

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

Kate Pennington[†]

[Latest version available here](#)

June 15, 2021

Abstract

Major cities around the world are gentrifying as high-income newcomers drive up housing prices and displace lower-income incumbent residents. Increasing the housing supply should mitigate rent increases, but new buildings could also stimulate demand for nearby housing by improving neighborhood quality. The net impact depends on how the relative sizes of these supply and demand effects play out over space. This paper identifies the causal impact of new construction on nearby rents, displacement, and gentrification in San Francisco by exploiting random variation in the location of new construction induced by serious building fires. I combine parcel-level data on fires and new construction with an original dataset of historic Craigslist rents and a panel of individual migration histories to test the impact of proximity to new construction. I find that rents fall by 2% for parcels within 100m of new construction. Renters' risk of displacement to a lower-income neighborhood falls by 17%. Both effects decay linearly to zero within 1.5km. Next, I identify a hyperlocal demand effect, with building renovations and business turnover spiking within 100m. Gentrification follows the pattern of this demand effect: parcels within 100m of new construction are 2.5 percentage points (29.5%) more likely to experience a net increase in richer residents. Affordable housing and endogenously located construction do not affect rents, displacement, or gentrification. These findings suggest that increasing the supply of market rate housing has beneficial spillover effects for incumbent renters, reducing rents and displacement pressure while improving neighborhood quality.

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, and Reed Walker for their thoughtful feedback. I am grateful for comments from my PhD cohort at Berkeley ARE, participants at UC Berkeley's Real Estate and Environmental and Resource Economics seminars, 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 housing prices in downtown areas 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 residents (Couture et al., 2019; Brummet and Reed, 2019; Ellen et al., 2019; Ding et al., 2016; Guerrieri et al., 2013).² Rising housing prices, displacement, and gentrification often occur together, but 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 and rates of displacement and gentrification nearby is ambiguous. Increasing the housing supply could ground soaring housing prices and slow demographic change. However, building new, high-quality housing could also increase demand for nearby housing by improving neighborhood quality. If these demand effects are larger than the supply effects, new construction could accelerate local displacement. Disagreement over the net effect and spatial dynamics has led to contentious policy debate (Monkkonen (2016); Zuk and Chapple (2016), [48 Hills 2020](#)³, [San Francisco Magazine 2018](#)⁴). Some housing advocates argue that all new construction should be affordable, that is, low-rent and income-restricted.⁵ 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

¹Qualitatively, 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.”

²In 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.

³The article 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.”

⁴The article is titled, “Is This Oakland Developer Building Sorely Needed Housing—or Dropping Gentrification Bombs?”

⁵For 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 2018⁶

impact of new market rate housing different from the impact of new affordable 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% of San Francisco Bay Area residents feel there is a housing crisis.⁷ Over my study period from 2003-2017, the average price of a one-bedroom apartment listed on Craigslist increased 97%. Systemic racial inequality means increasing 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%.⁸ 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). In this paper, I study the neighborhood impact of new housing construction in San Francisco from 2003-2017.

This paper overcomes two challenges to research on this topic. The first is an identification problem: it is well-documented that developers are more likely to build in areas that 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. I discuss this identification strategy in detail in Section 4.1.

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

⁷Quinnipiac University poll, 2019.

⁸SF City Planning Department analysis of IPUMS data

histories with proxies for income. First, I leverage data on individual address histories from the consumer data company Infutor. These data allow me to track the addresses of 1.24 million people who lived in San Francisco between 2003 and 2017. I focus my analysis on renters by excluding households that filed for an owner-occupation tax benefit. While these data do not include individual income, I can match each person's zipcode to the median zipcode income from the Internal Revenue Service (IRS). This allows me 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. Surveys from San Francisco, New York, Seattle, and Milwaukee all find that the need for cheaper housing is a primary driver of push migration.⁹ 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. Indeed, 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. Of course, not all moves to lower-income zipcodes are pushed, and some displaced households may move to higher-income zipcodes. As a robustness check, I show that the results are qualitatively the same when I use eviction notices as an alternative measure of displacement.

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 allow me to causally identify and compare the spatial impact of housing construction on rents, displacement, and gentrification. The microdata allow me to make key distinctions between displacement and gentrification, renters and owners,

⁹2019 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.

and market rate versus affordable construction¹⁰ that are not possible with aggregated data.

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 housing 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 buildings 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 assemble data on building owners' upgrade decisions, sales, and moves. If there are positive demand spillovers, building owners will internalize them by upgrading their own buildings (Hornbeck and Keniston, 2017). I find that building renovations and business turnover all increase within 100m. These findings match Glaeser et al. (2020)'s work showing that business turnover increases in gentrifying areas. However, the probability of owner moves and residential sales do not change. Given that new construction reduces rents for at least four years, this pattern may reflect a change in expectations about future housing price appreciation. Owners may see new construction as a signal of neighborhood upgrading, leading them to renovate their homes today so that they can achieve a higher sale price in the future.

I also explore demand effects by studying changes in the probability of endogenous new construction. If exogenous new construction creates a positive demand shock, then a standard supply and demand framework predicts that it will induce an additional supply increase. In other words, a positive demand shock would lead developers to 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. This finding supports the idea that owners may be anticipating future neighborhood change when they choose to renovate but not to sell.

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

¹⁰A 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.

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 residential renovations, business turnover, and the permitting of new endogenously located construction 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 or the probability of displacement and gentrification nearby. For policymakers who want to reduce both displacement and gentrification, new 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 a large and growing urban economics literature on the causes and consequences of 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). Low-wealth households are particularly likely to use their location asset rather than some other asset, because there are lower borrowing constraints on moving than on receiving a loan. In this way, displacement intensifies and perpetuates preexisting inequality.

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 impacts on rent, displacement, and gentrification in the same setting. Li (2019) finds that a 10% increase in New York City housing stock causes rents to decrease 1% within 500 feet. She also finds evidence of a smaller demand effect, with new high-rises attracting new restaurants. Combining data

across metro areas, Asquith et al. (2020) find that new construction decreases rents within 250m relative to 250-600 meters away by about \$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 completion time.

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 positive 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 (2016) 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. Sections 11 and 6 use the results to make welfare calculations and 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 than would occur endogenously. What are the impacts on people who live nearby? As in Asquith et al. (2020) and Diamond and McQuade (2016), 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 here given the small quantity of construction over the study period, but this framework and these results do not generalize to a large supply shock 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 :

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

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 traditional 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 data sets 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 and year built (and consequently rent control status) for all land parcels in San Francisco. I add City data on zoning at the parcel level. Of the 160,706 parcels in the data, 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 of every new construction project in San Francisco from 2003-2017. 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 new units are market rate, although the number of affordable units has been trending up. 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.¹¹ 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 building fire and its damage record from 2011, and Table 2 counts the number of these fires occurring in each year. In total, 158 fires serious enough to affect the probability of construction occurred from 2003-2017.

Combining the data on building fires with the data on new construction 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 will 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 full sample.

3.3 Displacement and gentrification

The heart of this paper relies on individual address histories provided by the consumer data company Infutor.¹² 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 anywhere in the United States. Diamond et al. (2018) 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 distinguish between owners and renters using the Office of the Assessor-Recorders secured property tax roll, which identifies addresses that received a tax break for owner occupation.¹³

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.¹⁴ I also use this data to proxy for the relative wealth of arrivers and leavers

¹¹I 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.

¹²I thank the Upjohn Institute for Employment Research for granting me a fellowship to access the Infutor data.

¹³California provides a Homeowners' Property Tax Exemption, which offers a \$7,000 reduction in the taxable value of a qualified owner-occupied home.

¹⁴This cutoff is arbitrary. Results are robust to alternative definitions, such as $\pm 1/2$ standard deviation. The goal is

when I calculate gentrification variables. Figure 9 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.¹⁵ 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. For households earning less than 30% of the Area Median Income (about \$83,000 in 2014), the problem is severe: the majority spend more than half of their monthly income on rent. Figure 6 shows rent burden by income group.¹⁶ I find supportive evidence for this approach 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 ($p = 0.00$).

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.¹⁷

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.

¹⁵2019 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.

¹⁶SF City Planning Department analysis of American Community Survey 2011-2014 estimates.

¹⁷This 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 Census blockgroup, has almost zero correlation with a motivational measure ($\rho = 0.06$). The individual approach performs better, with a correlation of $\rho = 0.64$.

3.4 Rental prices

The city of San Francisco does not track rental prices. I construct an original panel data set on historic rental prices by scraping archived Craigslist ads from 2003-2017. These ads are archived by a nonprofit called the Wayback Machine, which sporadically archives 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 11.1. Figure 7 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 are likely capturing different segments of 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 Craigslist data is \$2,759 compared to \$3,422 in the Zillow data over the period 2014-2017.¹⁸ Figure 8 in the Appendix 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 also 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

¹⁸Calculated using publicly-available Zillow data at the zipcode level.

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

4.1 Identification strategy

The clear identification challenge is that the timing and location of new construction are endogenous: developers are likely to build in the same areas that are already experiencing increased rents, displacement, and gentrification (Green et al., 2005; Li, 2019; Asquith et al., 2020).

I exploit exogenous variation in the location of new construction caused by serious building fires. Regulation and geography combine to make San Francisco one of the most difficult places to build housing in the United States (Albouy and Ehrlich, 2012; Saiz, 2010). Serious fires, 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 agreement to pay the tenant to leave. In 2015, the median buyout per tenant was \$18,000 and the maximum was \$325,000.¹⁹ 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 ($p=0.007$).

This identification strategy exploits exogenous variation in the location, but not the timing, of new construction. The limited study window means I cannot predict the timing of redevelopment based on the timing of a fire. There is wide variation in the time lag between fire and redevelopment: on average, 4.8 years pass between the fire and the permit application for new construction (sd 3.6); 7.2 years before completion (sd 4.2). Furthermore, not every burned parcel is redeveloped during my study period. The fire data starts in 2003, so there are undoubtedly post-fire construction projects in

¹⁹San Francisco Open Data, accessed 2 October 2019.

the data that I am not able to identify. Similarly, many – perhaps all – of the burned parcels in my data will ultimately be redeveloped sometime in the future. Figure 12 displays variation in time to construction.

For this reason, I do not claim that fires predict the timing of construction. Instead, I follow the literature to argue that project permit and completion dates are quasi-random (within a band of, say, 1-2 years) due to bureaucracy and construction management (Li, 2019; Asquith et al., 2020). Similarly, San Francisco's strict zoning laws mean that project size is determined by project location. Put differently, developers may want to build a certain quantity of a certain type of housing in any given year. The fires make it more likely that they build on parcel A compared to nearby parcel B.

I use the incidence of serious fires to identify a subset of new construction whose location is plausibly exogenous within a micro-neighborhood. I will estimate the effect of proximity to new construction using only these exogenously located projects, although I will also show results for endogenously located projects for comparison. Using this strategy, I identify 47 parcels that receive exogenously located market rate projects and 11 parcels that receive exogenously located affordable projects.²⁰

My identifying assumptions are that 1) serious fires increase the chance of construction on that parcel relative to other parcels within the same 1 km² micro-neighborhood, and 2) they are unrelated to displacement trends. The first assumption is easy to demonstrate. During my study period, 27.22% of burned parcels receive new construction compared to 1.11% of unburned parcels. Controlling for micro-neighborhood and year, this is a 32-fold increase in the probability of construction ($p=0.0000$).

To provide evidence for the second assumption, I conduct a series of balance tests. Table 3 shows baseline characteristics for parcels near burned versus unburned parcels and redeveloped versus not redeveloped parcels, controlling for micro-neighborhood-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 are no more likely to see residents move to poorer zipcodes than burned parcels that are not redeveloped within the study period. 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

²⁰Table 1 reports a total of 60 exogenous projects because one parcel receives multiple projects.

versus 0.006, $p = 0.011$). These differences do not exist between redeveloped and undeveloped parcels, which is the time comparison I exploit.

4.2 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. Asquith et al. (2020) and Li (2019) use differences in differences with near and far distance bins, which requires stable near and far areas. Diamond and McQuade (2016) use a nonparametric difference in differences strategy, constructing an empirical derivative by smoothing differences in housing prices over space. Hornbeck and Keniston (2017) nonparametrically estimate a distance gradient and a cutoff point for the spillover effects of the Great Boston Fire. These approaches require static measures of treatment intensity and clear and stable divisions between the pre- and post-periods. In fact, Asquith et al. (2020) intentionally limit their sample to housing units that are only treated by one project during their study period.

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 10 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 11 maps this approach over the city of San Francisco for the years 2015 and 2016. To deal with potential selection issues, I limit my 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 between this study and other studies 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; the average rate of moves to poorer zipcodes, my proxy for displacement, is only 1.03%. After moving, 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 micro-neighborhood (analogous 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 micro-neighborhood 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 micro-neighborhood 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 station²¹, 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 study-style plots 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-style 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 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

²¹ Caltrain is a train running from San Francisco to Silicon Valley, a second hub for high-paying jobs in the Bay Area.

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 ($mk100_{p,t,d,k}$ and $aff100_{p,t,d,k}$); distance bins from 1-2km are 200m wide ($mk200_{p,t,d,k}$ and $aff200_{p,t,d,k}$).

The estimating equation is:

$$\begin{aligned} move\ poorer_{ipt} = & \sum_{k=-2}^3 \left(\sum_{d=100}^{1000} \left[\alpha_{dt} mk100_{p,t,d,k} + \beta_{dt} aff100_{p,t,d,k} \right] + \right. \\ & \left. \sum_{d=1000}^{2000} \left[\alpha_{dt} mk200_{p,t,d,k} + \beta_{dt} aff200_{p,t,d,k} \right] \right) + \\ & \lambda_{sbc0}(t) + X_{ipt} + X_p + \epsilon_{pct} \end{aligned} \quad (4.1)$$

In addition to this survival model, I use ordinary least squares to study the effect of new construction on rents and other parcel-level outcomes. These specifications are run on panel data on land parcels. I include micro-neighborhood by year fixed effects γ_{ct} and the same set of parcel controls X_p including latitude and longitude, rent control status, distance to the financial district and Caltrain station, landuse zoning, 2010 Census tract median income tercile, and a quadratic in residential units:

$$\begin{aligned} rent_{pct} = & \sum_{k=-2}^3 \left(\sum_{d=100}^{1000} \left[\alpha_{dt} mk100_{p,t,d,k} + \beta_{dt} aff100_{p,t,d,k} \right] + \right. \\ & \left. \sum_{d=1000}^{2000} \left[\alpha_{dt} mk200_{p,t,d,k} + \beta_{dt} aff200_{p,t,d,k} \right] \right) + \\ & \gamma_{ct} + X_p + v_{pct} \end{aligned} \quad (4.2)$$

In both specifications, I cluster standard errors at the micro-neighborhood level to address spatial correlation. Alternatively, I can correct standard errors for spatial correlation using randomization inference as described in Appendix 11.2. I report clustered standard errors here because they are larger and more conservative.

