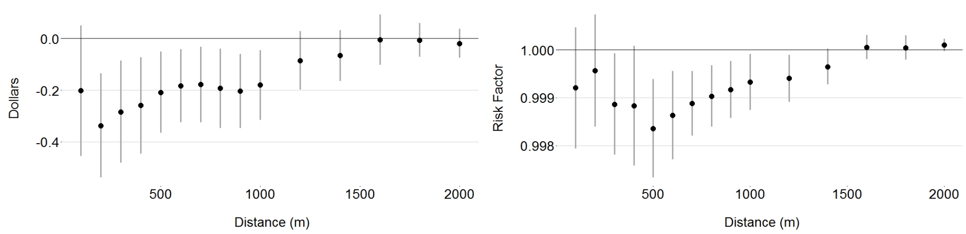
Note: This figure plots event-study style coefficients from running Equation 4.1 on a dummy for moving to a poorer zipcode, using exposure to new market rate housing projects as the treatment measure.



(a) 1BR Rents (b) Moves to poorer zipcodes

Figure 15: Impact of proximity to new projects on 1BR rents and the probability of displacement

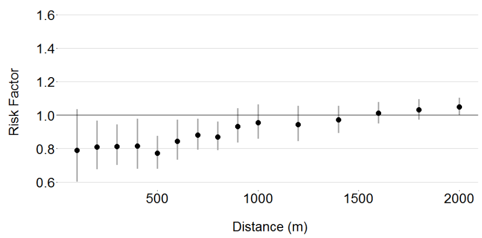
Note: Panel a shows the results from running specification 4.4 on one bedroom rents, using microneighborhood by year fixed effects and parcel-level controls including rent control, latitude and longitude, distance to the financial district and Caltrain station, landuse zoning, 2010 Census tract median income tercile, and a quadratic in residential units. Panel b shows the results from running 4.3 on a dummy for moving to a poorer zipcode, whose median income is at least 10% lower than individual i's current zipcode, including sex, birthyear, and microneighborhood strata; an interaction of rent control status with years lived at that parcel; and the same set of parcel-level controls. Mean 1BR rent = \$1,891; mean adverse move = 0.0103.



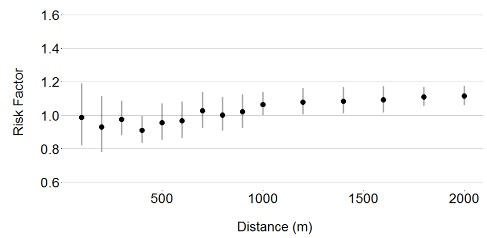
(a) 1BR Rents (b) Moves to poorer zipcodes

Figure 16: Impact of proximity to new units on 1BR rents and the probability of displacement

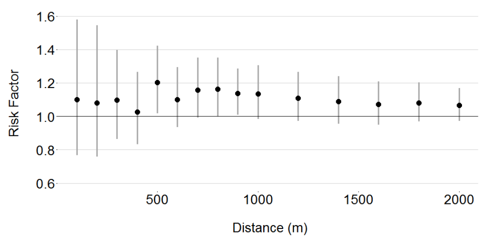
Note: Panel a shows the results from running specification 4.4 on one bedroom rents, using microneighborhood by year fixed effects and parcel-level controls including rent control, latitude and longitude, distance to the financial district and Caltrain station, landuse zoning, 2010 Census tract median income tercile, and a quadratic in residential units. Panel b shows the results from running 4.3 on a dummy for moving to a poorer zipcode, whose median income is at least 10% lower than individual i's current zipcode, including sex, birthyear, and microneighborhood strata; an interaction of rent control status with years lived at that parcel; and the same set of parcel-level controls. Mean 1BR rent = \$1,891; mean adverse move = 0.0103.



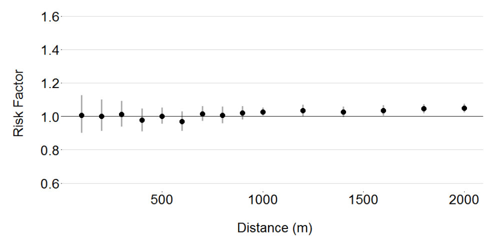
(a) Move to a poorer zipcode



(b) Leave the Bay Area



(c) Move to a richer zipcode



(d) Any move

Figure 17: Impact of New Projects on Moves by Type of Destination

Note: These plots show the results from running specification 4.3 on the named outcome variables. All specifications include sex, birthyear, and microneighborhood strata; an interaction of rent control status with years lived at that parcel; and parcel-level controls including rent control, latitude and longitude, distance to the financial district and Caltrain station, landuse zoning, 2010 Census tract median income tercile, and a quadratic in residential units. Mean move poorer = 0.0103; mean exit Bay Area = 0.0160; mean move richer = 0.00777; and mean any move = 0.0446. Mean 1BR rent = $1,891.

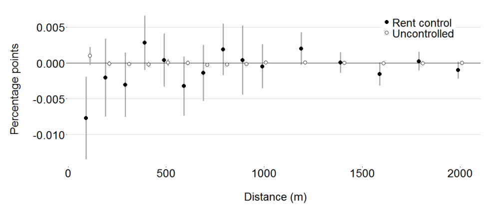


Figure 18: Impacts on Eviction Notices

Note: These plots show the results from running specification 4.3 on an indicator variable for parcel p receiving an eviction notice for rent controlled and uncontrolled parcels respectively. Both specifications use parcel panel data and include microneighborhood by year fixed effects, rent control, latitude and longitude, distance to the financial district and Caltrain station, landuse zoning, 2010 Census tract median income tercile, and a quadratic in residential units. Mean eviction for rent controlled parcels = 0.0247 mean eviction for uncontrolled parcels = 0.000263.

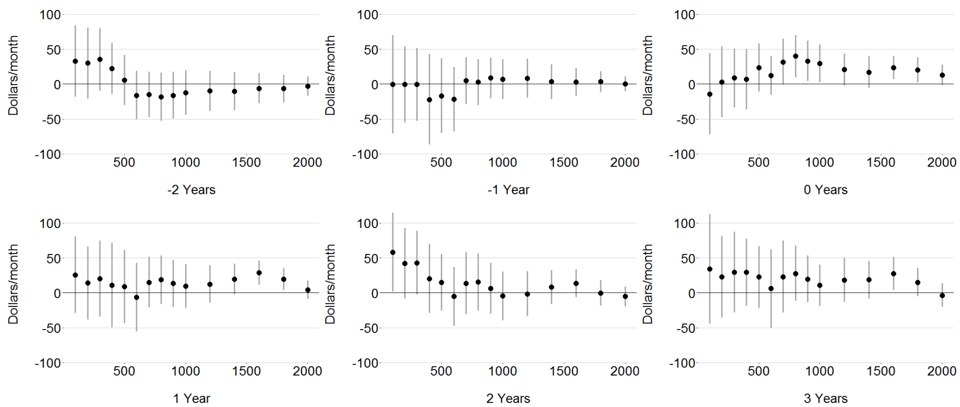


Figure 19: Event Study: Impact of new affordable projects on rents

Note: This figure plots event-study style coefficients from running Equation 4.2 on Craigslist rents for one-bedroom apartments, using exposure to new affordable housing units as the treatment measure. Mean move to a poorer zipcode = 0.0103.

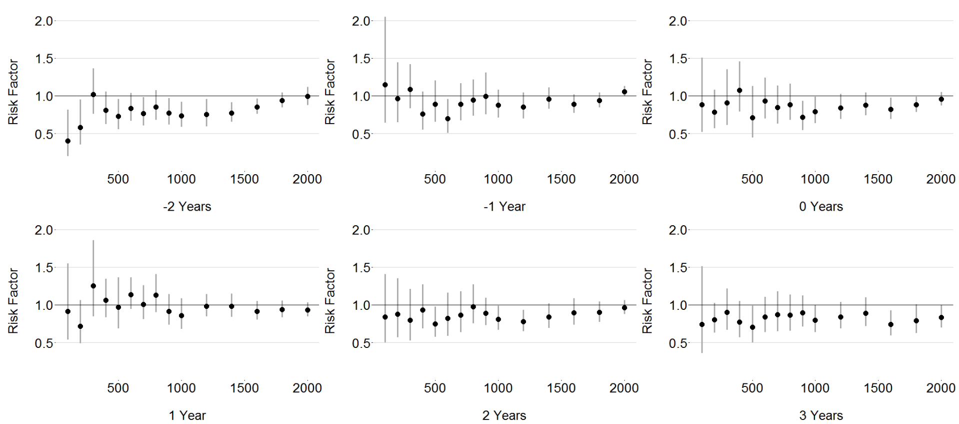
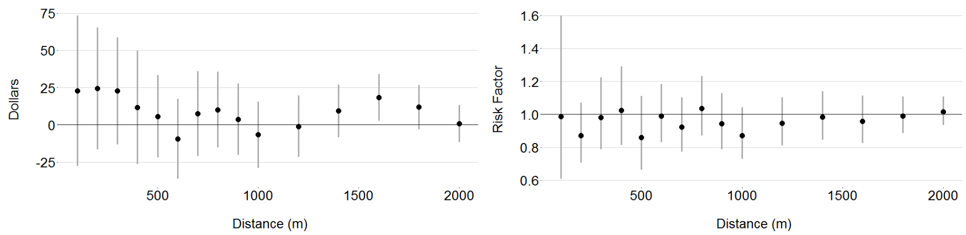


Figure 20: Event Study: Impact of affordable projects on moves to poorer zipcodes

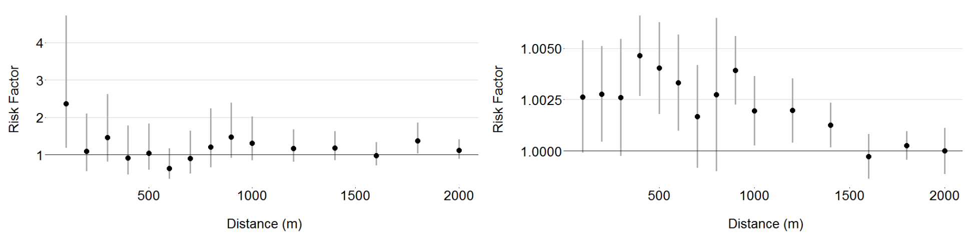
Note: This figure plots event-study style coefficients from running Equation 4.1 on a dummy for moving to a poorer zipcode, using exposure to new affordable projects as the treatment measure. Mean 1BR rent =$1,891 per month.



