

The Effects of Rent Control Expansion on Tenants, Landlords, and Inequality: Evidence from San Francisco[†]

By REBECCA DIAMOND, TIM MCQUADE, AND FRANKLIN QIAN*

Using a 1994 law change, we exploit quasi-experimental variation in the assignment of rent control in San Francisco to study its impacts on tenants and landlords. Leveraging new data tracking individuals' migration, we find rent control limits renters' mobility by 20 percent and lowers displacement from San Francisco. Landlords treated by rent control reduce rental housing supplies by 15 percent by selling to owner-occupants and redeveloping buildings. Thus, while rent control prevents displacement of incumbent renters in the short run, the lost rental housing supply likely drove up market rents in the long run, ultimately undermining the goals of the law. (JEL R23, R31, R38)

Steadily rising housing rents in many of the United State's large, productive cities has brought the issue of affordable housing to the forefront of the policy debate and reignited the discussion over expanding or enacting rent control provisions. While the details of rent control regulations vary some across places, they generally regulate rent increases and place restrictions on evictions. State lawmakers in California, Colorado, Illinois, and Oregon have considered repealing laws that limit cities' abilities to pass or expand rent control. Rent control is already extremely popular around the San Francisco Bay Area. Nine Bay Area cities already impose rent control regulations, two of which recently passed rent control laws through majority votes on the November 2016 ballot.

A substantial body of economic research has warned about potential negative efficiency consequences of limiting rent increases below market rates, including overconsumption of housing by tenants of rent-controlled apartments (Olsen 1972, Gyourko and Linneman 1989), misallocation of heterogeneous housing to heterogeneous tenants (Suen 1989, Glaeser and Luttmer 2003, Sims 2011, Bulow and Klemperer 2012), negative spillovers onto neighboring housing (Sims 2007; Autor, Palmer, and Pathak 2014) and neglect of required maintenance (Downs 1988). Yet, due to incomplete markets, in the absence of rent control, many tenants are unable to insure themselves against rent increases. Of course, individuals who have little connection to any specific area may be able to easily insure themselves against

*Diamond: Stanford Graduate School of Business, 655 Knight Way, Stanford, CA 94305, and NBER (email: diamondr@stanford.edu); McQuade: Stanford Graduate School of Business, 655 Knight Way, Stanford, CA 94305 (email: tmcquade@stanford.edu); Qian: Department of Economics, Stanford University, 579 Serra Mall, Stanford, CA 94305 (email: zqian1@stanford.edu). Thomas Lemieux was the coeditor for this article. We are grateful for comments from Ed Glaeser, Christopher Palmer, Paul Scott, and seminar and conference participants. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

[†]Go to <https://doi.org/10.1257/aer.20181289> to visit the article page for additional materials and author disclosure statements.

local rental price appreciation by simply moving to a cheaper location. However, if long-term tenants have developed neighborhood-specific capital, such as a network of friends and family, proximity to one's job, or proximity to the schools of one's children, then these tenants face large risks from rent appreciation. A variety of affordable housing advocates have argued that many tenants greatly value such insurance and that rent control can effectively provide it.

Despite the policy interest, due to a lack of detailed data and natural experiments, we have little well-identified empirical evidence evaluating how introducing local rent controls affects tenants, landlords, and the broader housing market.¹ In this paper, we bring to bear new microdata and exploit quasi-experimental variation in the assignment of rent control to fill this gap. We exploit an unexpected 1994 law change that suddenly rent-controlled a subset of San Francisco buildings and their tenants, based on the year each building was built. However, the law left very similar buildings and tenants without rent control. We find tenants covered by rent control do place a substantial value on the benefit, as revealed by their choice to remain in their apartments longer than those without rent control. Indeed, we find the vast majority of those incentivized to remain in their rent-controlled apartment would have been displaced from San Francisco had they not been covered.

However, landlords of properties affected by the law change respond over the long term by substituting to other types of real estate, in particular by converting to condos and redeveloping buildings so as to exempt them from rent control. In the long run, landlords' substitution toward owner-occupied and newly constructed rental housing not only lowered the supply of rental housing in the city, but also shifted the city's housing supply toward less affordable types of housing that likely cater to the tastes of higher income individuals. Ultimately, these endogenous shifts in the housing supply likely drove up citywide rents, damaging housing affordability for future renters, and counteracting the stated claims of the law.

In 1979, San Francisco imposed rent control on all standing buildings with five or more apartments. While all large buildings built as of 1979 would now be rent-controlled, new construction was exempt from the law, since legislators did not want to discourage new development. In addition, smaller multi-family buildings were exempt from rent control since they were viewed as more "mom and pop" ventures, and did not have market power over rents. However, this small multi-family exemption was lifted through a 1994 San Francisco ballot initiative. Proponents of this law change argued small multi-family housing was now primarily owned by large businesses and should face the same rent control restrictions of large multi-family housing. Since the initial 1979 rent control law only impacted properties built from 1979 and earlier, the removal of the small multi-family exemption also only affected properties built 1979 and earlier. This led to quasi-experimental rent control expansion in 1994 based on whether the small multi-family housing was built prior to or post 1980.

To examine rent control's effects on tenant migration and neighborhood choices, we make use of new panel data which provide address-level migration decisions

¹Notable exceptions to this are Sims (2007) and Autor, Palmer, and Pathak (2014) which use the repeal of rent control in Cambridge, Massachusetts to study its spillover effects onto nearby property values and building maintenance. Neither one of these papers, however, directly studies how rent control impacts tenants.

and housing characteristics for the majority of adults living in San Francisco in the early 1990s. This allows us to define our treatment group as renters who lived in small multi-family apartment buildings built prior to 1980 and our control group as renters living in small multi-family housing built between 1980 and 1990. Using our data, we can follow each of these groups over time up until the present, regardless of where they migrate.

We find that between five and ten years after the law change, the beneficiaries of rent control are, on average, 3.5 percentage points more likely to still remain at their 1994 address relative to the control group. Since only 18 percent of the control group still remained at their 1994 address for this long, this estimate represents a 19.4 percent increase in not moving ($3.5/18$) relative to the control group. We further find that the beneficiaries are 4.5 percentage points more likely to remain in San Francisco relative to the control group, indicating that a large share of the renters who remained at their 1994 address due to rent control would have left San Francisco had they not been covered by rent control. This would likely be viewed as a desirable outcome by rent control advocates.

We next analyze treatment effect heterogeneity along a number of dimensions. We first find that our estimated effects are significantly stronger among older households and among households that have already spent a number of years at their address prior to treatment. This is consistent with the idea that both of these populations are less likely to experience personal shocks requiring them to change residence and thus, are better able to take advantage of the potential savings offered by rent control.

We then examine whether the effects we estimate vary across racial groups. We do not directly observe race in our data, so we use an imputation procedure based on renters' names and addresses.² We find rent control has an especially large impact on preventing the displacement of racial minorities from San Francisco, suggesting that rent control helps to foster the racial diversity of San Francisco, at least among the initial cohort of renters covered by the law.

Finally, we analyze whether rent control enables tenants to live in neighborhoods with better amenities. One might expect neighborhoods with the largest increases in market prices and amenities would be ones where tenants would remain in their rent-controlled apartments the longest, since their outside options in the neighborhood would be especially expensive. However, for these same reasons, landlords in these high-rent, high-amenity neighborhoods would have large incentives to remove tenants.³ They then could either reset rents to market rates with a new tenant or redevelop the building as condos or new construction, both of which are exempt from rent control. These landlord incentives would push rent control tenants out of the nicest neighborhoods. In fact, we find the landlords' incentives appear to dominate. The average tenant treated by rent control lives in a census tract with worse observable amenities, as measured by the census tract's median household income, share of the population with a college degree, median house value, and share unemployed.

²We impute race by combining imputed race based on first and last name (Ye et al. 2017) and the racial mix of one's census block of residence in 1990. See Section II for more details.

³In practice, landlords use a number of legal means to remove their tenants, including owner move-in eviction, Ellis Act eviction, or monetary compensation. Landlords may also engage in various pressure tactics, such as tardy maintenance, to pressure tenants to leave.

Thus, while rent control does prevent displacement from San Francisco, it does not provide access to the best neighborhoods in the city.

The evidence above suggests that landlords do not passively accept the burdens of the law. To further study the landlord response to the rent control expansion and to understand the impact of rent control on rental supply, we merge in historical parcel history data from the San Francisco Assessor's Office, which allows us to observe parcel splits and condo conversions. We find that rent-controlled buildings were 8 percentage points more likely to convert to a condo or a Tenancy in Common (TIC) than buildings in the control group. Consistent with these findings, we find that rent control led to a 15 percentage point decline in the number of renters living in treated buildings and a 25 percentage point reduction in the number of renters living in rent-controlled units, relative to 1994 levels. This large reduction in rental housing supply was driven by both converting existing structures to owner-occupied condominium housing and by replacing existing structures with new construction.