4.3 Event study results

Since I expect new construction to affect displacement through housing prices, I begin with event-study style plots of results 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 begins in the year of completion, with rents within 100m falling by \$0.48 per new housing unit and decaying to zero over

distance. The rent reduction persists for at least four years. These findings imply that the risk of moving may fall as early as $k = 0$ and remain lower for at least four years.

Alternatively, I can measure treatment exposure using binned counts of the number of completed projects, rather than completed units. Figure 15 displays a similar pattern: there is no impact on rents in the pre-period, but a strong distance gradient emerges the year before completion and persists for at least four years. In $k = -1$, monthly rents fall by \$100 per project for people living within 100m. Seeing effects beginning the year before completion is consistent both with nuisance effects of construction and with the sale of units before completion. This effect decays in distance and time.

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, and persists for several years. Figure 16 displays a similar pattern for the impact of the count of completed projects, with displacement risk falling beginning in year 0. These estimates are noisier than the estimates for rent because there is less variation in the moving dummy than in rents.

Plots 17a and 17b show these results as smoothed surfaces. Distance is shown on the x-axis, time relative to project completion is shown on the y-axis, and the coefficient is shown on the z-axis. Standard errors are shown as transparent surfaces. Both plots show the same relationship: the effect is zero for all distances in the pre-period. Starting by year 0, rents and displacement risk fall for nearby parcels.

The effects identified by Equations 4.1 and 4.2 may continue beyond $k = 3$. To determine the longer-term impacts of new construction, I run the specifications again using lags from $k \in [4, 9]$. 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 my study window only runs from 2003-2017. These longer-term plots (Figures 18, 19, 20, 21) suggest that impacts on rents persist for at least 9 years, while impacts on displacement risk decay to zero by $k = 5$.

4.4 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:

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

The corresponding OLS specification is:

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

5 Results

5.1 Displacement

The results from Equation 4.3 show a clear distance gradient. Figure 22 shows that both rents and the risk of adverse moves plunge for people living near new market rate construction. For each additional housing unit within 100m, rents fall by \$0.20 and displacement risk falls by 0.10%. Figure 23 shows results from measuring exposure to total projects, rather than total units. 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.

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 24 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. It is important to note that eviction notices are not evictions: tenants can redress the causes stated in a just-cause eviction notice (overcounting) and landlords can pressure tenants to leave without going through the formal eviction process (undercounting). Still, eviction notices provide a useful additional source of information about displacement. I find that the probability of eviction drops by 0.77 percentage points (31.09%) for tenants living in rent controlled apartments within 100m of a new project (Figure 25). Consistent with the conceptual framework and with prior research, the probability of eviction does not change for tenants of uncontrolled apartments. While landlords of uncontrolled units can simply raise rents, landlords of rent controlled units can only raise rents in between occupancies. San Francisco's rent control policy sets a maximum annual rent increase for units in multifamily buildings built before 1979. When tenants move out, landlords are free to set a new rent in their agreement with their next tenant, which will then also be limited to a modest annual increase. When prices rise, this creates an incentive to remove tenants through buyouts or eviction. The finding that evictions decrease only for rent controlled units is consistent with a supply effect that has reduced the opportunity cost of a low-paying 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 compare the impact of exogenous market rate construction with three other types of construction: exogenously located affordable housing, endogenously located market rate housing, and endogenously located affordable housing. Figure 26 displays the event study of affordable projects on rents. There is no clear change in this spatial pattern after new construction. Rents near new construction are roughly \$50 higher in year 2, but this matches the pattern in year -2. Similarly,

Figure 27 shows proximity to affordable projects does not affect moves to poorer zipcodes.

Figure 28 compares the average total impact of exogenously located market rate projects with the average total impact of exogenously located affordable and endogeneously located projects. The only type of construction that affects prices and displacement is exogenously located market rate. 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. Endogeneously located housing does not affect prices either. This finding agrees with previous work showing that developers build where prices are already appreciating. In San Francisco at least, finding a source of exogenous variation in the location of new construction is necessary for identifying causal effects.

The rent elasticity of displacement

The results shown in Figures 23a and 23b 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 $\Delta rent_t = (rent_t - rent_{t-1})/rent_{t-1}$:

$$\begin{aligned} \Delta rent_{pct} = & \sum_{d=100}^{1000} \alpha_d mkt100_{p,t,d,k} + \alpha_{1200} mkt200_{p,1200,t} + \\ & \sum_{d=100}^{1000} \alpha_d aff100_{p,t,d,k} + \alpha_{1200} aff200_{p,1200,t} + \\ & \gamma_{ct} + X_p + X_i + X_{it} + e_{ipct} \end{aligned} \quad (\text{First stage})$$

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

$$move\ poorer_{ipct} = \widehat{\Delta rent}_{pct} + \gamma_{ct} + X_p + e_{pct} \quad (\text{Second stage})$$

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

$$\begin{aligned} move\ poorer_{ipct} = & \sum_{d=100}^{1000} \alpha_d mkt100_{p,t,d,k} + \alpha_{1200} mkt200_{p,1200,t} + \\ & \sum_{d=100}^{1000} \alpha_d aff100_{p,t,d,k} + \alpha_{1200} aff200_{p,1200,t} + \\ & \gamma_{ct} + X_p + X_i + X_{it} + e_{ipct} \end{aligned} \quad (\text{Reduced form})$$

Figures 29a and 29 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 to 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.²² The IV estimate is modestly larger than the implied elasticity discussed above, suggesting a rent elasticity of displacement of 14.4.

²²To 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 $(\hat{y} - \bar{y})/\bar{y}$.

5.2 Demand effects

We have seen that the net effect of proximity to new construction on rents is negative. This negative net effect suggests that the supply effect dominates, but the demand effect may still be nonzero. I assemble a set of alternative dependent variables to pinpoint changes in demand.

If new construction increases neighborhood quality, then the traditional supply and 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 variable for new permits. I find that the probability of new endogenous construction more than doubles within 100m of new projects (Figure 31).

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 30 provides evidence of 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.

Next, I examine impacts on residential sales and sales prices. I find no evidence of a change in the likelihood of a sale or in the residential sales price. Accordingly, I do not find evidence of impacts on the likelihood of owner moves (Figure 32).

These findings suggest that new construction may have changing dynamics over time. Shortly following completion, prices fall and renters are more likely to be able to afford to stay. But the neighborhood upgrade may also signal to building owners that the area may appreciate over time. Owners prepare for this appreciation by renovating buildings, and new businesses open to serve a changing population. The results on gentrification in the next section support this idea.

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 my 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:

$$from_richer_{ip} = \alpha + \beta exog_p + \gamma aff_p + \delta exog_p \times aff_p + e_i \quad (5.1)$$

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 micro-neighborhood by year, individual, and parcel controls:

$$from_richer_{ipct} = \tilde{\beta} exog_p + \tilde{\gamma} aff_p + \tilde{\delta} exog_p \times aff_p + X_i + X_p + \gamma_{ct} + \tilde{e}_i \quad (5.2)$$

Table 5 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 observe the total number of arrivers and movers, and the number of arrivers and movers from richer or poorer zipcodes. Then I construct a parcel-level indicator variable equal to 1 if the net increase in wealthy people, captured 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):

$$gent_{pt} = 1\{(arrivers_richer_{pt} - movers_richer_{pt}) - (arrivers_poorer_{pt} - movers_poorer_{pt}) > 0\} \quad (5.3)$$

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 33 shows that parcels within 100m of market rate construction are 2.5 percentage points (29.5%) more likely to gentrify, with the effect decaying to zero within 700m. Neither exogenously located affordable construction nor endogenously located construction has any differential impact on gentrification.

What is driving this increase in gentrification? I decompose the gentrification dummy by studying each term separately. Figure 34 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 11.

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: $\sum_0^{2000} \hat{\alpha}_d * \overline{mkt100}_{p,d,t} \times n$. 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 gentri-

fication. 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 quality²³ 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. If the city continues to gentrify 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.

In conclusion, policymakers who want to slow displacement and gentrification should accelerate both market rate and affordable housing construction. The high rent elasticity of displacement also suggests that policies like rental assistance and a universal basic income (UBI) could be efficient, cost-effective ways to meaningfully reduce displacement and preserve income diversity.

Author affiliations: University of California, Berkeley Department of Agricultural and Resource Economics. Fellow at UC Berkeley's Institute for Research on Labor and Employment.

²³Neighborhood 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

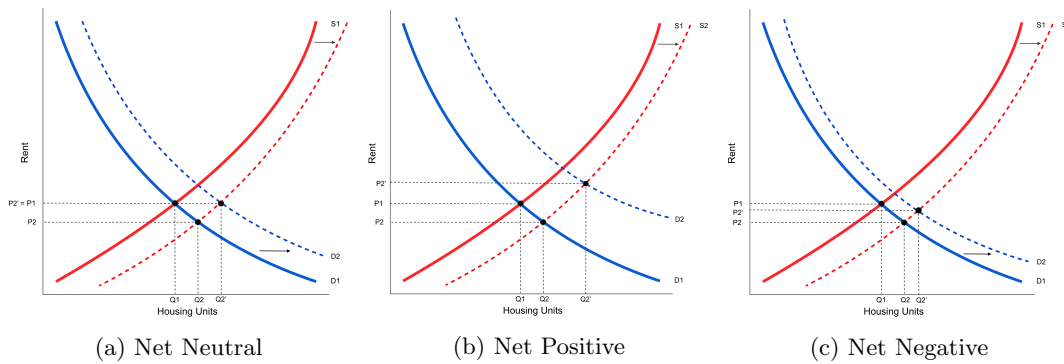


Figure 1: Supply and Demand Scenarios

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.

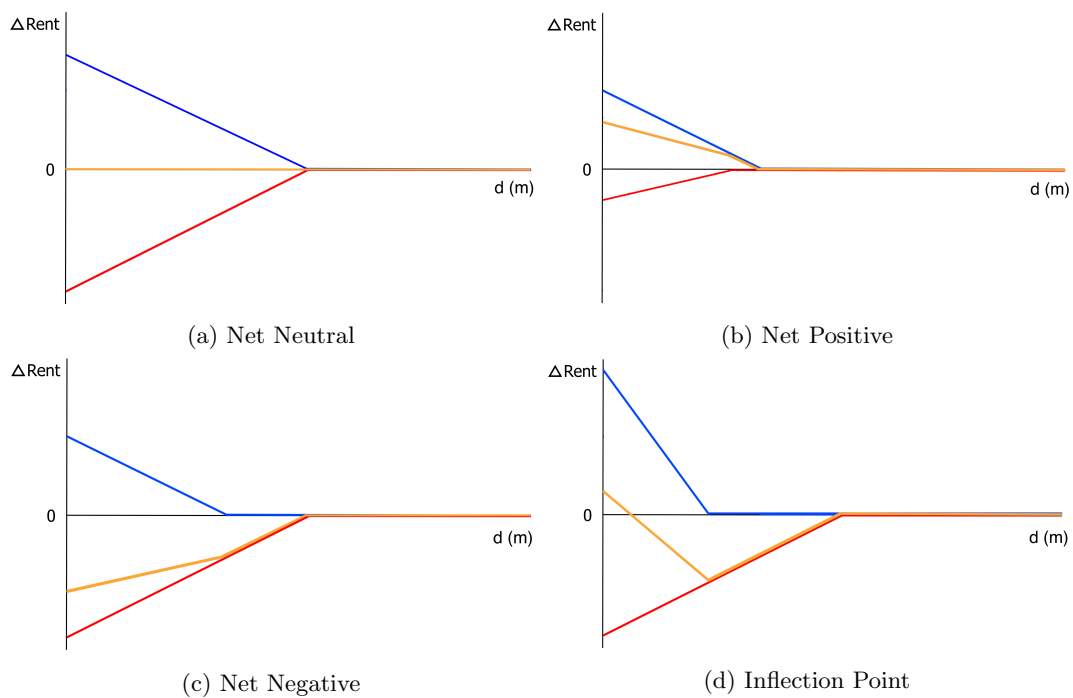


Figure 2: Spatial Supply and Demand Scenarios

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.



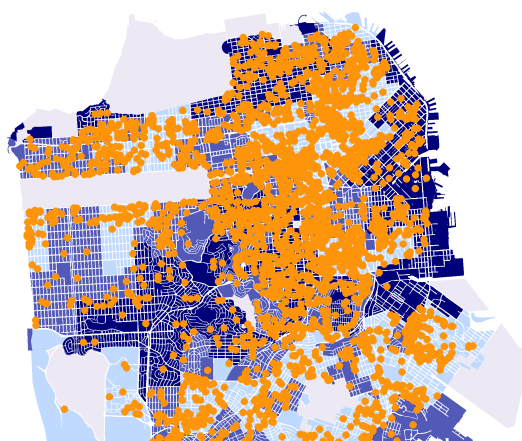
(a) Market Rate



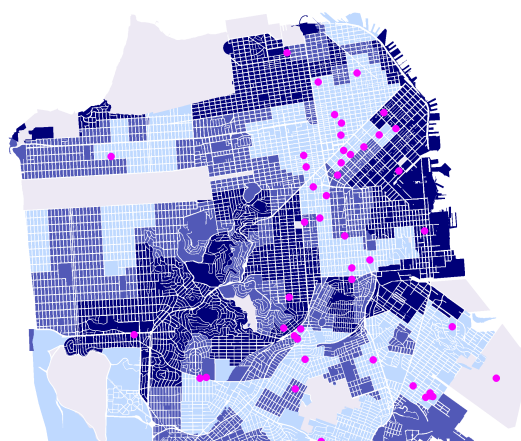
(b) Affordable

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

City and County of
San Francisco

Home Permit Services Plan Review Inspection Services Most Requested Key Pri

Home > Most Requested

Welcome to our Permit / Complaint Tracking System!