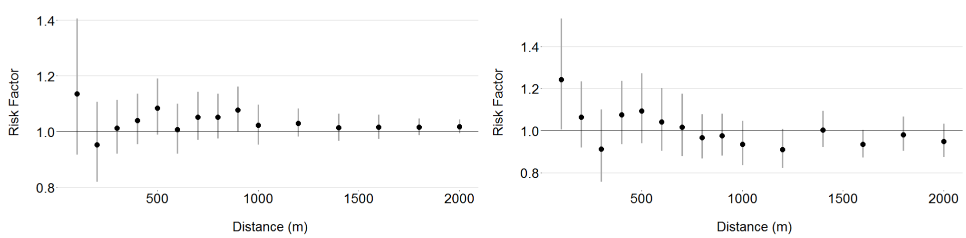
(a) 1BR Rents (b) Moves to poorer zipcodes

Figure 21: Impact of affordable projects on 1BR rents and the probability of displacement

Note: Panel a shows results from running specification 4.4 on 1BR rents, using microneighborhood by year fixed effects and parcel controls including rent control, latitude and longitude, distance to the financial district and Caltrain station, landuse zoning, 2010 Census tract median income tercile, and a quadratic in residential units. Panel b shows results from running 4.3 on a dummy for moving to a poorer zipcode, whose median income is at least 10% lower than individual i's current zipcode, including sex, birthyear, and microneighborhood strata; an interaction of rent control status with years lived at that parcel; and the same set of parcel controls. Mean 1BR rent = \$1,891; mean move poorer = 0.0103.



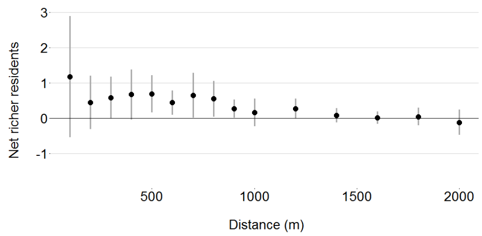
(a) Permits for New Projects (b) Permits for New Units



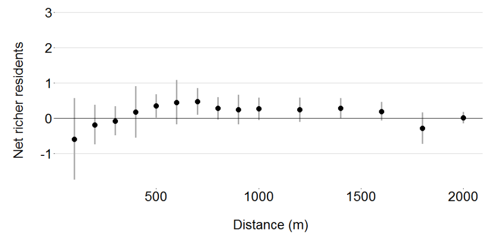
(c) Probability of a Residential Renovation (d) Business turnover

Figure 22: Impact of market rate projects on residential renovations, business turnover, and crowd-in

Note: These plots show results from running specification 4.3 on the named dependent variables. All specifications use parcel panel data restricted to parcels zoned for residential or commercial use, and include microneighborhood strata and parcel controls including rent control, latitude and longitude, distance to the financial district and Caltrain station, landuse zoning, 2010 Census tract median income tercile, and a quadratic in residential units. The specification for residential sales prices only uses properties that have been sold more than once. Mean residential renovation = 0.0638; mean business turnover = 0.1078; mean new permit = 0.000682.



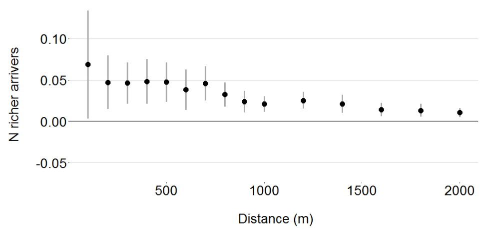
(a) Exogenous Market Rate



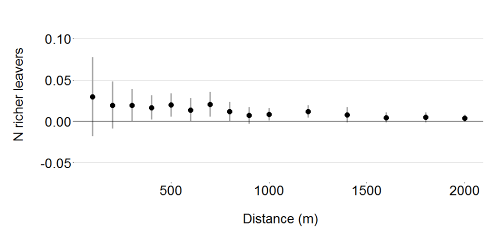
(b) Exogenous Affordable

Note: These plots show results from running specification 4.3 on gentrification, using cumulative binned exposure to exogenous market rate and affordable construction, respectively. All specifications include parcel and year fixed effects and microneighborhood trends. Gentrification is measured as the net change in richer people (arrivers from richer zipcodes minus movers to richer zipcodes) is greater than the net change in poorer people (arrivers from poorer zipcodes minus movers to poorer zipcodes). Mean gent = 0.0712.

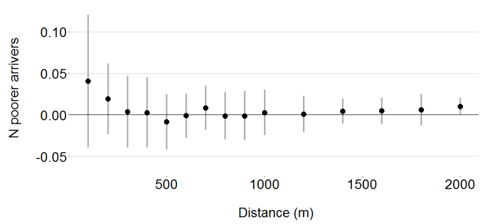
Figure 23: Impact of new projects on gentrification by construction type



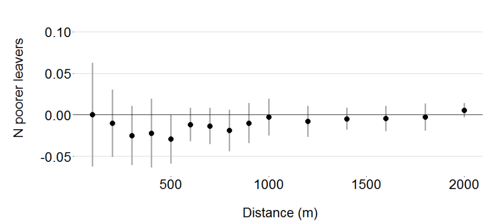
(a) Richer Arrivers



(b) Richer Leavers



(c) Poorer Arrivers



(d) Poorer Leavers

Note: These plots show the results from running specification 4.3 on each outcome variable. All specifications include parcel and year fixed effects and microneighborhood trends. Mean richer arrivers = 0.1189 ; mean richer leavers = 0.03896; mean poorer arrivers = 0.133 ; mean poorer leavers = 0.05775.

Figure 24: Impact of new projects on leavers and arrivers by income type

9 Tables

Table 1: Construction and Serious Building Fires

Year	All construction			Exogenous construction			Fires
	Completed	Net Units	Affordable Units	Completed	Net Units	Affordable Units	
2003	94	308	7	1	2	0	13
2004	113	1,047	11	1	1	0	9
2005	129	1,369	682	2	147	147	13
2006	147	1,421	450	1	1	1	18
2007	146	2,567	693	1	8	0	17
2008	171	3,390	812	7	512	13	16
2009	148	3,459	921	4	507	76	7
2010	93	1,303	580	3	5	1	13
2011	72	376	186	4	6	3	17
2012	82	1,059	565	5	350	201	14
2013	101	2,169	728	8	557	136	18
2014	119	3,271	589	5	35	26	1
2015	119	2,768	459	4	100	9	1
2016	133	5,752	727	8	670	99	1
2017	164	4,553	1,621	6	1,043	486	0
Total	2,431	34,812	9,031	60	3,944	1,198	158

Note: This table reports summary statistics for new construction and fires. New construction includes all new buildings that added at least one unit to the housing stock. It differs from the annual Housing Inventory: it does not include demolitions, mergers or splits of existing units within a building, reclassification of illegal units, or corrections of the record. Exogenous construction refers to the subset of new construction that occurred on a burned parcel. The final column reports the count of serious building fires each year. To qualify, the fire incident must be classified as an unintentional building fire requiring at least 10 units to be dispatched and to appear in records of building complaints or in the description of new construction.

Table 2: Balance Table: Parcel Characteristics

	Within 100m of Construction			Within 100m of Fire		
	Yes	No	Pr(>|t|)	Yes	No	Pr(>|t|)
1BR rent	1,960	1,892	0.382	1,642	1,881	0.697
Mean zipcode income	83,261	101,578	0.121	86,060	101,076	0.368
Residential units	9.235	4.170	0.456	6.566	4.053	0.779
Infutor population	10.914	7.401	0.607	9.161	7.080	0.517
Year built	1936	1932	0.969	1927	1933	0.043
Km to Caltrain station	3.970	3.694	0.866	3.406	3.730	0.948
Km to Financial District	4.936	5.349	0.717	4.118	5.462	0.822
Move poorer	0.054	0.048	0.900	0.050	0.047	0.465
Move richer	0.047	0.035	0.773	0.060	0.033	0.913
Leave Bay Area	0.083	0.068	0.672	0.105	0.066	0.957
Any move	0.161	0.145	0.657	0.212	0.140	0.172
Residential renovation	0.052	0.065	0.145	0.068	0.064	0.815
Commercial renovation	0.012	0.006	0.907	0.009	0.006	0.563
Eviction notice	0.008	0.006	0.779	0.010	0.006	0.011
Gentrify	0.040	0.036	0.635	0.050	0.034	0.662

Note: The first section of this table compares the pre-treatment characteristics of parcels within 100m of future exogenous construction projects with the characteristics of parcels that are more than 100m away from future construction. The second section compares the pre-treatment characteristics of parcels within 100m of a serious building fire with the characteristics of parcels that are more than 100m away from serious fires. Mean zipcode income data come from the Internal Revenue Service. Move variables are dummies equal to 1 if any person living on that parcel moves. P-values are computed after controlling for microneighborhood by year fixed effects and clustering standard errors at the microneighborhood level, as is done in every specification.