This 15 percentage point reduction in the rental supply of small multi-family housing likely led to rent increases in the long run, consistent with standard economic theory. In this sense, rent control operated as a transfer between the future renters of San Francisco (who would pay these higher rents due to lower supply) to the renters living in San Francisco in 1994 (who benefited directly from lower rents). Furthermore, since many of the existing rental properties were converted to higher-end, owner-occupied condominium housing and new construction rentals, the passage of rent control ultimately led to a housing stock which caters to higher income individuals. We directly test whether rent control led to in-migration of higher income residents by imputing household income as the per capita income of the census block groups in which the building occupants resided in five year prior. We find that this high-end housing, developed in response to rent control, attracted residents with at least 18 percent higher income, relative to control group buildings in the same zip code.

Taking all of these points together, it appears rent control has actually contributed to the gentrification of San Francisco, the exact opposite of the policy's intended goal. Indeed, by simultaneously bringing in higher income residents and preventing displacement of minorities, rent control has contributed to widening income inequality of the city. For a full quantitative analysis of the welfare gains and losses due to rent control, see our companion paper (Diamond, McQuade, and Qian 2018), which estimates a dynamic discrete choice model of tenant migration and performs general equilibrium counterfactual analysis of the impacts of rent control.

Our paper is part of the literature on rent control. The two papers most closely related to ours are Sims (2007) and Autor, Palmer, and Pathak (2014), both of which study the effects of *ending* rent control in the Boston metropolitan area. Sims (2007) uses American Housing Survey (AHS) data to show that towns in the Boston metropolitan area in which rent control was abolished saw increases in rental supply and increased housing maintenance. Sims (2007) also shows some evidence of spillover effects on non-controlled properties. Autor, Palmer, and Pathak (2014) use property-level data on assessed values and transaction prices in Cambridge, Massachusetts to investigate these spillover effects more directly. They show that decontrol led to price appreciation at decontrolled and never-controlled units.

Our paper is different on a number of important dimensions. First, our paper uses a different natural experiment which has the nice feature of generating quasi-random assignment of rent control within narrowly defined neighborhoods. More substantively, by bringing to bear a unique, rich, and previously unused dataset, our paper is the first in this literature to be able to study how rent control impacts the behavior of the actual tenant beneficiaries. These estimates reveal a number of important insights regarding the value tenants place on rent control protections and rent control's ability to limit displacement, but also potential limitations in the ability of tenants to realize rent savings due to landlord responses.

Finally, since our unique data provide property-level information on renovations, condo conversions, and redevelopment, our paper shows that rent control can lead to an upgraded housing stock catering to higher income individuals. Indeed, the previous literature has shown that ending rent control leads to higher maintenance and higher nearby property values. To reconcile these seemingly conflicting points, it is crucial to understand that decontrol studies the effects of removing rent control on buildings which *still remain* covered. In fact, one of our key points is to show that a large share of landlords substitute away from supply of rent-controlled housing, making those properties which remain subject to rent control a selected set. In this way, studying the introduction of rent control, which our paper does, is not the same as studying the abolishment of rent control.

There also exists an older literature on rent control combining applied theory with cross-sectional empirical methods. These papers test whether the data are consistent with the theory being studied, but usually cannot quantify causal effects of rent control (Early 2000, Glaeser and Luttmer 2003, Gyourko and Linneman 1989, Gyourko and Linneman 1990, Moon and Stotsky 1993, Olsen 1972).

The remainder of the paper proceeds as follows. Section I discusses the history of rent control in San Francisco. Section II discusses the data used for the analysis. Section III presents our empirical results. Section IV concludes.

I. A History of Rent Control in San Francisco

Regulations are widespread in housing markets, and rent controls are arguably among the most important historically (Stigler and Friedman 1946, Gyourko and Glaeser 2008). The modern era of US rent controls began as a part of World War II era price controls and as a reaction to housing shortages following demographic changes immediately after the war (Fetter 2016). These "hard price controls" that directly regulate the exact price of housing have been replaced by newer policies that regulate rent increases (Arnott 1995). This "newer style" policy is what exists in San Francisco.

Rent control in San Francisco began in 1979, when acting Mayor Dianne Feinstein signed San Francisco's first rent control law. Pressure to pass rent control measures was mounting due to high inflation rates nationwide, strong housing demand in San Francisco, and recently passed Proposition 13.⁴ This law capped annual nominal rent increases to 7 percent and covered all rental units built before June 13,

⁴Proposition 13, passed in 1978, limited annual property tax increases for owners. Tenants felt they were entitled to similar benefits in the form of capped annual rent increases.

1979 with one key exemption: owner-occupied buildings containing 4 units or less.⁵ These “mom and pop” landlords were cast as being less profit-driven than large-scale, corporate landlords, and more similar to the tenants being protected. These small multi-family structures made up about 44 percent of the rental housing stock in 1990, making this a large exemption to the rent control law.

While this exemption was intended to target “mom and pop” landlords, in practice small multi-families were increasingly purchased by larger businesses who would then sell a small share of the building to a live-in owner so as to satisfy the rent control law exemption. This became fuel for a new ballot initiative in 1994 to remove the small multi-family rent control exemption. This ballot initiative barely passed in November 1994. Suddenly, all multi-family structures with four units or less built in 1979 or earlier were now subject to rent control. These small multi-family structures built prior to 1980 remain rent-controlled today, while all of those built from 1980 or later are still not subject to rent control. San Francisco rent control laws have remained stable since then, possibly due to the statewide Costa-Hawkins Act. This law precludes any California city from rent controlling any housing stock built 1994 or later and regulates the scope of rent control allowed. For example, it requires rent-controlled apartment rents to be unregulated between tenants.

II. Data

We bring together data from multiple sources to enable us to observe property characteristics, determine treatment and control groups, track the migration decisions of tenants, and observe the property decisions of landlords. Our first dataset is from Infutor, which provides the entire address history of individuals who resided in San Francisco at some point between the years of 1980 and 2016.⁶ The data include not only individuals’ San Francisco addresses, but any other address within the United States at which that individual lived during the period of 1980–2016. The dataset provides the exact street address, the month and year in which the individual lived at that particular location, the name of the individual, and some demographic information including age and gender.

We link these data to property records provided by DataQuick. These data provide us with a variety of property characteristics, such as the use-code (single-family, multi-family, commercial, etc.), the year the building was built, and the number of units in the structure. For each property, the data also detail its transaction history since 1988, including transaction prices, as well as the buyer and seller names. By comparing last names in Infutor to the listed owners of the property in DataQuick, we are able to distinguish owners from renters.

Next, we match each address to its official parcel number from the San Francisco Assessor’s office. Using the parcel ID number from the Secured Roll data, we merge in any building permits that have been associated with that property since 1980. These data come from the San Francisco Planning office. This allows us to track

⁵The annual allowable rent increase was cut to 4 percent in 1984 and later to 60 percent of the CPI in 1992, where it remains today.

⁶Infutor is a data aggregator of address data using many sources including sources such as phone books, voter files, property deeds, magazine subscriptions, credit header files, and others.

large investments in renovations over time based on the quantity and type of permit issued to each building.

Finally, the parcel number also allows us to link to the parcel history file from the Assessor's office. This allows us to observe changes in the parcel structure over time. In particular, this allows us to determine whether parcels were split off over time, a common occurrence when a multi-family apartment building (one parcel) splits into separate parcels for each apartment during a condo conversion.

Summary statistics are provided in Table 1. We see the average renter in our sample in 1994 is about 37 years old and has lived at their current address for 6 years. We also see that these small multi-family properties are made up of 82 percent (0.74/0.9) renters and 18 percent owner occupants prior to 1994.

A. Data Representativeness

To examine the representativeness of the Infutor data, we link all individuals reported as living in San Francisco in 1990 to their census tract, to create census tract population counts as measured in Infutor. We make similar census tract population counts for the year 2000 and compare these San Francisco census tract population counts to those reported in the 1990 and 2000 Census for adults 18 years old and above. Regressions of the Infutor populations on census population are shown in Figure 1.⁷ Panel A shows that for each additional person recorded in the 1990 Census, Infutor contains an additional 0.44 people, suggesting we have a 44 percent sample of the population. While we do not observe the universe of San Francisco residents in 1990, the data appear quite representative, as the census tract population in the 1990 Census can explain 69 percent of the census tract variation in population measured from Infutor. Our data are even better in the year 2000. Panel B shows that we appear to have 1.1 people in Infutor for each person observed in the 2000 US Census. We likely overcount the number of people in each tract in Infutor since we are not conditioning on year of death in the Infutor data, leading to overcounting of alive people. However, the Infutor data still tracks population well, as the census tract population in the 2000 Census can explain 90 percent of the census tract variation in population measured from Infutor.