COMPLAINT DATA SHEET

Complaint Number:	201178596	Date Filed:	
Owner/Agent:	OWNER DATA SUPPRESSED	Location:	1502 GOLDEN GATE AV
Owner's Phone:	---	Block:	0753
Contact Name:	---	Lot:	032
Contact Phone:	---	Site:	
Complainant:	COMPLAINANT DATA SUPPRESSED	Rating:	
		Occupancy Code:	
Complainant's Phone:		Received By:	Christina Wang
Complaint Source:	TELEPHONE	Division:	BID
Assigned to:	CES		
Division:			
Description:	Building fire causing major damage to apartment building		

(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.

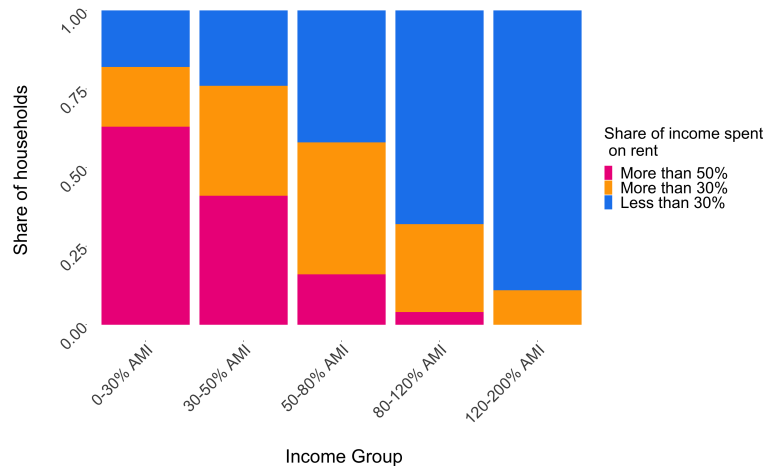


Figure 6: Rent Burden and Income

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.

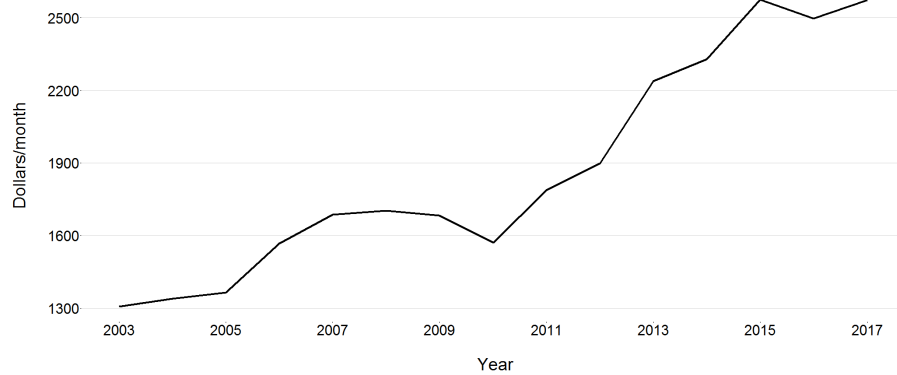
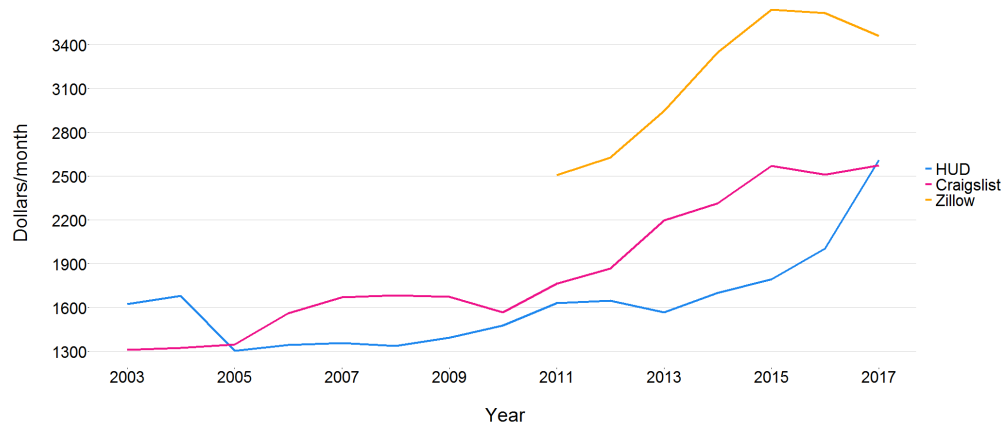


Figure 7: Average Monthly 1BR Rent, 2003-2017

Note: This figure plots average Craigslist rents for a 1 bedroom apartment by year.

Figure 8: Comparing Median 1BR Rents from Craigslist with Department of Housing and Urban Development (HUD) and Zillow



Note: This plot compares the median 1-bedroom Craigslist rent from my data collection process with the median 1-bedroom rent from HUD and the mean 1-bedroom rent from Zillow (Zillow does not make median rent available). HUD draws from several data sources, including gross rents data from the U.S. Census Bureau, gross rent information from HUDs American Housing Survey, and yearly telephone surveys. Zillow's rental price calculation methodology is discussed [here](https://www.zillow.com/research/rental-price-calculation/).

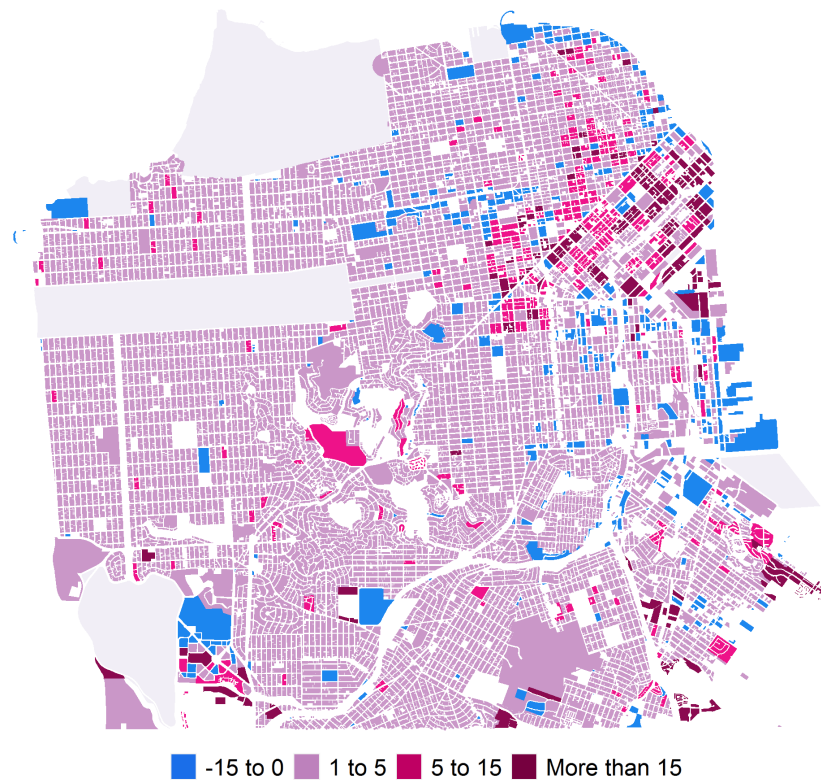


Figure 9: Change in Residents from Richer Zipcodes, 2003-2017

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, since parcels are difficult to see at this scale.

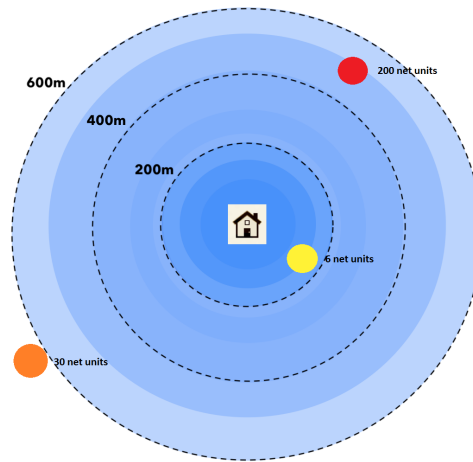


Figure 10: Measuring Treatment Exposure

Note: This figure 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.

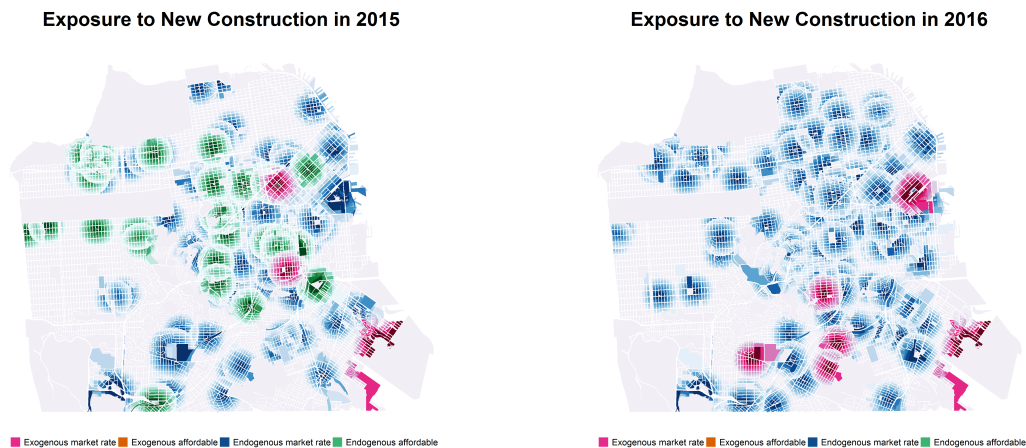


Figure 11: Variation in Treatment Exposure by Distance Bin

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.

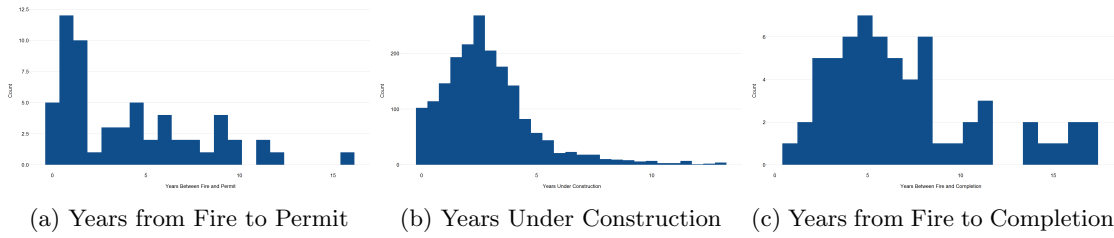


Figure 12: Project Duration

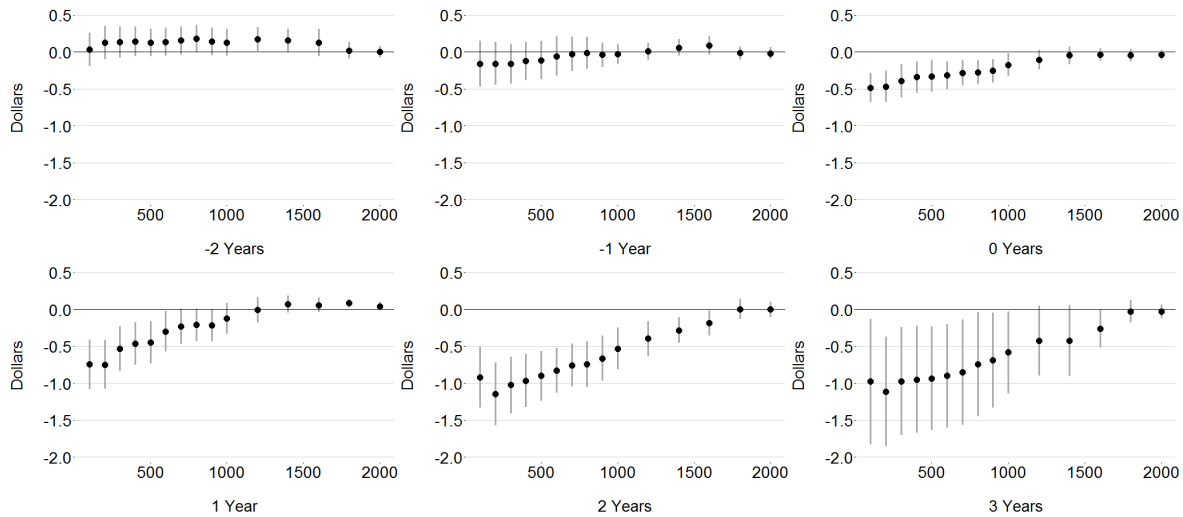


Figure 13: Event Study: Impact of new market rate units on rents

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.

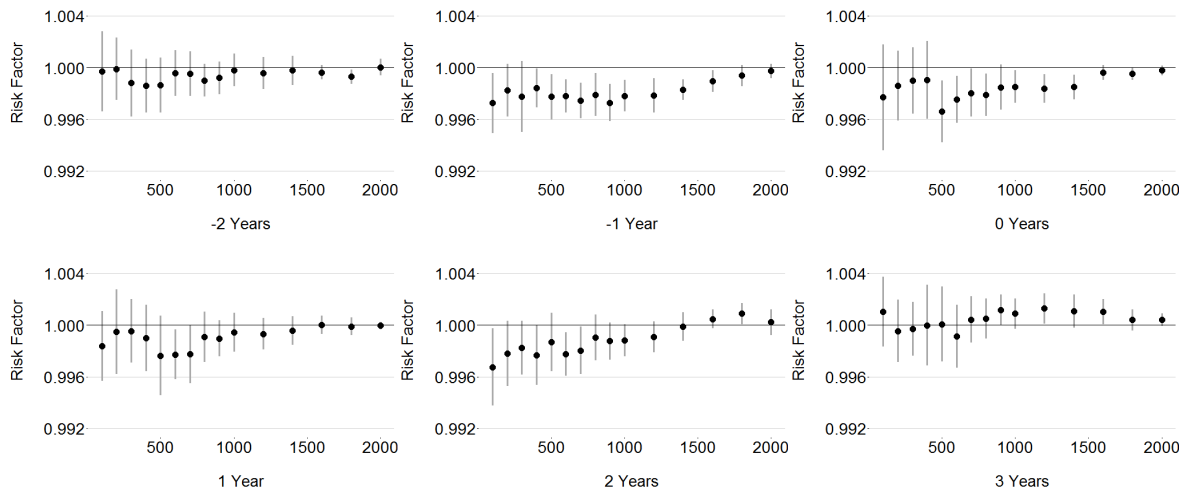


Figure 14: Event Study: Impact of new market rate units on moves to poorer zipcodes

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.