Table 3: Direct gentrification effects: Who moves into new housing?

	(Cross-Section)	(Panel)
$\mathbb{1}$(Exogenous)	0.035	0.096
	(0.017)	(0.032)
$\mathbb{1}$(Affordable)	−0.118	−0.037
	(0.009)	(0.030)
$\mathbb{1}$(Exogenous) · $\mathbb{1}$(Afforable)	−0.156	−0.181
	(0.034)	(0.069)
R^2	0.011	0.090
Adj. R^2	0.011	−0.003
Num. obs.	22730	362873

Note: The dependent variable is a dummy equal to one if the newcomer comes from a richer sending zipcode. The cross-sectional specification includes only the people who moved into a new building. The panel specification includes includes people who live in parcels that are within 2km of exogenous construction at some point in the study period, in the year they arrived. It includes microneighborhood by year fixed effects and controls for individual characteristics (sex and birth decade) and parcel characteristics (quadratics in distance to the Financial District and Caltrain station, residential units, landuse zoning, 2010 Census block income tercile, year built, rent control status, and latitude and longitude). Standard errors are clustered by microneighborhood.

References

Ahlfeldt, G. M. and Maennig, W. (2010). Impact of sports arenas on land values: evidence from berlin. *The Annals of Regional Science*, 44(2):205–227.

Ahlfeldt, G. M., Redding, S. J., Sturm, D. M., and Wolf, N. (2015). The Economics of Density: Evidence from the Berlin Wall. *Econometrica*, 83(6):2127–2189.

Albouy, D. and Ehrlich, G. (2012). Metropolitan land values and housing productivity. *National Bureau of Economic Research.*

Albouy, D., Ehrlich, G., and Liu, Y. (2016). Housing demand, cost-of-living inequality, and the affordability crisis. Technical report, National Bureau of Economic Research.

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. Washington: Board of Governors of the Federal Reserve System.*

Asquith, B. (2016). Rent Control and Evictions: Evidence from San Francisco. *Working Paper.*

Asquith, B. J., Mast, E., and Reed, D. (2021). Local effects of large new apartment buildings in low-income areas. *The Review of Economics and Statistics*, pages 1–46.

Autor, D. H., Palmer, C. J., and Pathak, P. A. (2014). Housing Market Spillovers: Evidence from the End of Rent Control in Cambridge, Massachusetts. *Journal of Political Economy*, 122(3):661–717.

Bilal, A. and Rossi-Hansberg, E. (2018). Location as an asset. *Working paper.*

Blattman, C., Green, D. P., Ortega, D., and Tobón, S. (2017). Place-based interventions at scale: The direct and spillover effects of policing and city services on crime. *Journal of the European Economic Association.*

Boustan, L. P., Margo, R. A., Miller, M. M., Reeves, J. M., and Steil, J. P. (2019). Does condominium development lead to gentrification? *NBER Working Paper.*

Brummet, Q. and Reed, D. (2019). The effects of gentrification on the well-being and opportunity of original resident adults and children. *Working paper.*

Carlson, H. J. (2020). Measuring displacement: Assessing proxies for involuntary residential mobility. *City & Community.*

Chapple, K., Waddell, P., Chatman, D., Zuk, M., Loukaitou-Sideris, A., Ong, P., Gonzalez, S. R., Pech, C., and Gorska, K. (2017). Developing a new methodology for analyzing potential displacement. *California Air Resources Board and the California Environmental Protection Agency.*

City and County of San Francisco (2015). Policy Analysis Report: Displacement in the Mission District. *Budget and Legislative Analyst.*

Couture, V., Gaubert, C., Handbury, J., and Hurst, E. (2019). Income growth and the distributional effects of urban spatial sorting. *Working paper.*

Couture, V. and Handbury, J. (2017). Urban revival in america, 2000 to 2010. Technical report, National Bureau of Economic Research.

Depro, B., Timmins, C., and O'Neil, M. (2015). White Flight and Coming to the Nuisance: Can Residential Mobility Explain Environmental Injustice? *Journal of the Association of Environmental and Resource Economists.*

Desmond, M. (2016). *Evicted: Poverty and profit in the American city.* Broadway books.

Desmond, M. and Shollenberger, T. (2015). Forced displacement from rental housing: Prevalence and neighborhood consequences. *Demography*, 52(5):1751–1772.

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):1063–1117.

Diamond, R., McQuade, T., and Qian, F. (2019). The effects of rent control expansion on tenants, landlords, and inequality: Evidence from san francisco. *American Economic Review*, 109(9):3365–94.

Ding, L., Hwang, J., and Divringi, E. (2016). Gentrification and residential mobility in Philadelphia. *Regional science and urban economics*, 61:38–51.

DiPasquale, D. (1999). Why don't we know more about housing supply? *The Journal of Real Estate Finance and Economics*, 18(1):9–23.

Dragan, K., Ellen, I., and Glied, S. A. (2019). Does gentrification displace poor children? new evidence from new york city medicaid data. *NBER Working Paper*.

Egan, T. (2014). The Economics of San Francisco Housing. *Office of the Controller - City and County of San Francisco*.

Ellen, I. G., Horn, K. M., and Reed, D. (2019). Has falling crime invited gentrification? *Journal of Housing Economics*, 46:101636.

Englander, G. (2020). Information and Spillovers from Targeting Policy in Peru's Anchoveta Fishery. *Working paper*.

Environmental Protection Agency (2015). Bay Area Housing and Community Risk Assessment Project: Creating Safe Growth Strategies for the San Francisco Bay Area. *Office of Sustainable Communities Smart Growth Program*.

Freeman, L. (2005). Displacement or succession? residential mobility in gentrifying neighborhoods. *Urban Affairs Review*, 40(4):463–491.

Glaeser, E. L. and Gottlieb, J. D. (2008). The economics of place-making policies. Technical report, National Bureau of Economic Research.

Glaeser, E. L., Luca, M., and Moszkowski, E. (2020). Gentrification and neighborhood change: Evidence from yelp. *Harvard Business School NOM Unit Working Paper*, (21-074).

Glass, R. (1964). Aspects of change. *The Gentrification Debates: A Reader*.

Green, R. K., Malpezzi, S., and Mayo, S. K. (2005). Metropolitan-specific estimates of the price elasticity of supply of housing, and their sources. *American Economic Review*, 95(2):334–339.

Grier, G. and Grier, E. (1980). Urban Displacement: A reconnaissance. *Back to the City: Issues in Neighbourhood Renovation Eds S Laska, D Spain (Pergamon, New York) pp*, pages 252–268.

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

Gyourko, J., Mayer, C., and Sinai, T. (2013). Superstar cities. *American Economic Journal: Economic Policy*, 5(4):167–99.

Gyourko, J. and Molloy, R. (2015). Regulation and housing supply. In *Handbook of regional and urban economics*, volume 5, pages 1289–1337. Elsevier.

Gyourko, J. and Saiz, A. (2006). Constuction Costs and the Supply of Housing Structure. *Journal* of Regional Science.

Hankinson, M. (2018). When do renters behave like homeowners? high rent, price anxiety, and nimbyism. *American Political Science Review*, 112(3):473–493.

Hanushek, E. A. and Quigley, J. M. (1980). What is the price elasticity of housing demand? *The Review of Economics and Statistics*, pages 449–454.

Henderson, J. V., Regan, T., and Venables, A. J. (2016). Building the City: Sunk Capital, Sequencing, and Institutional Frictions.

Hornbeck, R. and Keniston, D. (2017). Creative destruction: Barriers to urban growth and the Great Boston Fire of 1872. *American Economic Review*, 107(6):1365–98.

Jackson, C. (2018). Is This Oakland Developer Building Sorely Needed Housing or Dropping Gentrification Bombs? *San Francisco Magazine*.

Kinney, S. and Karr, A. (2017). Public-Use vs. Restricted-Use: An Analysis Using the American Community Survey. *US Census Bureau CES Working Paper*.

Kok, N., Monkkonen, P., and Quigley, J. M. (2014). Land use regulations and the value of land and housing: An intra-metropolitan analysis. *Journal of Urban Economics*, 81:136–148.

Lees, L., Slater, T., and Wyly, E. (2008). Gentrification. *New York*.

Li, X. (2019). Do new housing units in your backyard raise your rents? *Working paper*.

Lord, T. (2017). Why ABAG housing quotas lead to displacement. *48 Hills*.

Marcuse, P., Smith, N., and Williams, P. (1986). *Abandonment, Gentrification, and Displacement: the Linkages in New York City*.

Mense, A. (2021). The impact of new housing supply on the distribution of rents. *Working paper*.

Mok, G. and Wang, W. (2020). Do evictions cause income changes? an instrumental variables approach. *Working paper*.

Monkkonen, P. (2016). Understanding and challenging opposition to housing construction in california's urban areas. *Available at SSRN 3459823*.

Qiang, A. J., Timmins, C., and Wang, W. (2020). Displacement and the consequences of gentrification. *Working paper*.

Redding, S. and Sturm, D. (2016). Estimating Neighborhood Effects: Evidence from War-Time Destruction in London. *Working Paper*.

Rossi-Hansberg, E., Sarte, P.-D., and Owens III, R. (2010). Housing Externalities. *Journal of political Economy*, 118(3):485–535.

Saiz, A. (2010). The geographic determinants of housing supply. *The Quarterly Journal of Economics*, 125(3):1253–1296.

Siodla, J. (2015). Razing San Francisco: The 1906 disaster as a natural experiment in urban redevelopment. *Journal of Urban Economics*, 89:48–61.