Infutor also provides information on age. As additional checks, we compare the population counts within decadal age groups living in a particular census tract as reported by Infutor to that reported by the Census. We again report the results for both 1990 and 2000. Unlike the prior analysis, we must drop Infutor observations missing birth date information for this, making our sample smaller. As shown in panel A of Table 2, the slopes of the regression lines for the 18–29, 30–39, 40–49, 50–59, and 60–69 age groups are 0.31, 0.44, 0.42, 0.24, and 0.16, respectively. This indicates the Infutor coverage is strongest for 30–49-year-olds in 1990. The R^2 values are also the highest in this age range at 65 to 76 percent. The coverage of the data improves dramatically by 2000, as shown in panel A of Table 2. The regression line slopes for the respective age groups are now 0.33, 0.74, 0.72, 0.70, 0.45. The R^2 values range from 0.61–0.85. It is clear the data disproportionately undersamples

⁷We only can do data validation relative to the US Censuses for census tracts in San Francisco because we only have address histories for people who lived in San Francisco at some point in their life.

TABLE 1—SAMPLE CHARACTERISTICS OF MULTI-FAMILY PROPERTIES (2–4 UNITS) AND THEIR TENANTS

	1990–1993			1994–2016		
	Treat	Control	Difference	Treat	Control	Difference
<i>Panel A. Tenants living in multi-family residence (2–4 units)</i>						
A1. Demographics						
Age in 1993	37.708 (10.438)	37.120 (10.639)	0.587 (0.247)	37.708 10.438	37.120 (10.639)	0.587 (0.247)
A2. Residency						
In San Francisco	0.954 (0.210)	0.954 (0.210)	0.000 (0.002)	0.569 (0.495)	0.538 (0.499)	0.032 (0.002)
Same address	0.870 (0.336)	0.867 (0.340)	0.003 (0.004)	0.261 (0.439)	0.240 (0.427)	0.021 (0.002)
Years at address	6.015 (3.958)	5.825 (3.927)	0.190 (0.047)	6.590 (5.898)	6.267 (5.530)	0.324 (0.029)
Number of persons	44,502	1,861	46,363	44,502	1,861	46,363
<i>Panel B. Multi-family properties (2–4 units)</i>						
B1. Residency						
Conversion	0.000 (0.009)	0.000 (0.000)	0.000 (0.000)	0.096 (0.294)	0.044 (0.206)	0.051 (0.002)
B2. Population, 1990–1994						
Population/avg. population	0.898 (0.436)	0.905 (0.426)	–0.008 (0.007)	2.282 (4.029)	2.252 (2.998)	0.030 (0.028)
Renters/avg. population	0.741 (0.484)	0.737 (0.482)	0.004 (0.008)	1.680 (3.555)	1.700 (2.517)	–0.020 (0.025)
Renters in rent-controlled buildings/avg. population	0.741 (0.484)	0.737 (0.482)	0.004 (0.008)	1.404 (1.927)	1.570 (2.053)	–0.165 (0.014)
Renters in redeveloped buildings/avg. population	0 (0)	0 (0)	0 (0)	0.129 (0.740)	0.060 (0.541)	0.069 (0.005)
Owners/avg. population	0.157 (0.329)	0.168 (0.335)	–0.012 (0.006)	0.602 (1.581)	0.552 (1.348)	0.050 (0.011)
B3. Permits						
Cumulative Add/alter/repair per unit	0.072 (0.231)	0.088 (0.287)	–0.016 (0.004)	0.290 (0.511)	0.254 (0.536)	0.035 (0.004)
Number of parcels	25,925	892	26,817	25,925	892	26,817

Notes: Panel A reports the summary statistics of the demographic characteristics and residency outcomes during 1990–2016 of our tenant sample. The sample consists of all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2–4 units that were built during 1900–1990. Panel B reports the summary statistics of the outcomes variables related to residency, population changes, and permit issuance during 1990–2016 of our property sample. The sample consists of all parcels that are multi-family residence with 2–4 units in San Francisco that were built during 1900–1990. The *Treat* and *Control* columns report the mean and standard deviation (in parentheses) of each outcome variable at the tenant level in panel A and at the property level in panel B. The *Difference* column reports the coefficient and standard error (in parentheses) of a regression of each outcome variable on the treatment dummy at the tenant level in panel A and at the property level in panel B.

the youngest group, but this is unsurprising as these data come from sources such as credit header files, voter files, and property deeds. Eighteen-year-olds are less likely to show up in these sources right away. Overall the data coverage looks quite good.

As described above, we merge the Infutor data with public records information provided by DataQuick about the particular property located at a given address, such as use-code and age of the property. We assess the quality of the matching procedure by comparing the distribution of the year buildings were built across census tracts among addresses listed as occupied in Infutor versus the 1990 and 2000 Censuses.

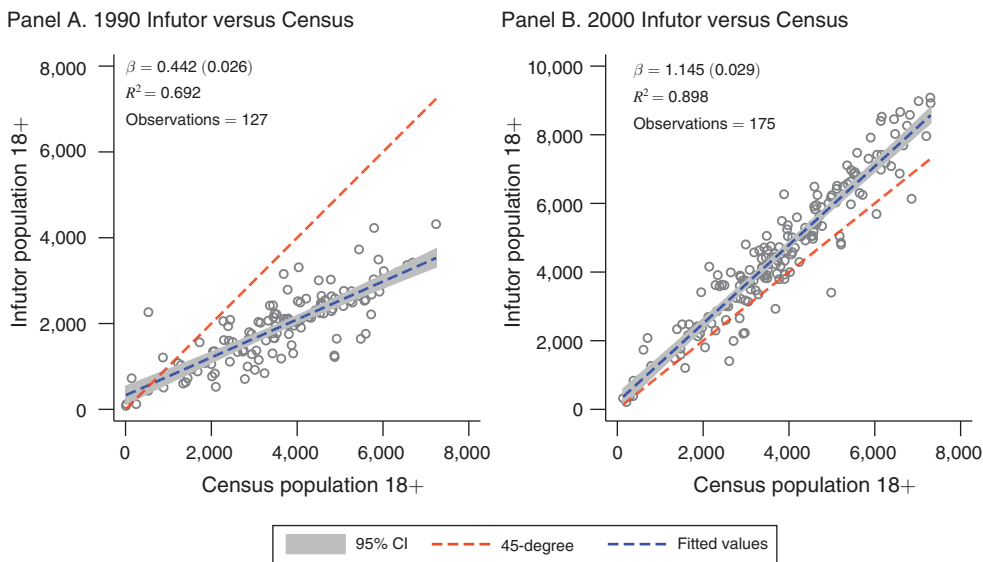


FIGURE 1. VALIDATION OF INFUTOR POPULATION VERSUS US CENSUS POPULATION

Notes: Plot shows the population of 18 and over in each census tract in 1990 and 2000 from Infutor data against that from 1990 and 2000 Censuses, respectively. The fitted line is by OLS.

TABLE 2—REPRESENTATIVENESS OF INFUTOR DATA: POPULATION BY AGE GROUPS AND AGE OF OCCUPIED HOUSING STOCKS

Age group	1990			Age group	2000		
	Slope	SE	R ²		Slope	SE	R ²
<i>Panel A. Population by age group</i>							
18–29	0.314	0.026	0.534	18–29	0.325	0.016	0.696
30–39	0.444	0.022	0.758	30–39	0.744	0.024	0.850
40–49	0.416	0.027	0.649	40–49	0.715	0.032	0.741
50–59	0.237	0.023	0.458	50–59	0.695	0.033	0.723
60–69	0.159	0.015	0.469	60–69	0.447	0.027	0.611
<i>Panel B. Age of occupied housing</i>							
Year built	Slope	SE	R ²	Year built	Slope	SE	R ²
1970–1990	0.639	0.046	0.667	1980–2000	0.813	0.024	0.876
1950–1969	0.928	0.046	0.807	1960–1979	1.083	0.036	0.853
1940–1949	1.111	0.035	0.911	1950–1959	0.955	0.049	0.711
1939 or earlier	1.024	0.040	0.872	1940–1949	1.323	0.042	0.863
				1939 or earlier	1.144	0.036	0.863

Notes: Panel A reports the coefficients, standard errors, and R² values of regressing the population counts within various age groups in each census tract from Infutor data against those from the Census in the year 1990 and 2000 respectively. Panel B reports the coefficients, standard errors, and R² values of regressing the fraction of buildings built in various time periods in each census tract from Infutor data against those from the Census in the year 1990 and 2000 respectively. In panel B, the regressions are weighted by the number of occupied housing units in each census tract from the Census.