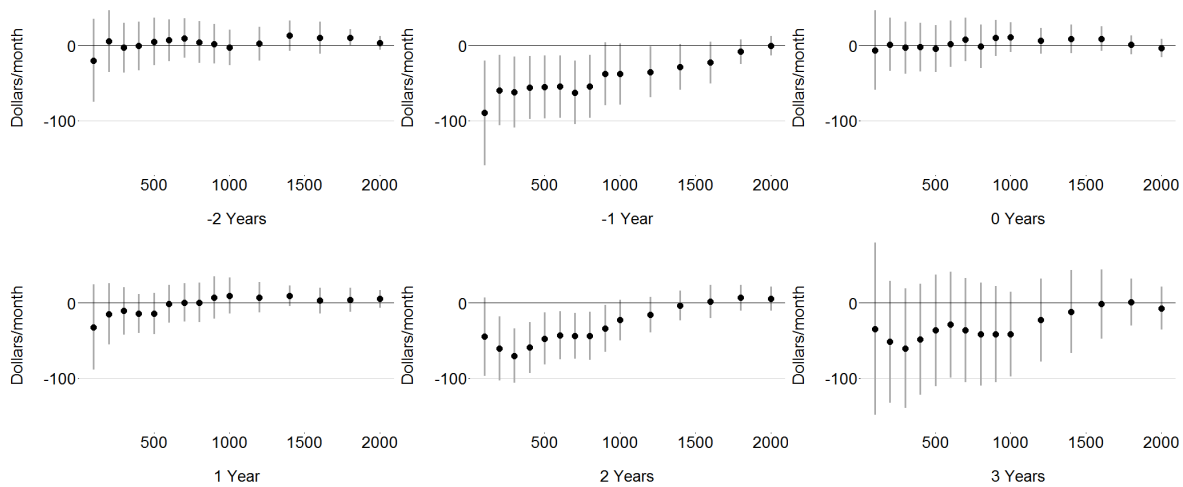


Figure 15: Event Study: Impact of new market rate projects on rents

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.

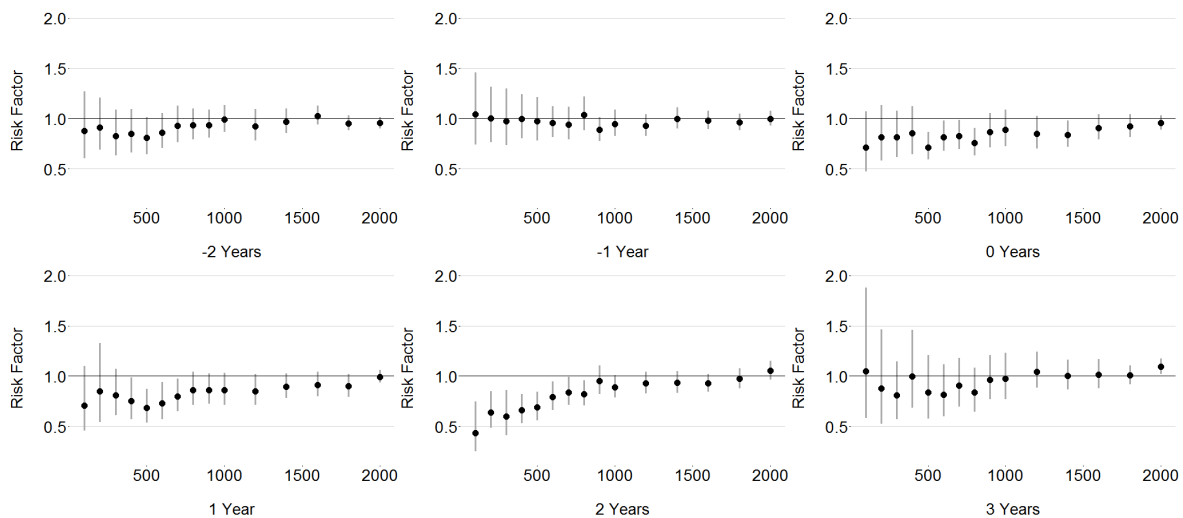


Figure 16: Event Study: Impact of new market rate projects on moves to poorer zipcodes

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.

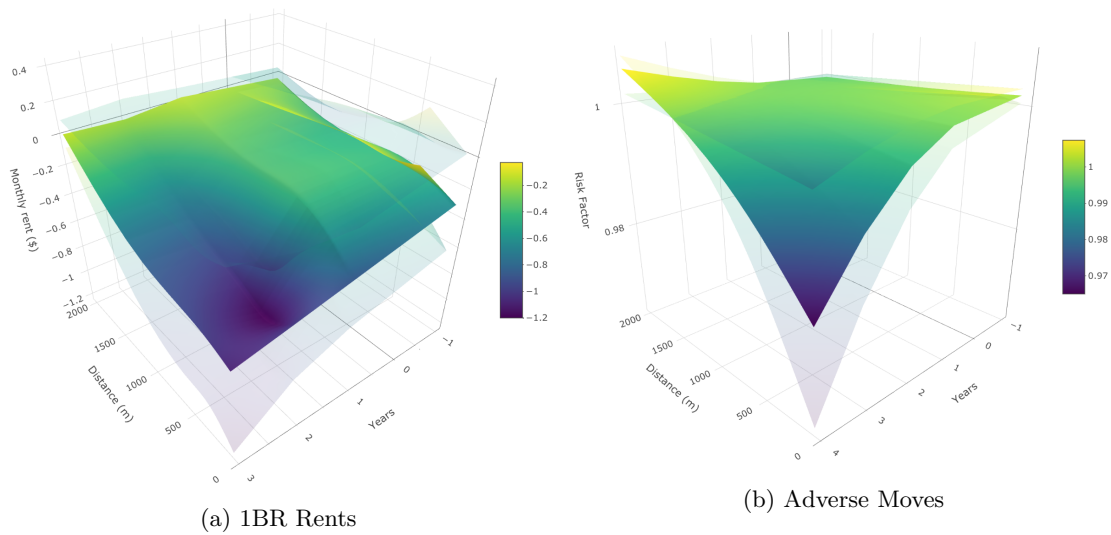


Figure 17: Impact of New Units over Time and Distance

Note: This figure plots smooths of the distance bin coefficients from Equation 4.2 and 4.1. Year 0 is the year of project completion. The rent specification includes micro-neighborhood by year fixed effects and parcel controls including rent control, distance to the financial district, Caltrain station, landuse zoning, and a quadratic in residential units. The displacement specification is a Cox proportional hazards model including micro-neighborhood, sex, and birth decade strata.

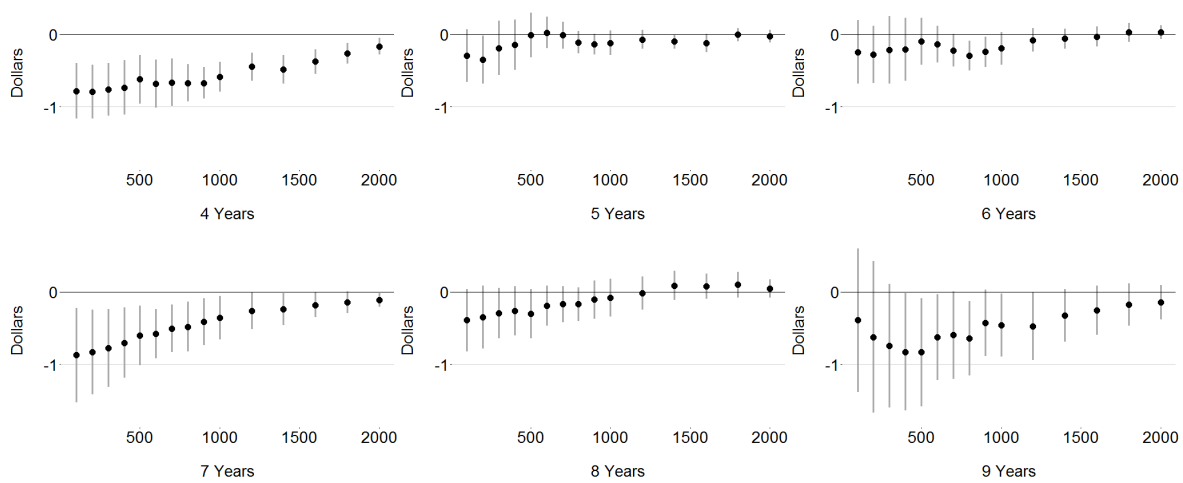


Figure 18: Event Study: Long Term Impact of New Units on Rents

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.

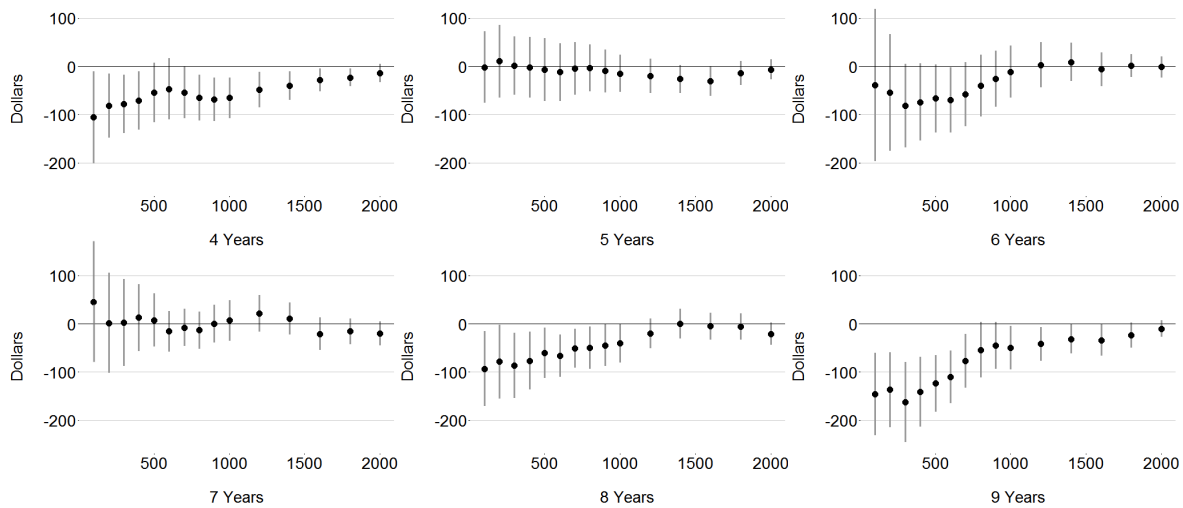


Figure 19: Event Study: Long Term Impact of New Market Rate Projects on Rents

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.

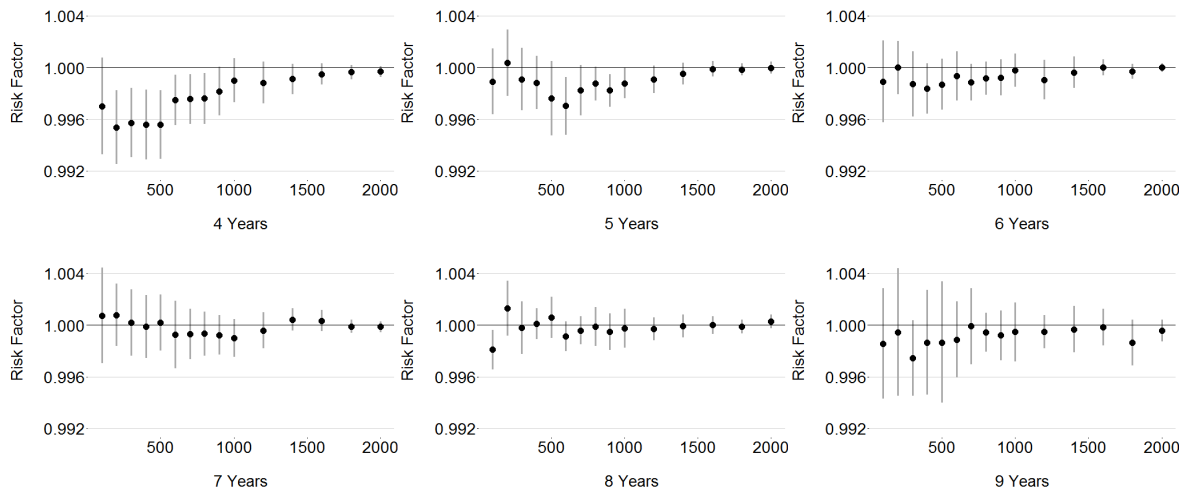


Figure 20: Event Study: Long term impact of new market rate units on moves to poorer zipcodes

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.