Zuk, M. and Chapple, K. (2016). Housing Production, Filtering and Displacement: Untangling the Relationships. *Research Brief*.

10 Appendix: For Online Publication

10.1 Additional event study plots

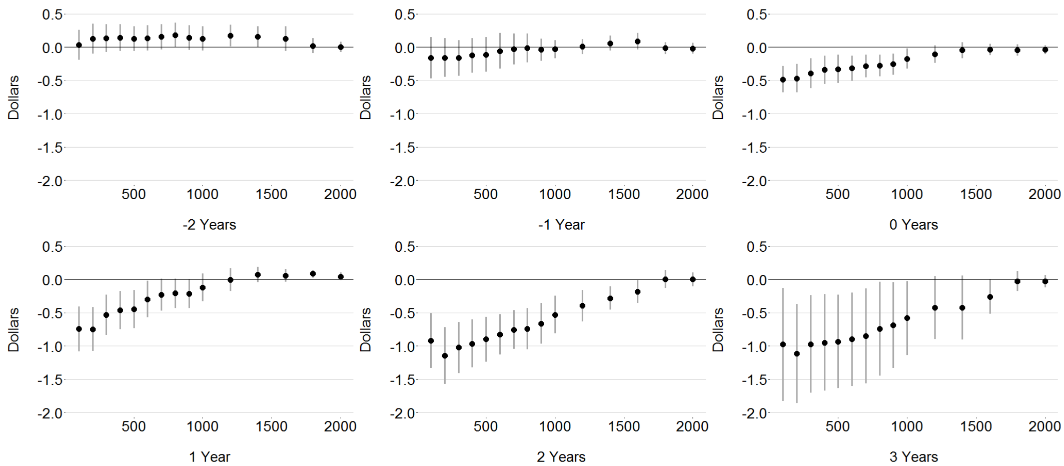


Figure 25: Event Study: Impact of new market rate units on rents

Note: This figure plots event-study style coefficients from running Equation 4.2 on Craigslist rents for one-bedroom apartments, using exposure to new market rate housing units as the treatment measure.

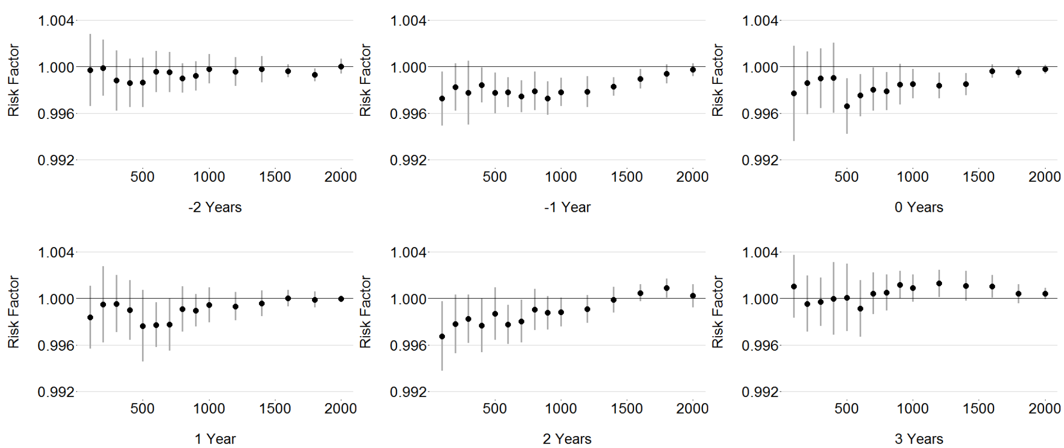


Figure 26: Event Study: Impact of new market rate units on moves to poorer zipcodes

Note: This figure plots event-study style coefficients from running Equation 4.1 on a dummy for moving to a poorer zipcode, using exposure to new market rate housing units as the treatment measure.

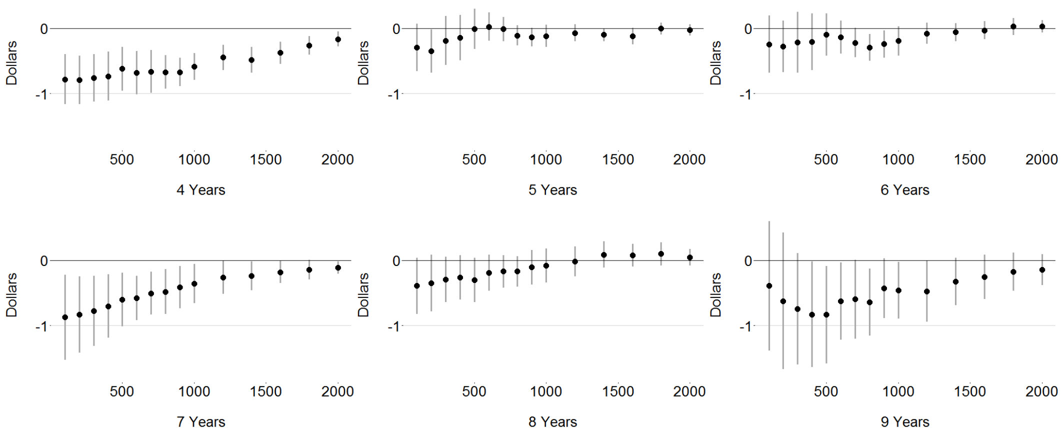


Figure 27: Event Study: Long Term Impact of New Units on Rents

Note: This figure plots event-study style coefficients from running Equation 4.2 on Craigslist rents for one-bedroom apartments, using exposure to new market rate housing units as the treatment measure.

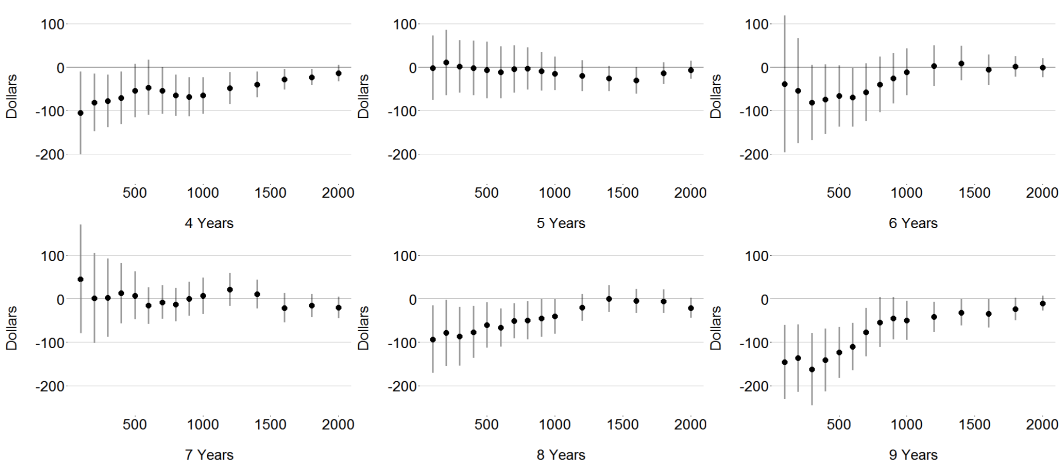


Figure 28: Event Study: Long Term Impact of New Market Rate Projects on Rents

Note: This figure plots event-study style coefficients from running Equation 4.2 on Craigslist rents for one-bedroom apartments, using exposure to new market rate housing units as the treatment measure.

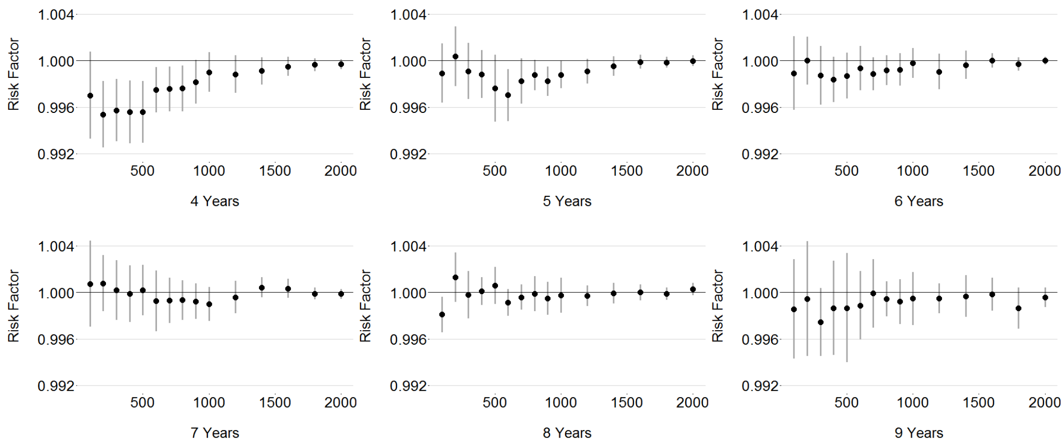


Figure 29: Event Study: Long term impact of new market rate units on moves to poorer zipcodes

Note: This figure plots event-study style coefficients from running Equation 4.1 on a dummy for moving to a poorer zipcode, using exposure to new market rate housing units as the treatment measure.