If a building is constructed after 1993 according to its current day use-code, but we observe a person living there in 1993, we include it in the treatment group for rent control. Panel B of Table 2 shows the age distribution of the occupied stock by census tract. In both of the years 1990 and 2000, our R² values range from 67 percent

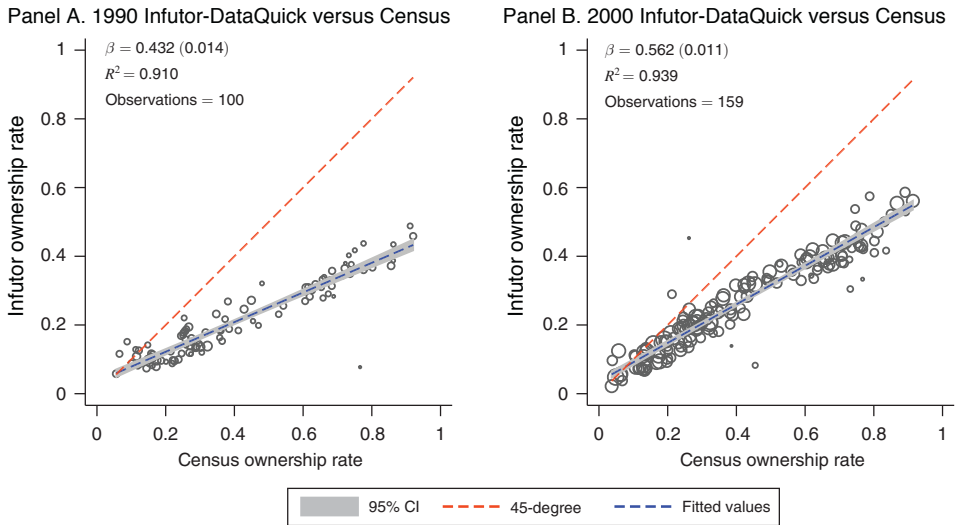


FIGURE 2. HOME OWNERSHIP RATES IN INFUTOR-DATAQUICK VERSUS US CENSUS

Notes: Plot shows census tract average owner occupant rates in 1990 and 2000 from Infutor-DataQuick data versus that from 1990 and 2000 Censuses. The size of marker is proportional to the number of occupied housing units in each census tract. The fitted line is by weighted least squares.

to 91 percent and we often cannot reject a slope of 1.⁸ This highlights the extremely high quality of the linked Infutor-DataQuick data, as the addresses are clean enough to merge in the outside data source DataQuick and still manage to recover the same distribution of building ages as reported in both the 1990 and 2000 Censuses.

To measure whether Infutor residents were owners or renters of their properties, we compare the last names of the property owners list in DataQuick to the last names of the residents listed in Infutor. Since property can be owned in trusts, under a business name, or by a partner or spouse with a different last name, we expect to underclassify residents as owners. Figure 2 plots the Infutor measure of ownership rates by census tract in 1990 and 2000, respectively, against measures constructed using the 1990 and 2000 Censuses. In 1990 (2000), a 1 percentage point increase in the owner-occupied rate leads to a 0.43 (0.56) percentage point increase in the ownership rate measured in Infutor. Despite the undercounting, our cross-sectional variation across census tract matches the 1990 and 2000 censuses extremely well, with R^2 values over 90 percent in both decades. This further highlights the quality of the Infutor data.

B. Imputing Tenant Race

We use a two-step procedure to impute the race/ethnicity of individuals in our main sample of analysis: all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993. In the first step, we use NamePrism, a

⁸ Since year built comes from the Census long form, these data are based only on a 20 percent sample of the true distribution of building ages in each tract, creating measurement error that is likely worse in the census than in the merged Infutor-DataQuick data.

non-commercial ethnicity/nationality classification tool intended to support academic research (Ye et al. 2017), to compute baseline probabilities of race/ethnicity for each tenant based on her first name and last name. In the second step, we use Bayes' rule to update the name-based probabilities for race and ethnicity using the local racial distribution at each tenant's place of residence in 1990, following a similar methodology used by the Consumer Financial Protection Bureau (CFPB 2014). More details about each step are provided below.

In step 1, for each tenant, we use both her first and last name to query the NamePrism online tool and obtain baseline probabilities for the six ethnic categories defined by the US Census Bureau: Hispanic; non-Hispanic white; non-Hispanic black or African American; non-Hispanic Asian/Pacific Islander; non-Hispanic American Indian and Alaska Native; and non-Hispanic Multi-racial.⁹ NamePrism employs a training dataset of 57 million contact lists from a major internet company, US Census data on the distribution of last names by race, and trains its algorithm using the homophily principle exhibited in communication as the basis for its ethnicity classifier.¹⁰ In this step, each tenant is assigned a probability, ranging from 0 percent to 100 percent, of belonging to each of the six ethnic groups, and the six probabilities sum to 1.

In step 2, we update each tenant's baseline racial probabilities with the racial and ethnic characteristics of the census block associated with her place of residence in 1990 using Bayes' rule to obtain posterior probabilities for the six ethnic groups.¹¹ In particular, for a tenant with name s who resides in geographic area g , we calculate the probability of race or ethnicity r for each of the six categories for a given name s , denoted as $\Pr(r|s)$. From the Summary File 1 (SF1) from Census 1990, we obtain the proportion of the population belonging to race or ethnicity r that lives in geographic area g , denoted as $\Pr(g|r)$. Bayes' rule then gives the probability that a tenant with name s residing in geographic area g belongs to race or ethnicity r :

$$\Pr(r|g,s) = \frac{\Pr(r|s)\Pr(g|r)}{\sum_{r' \in R} \Pr(r'|s)\Pr(g|r')}$$

where R denotes the set of six ethnic categories. An assumption necessary for the validity of the Bayesian updating procedure is that the probability of living in a given geographic area, given one's race, is independent of one's name. For example, it assumes that blacks with the name John Smith are just as likely to live in a certain neighborhood as blacks in general.

For each tenant, we then assign a final racial probability if the maximum of the six posterior probabilities is equal to or above 0.8, and a final racial/ethnic category corresponding to the maximum posterior; otherwise a tenant's race/ethnicity is unclassified. Table 3 shows the breakdown of our racial and ethnic classification for our main sample of analysis.

⁹This classification considers Hispanic as mutually exclusive from the race categories, with individuals identified as Hispanic belonging only to that category, regardless of racial background.

¹⁰People tend to communicate more frequently with others of similar age, language, and location.

¹¹In practice, census block level information on the racial and ethnic composition is available for 94.7 percent of our sample. For the rest of sample, we use racial and ethnic composition at the census block group (4 percent), census tract (0.2 percent), and 5-digit zip code levels (1 percent), whichever one is first available in the order listed. We set the posterior probabilities equal to the baseline probabilities from NamePrism for the rest: 0.1 percent of our sample.

TABLE 3—2010 CENSUS BLOCK RACIAL DISTRIBUTION BY TENANTS' RACE AMONG 1994 RENT CONTROL COHORT

	Average share in 2010 census block				Sample share (5)	SF overall	
	White (1)	Black (2)	Hispanic (3)	Asian (4)		1990 census (6)	2010 census (7)
<i>Predicted race</i>							
White	63.4	4.2	12.1	16.4	75.01	57.36	52.26
Black	24.8	24.0	24.4	22.8	1.40	7.72	4.69
Hispanic	33.7	6.3	31.4	24.9	8.20	14.18	18.28
Asian	38.1	4.1	13.2	40.8	15.39	20.16	24.51

Notes: Sample consists of all tenants with a classified race/ethnicity between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2–4 units that were built during 1900–1990. We geocode the 2010 addresses of tenants in our sample to the census block level. Columns 1–4 report the average shares of white, black, Hispanic, and Asian population in the census blocks containing the 2010 addresses of tenants in each classified racial/ethnic category. Column 5 reports the share of our sample by predicted race. Columns 6 and 7 report the share of tenants in San Francisco between 20 and 65 years old who were living in small multi-family residences by racial/ethnic categories according to the 1990 and 2010 US censuses.