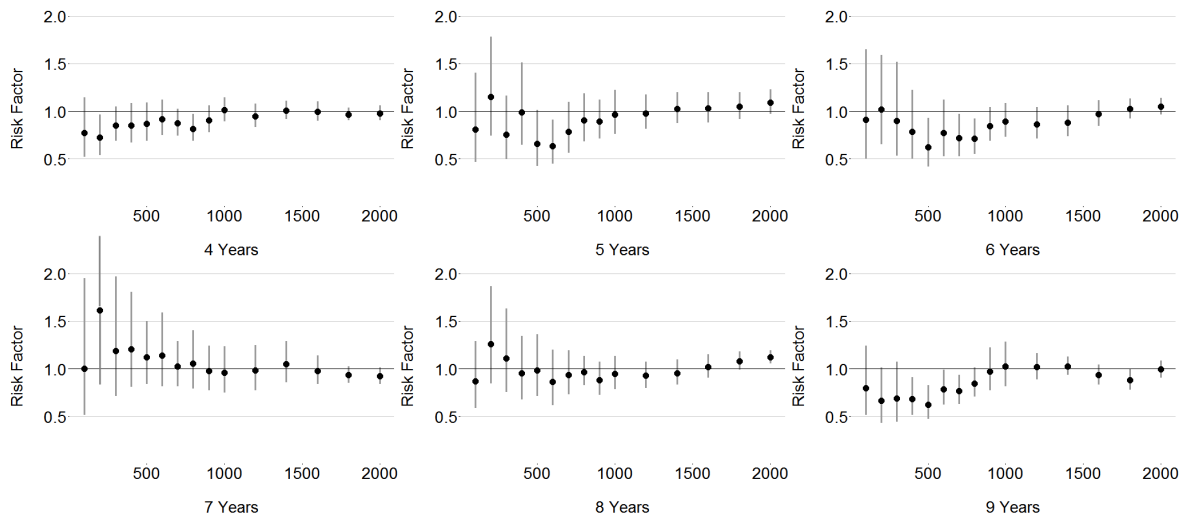


Figure 21: Event Study: Long term impact of new market rate projects on moves to poorer zipcodes

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.

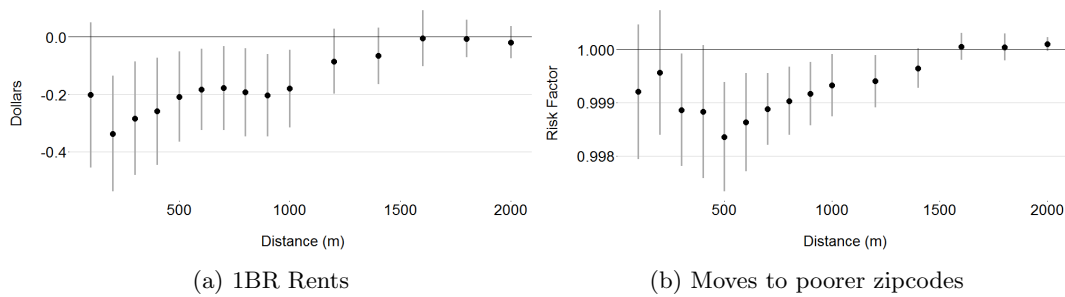


Figure 22: Impact of proximity to new units on 1BR rents and the probability of moving to a poorer zipcode

Note: Panel a shows the results from running specification 4.4 on one bedroom rents, using micro-neighborhood 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 micro-neighborhood 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.

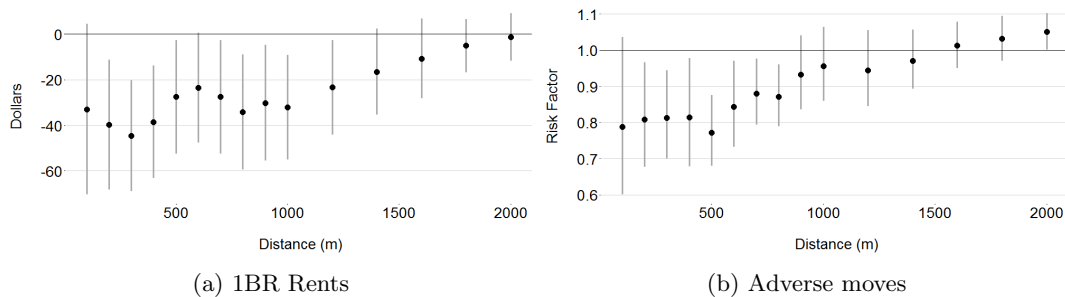


Figure 23: Impact of proximity to new projects on 1BR rents and the probability of an adverse move

Note: Panel a shows the results from running specification 4.4 on one bedroom rents, using micro-neighborhood 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 micro-neighborhood 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.

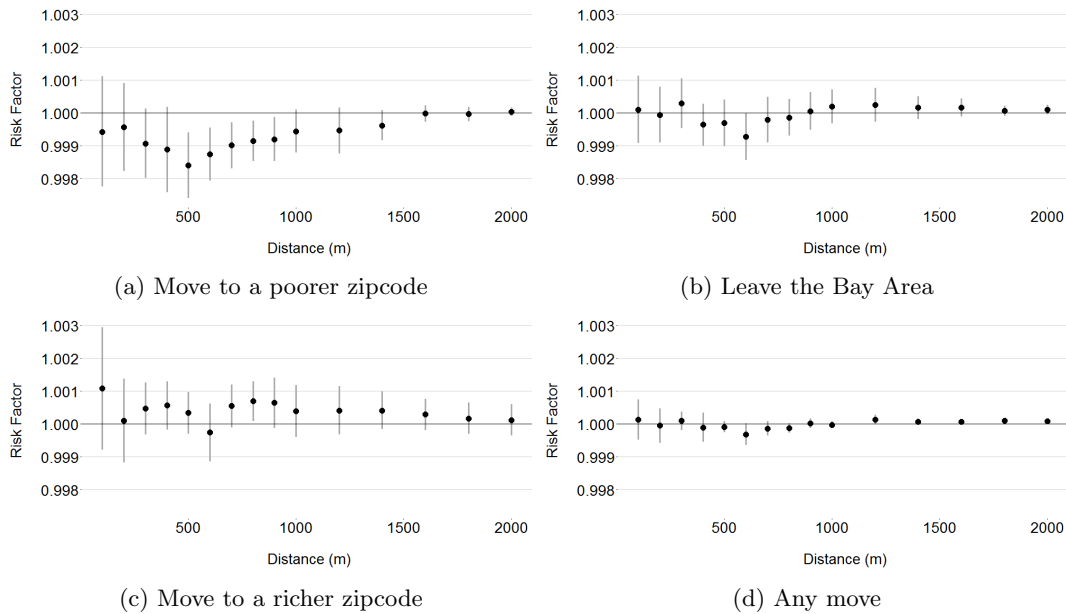


Figure 24: Results by Type of Destination

Note: These plots show the results from running specification 4.3 on the named outcome variables. All specifications include sex, birthyear, and micro-neighborhood 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.

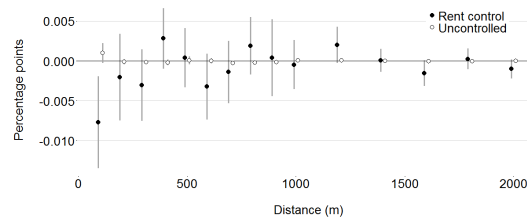


Figure 25: Impacts on Eviction Notices

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 micro-neighborhood 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.

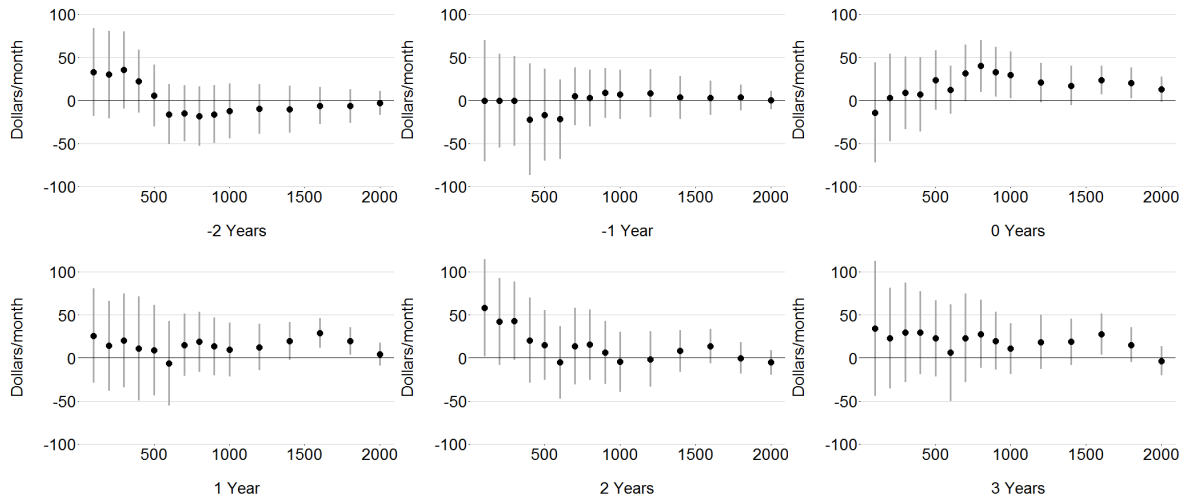


Figure 26: Event Study: Impact of new affordable projects on rents

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.

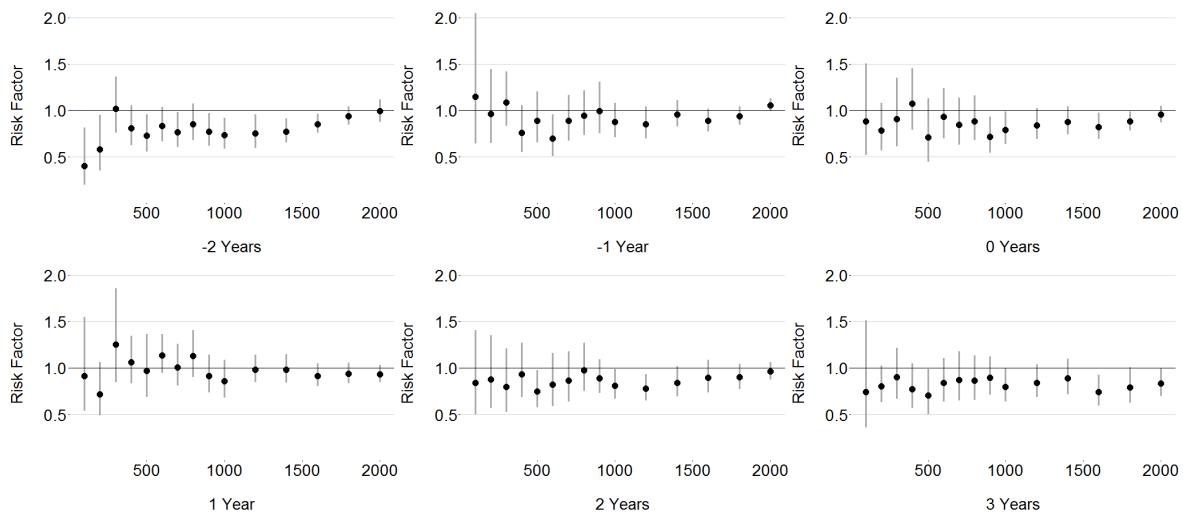


Figure 27: Event Study: Impact of new affordable projects on moves to poorer zipcodes

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.

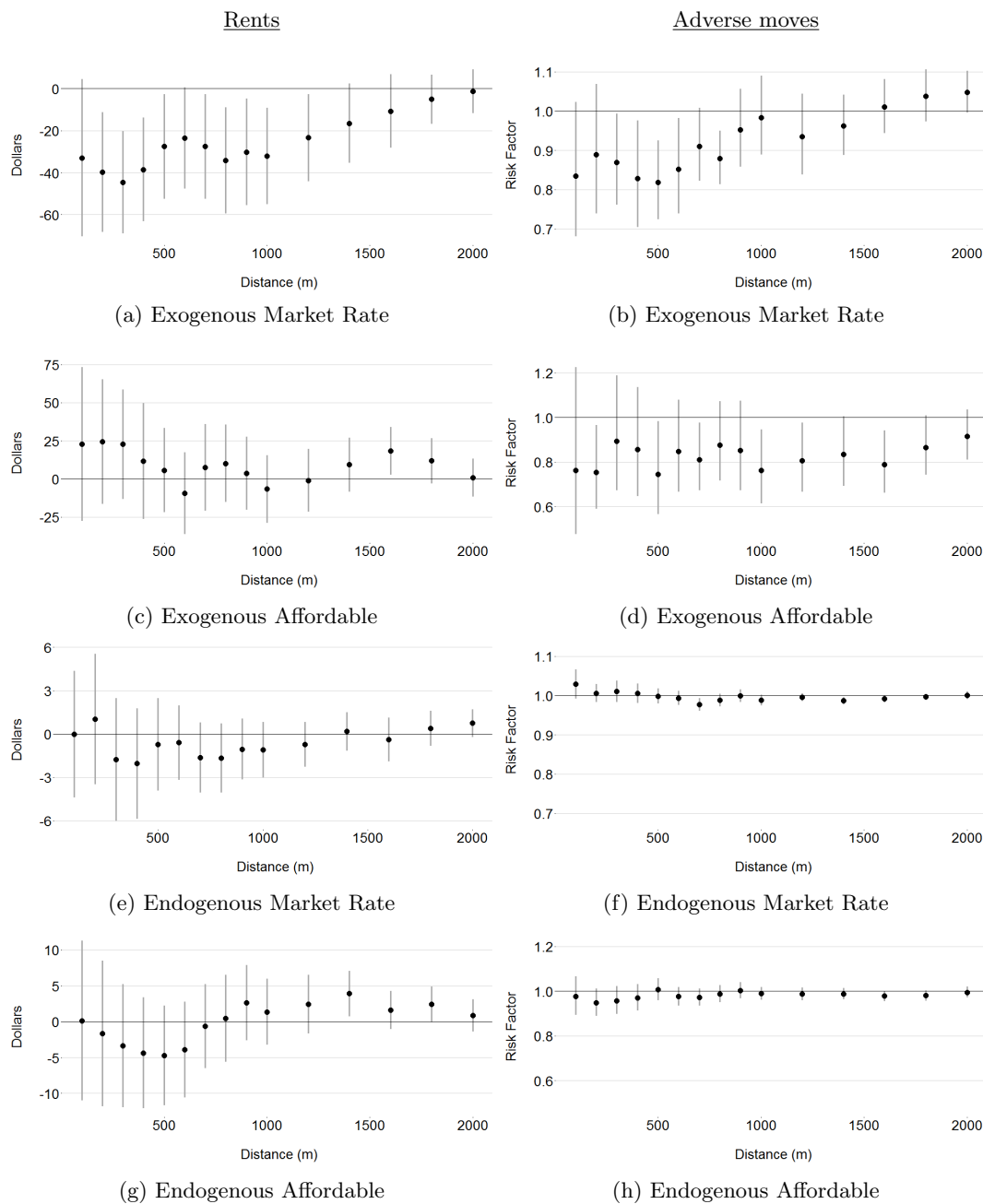


Figure 28: Impacts on Rents and Adverse Moves by Type of Construction

Note: These plots show the results from running specification 4.3 on 1 bedroom rents and moves to poorer zipcodes, using cumulative binned exposure to new units of exogenous market rate, exogenous affordable, endogenous market rate, and endogenous affordable projects, respectively. All rent specifications use parcel panel data and include micro-neighborhood 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 include including sex, birthyear, and micro-neighborhood 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.