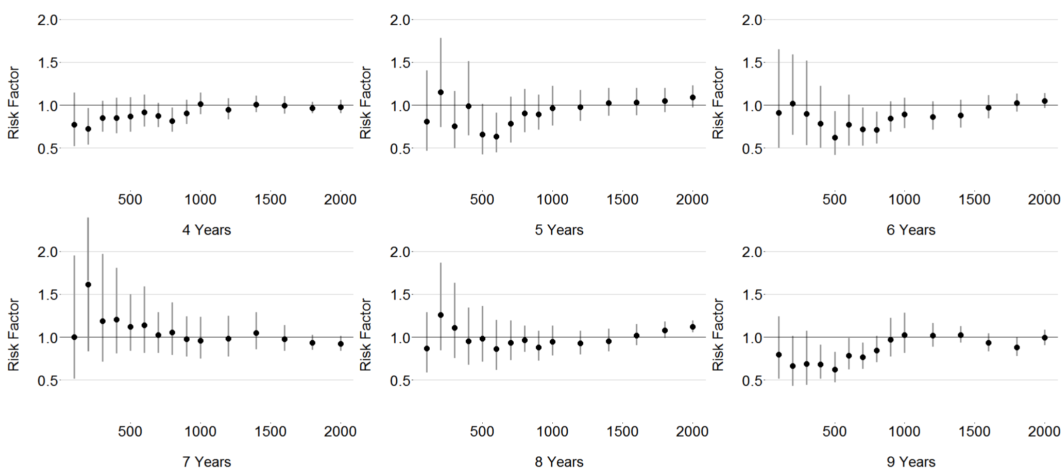


Figure 30: Event Study: Long term impact of new market rate projects on moves to poorer zipcodes

Note: This figure plots event-study style coefficients from running Equation 4.1 on a dummy for moving to a poorer zipcode, using exposure to new market rate housing projects as the treatment measure.

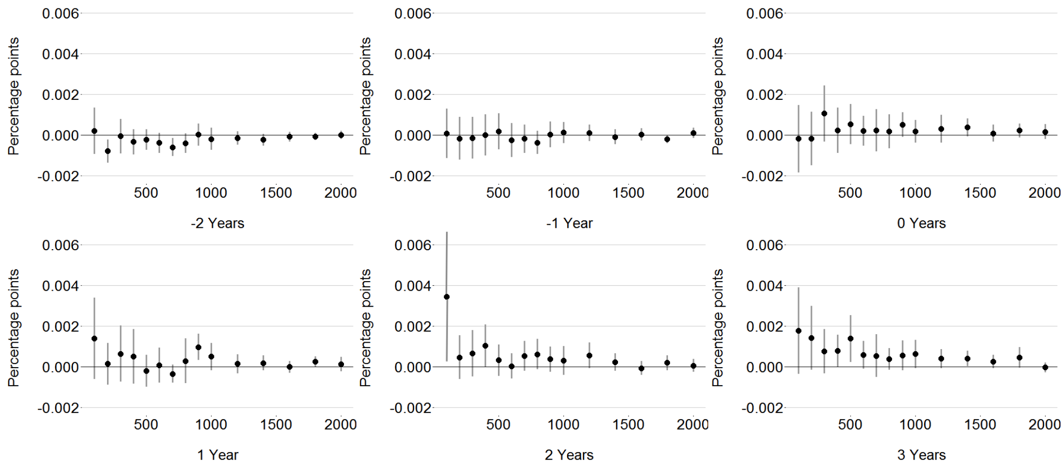


Figure 31: Impact of projects on probability of new building permit

Note: These plots show results from running specification 4.2 on the named outcome variables. A poorer zipcode has median income at least 10% lower than individual i's current zipcode. All specifications include individual, year, and microneighborhood fixed effects; an interaction of rent control status with years lived at that parcel; and parcel-level controls including rent control, distance to the financial district, Caltrain station, landuse zoning, and a quadratic in residential units.

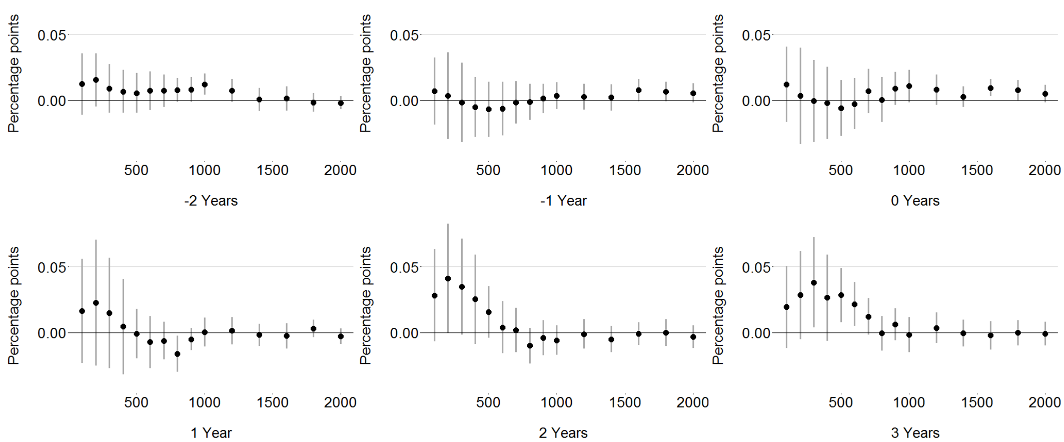


Figure 32: Impact of projects on gentrification

Note: These plots show results from running specification 4.2 on the named outcome variables. A poorer zipcode has median income at least 10% lower than individual i's current zipcode. All specifications include individual, year, and microneighborhood fixed effects; an interaction of rent control status with years lived at that parcel; and parcel-level controls including rent control, distance to the financial district, Caltrain station, landuse zoning, and a quadratic in residential units.

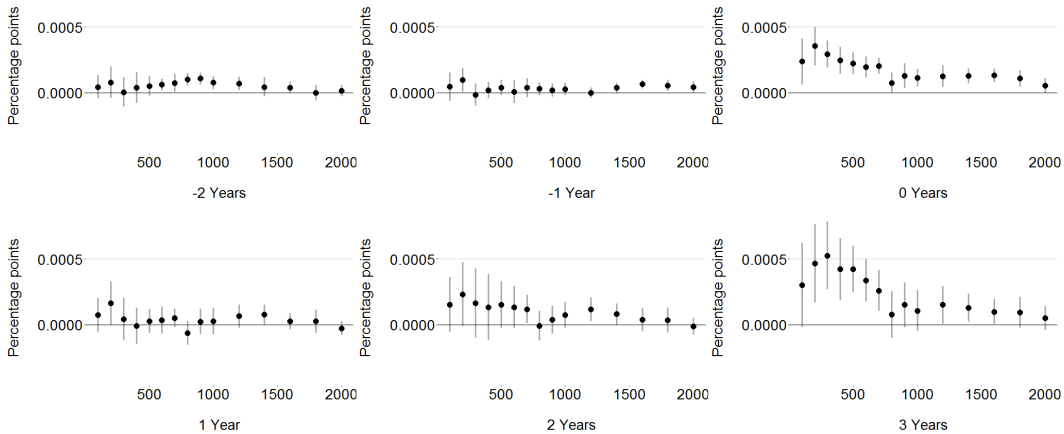


Figure 33: Impact of net units on gentrification

Note: This figure plots event-study style coefficients from running Equation 4.2 on a dummy for gentrification, using exposure to new market rate housing units as the treatment measure.

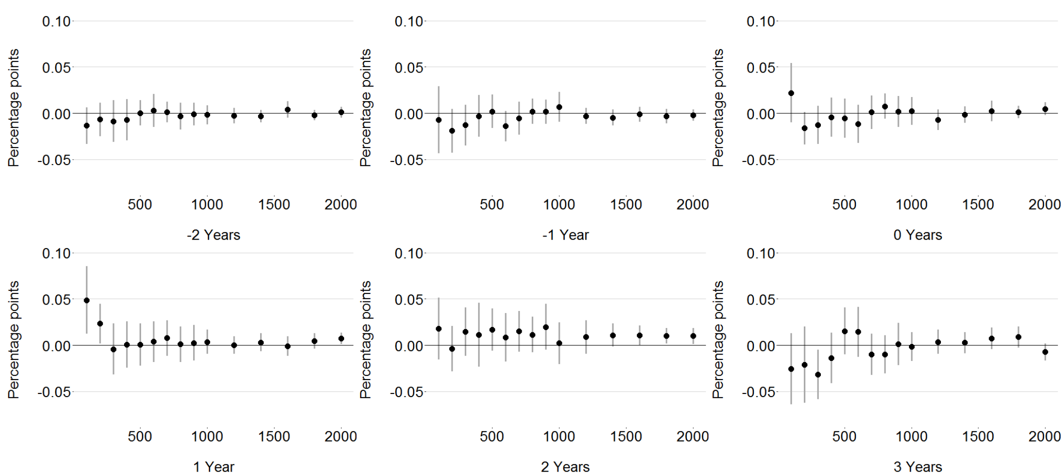
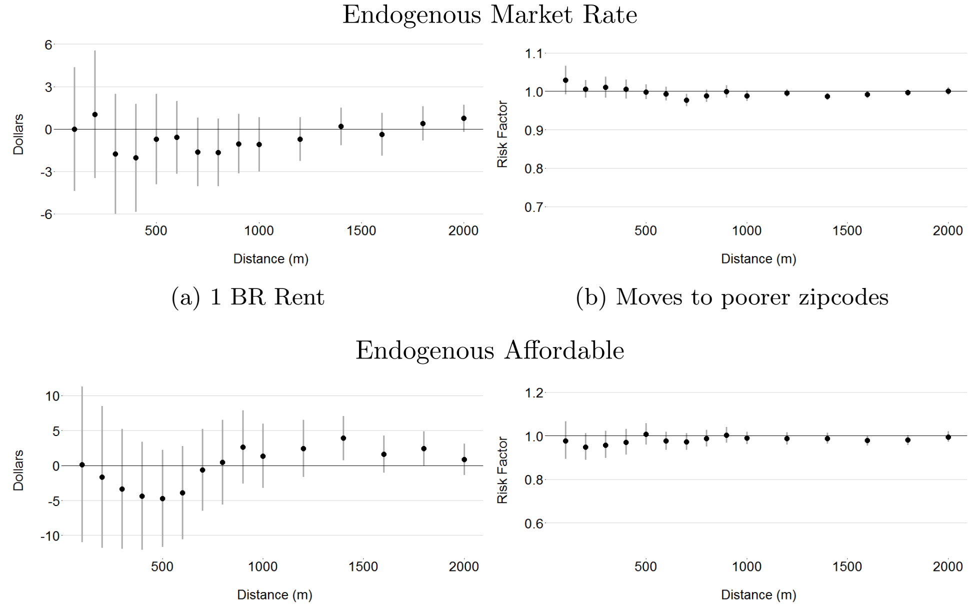


Figure 34: Impact of projects on residential renovations

Note: This figure plots event-study style coefficients from running Equation 4.2 on a dummy for residential renovation, using exposure to new market rate housing projects as the treatment measure.