Our methodology is similar to what's used by the CFPB to construct proxy consumer race in order to conduct fair lending analysis. CFPB (2014) and Elliott et al. (2009) demonstrate that combining geography- and name-based information into a single proxy probability for race/ethnicity significantly outperforms traditional classification methods based on names or geography alone. The key difference between our method and CFPB's method is that we use NamePrism to compute "prior" probabilities, whereas CFPB relies on the racial distribution for common last names in the United States published by the Census Bureau (Comenetz 2016). Since NamePrism uses both first and last names from a much larger name database, it is able to classify race/ethnicity for a much wider range of names at higher accuracy. Moreover, we use census block level racial composition for Bayesian updating of racial probabilities whenever possible, whereas CFPB uses racial distribution at the census block group level, which is a larger geographic unit, and thus less refined.

Validation of Race Imputation.—We report some summary statistics regarding our race imputation methodology and perform a few validation checks. Using our imputation procedure and the linked Infutor-DataQuick data, we first report in column 5 of Table 3 the racial distribution of all tenants aged 20–65 living in multi-family residences with 2–4 units as of December 31, 1993. Column 6 of Table 3 reports the 1990 Census measure of this distribution. As in the census, we find that Asians are the most numerous minority, followed by Hispanics and then blacks. This table also shows that our procedure somewhat overrepresents whites in San Francisco and underrepresents the number of minorities. This is because we only assign a race to an individual if the probability of that race is above 80 percent. In practice, this means 8,009 tenants are not assigned a race, equal to 17.27 percent of our tenant sample. Many of these unassigned individuals are likely minorities, as a large fraction of the unassigned are those with minority-sounding names but who live in relatively racially integrated neighborhoods.¹²

¹²If we do not impose this cutoff and instead simply calculate raw means of each racial group's probabilities, our racial distribution looks much closer to the distribution reported by the Census. We feel that imposing the cutoff is appropriate, however, since it ameliorates concerns regarding measurement error in our regression analysis by

To further validate our methodology, we examine the average racial makeup of the 2010 census block in which our assigned individuals live. Note that this is an out-of-sample check since we use an individual's 1990 address, not their 2010 address, in our imputation procedure. The results are reported in columns 1 through 4 of Table 3. Consistent with what one would expect from some degree of continued racial sorting, individuals we classify as white live in neighborhoods with the greatest fraction of whites (as of 2010), those we classify as black live in neighborhoods with the greatest fraction of blacks (as of 2010), and similarly for Hispanics and Asians. The same sorting result appears when we regress racial shares of an individual's 2010 census block on the individual's assigned race. The results are reported in online Appendix Table A2, with black being the omitted category. For example, being white is the strongest positive predictor of the 2010 white share, being Hispanic is the strongest positive predictor of the 2010 Hispanic share, and similarly for Asians and blacks.

III. Empirical Results

Studying the effects of rent control is challenged by the usual endogeneity issues. The tenants who choose to live in rent-controlled housing, for example, are likely a selected sample. To overcome these issues, we exploit the successful 1994 ballot initiative which removed the original 1979 exemption for small multi-family housing of four units or less, as discussed in Section I.

In 1994, as a result of the ballot initiative, tenants who happened to live in small multi-family housing built prior to 1980 were, all of a sudden, protected by statute against rent increases. Tenants who lived in small multi-family housing built 1980 and later continued to not receive rent control protections. We therefore use as our treatment group those renters who, as of December 31, 1993, lived in multi-family buildings of less than or equal to four units, built between years 1900 and 1979. We use as our control group those renters who, as of December 31, 1993, lived in multi-family buildings of less than or equal to four units, built between the years of 1980 and 1990. We exclude those renters who lived in small multi-family buildings constructed post-1990 since individuals who choose to live in new construction may constitute a selected sample and exhibit differential trends. We also exclude tenants who moved into their property prior to 1980, as none of the control group buildings would have been constructed at the time.

When examining the impact of rent control on the parcels themselves, we use small multi-family buildings built between the years of 1900 and 1979 as our treatment group and buildings built between the years of 1980 and 1990 as our control group. We again exclude buildings constructed in the early 1990s to remove any differential effects of new construction. Figure 3 shows the geographic distribution of treated buildings and control buildings in San Francisco. Since our control group was built over a narrow time span, the sample size of the treatment group is much larger than the control group. However, the control group buildings cover many

restricting to those individuals whose racial classification is more precise. We investigate using the entire sample as a robustness check in the online Appendix.



FIGURE 3. GEOGRAPHIC DISTRIBUTION OF TREATED AND CONTROL BUILDINGS IN SAN FRANCISCO

Notes: The purple dots represent parcels in the treatment group, which are parcels corresponding to multi-family residences with 2–4 units in San Francisco that were built between 1900–1979. The green dots represent parcels in the control group, which are parcels corresponding to multi-family residences with 2–4 units in San Francisco that were built between 1980–1990. The gray dots represent other types of housing stocks such as single-family residences and multi-family residences with five or more units.

neighborhoods across San Francisco, giving the treatment and control samples good overlap.

We next estimate balance tests between our treatment and control samples to evaluate whether rent control status was as good as randomly assigned. Table 1 compares the characteristics of tenants in treatment and control buildings, from 1990–1993, prior to treatment. The comparisons in raw means do not control for the zip code of the building, which we will always condition on in our analysis. Panel A shows that tenants in the treated buildings are 0.6 years older than tenants in control buildings. This is unsurprising as the older buildings have been around much longer, allowing for longer tenancies and thus older residents. Indeed, we also see that the average tenant in the treatment building has lived there for 6 years prior to treatment, while control group tenants have lived there for 5.8 years. To account for this differences, we will always condition on the length of tenancy, measured at the time of treatment, when comparing treatment and control groups in the following analysis.

We begin our analysis by studying the impact of rent control provisions on its tenant beneficiaries. Policy advocates argue that tenants covered by rent control will be dramatically helped by lower housing costs, thereby enabling them to stay in communities that they have lived in for a number years and grown attached to. We

evaluate these claims first by quantifying rent control's impact on the initial cohort of tenants living in the properties newly covered by the law. Later, in Section IIIB we examine how landlords' responses to the law change impacted the long-run housing supply of rental properties. In light of these findings, we then return to and evaluate the claim that rent control helps tenants by lowering housing costs and preventing displacement.

A. Tenant Effects

We first examine whether rent control "locks tenants into their apartments," extending the duration of time they live at the address where they were first covered by rent control. On the one hand, locking tenants into their apartments could be viewed as a cost of rent control. Tenants might not be able to move to different types of housing as their needs change, such as when they get married or have a child. On the other hand, if tenants' lack of migration not only keeps them in the same apartment but enables them to stay in San Francisco overall, then this could be viewed as a success in that rent control prevents displacement.

To evaluate these effects we use a difference-in-differences design described above, with the following exact specification:

$$(1) \quad Y_{iszt} = \delta_{zt} + \alpha_i + \beta_t T_i + \gamma_{st} + \epsilon_{it}.$$

Here, Y_{iszt} are outcome variables equal to 1 if, in year t , the tenant i is still living at either the same address as they were at the end of 1993, or, alternatively, if the tenant is still living in San Francisco. The variables α_i denote individual tenant fixed effects. The variable T_i denotes treatment, equal to 1 if, on December 31, 1993, the tenant is living in a multi-family building with less than or equal to four units built between the years 1900 and 1979.

We include fixed effects γ_{st} denoting the interaction of dummies for the year s the tenant moved into their 1993 apartment with calendar year t time dummies. These additional controls are needed since older buildings are mechanically more likely to have long-term, low-turnover tenants; not all of the control group buildings were built when some tenants in older buildings moved in. Finally, note we have included a full set of zip-code-by-year fixed effects, δ_{zt} . In this way, we control for any differences in the geographic distribution of treated buildings versus control buildings, ensuring that our identification is based off of individuals who live in the same neighborhood, as measured by zip code. Our coefficient of interest, quantifying the effect of rent control on future residency, is denoted by β_t .