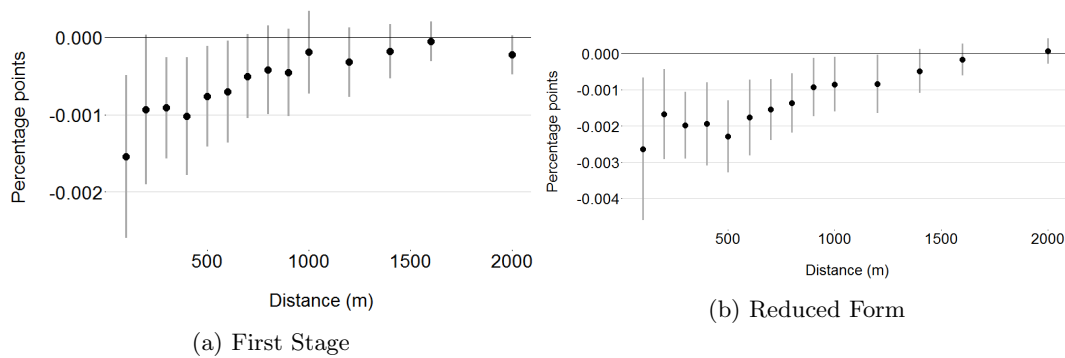


Figure 29: IV: First Stage and Reduced Form

Note: These plots show the results from running the [First stage](#) and [Reduced form](#) specifications, respectively. Both specifications use individual panel data and include micro-neighborhood 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.

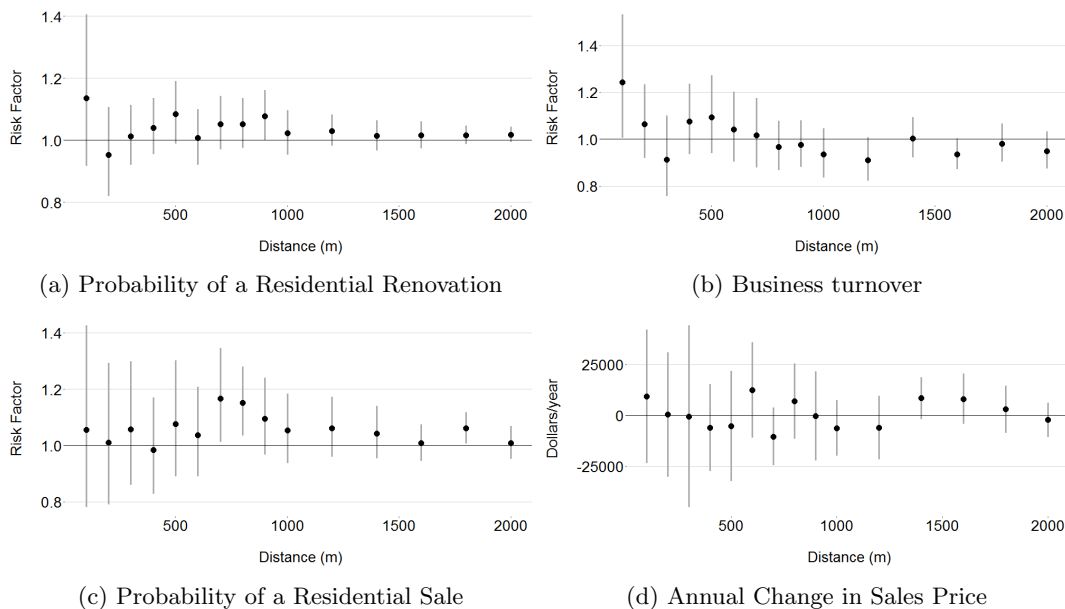


Figure 30: Demand Effects?: Impact on Residential Renovations, Sales, and Business Turnover

Note: These plots show the results from running specification [4.3](#) on the named dependent variables. All specifications use parcel panel data restricted to parcels zoned for residential or business use, and include micro-neighborhood strata 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. The specification for residential sales prices only uses properties that have been sold more than once. Samples are restricted by landuse type, so that only residential properties are included in the residential renovation and sale specifications and only commercial properties are included in the business turnover specification. Mean residential renovation = 0.0638; mean residential sale = 0.0235; mean business turnover = 0.1078.

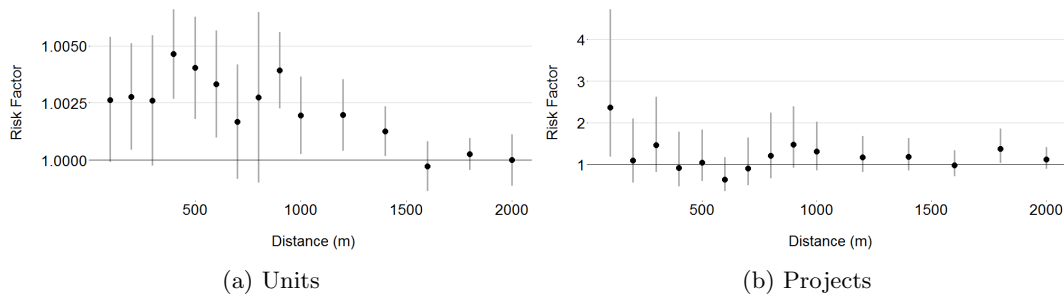


Figure 31: Impacts on New Construction

Note: These plots show the results from running specification 4.3 on an indicator variable for new construction getting permitted on parcel p . Both specifications use parcel panel data and include micro-neighborhood strata 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 new = 0.000682.

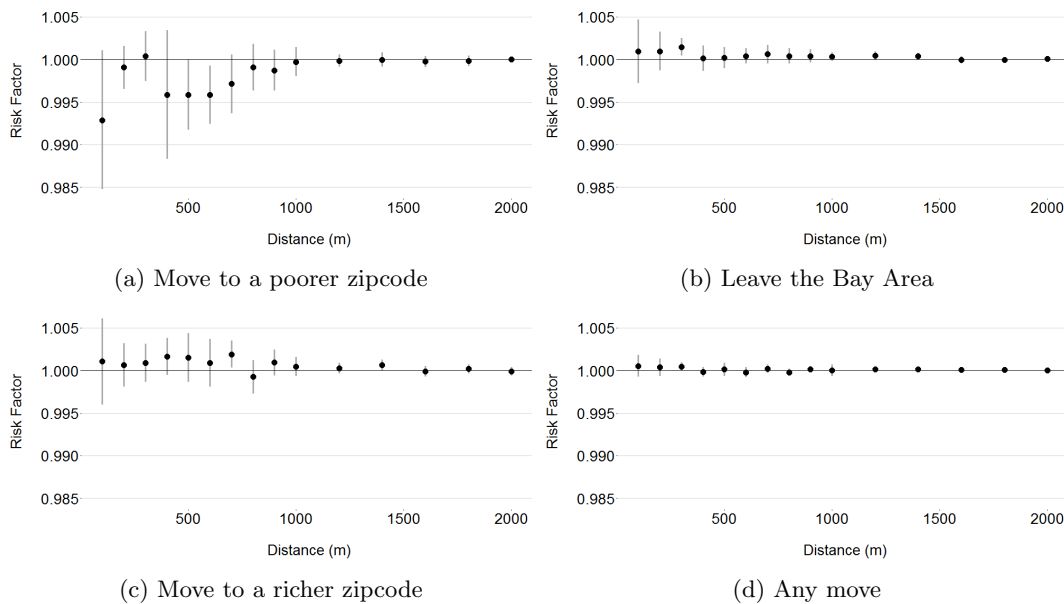
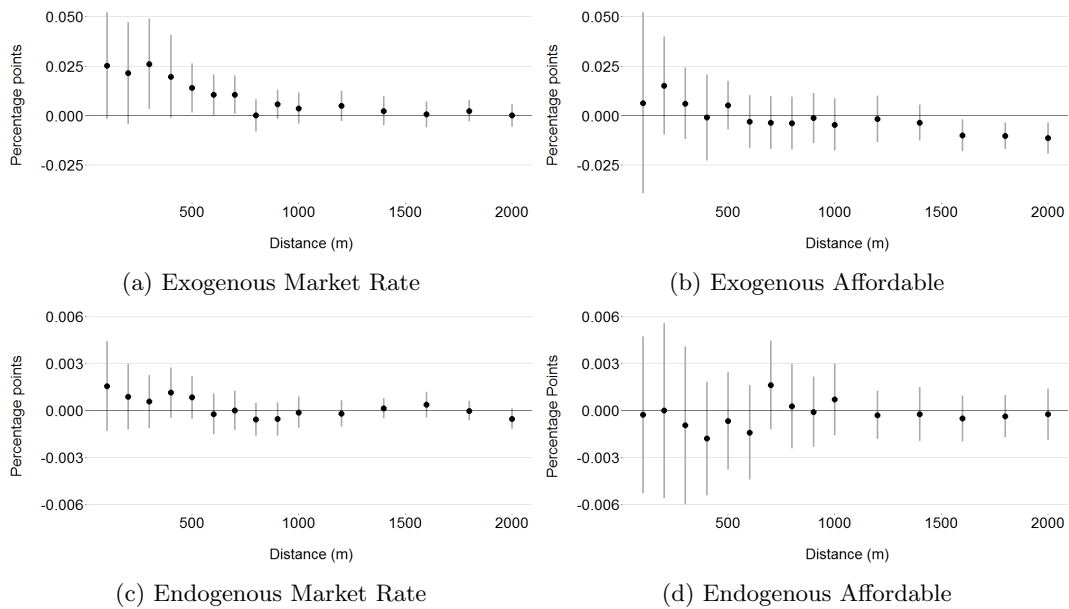


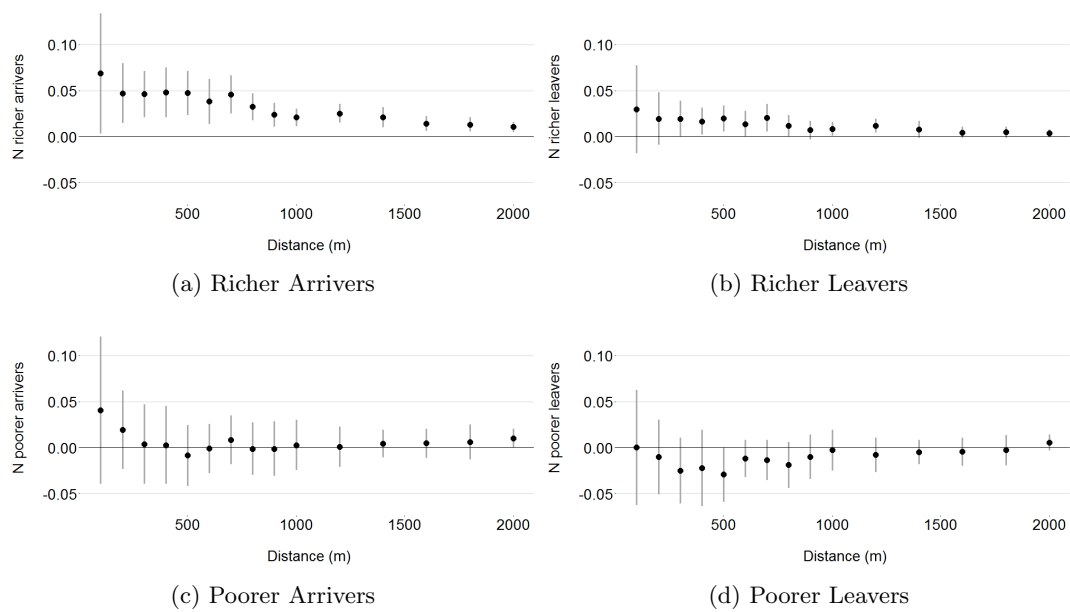
Figure 32: Demand effects?: Impact on owners' moves

Note: These plots show the results from running specification 4.3 on the named dependent variables. All specifications use individual panel data restricted to owners, and include sex, birth decade, and micro-neighborhood strata; years person i has lived at parcel p ; 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 = 0.0385; mean move poorer = 0.0103; mean exit Bay Area = 0.0128; and mean move richer = 0.00545.



Note: These plots show the results from running specification 4.3 on an indicator variable for gentrification, using cumulative binned exposure to exogenous market rate, exogenous affordable, endogenous market rate, and endogenous affordable construction, respectively. All specifications include parcel and year fixed effects and micro-neighborhood 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 33: Impact of new projects on gentrification by construction type



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

Figure 34: 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: Fires by Year

Year	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Fires	13	9	13	18	17	16	7	13	17	14	18	1	1	1	0
Cumulative	13	22	35	53	70	86	93	106	123	137	155	156	157	158	158

Note: This table reports the count of serious building fires by year and the cumulative number of serious fires. This cumulative number is the count of parcels that are eligible for exogenously located redevelopment. I identify serious building fires from the universe of calls to the fire department by crossreferencing 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 fire units to be dispatched and to appear in records of building complaints or in the description of new construction.

Table 3: 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 micro-neighborhood by year fixed effects and clustering standard errors at the micro-neighborhood level, as is done in every specification.

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

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

Note: Both specifications include micro-neighborhood 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 micro-neighborhood. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 5: Direct gentrification effects: Who moves into new housing?