10.2 Correlations with endogenous construction

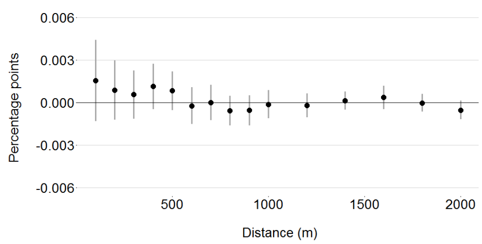


(a) 1 BR Rent (b) Moves to poorer zipcodes

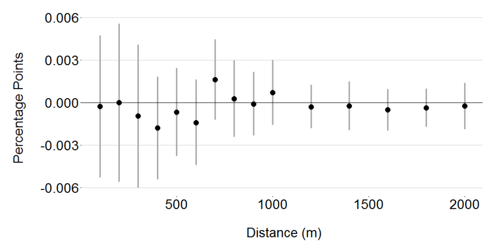
(c) 1 BR Rent (d) Moves to poorer zipcodes

Figure 35: Endogenous Construction, Rents, and Adverse Moves

Note: These plots show results from running specification 4.3 on 1 bedroom rents and moves to poorer zipcodes, using cumulative binned exposure to new endogenous market rate and affordable projects. All rent specifications use parcel panel data and include microneighborhood by year fixed effects, rent control, latitude and longitude, distance to the financial district and Caltrain station, landuse zoning, 2010 Census tract median income tercile, and a quadratic in residential units. All displacement specifications use individual panel data and control for sex, birthyear, and microneighborhood strata; an interaction of rent control status with years lived at that parcel; and the same set of parcel-level controls. Mean 1BR rent =\$1,891 per month; mean move to a poorer zipcode = 0.0103.



(a) Endogenous Market Rate

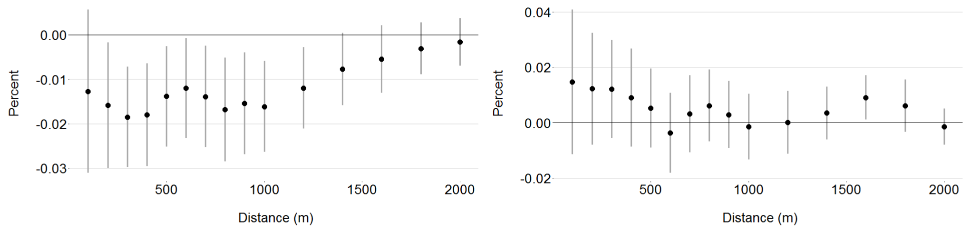


(b) Endogenous Affordable

Note: These plots show the results from running specification 4.3 on an indicator for gentrification, using cumulative binned exposure to endogenous market rate and affordable projects. All specifications include parcel and year fixed effects and microneighborhood trends. The indicator is equal to one if the net change in richer people (arrivers from richer zipcodes minus movers to richer zipcodes) is greater than the net change in poorer people (arrivers from poorer zipcodes minus movers to poorer zipcodes). Mean gent = 0.0712.

Figure 36: Endogenous construction and gentrification

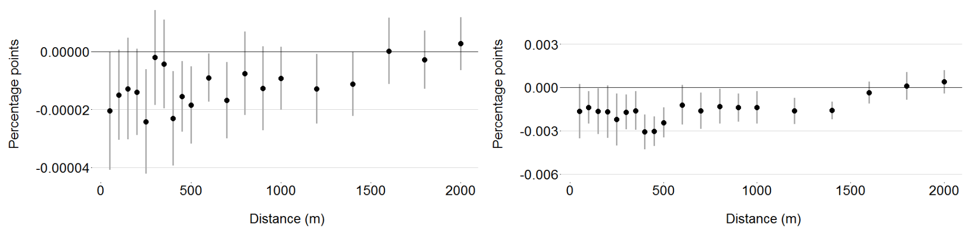
10.3 Alternative specifications



(a) Exogenous Market Rate (b) Exogenous Affordable

Figure 37: Impact of proximity to new construction on log rent

Note: These plots show the results from running specification 4.3 on log rent, instead of rent, for exogenous market rate and affordable projects. The specification includes individual, year, and microneighborhood fixed effects; an interaction of rent control status with years lived at that parcel; and parcel-level controls including rent control, distance to the financial district, Caltrain station, landuse zoning, and a quadratic in residential units.



(a) Units (b) Projects

Figure 38: Linear Probability Model: Impact of proximity to new construction on the probability of moving to a poorer zipcode

Note: These plots show the results from running specification 4.3 on the named outcome variables. A poorer zipcode has median income at least 10% lower than individual i's current zipcode. All specifications include individual, year, and microneighborhood fixed effects; an interaction of rent control status with years lived at that parcel; and parcel-level controls including rent control, distance to the financial district, Caltrain station, landuse zoning, and a quadratic in residential units. Mean move poorer = 0.0103.

11 The rent elasticity of displacement

The results shown in Figures 15a and 15b suggest that displacement risk is highly price elastic. When rents fall by roughly 2% (\$40), the risk of moving to a poorer zipcode falls by about 20%, an elasticity of approximately 10. This high price elasticity is consistent with San Francisco's very high levels of rent burden, especially among households earning less than the Area Median Income (AMI). For the majority of households who earn less than half of the AMI, rent takes up more than 30% of their monthly income and a small reduction could plausibly make the difference between managing to stay in their current housing and being displaced (see Figure 6 for details on rent burden and income).

Instrumental variables offers another strategy for identifying this elasticity. The IV recharacterizes the rent results as the first stage and the displacement results as the reduced form. In the first stage, I use variation in exposure to new construction to instrument for exogenous price shocks, measured as the annual percent change in rent $\Delta rent_t = (rent_t - rent_{t-1})/rent_{t-1}$:

$$\begin{aligned}\Delta rent_{pct} = &\sum_{d=100}^{1000} \alpha_d mkt100_{p,t,d,k} + \alpha_{1200} mkt200_{p,1200,t} + \\ &\sum_{d=100}^{1000} \alpha_d \mathit{aff}100_{p,t,d,k} + \alpha_{1200} \mathit{aff}200_{p,1200,t} + \\ &\gamma_{ct} + X_p + X_i + X_{it} + e_{ipct}\end{aligned} \qquad \text{(First stage)}$$

In the second stage, I use this exogenous price variation to estimate the impact on the probability of moving to a poorer zipcode:

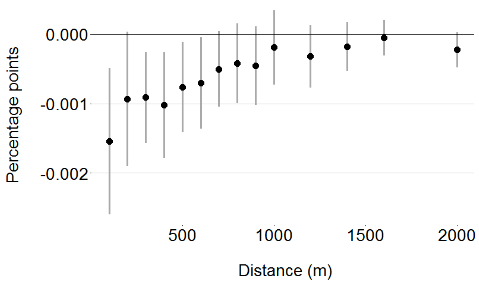
$$move\,poorer_{ipct} = \widehat{\Delta rent_{pct}} + \gamma_{ct} + X_p + e_{pct} \qquad \text{(Second stage)}$$

The reduced form directly regresses displacement on proximity of new construction:

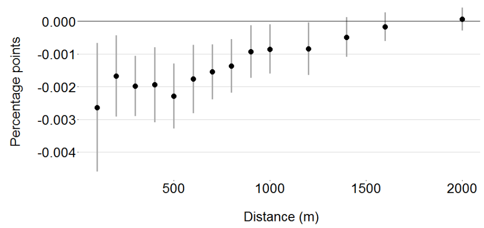
$$\begin{aligned}move\,poorer_{ipct} = &\sum_{d=100}^{1000} \alpha_d mkt100_{p,t,d,k} + \alpha_{1200} mkt200_{p,1200,t} + \\ &\sum_{d=100}^{1000} \alpha_d \mathit{aff}100_{p,t,d,k} + \alpha_{1200} \mathit{aff}200_{p,1200,t} + + \\ &\gamma_{ct} + X_p + X_i + X_{it} + e_{ipct}\end{aligned} \qquad \text{(Reduced form)}$$

Figures 39a and 39 show the first stage and reduced form results. These plots are familiar, showing the same qualitative relationships seen in the previous section. There are four differences. First, the first stage is now built using the individual panel, rather than the parcel panel, so that each rent observation is multiplied by the number of inhabitants in that parcel. Second, rent is measured as the annual percent change in rent, rather than levels. Third, the reduced form is now estimated using a linear probability model rather than a Cox proportional hazards model to make the standard error calculation straightforward. Fourth, I now include cubics instead of quadratics of parcel-level controls, like residential units and distance ot the Financial District, to increase the F-statistic of the instrument.