Our estimated effects are shown in Figure 4, along with 90 percent confidence intervals. As further evidence of random assignment, we see no pre-trends leading up to time of treatment. Exactly at time of treatment we see a large spike in the probability that the treatment group remains at their 1993 address, versus the control group. We can see that tenants who receive rent control protections are persistently more likely to remain at their 1993 address relative to the control group. This effect decays over time, which likely reflects that as more years go by, all tenants are increasingly likely to move away from where they lived in 1993. Further, we find that treated tenants are also more likely to be living in San Francisco. This result

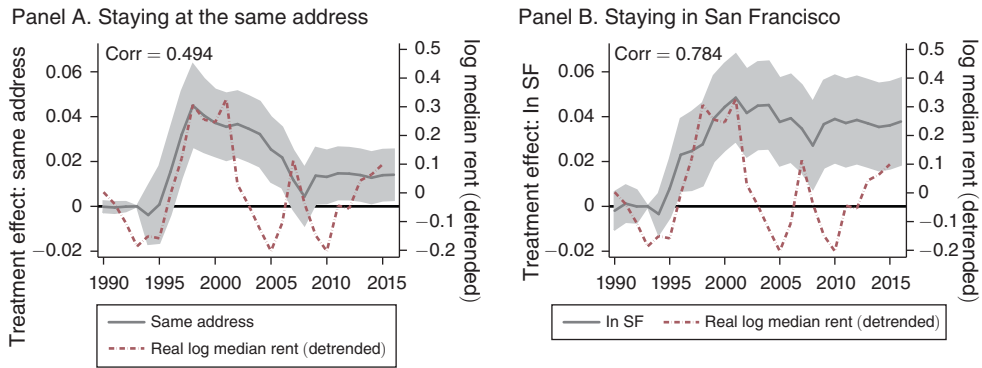


FIGURE 4. TREATMENT EFFECT FOR TENANTS IN MULTI-FAMILY RESIDENCE (2–4 UNITS)

Notes: Sample consists of all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2–4 units that were built during 1900–1990. The solid line plots the treatment effects for staying at the same address in panel A and staying in San Francisco in panel B along with 90 percent CI in shaded area. The dotted line plots the yearly deviation from the log trend in median rental rates. Standard errors are clustered at the person level.

indicates that the assignment of rent control not only impacts the type of property a tenant chooses to live in, but also their choice of location and neighborhood type.

These figures also illustrate how the time pattern of our effects correlates with rental rates in San Francisco.¹³ We would expect our results to be particularly strong in those years with quickly rising rents and thus large potential savings. Along with our yearly estimated effect of rent control, we plot the yearly deviation from the log trend in rental rates against our estimated effect of rent control in that given year. We indeed see that our effects grew quite strongly in the mid- to late-1990s in conjunction with quickly rising rents, relative to trend. Our effects then stabilize and slightly decline in the early 2000s in the wake of the dot-com bubble crash, which led to falling rental rates relative to trend. Overall, we measure a correlation of 49.4 percent between our estimated same address effects and median rents, and a correlation of 78.4 percent between our estimated SF effects and median rents.

In Table 4, we collapse our estimated effects into a short-term 1994–1999 effect, a medium-term 2000–2004 effect, and a long-term post-2005 effect. We find that in the short run, tenants in rent-controlled housing are 2.18 percentage points more likely to remain at the same address. This estimate reflects a 4.03 percent increase relative to the 1994–1999 control group mean of 54.10 percent. In the medium term, rent-controlled tenants are 3.54 percentage points more likely to remain at the same address, reflecting a 19.38 percent increase over the 2000–2004 control group mean of 18.27 percent. Finally, in the long term, rent-controlled tenants are 1.47 percentage points more likely to remain at the same address. This is a 12.95 percent increase over the control group mean of 11.35 percent. Whether these effects should widen

¹³ Annual advertised rents from the San Francisco Chronicle and Craigslist have been collected by Eric Fischer (<https://github.com/ericfischer/housing-inventory/>). Since we do not have the microdata, this gives us an aggregate San Francisco-wide annual time series of rents. Given that these data are based on actual listings, this is likely the most accurate measure of true market rate rents, among all possible data sources.

TABLE 4—TREATMENT EFFECT FOR TENANTS OF MULTI-FAMILY RESIDENCE (2–4 UNITS)

	In SF (1)	Same address (2)
Treat × period		
1994–1999	0.0200 (0.0081)	0.0218 (0.0083)
2000–2004	0.0451 (0.0115)	0.0354 (0.0088)
Post 2005	0.0366 (0.0109)	0.0147 (0.0063)
Control mean, 1994–1999	0.7641	0.5410
Control mean, 2000–2004	0.5138	0.1827
Control mean, post-2005	0.4346	0.1135
Adjusted R^2	0.586	0.608
Observations	1,251,801	1,251,801

Notes: Sample consists of all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2–4 units that were built during 1900–1990. Table reports the mean of dependent variables for the control group during 1990–1994, 2000–2004, and post-2005. Standard errors are clustered at the person level.

or narrow over time is ambiguous. On one hand, the wedge between market rate rents and rent control rents diverge, the longer one remains at one's rent-controlled address. On the other hand, the mismatch between one's 1993 address and the ideal location and type of housing is likely to grow over time, pushing tenants to give up their rent control. Since our long-term results are smaller than our medium-term findings, it appears the mismatch effect begins to grow faster than the below market rent effect over the medium to long term.

Tenants who benefit from rent control are 2.00 percentage points more likely to remain in San Francisco in the short-term, 4.51 percentage points more likely in the medium-term, and 3.66 percentage points more likely in the long term. Relative to the control group means, these estimates reflect increases of 2.62 percent, 8.78 percent, and 8.42 percent, respectively. Since these numbers are of the same magnitude as the treatment effects of staying at one's exact 1993 apartment, we find that absent rent control a large share of those incentivized to stay in their apartments would have otherwise moved out of San Francisco. Since most of the tenants "locked" into their apartments by rent control would have otherwise left the city rather than select a different apartment in the same neighborhood, the allocative inefficiency effects of rent control might be smaller than its impacts on preventing displacement.

Robustness.—A key identifying assumption for our analysis is that once neighborhood characteristics have been controlled for, as well as the number of years lived in the apartment as of December 31, 1993, those living in older versus newer buildings would not exhibit differential trends in migration. As a robustness test, in panel A of Table 5, we have restricted our treatment group to individuals who lived in structures built between 1960 and 1979, thereby comparing tenants in buildings built slightly before 1979 to tenants in buildings built slightly after 1979. We find statistically indistinguishable results from our main analysis, with point estimates actually 5 percent to 63 percent larger across the six point estimates.

TABLE 5—ROBUSTNESS CHECKS: TREATMENT EFFECT FOR TENANTS OF SMALL MULTI-FAMILY RESIDENCES

	Panel A. Treatment group: buildings built between 1960 and 1979		Panel B. Census tract fixed effects	
	In SF (1)	Same address (2)	In SF (3)	Same address (4)
Treat × period				
1994–1999	0.0326 (0.0105)	0.0289 (0.011)	0.0175 (0.0084)	0.0157 (0.0087)
2000–2004	0.0642 (0.0151)	0.0370 (0.0118)	0.0426 (0.012)	0.0284 (0.0092)
Post-2005	0.0531 (0.0145)	0.0164 (0.0084)	0.0364 (0.0114)	0.0113 (0.0066)
Control mean, 1994–1999	0.7641	0.541	0.7641	0.541
Control mean, 2000–2004	0.5138	0.1827	0.5138	0.1827
Control mean, post-2005	0.4346	0.1135	0.4346	0.1135
Adjusted R^2	0.584	0.609	0.588	0.609
Observations	135,594	135,594	1,243,242	1,243,242
	Panel C. Control group lives in buildings with 5–10 units		Panel D. Control group lives in buildings with 2–10 units	
Treat × period				
1994–1999	0.0319 (0.0096)	0.0162 (0.0094)	0.0256 (0.0063)	0.0201 (0.0064)
2000–2004	0.0424 (0.0132)	0.0291 (0.0099)	0.0452 (0.0089)	0.0340 (0.0067)
Post-2005	0.0400 (0.0124)	0.0167 (0.0071)	0.0387 (0.0084)	0.01575 (0.0048)
Control mean, 1994–1999	0.7356	0.541	0.7507	0.541
Control mean, 2000–2004	0.4935	0.178	0.5043	0.1805
Control mean, post-2005	0.4092	0.1064	0.4227	0.1101
Adjusted R^2	0.587	0.608	0.587	0.608
Observations	1,246,023	1,246,023	1,296,270	1,296,270

Notes: In panel A, we change our tenant sample to all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2–4 units that were built during 1960–1990. Hence, we have restricted our treatment group to individuals who lived in buildings built between 1960 and 1979. In panel B, the sample of tenants is the same as in our baseline regressions. Instead of using zip-code-by-year fixed effects in our baseline regressions, we use census tract by year fixed effects. In panel C, we have changed our control group to individuals who lived in multi-family residences with 5–10 units that were built during 1980–1990. The treatment group is the same as in our baseline regressions. In panel D, we have changed our control group to individuals who lived in multi-family residences with 2–10 units that were built during 1980–1990. The treatment group is the same as in our baseline regressions. Table reports the mean of dependent variables for the control group during 1990–1994, 2000–2004, and post-2005. Standard errors are clustered at the person level.