	(Cross-Section)	(Panel)
$\mathbb{1}(\text{Exogenous})$	0.035* (0.017)	0.096** (0.032)
$\mathbb{1}(\text{Affordable})$	-0.118*** (0.009)	-0.037 (0.030)
$\mathbb{1}(\text{Exogenous}) \cdot \mathbb{1}(\text{Affordable})$	-0.156*** (0.034)	-0.181** (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 micro-neighborhood by year fixed effects and control for person 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 micro-neighborhood. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

10 Appendix

10.1 Additional event study plots

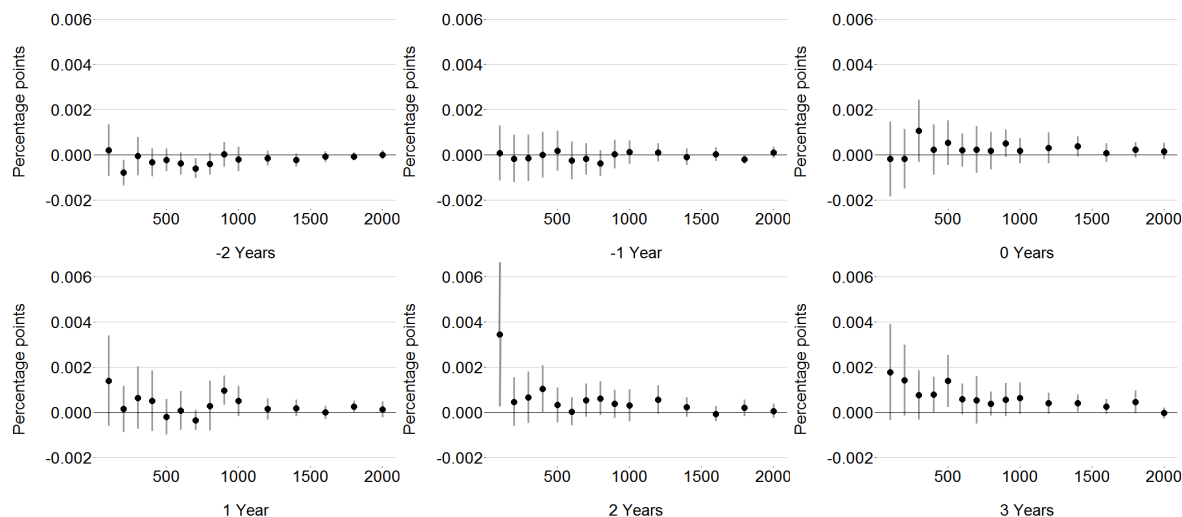


Figure 35: Impact of projects on probability of new building permit

Note: These plots show the 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 micro-neighborhood 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.

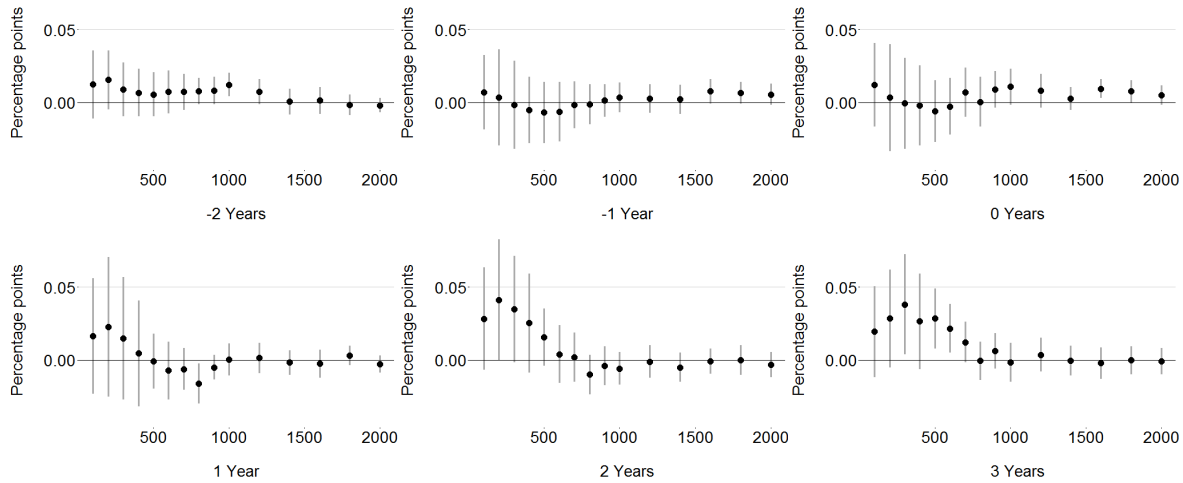


Figure 36: Impact of projects on gentrification

Note: These plots show the 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 micro-neighborhood 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.

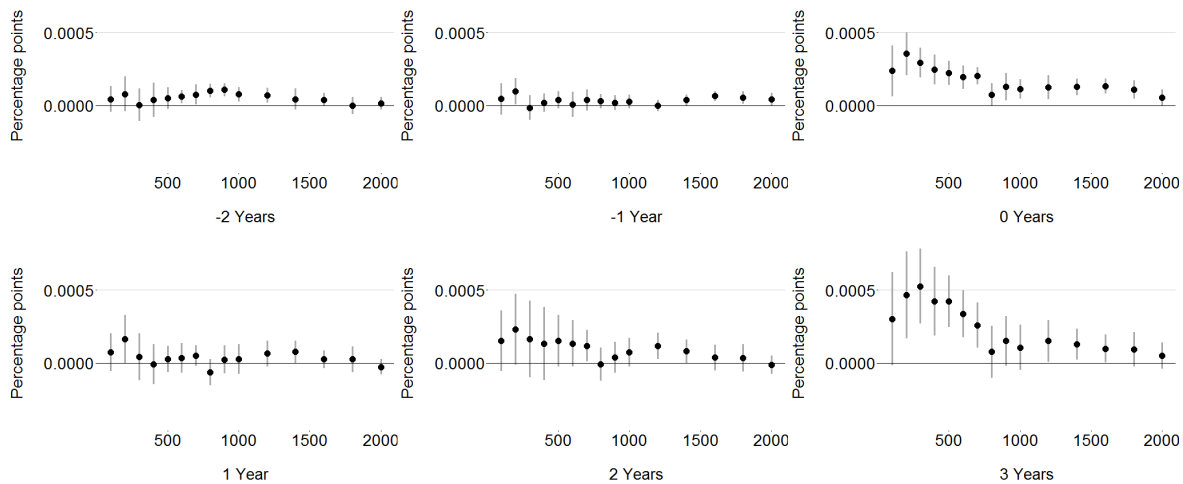


Figure 37: Impact of net units on gentrification

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.

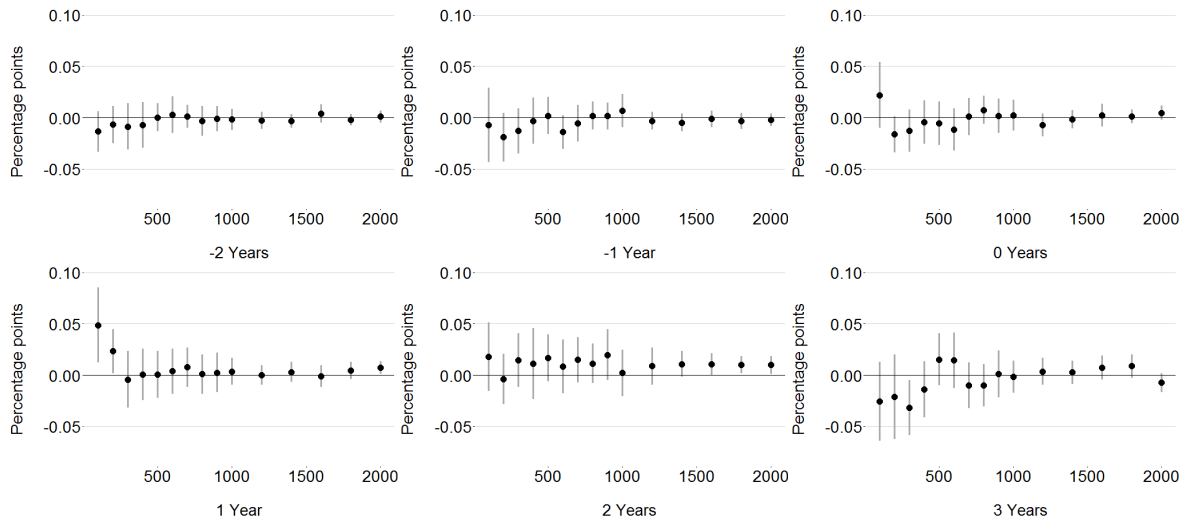


Figure 38: Impact of projects on residential renovations

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.

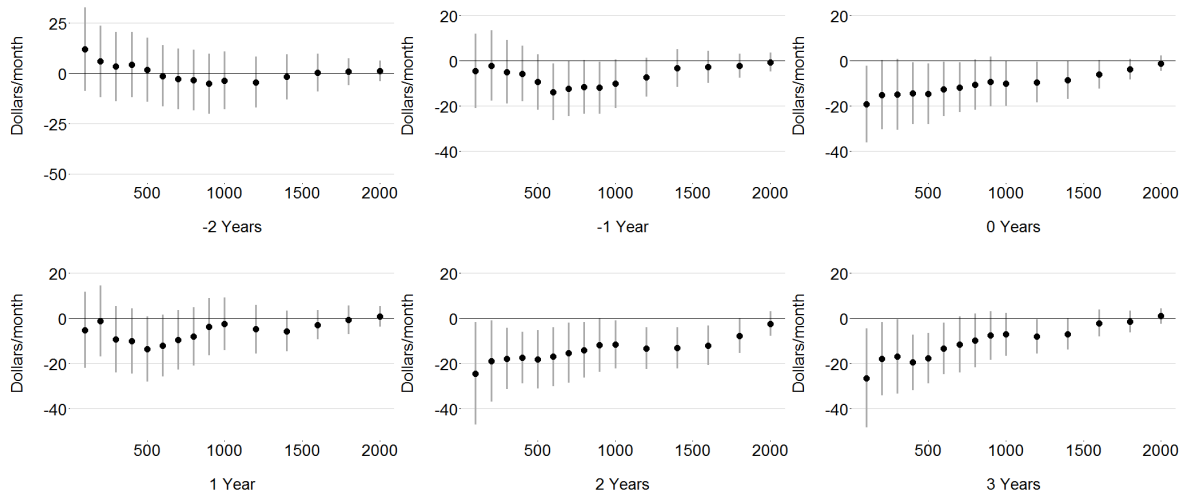


Figure 39: Impact of serious building fires on nearby rents

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

10.2 Rent results using log rent

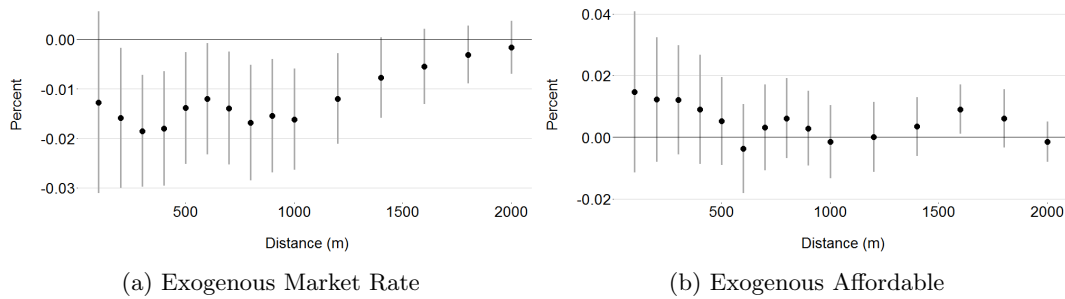


Figure 40: Impact of proximity to new construction on log rent

Note: These plots show the results from running specification 4.3 on log rent, instead of rent. Panel a plots coefficients on exogenous market rate projects and panel b plots coefficients on exogenous affordable projects. The specification includes individual, year, and micro-neighborhood 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.

10.3 Linear probability model results

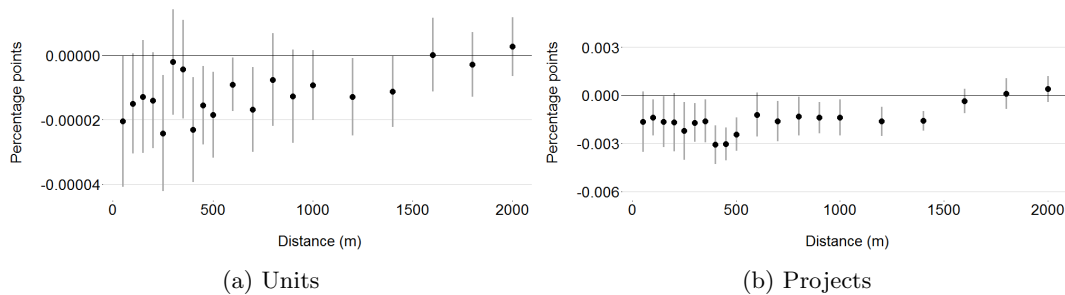


Figure 41: Linear Probability Model: Impact of proximity to new construction on the probability of moving to a poorer zipcode

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 micro-neighborhood 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 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 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 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.²⁴

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 estimate 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.