Table 4 shows results from the second stage and a naive regression of displacement on rents. In the naive regression, a 1% decrease in the change in price *increases* displacement by 0.00025 percentage points, or about 2.4%. This negative sign reflects the endogeneity problem: higher prices are associated with less displacement, because developers are more likely to build in areas that are already gentrifying, prices are already rising, and residents are already more likely to be richer. The IV results in the second column reverse the sign. Using only exogenous variation in the annual change in rents, a 1% decrease in monthly rent would cause a 0.0129 percentage point decrease in displacement risk, or a 14.44% decrease.[27] The IV estimate is modestly larger than the implied elasticity discussed above, suggesting a rent elasticity of displacement of 14.4.



(a) First Stage



(b) Reduced Form

Figure 39: IV: First Stage and Reduced Form

Note: These plots show the results from running the First stage and Reduced form specifications, respectively. Both specifications use individual panel data and include microneighborhood by year fixed effects, sex and birthyear fixed effects, and interaction of rent control with a cubic in years lived at that address, and parcel-level controls including latitude and longitude, cubics in distance to the financial district and Caltrain station, landuse zoning, 2010 Census tract median income tercile, and a cubic in residential units.

12 Welfare calculations

This paper has identified the net effect of a joint shock to the supply and demand for housing near new construction. The net effect is negative, indicating that the supply effect dominates any potential demand effect, but there is also evidence that the demand effect is nonzero. The natural next step would be to decompose the net effect into separate supply and demand effects and calculate changes in welfare.

However, decomposing the net effect would require me to find supply and demand shifters that separately identify each elasticity η_S and η_D and the intercepts of each curve. Since I do not have either, I turn to the literature.

I use three different estimates for San Francisco's housing supply elasticity to compute a range of estimates of changes to landlord surplus. First, Green et al. (2005) estimate that San Francisco's housing supply elasticity is 0.14. Second, Saiz (2010) estimates San

[27] To estimate the percent decrease in displacement risk, I first calculate the predicted probability for a 1% decrease in rents: $\hat{y} = \beta \cdot \Delta P/P$. Then I calculate the percent change in risk as $(\hat{y} - \bar{y})/\bar{y}$.

Francisco's housing supply elasticity to be 0.66. Third, I use Asquith (2016)'s estimates to calculate a pseudo-supply elasticity of 0.277, discussed below.

The authors use different approaches to arrive at their estimates. Green et al. (2005) apply MSA-level data to a a simple theoretical model in which housing supply elasticity is a function of the cost of capital, city population, density, transportation costs, property taxes, and housing prices. Saiz (2010) uses relative shocks to labor productivity or to amenities as demand shifters, using detailed data from nearly 100 cities. Asquith (2016) instruments for demand shocks using proximity to potential tech bus stops in San Francisco, identifying the impact on evictions from rent-controlled units. To estimate the implied supply elasticity, I treat this eviction response as an expansion of the housing supply available on the market.[28]

Notably, all three of these estimates are considerably larger than the estimate of 0.09 used by the City of San Francisco (Egan, 2014). The City uses an esimate of 0.09, derived by regressing $ln(Q) = \alpha + \beta \ln(p)$ where Q is the total number of housing units as reported by Census counts incremented by annual HUD building permits, and p is the average housing price from Zillow. For comparison, I will also calculate changes in landlord surplus using this elasticity.

For the elasticity of housing demand, I take an upper bound from the literature[29] and calculate a lower bound based on my findings. Albouy et al. (2016) estimate an average demand elasticity of 0.66 across major US metro-areas. Housing demand in San Francisco is likely to be less elastic both because San Francisco's unusual job market means that few other cities are good substitutes, and because geographical constraints mean that there are

[28] Asquith estimates that a 6.4% increase in housing prices drives an additional 6,892 evictions. To calculate the percent change in the housing stock, I use the 2008 estimate of the number of housing units in the city: 389,787. The results are qualitatively unaffected by using the estimate of the average number of units over Asquith's study period, 390,663, or the average over my study period, 391,007.

[29] If there were no demand shift, then the elasticity of demand implied by the changes in price and quantity is 1.61. I do not use this as an upper bound because it is more than double the highest estimates in the literature.

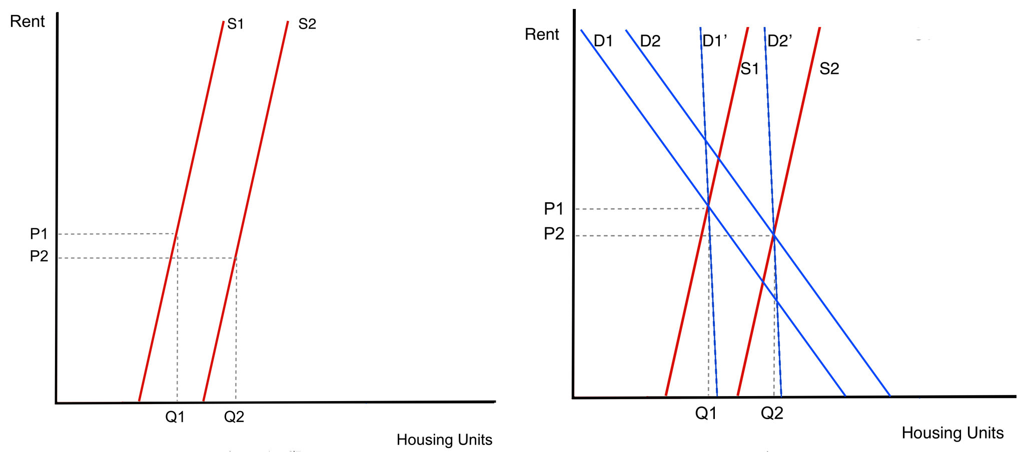
Table 4: IV: Impact of annual percent change in rent on displacement risk

	(Naive)	(2SLS)
pchange	-0.025	1.285
	(0.003)	(0.345)
Observations	6369866	6369866
F statistic (full model)	65.463	50.885
F statistic (instrument)	-	13.89

Note: Both specifications include microneighborhood by year fixed effects and control for person characteristics (sex, birth decade, and a cubic in years lived at current address) and parcel characteristics (cubics in distance to the Financial District and Caltrain station, a cubic in residential units, landuse zoning, 2010 Census block income tercile, year built, rent control status, and latitude and longitude). The sample is restricted to people who live in parcels that are within 2km of exogenous construction at some point in the study period, and who have been living at their address for at least one year. Standard errors are clustered by microneighborhood.

few substitutes for living in the San Francisco metro-area for people who have chosen to work in San Francisco. In fact, the City of San Francisco estimates that its elasticity of rental housing demand is 0.6 (Egan, 2014).[30]

Figure 40a displays known information: assuming that the estimates for η_S from the literature are accurate, I know the slope and intercept of the supply curves S_1 and S_2 and the equilibria (P_1,Q_1) and (P_2,Q_2) . Figure 40b shows the bounds that I can put on the slope and intercept of the demand curves, with D_1 and D_2 defined by Albouy et al. (2016)'s estimate of η_D and $D_1^{'}$ and $D_2^{'}$ defined by my findings.



(a) Known Supply Curves and Equilibria

(b) Range of Possible Demand Curves

Using these estimates allows me to compute a range of changes to landlord and renter surplus under several key assumptions. First, I assume that the estimates for η_S are accurate. Second, I must assume that the supply and demand curves are linear and that their slopes are constant over time. Third, I abstract from neighborhood versus aggregate effects, calculating an average effect rather than a local one.

Under these assumptions, I can calculate the welfare impact of the rent reduction. From η_S, η_D, and the original housing supply and price level observed in the data, I can calculate the slopes of the supply and demand curves and their intercepts:

$$m_{S,D} = \eta_{S,D}^{-1} \cdot \frac{P}{Q} \tag{12.1}$$

$$P = m_{S,D} \cdot Q + a_{S,D} \tag{12.2}$$

Assuming that the slopes of the supply and demand curves do not change, I can calculate the intercepts at the beginning and end of the study period as shown above, and then

[30]The City calculates the demand elasticity by regressing $ln(pQ) = ln() + (1 + 1)ln(p) + 2ln(y)$, using 2005-2011 Public-Use Microdata for household income and a price index p constructed from Zillow's average housing value for San Francisco for the same period. As in their supply estimation, Q is the total number of housing units as reported by Census counts incremented by annual HUD building permits.