As further robustness, we redefine the neighborhood more finely, using census tracts instead of zip codes. Panel B of Table 5 repeats the analysis using census tract by year fixed effects. The results are also statistically indistinguishable from our main results, although the point estimates are between 1 percent and 28 percent smaller across the six point estimates. Dropping the zip-code-by-year fixed effects also produces similar results.

As a final robustness check, we use an alternative control group of renters living in larger multi-family apartment buildings not subject to rent control. Specifically, we create a control group of renters living in buildings with between 5 and 10 apartment units built between 1980 and 1990. We exclude large multi-family buildings built prior to 1980 from the control group because they have been covered by rent

control since 1979. Using residents of these slightly larger buildings built in the 1980s should also act as a valid control group if the sorting of tenants to buildings within neighborhoods did not depend on the exact number of units in the buildings. Panel C of Table 5 reports the treatment effect using this alternative control group. The effects are statistically indistinguishable from our main effects. Panel D of Table 5 combines our control groups, creating a larger control group of renters living in buildings with two to ten apartments building in the 1980s. Unsurprisingly, these effects are also statistically indistinguishable from our main estimates, but the standard errors are smaller due to the increased sample size of our control group.

Treatment Effect Heterogeneity.—These estimated overall effects mask economically interesting heterogeneity. We begin by repeating our analysis separately within each racial group. Racial minorities may face discrimination in the housing market, indicating that rent control may be especially impactful on limiting their displacement. Figure 5 shows the treatment effects of remaining in one's 1993 address for whites, and then the differential effects for each racial group. Since our sample sizes within any given racial group are smaller, we will focus on the overall "post" impact of rent control, not separating out the short-, medium-, and long-term effects. Whites are 2.1 percentage points more likely to remain at their treated address due to rent control. For both blacks and Hispanics, we find larger treatment effects of 10.7 and 7.1 percentage point increases for these groups, respectively.¹⁴ This suggests these minority groups disproportionately valued rent control. In contrast, the effect for Asians is statistically indistinguishable from the whites effect, with a point estimate of 0.9 percentage points.

We see further evidence that racial minorities disproportionately benefited from rent control when looking at the impact of the law on remaining in San Francisco. Rent control leads treated whites to be 2.8 percentage points more likely to remain in San Francisco, while blacks, Hispanics, and Asians are 10.7, 10.1, and 6.4 percentage points more likely to remain in San Francisco, respectively.¹⁵ This suggests that rent control had a substantial impact on limiting displacement of minorities from the city, an additional sign that rent control strongly benefits the initial cohort of renters who are covered by the law.

We next examine treatment effect heterogeneity across neighborhoods, duration of tenancy, and age.¹⁶ The goal of this exercise is two-fold. First we want to examine whether tenants who have lived in their neighborhoods for a long time disproportionately value rent control, as would be expected if these long-term tenants had built up a stock of neighborhood-specific capital. Second, we want to examine whether the value of rent control varies across tenant age. It is well known that younger individuals move more often. If young people need to move often for personal reasons, it

¹⁴ Since our sample of blacks is quite small, the differential effects for blacks are not statistically indistinguishable from whites.

¹⁵ As a robustness check, we repeat this analysis on the entire sample, including the renters whose probabilities for their most likely imputed race were below 80 percent. These results are in online Appendix Figure A1. The result are statistically indistinguishable from our main results, but the differences in the point estimates across races are smaller. This is consistent with the fact we have much more measurement error in the imputed races for these additional renters.

¹⁶ We do not cut on race here as well, as the samples would become too thin.

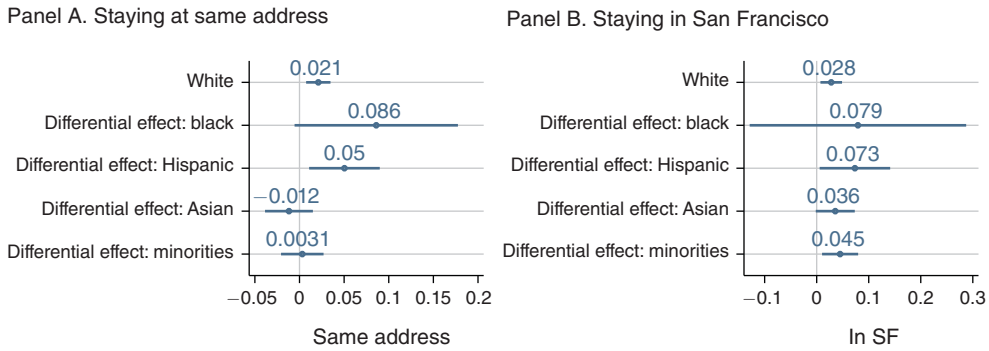


FIGURE 5. HETEROGENEITY BY TENANT'S RACE IN TREATMENT EFFECT FOR TENANTS

Notes: Sample consists of all tenants with a classified race/ethnicity between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2–4 units that were built during 1900–1990. For white tenants, we report the average treatment effect in the post-1994 period along with 90 percent CI. For the other ethnic categories, we report the differential treatment effect in the post-1994 period between white and each ethnic category along with 90 percent CI. Minorities consist of all ethnic groups other than white. Standard errors are clustered at the person level.

will be hard for them to benefit from rent control since they cannot stay in one place long enough to access the insurance value of rent control.

To examine these effects, we cut the data by age, sorting individuals into two groups, a young group who were aged 20–39 in 1993 and an old group who were aged 40–65 in 1993. We also sort the data based on the number of years the individual has been living at their 1993 address. We create a “short-tenure” group of individuals who had been living at their address for less than four years and a “long-tenure” group of individuals who had been living at their address for between 4 and 14 years. Finally, we cut the sample of zip codes based on whether their housing price appreciation from 1990 to 2000 was above or below the median, as measured by the housing transactions observed in DataQuick. Ideally, we would measure market rental price appreciation across neighborhoods, but no data source for this exists. While rents and house prices need not be perfectly correlated, house prices and market rents tend to move together. We form eight subsamples by taking the $2 \times 2 \times 2$ cross across each of these three dimensions and re-estimate our effects for each subsample.

The results are reported in Table 6 and plotted in online Appendix Figures A2 and A3. We summarize the key implications. First, we find that the effects are weaker for younger individuals. We believe this is intuitive. Younger households are more likely to face larger idiosyncratic shocks to their neighborhood and housing preferences (such as changes in family structure and employment opportunities), which makes staying in their current location particularly costly, relative to the types of shocks older households receive. Thus, younger households may feel more inclined to give up the benefits afforded by rent control to secure housing more appropriate for their circumstances.

Moreover, among older individuals, there is a large gap between the estimated effects based on tenure duration. Older, long-tenure households have a strong, positive response to rent control. That is, they are more likely to remain at their 1993

TABLE 6—HETEROGENEITY BY AGE, TENURE, AND NEIGHBORHOOD HOUSE PRICE APPRECIATION IN TREATMENT EFFECT OF STAYING AT SAME ADDRESS

	Older tenants		Younger tenants	
	(1)	(2)	(3)	(4)
<i>Panel A. Above-median house price appreciation zip codes</i>				
Treat × post	0.062 (0.019)	-0.107 (0.042)	0.018 (0.012)	-0.003 (0.032)
Tenant tenure duration	Long	Short	Long	Short
<i>Panel B. Below-median house price appreciation zip codes</i>				
Treat × post	0.041 (0.015)	0.010 (0.033)	0.007 (0.009)	0.039 (0.018)
Tenant tenure duration	Long	Short	Long	Short

Notes: Sample consists of all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2–4 units that were built during 1900–1990. We first divide individuals into two groups by whether their 1993 zip code experienced above- or below-median house price appreciation during 1990–2000. We further sort the sample by age group. The young group refers to residents who were aged 20–39 in 1993 and the old group are residents who were aged 40–65 in 1993. Finally, we cut the data by number of years the individual has been living at their 1993 address. We define a *long-tenure* group of individuals who had been living at their 1993 address for greater than or equal to four years and a *short-tenure* group of individuals who had been living at their address for less than four years. The coefficients represent average treatment effects in the post-1994 period. Standard errors are clustered at the person level. See online Appendix Figures A2 and A3 that plot the full dynamics of these treatment effects.

address relative to the control group. In contrast, older, short-tenure individuals are estimated to have a weaker response to rent control. They are less likely to remain at their 1993 address relative to the control group.