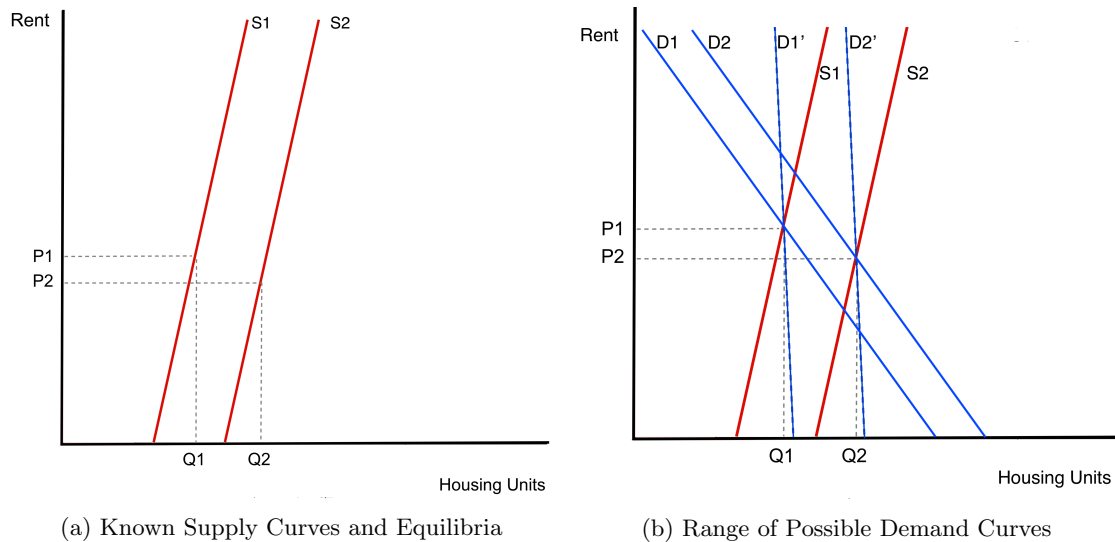
For the elasticity of housing demand, I take an upper bound from the literature²⁵ and calculate a lower bound based on my findings. Albouy et al. (2016) estimate an average demand elasticity of 0.66 across major US metro-areas. Housing demand in San Francisco is likely to be less elastic both because San Francisco's unusual job market means that few other cities are good substitutes, and

²⁴Asquith 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.

²⁵If 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.

because geographical constraints mean that there are 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).²⁶

Figure 42a 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 42b 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.



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

²⁶The City calculates the demand elasticity by regressing $\ln(pQ) = \ln() + (1 + 1)\ln(p) + 2\ln(y)$, 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.

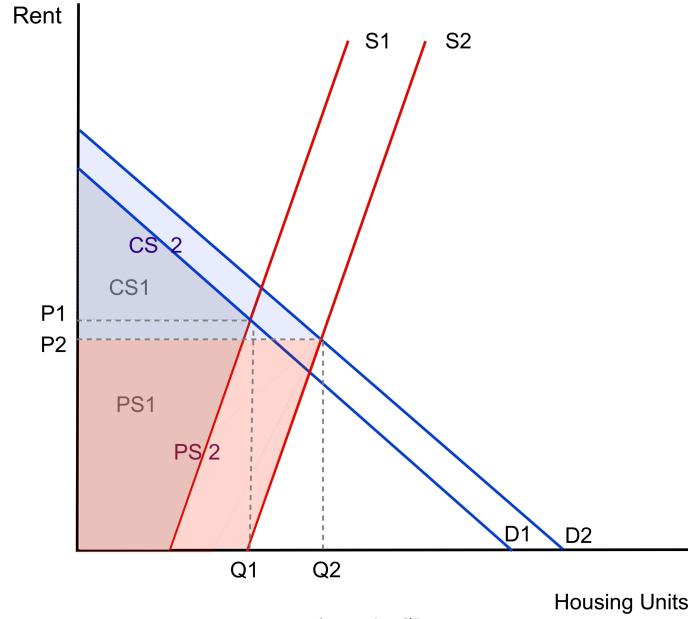


Figure 43: Calculating Consumer and Producer Surplus

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.

supply and demand curves and their intercepts:

$$m_{S,D} = \eta_{S,D}^{-1} \cdot \frac{P}{Q} \quad (11.1)$$

$$P = m_{S,D} \cdot Q + a_{S,D} \quad (11.2)$$

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 calculate the change in renter surplus and landlord surplus, as depicted in Figure 43:

$$\Delta CS = 0.5(Q_2 \cdot (a_{D2} - P_2) - Q_1 \cdot (a_{D1} - P_1)) \quad (11.3)$$

$$\Delta PS = (a_{S2}/m_S) \cdot P_2 + 0.5 \cdot (Q_2 - a_{S2}/m_S) \cdot P_2 - (a_{S1}/m_S) \cdot P_1 + 0.5 \cdot (Q_1 - a_{S1}/m_S) \cdot P_1 \quad (11.4)$$

Estimates for the change in renter and landlord surplus implied by each estimate of η_S and η_D are shown in Tables 6 and 7. 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 6: 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 7: 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.

11.1 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 list-

ings 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 number 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, it's not possible to tell which Bay Area town it's 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 .

11.2 Randomization Inference

Figure 11 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., 2020; Li,

2019; Diamond and McQuade, 2016), 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 my 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 micro-neighborhood-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 44 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 micro-neighborhood level. Figure ?? plots the distribution of coefficients from equation 4.3. In the main paper, I report the more conservative clustered standard errors.

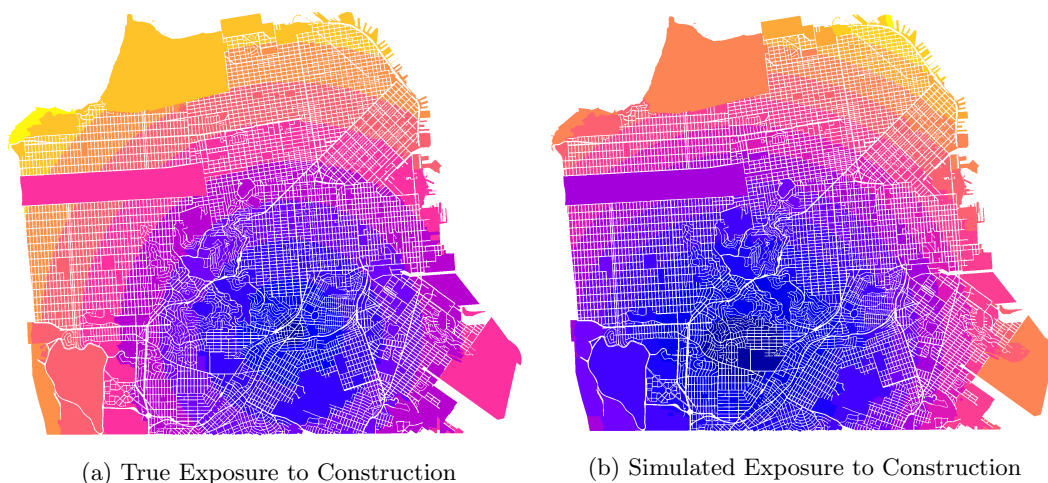


Figure 44: Randomly Translating the Treatment Surface

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.

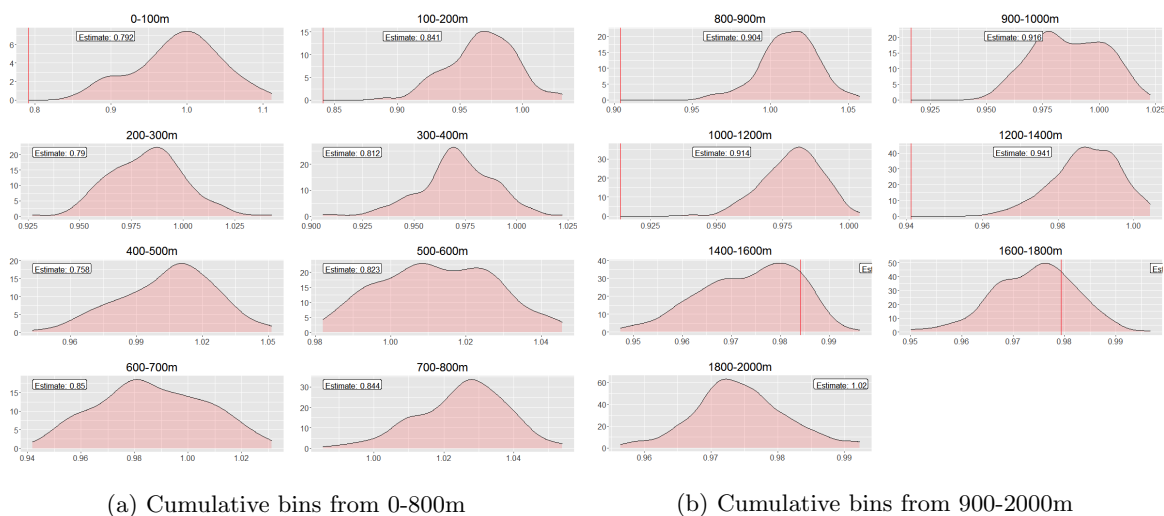


Figure 45: Distribution of Coefficients from Randomization Inference

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 11.2.

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.
- Albouy, D. and Zabek, M. (2016). Housing Inequality. Technical report.
- 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., Mast, E., and Reed, D. (2020). Does luxury housing construction increase rents in gentrifying areas? *Working paper*.
- 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.
- Bandiera, O., Burgess, R., Ghatak, M., Balboni, C., and Heil, A. (2020). Why do people stay poor?
- Banzhaf, H. S. and Walsh, R. P. (2008a). Do people vote with their feet? An empirical test of Tiebout. *American Economic Review*, 98(3):843–63.
- Banzhaf, S. and Walsh, R. (2008b). Do People Vote with Their Feet? An Empirical Test of Tiebout’s Mechanism. *American Economic Review*.
- Bartik, A. W. (2018). Moving costs and worker adjustment to changes in labor demand: Evidence from longitudinal census data. *Manuscript, University of Illinois at Urbana-Champaign*.
- Bayer, P. and Timmins, C. (2007). Estimating Equilibrium Models of Sorting Across Locations. *The Economic Journal*, 117(518):353–374.
- Berry, S., Levinsohn, J., and Pakes, A. (1995). Automobile Prices in Market Equilibrium. *Econometrica: Journal of the Econometric Society*, pages 841–890.
- Bilal, A. and Rossi-Hansberg, E. (2018). Location as an asset. *Working paper*.
- Blattman, C., Green, D., Ortega, D., and Tobón, S. (2017). Place-based interventions at scale: The direct and spillover effects of policing and city services on crime. Technical report, National Bureau of Economic Research.
- Boustan, L. P., Margo, R. A., Miller, M. M., Reeves, J. M., and Steil, J. P. (2019). Does condominium development lead to gentrification? Technical report, National Bureau of Economic Research.
- 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 mo-

bility. *City & Community*.

- Chan, S. and Jush, G. K. (2017). 2017 National Rental Housing Landscape: Renting in the Nation's Largest Metros. *Furman Center*.
- 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. (2016). Who Wants Affordable Housing in Their Backyard? An Equilibrium Analysis of Low Income Property Development. Technical report.
- Diamond, R., McQuade, T., and Qian, F. (2018). The Effects of Rent Control Expansion on Tenants, Landlords, and Inequality: Evidence from San Francisco. Technical report, National Bureau of Economic Research.
- 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. Technical report, National Bureau of Economic Research.
- 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. (2017). Has Falling Crime Invited Gentrification? Technical report, US Census Bureau CES Working Paper.
- 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.
- Fry, R. and Taylor, P. (2012). The Rise of Residential Segregation by Income. *Pew Research Center*.
- Glaeser, E. and Gyourko, J. (2003). The impact of building restrictions on housing affordability.
- Glaeser, E., Gyourko, J., and Saks, R. (2005a). Why Is Manhattan So Expensive? Regulation and the Rise in Housing Prices. *The Journal of Law & Economics*.
- Glaeser, E. L. and Gottlieb, J. D. (2008). The economics of place-making policies. Technical report, National Bureau of Economic Research.
- Glaeser, E. L., Gyourko, J., and Saks, R. E. (2005b). Why have housing prices gone up? *NYU Furman Center*.
- 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).
- Glaeser, E. L. and Luttmer, E. F. (2003). The Misallocation of Housing Under Rent Control. *The American Economic Review*, 93(4):1027–1046.
- 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.
- Greenstone, M. and Hanna, R. (2014). Environmental Regulations, Air and Water Pollution, and Infant Mortality in India. *American Economic Review*, 104(10):3038–72.
- 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. and Linneman, P. (1989). Equity and Efficiency Aspects of Rent Control: An Empirical Study of New York City. *Journal of Urban Economics*.
- Gyourko, J., Mayer, C., and Sinai, T. (2013). Superstar cities. *American Economic Journal: Economic Policy*, 5(4):167–99.
- Gyourko, J. and Molloy, R. (2014). Regulation and Housing Supply. *NBER Working Paper 20536*.
- Gyourko, J. and Saiz, A. (2006). Constuction Costs and the Supply of Housing Structure. *Journal of Regional Science*.
- Gyourko, J., Saiz, A., and Summers, A. (2008a). A new measure of the local regulatory environment for housing markets: Wharton residential land use regulatory index. *Urban Studies*.
- Gyourko, J. and Sinai, T. (2003). The spatial distribution of housing-related ordinary income tax benefits. *Real Estate Economics*.
- 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, Se-

quencing, and Institutional Frictions.

- Ho, D., Imai, K., King, G., and Stuart, E. (2007). Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference. *Political Analysis*.
- 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.
- Ihlanfeldt, K. and Mayock, T. (2014). Housing Bubbles and Busts: The Role of Supply Elasticity. *Land Economics*.
- 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*.
- 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*.
- Redding, S. J. and Rossi-Hansberg, E. (2017). Quantitative Spatial Economics. *Annual Review of Economics*, 9:21–58.
- 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.
- Sander, R. and Kucheva, Y. (2017). Structural versus Ethnic Dimensions of Housing Segregation. *Journal of Urban Affairs*.
- Schmidheiny, K. and Siegloch, S. (2020). On event studies and distributed-lags in two-way fixed effects models: Identification, equivalence, and generalization. *ZEW-Centre for European Economic Research Discussion Paper*, (20-017).
- Siodla, J. (2015). Razing San Francisco: The 1906 disaster as a natural experiment in urban redevelopment. *Journal of Urban Economics*, 89:48–61.

- Tiebout, C. M. (1956). A Pure Theory of Local Expenditures. *The Journal of Political Economy*.
- Voorheis, J. (2017). Longitudinal Environmental Inequality and Environmental Gentrification: Who Gains From Cleaner Air? Technical report, US Census Bureau CES Working Paper.
- Walker, W. R. (2013). The transitional costs of sectoral reallocation: Evidence from the clean air act and the workforce. *The Quarterly journal of economics*, 128(4):1787–1835.
- Zuk, M. and Chapple, K. (2016). Housing Production, Filtering and Displacement: Untangling the Relationships. *Research Brief*.