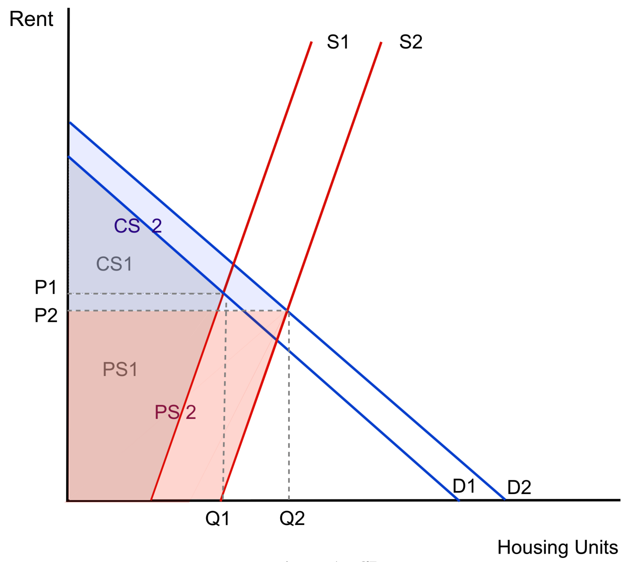


Figure 41: Calculating Consumer and Producer Surplus

Note: This figure shows the calculation of the change in consumer and producer surplus, assuming linear supply and demand curves that can shift in intercept but not in slope. It is not drawn to scale: the true supply and demand curves are so inelastic that it would be difficult to see the changes in an illustration drawn to scale.

calculate the change in renter surplus and landlord surplus, as depicted in Figure 41:

$$\Delta CS = 0.5(Q_2 \cdot (a_{D2} - P_2) - Q_1 \cdot (a_{D1} - P_1)) \tag{12.3}$$

$$\Delta PS = (a_{S2}/m_S) \cdot P_2 + 0.5 \cdot (Q_2 - a_{S2}/m_S) \cdot P_2 - (a_{S1}/m_1) \cdot P_1 + 0.5 \cdot (Q_1 - a_{S1}/m_S) \cdot P_1 \tag{12.4}$$

Estimates for the change in renter and landlord surplus implied by each estimate of η_S and η_D are shown in Tables 5 and 6. Landlord surplus increases modestly by \$3.1-5.8 million. Although incumbent landlords are made worse off by the reduction in rents, these damages are mitigated by the gains to the landlords of the new buildings. Renter surplus increases by at least \$11.3 million. Taking the most conservative estimate of renter surplus and the most generous estimate of landlord surplus, the increase in renter welfare is at least double the decrease in landlord surplus.

Table 5: Changes in Landlord Surplus

η_S	ΔPS	Source for η_S
0.14	3,356,669	Green et al. (2005)
0.28	4,013,364	Asquith (2016)
0.66	5,795,822	Saiz (2010)
0.09	3,122,135	Egan (2014)
0.29	4,060,271	Mean

Note: This table shows estimates of the housing supply elasticity in San Francisco from four other papers and the implied changes in landlord surplus.

Table 6: Changes in Renter Surplus

η_D	ΔCS	Source for η_D
0.66	11,361,704	Albouy et al. (2016)
0.6	12,497,875	Egan (2014)

Note: This table shows estimates of the housing demand elasticity in San Francisco and the implied changes in renter surplus.

12.1 Craigslist data creation

Craigslist has become a major platform for the rental housing market in the United States. The site connects potential tenants with landlords who post listings containing information like price, bedrooms, square footage, photos, and descriptions.

Listings expire, but many of them have been archived by the Wayback Machine, a non-profit that maintains a library of past internet content by taking repeated snapshots of webpages. I wrote python code using the packages BeautifulSoup and Selenium to navigate through all Bay Area apartment listings archived by the Wayback Machine from September 2000 to July 2018. Full details on the methodology and a walkthrough of the python code are available at https://www.katepennington.org/clmethod.

Limitations. There are three main drawbacks to using this data. First, Craigslist data do not capture the entire rental market. It is likely to systematically miss the highest end of the market, which may be dominated by real estate agents, and the lowest end of the market, which may be dominated by word of mouth.

Second, it is not complete in time: the Wayback Machine only archives websites sporadically. Luckily, the timing of archive events is plausibly random, so the data can still be used for causal inference. (It's unclear exactly how Wayback decides when to archive which pages.)

In addition, the Wayback Machine does not archive every listing on every date. Usually, it only archives the first 100-120 results (that is, you cannot click 'next' on the archived pages). While this reduces the numer of recoverable listings, it probably doesn't introduce bias: the top 100-120 results are whichever results were most recently posted when the archive event began.

Third, the data are not continuous (or perfectly reliable) in space, either. Some areas, like the Mission district, have hundreds of postings over the entire 2000-2018 period. But for other areas, like the mostly single-family home Sunset neighborhood, postings are sparser. Other times, Craigslist users failed to enter accurate location data or the location data cannot be confidently matched to a specific place. For example, if someone entered '4th St' instead of a neighborhood, its not possible to tell which Bay Area town its in.

Data cleaning and interpolation. The goal of the data cleaning and interpolation exercise is to move from a listing-date data set to a parcel-year data set. I begin by matching each listing to a realtor neighborhood shapefile that corresponds with Craigslist neighborhoods. Then I create a set of distance weights for each Craigslist neighborhood for each parcel by calculating the distance from the neighborhood centroid to the parcel centroid. The interpolated rent at parcel p in year t is the distance-weighted average of rents in all Craigslist neighborhoods within 2.5 kilometers in year t.

12.2 Randomization Inference

Figure 10 shows the spatial correlation of treatment in two consecutive years. Unlike other studies which consider the effect of a single treatment on each unit of observation (Asquith et al., 2021; Li, 2019; Diamond and McQuade, 2019), many of the parcels in this study are exposed to repeated treatments as additional new buildings are completed over the study period. The spatial correlation of the error terms changes over time. For this reason, it may not be sufficient to cluster standard errors within a static spatial area. I address this problem by using randomization inference to create a distribution of log-likelihood statistics under the null hypothesis from simulations of spatially correlated random assignment. I calculate p-values by locating the estimated log-likelihoods in this simulated distribution.

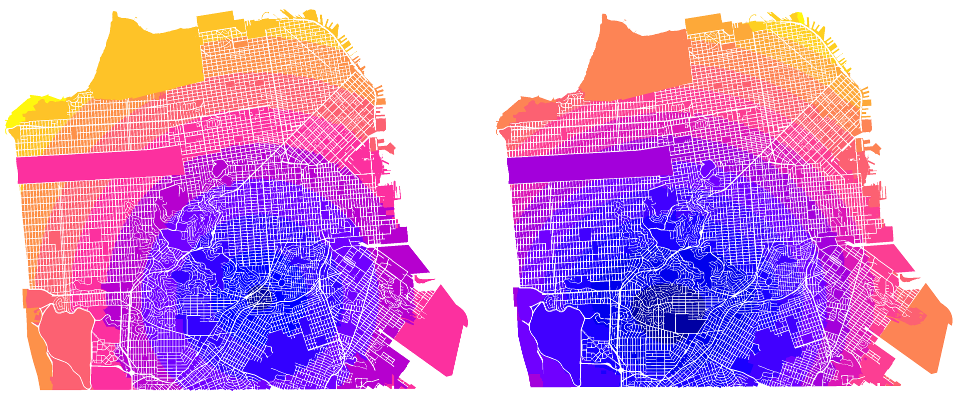
To mimic the data generating process for randomization inference, I need to preserve the spatial relationships in the data but randomly vary treatment location. I do this in two steps. First, I use the real data to create a treatment surface. Second, I create a rule for overlaying this treatment surface on the map of parcels in a random location. Imagine that the treatment surface is one sheet of paper and the map of land parcels is another. To create each simulated data set, I randomly slide the treatment sheet over the parcel sheet. This process preserves the spatial relationship between treatments.

The rule for where to locate the treatment surface is as follows. First, I randomly vary which parcel receives simulated new construction within a microneighborhood-year. A neighborhood-year that truly had no construction will also have zero simulated construction. A neighborhood-year that had one project will have one simulated project of that same size, translated to a randomly selected location.

This new location for simulated construction determines how the entire treatment surface shifts. I calculate the distance and direction between the real construction parcel A and the simulated construction parcel B. Then I translate the treatment surface by this distance and direction.

Figure 42 shows how recentering the treatment surface from parcel A to parcel B affects treatment status across the whole city. Now each parcel is exposed to a simulated treatment intensity that follows the same pattern of spatial correlation as the true treatment surface.

This procedure generates standard errors that are generally smaller than the standard errors from clustering at the microneighborhood level. Figure ?? plots the distribution of coefficients from equation 4.3. In the main paper, I report the more conservative clustered standard errors.

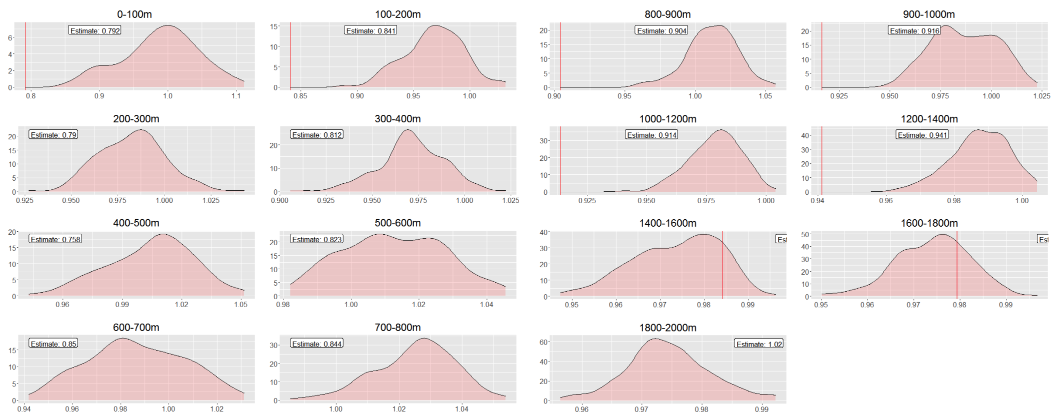


(a) True Exposure to Construction

(b) Simulated Exposure to Construction

Figure 42: Randomly Translating the Treatment Surface

Note: This figure gives an example of how I define a treatment surface over the city and then randomly translate it to conduct randomization inference. Darker colors are closer to the new construction. This strategy preserves the spatial correlation of treatment and ensures that significance is calculated based on small differences in exposure.



(a) Cumulative bins from 0-800m

(b) Cumulative bins from 900-2000m

Figure 43: Distribution of Coefficients from Randomization Inference

Note: These figures plot the distribution of coefficients from running 1,000 iterations of equation 4.3 using simulated data following the randomization procedure described in Appendix 12.2.