To further explore the mechanism behind this result, we now investigate these effects based on the 1990–2000 price appreciation of their 1993 zip codes. Among older, long-tenure individuals, we find that the effects are always positive and strongest in those areas which experienced the most price appreciation between 1990 and 2000, as one might expect. For older, short-tenure households, however, the results are quite different. For this subgroup, the effects are actually *negative* in the areas which experienced the *highest* price appreciation. They are positive in the areas which experienced below-median price appreciation.¹⁷

This result suggests that landlords actively try to remove tenants in those areas where rent control affords the most benefits, i.e., high price appreciation areas. There are a few ways a landlord could accomplish this. First, landlords could try to legally evict their tenants by, for example, moving into the properties themselves, known as owner move-in eviction. Alternatively, landlords could evict tenants according to the provisions of the Ellis Act, which allows evictions when an owner wants to remove units from the rental market: for instance, in order to convert the units into condos or a tenancy in common.¹⁸ Finally, landlords are legally allowed to negotiate with tenants over a monetary transfer convincing them to leave. In this way, tenants may “bring their rent control with them” in the form of a lump sum tenant buyout. Of

¹⁷ A similar pattern holds for younger individuals as well, although the results are weaker.

¹⁸ Asquith (2018) studies the use of Ellis Act evictions in the 2000s by landlords of rent-controlled properties in San Francisco.

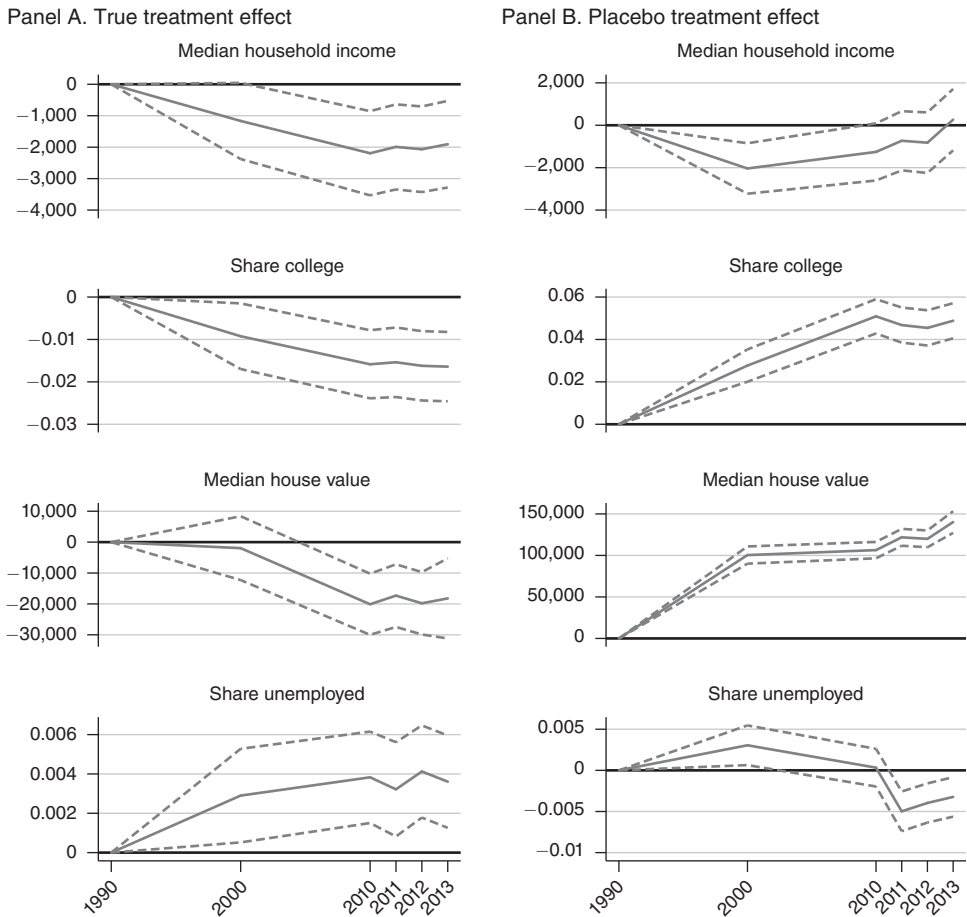


FIGURE 6. TREATMENT EFFECT ON NEIGHBORHOOD QUALITY FOR TENANTS OF MULTI-FAMILY RESIDENCE (2–4 UNITS)

Notes: Sample consists of all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2–4 units that were built during 1900–1990. Median household income, share of residents with college education and above, median house value, and share of unemployed are measured in the census tract that an individual is living in a given year. The data sources are decennial censuses in 1990 and 2000, as well as 5-year pooled ACS for 2010 to 2013. Panel A plots the true treatment effects for various proxies of neighborhood quality. Panel B plots the placebo treatment effects where we assume those treated by rent control remain at their 1993 addresses, but allow the control group to migrate as seen in the data. The treatment effects along with 90 percent CI are plotted. Standard errors are clustered at the person level.

course, if landlords predominantly use evictions, tenants are not compensated for their loss of rent protection, weakening the insurance value of rent control.

Effects on Neighborhood Quality.—The results from the previous subsection help to rationalize some additional, final findings. In panel A of Figure 6, we examine the impact that rent control has on the types of neighborhoods in which tenants live. We find that those who received rent control ultimately live in census tracts with lower house prices, lower median incomes, lower college shares, and higher unemployment rates than the control group. As panel B shows, this is not a function of the areas in which treated individuals lived in 1993. In this figure, we fix

the location of those treated by rent control at their 1993 locations, but allow the control group to migrate as seen in the data. If rent-controlled renters were equally likely to remain in their 1993 apartments across all locations in San Francisco, we would see the sign of the treatment effects on each neighborhood characteristic to be the same as in the previous regression. Instead, we find strong evidence that the out-migration of rent-controlled tenants came from very selected neighborhoods. Had treated individuals remained in their 1993 addresses, they would have lived in census tracts which had significantly higher college shares, higher house prices, lower unemployment rates, and similar levels of household median income relative to the control group.

This evidence is consistent with the idea that landlords undertake efforts to remove their tenants or convince them to leave in improving, gentrifying areas. In addition, the rent control tenants are more likely to remain at their address within the less gentrifying areas, as we saw in the previous analysis in Table 6. These combined effects lead tenants treated by rent control to live in lower quality areas. Further, it highlights that rent control does not appear to be an effective means of providing tenants access to neighborhoods with better amenities. The better locales are where landlords have the most to gain from removing rent-controlled tenants and these landlords apparently work hard to make this happen. Having said that, our prior results did show that rent control helped tenants remain in San Francisco overall. Thus, while they are unable to live in the nicest parts of the city, it is possible that by being able to remain in San Francisco, they are able to enjoy lower commute times or work at better jobs than they otherwise would have had they been displaced. These types of amenities cannot be observed in our data.

B. *Parcel and Landlord Effects*

The results above strongly suggest that while tenants value and take advantage of the protections offered by rent control, landlords actively take steps to reduce the burdens of the law, especially in those areas in which it would be most profitable to do. Motivated by these findings, in this section, we continue our analysis by studying and quantifying the landlord response more directly. To do so, we examine the impact of rent control on the properties themselves. In particular, we study how rent control affects the type of residents who live in the buildings, as well as how it impacts the investments that landlords choose to make in the properties. This analysis will enable us to understand the effects of rent control on long-term rental housing supply. Such changes in housing supply will ultimately impact equilibrium market rents and thus housing affordability for future renters.

Summary statistics for our key outcomes are in panel B of Table 1. This table shows that treatment and control properties are balanced in the pre-period in terms of total residents and number of renter residents. We see 1.2 percentage points more owners in the control group and 1.6 percentage points more construction/renovation permits. These small differences reflect that fact that the control buildings are slightly newer.

We run a specification similar to (1):

$$(2) \quad Y_{kzt} = \delta_{zt} + \lambda_k + \beta_t T_k + \epsilon_{kt}$$

where k now denotes the individual parcel and λ_k represent parcel fixed effects. The variable T_k denotes treatment, equal to one if, on December 31, 1993, the parcel is a multi-family building with less than or equal to four units built between the years 1900 and 1979. The δ_{zt} variables once again reflect zip-code-by-year fixed effects. Our outcome variables Y_{kzt} now include the number of renters and owners living in the building, the number of renovation permits associated with the building, and whether the building is ever converted to a condo or TIC. The permits we look at specifically are addition/alteration permits, taken out when major work is done to a property.

We begin by plotting in panel A of Figure 7 the effects of rent control on the number of individuals living at a given parcel, calculated as a percentage of the average number of individuals living at that parcel between the years 1990–1994. We estimate a decline of approximately 6.4 percent over the long run, although this effect is not statistically significant.

We next decompose this effect into the impact on the number of renters and the number of owners living at the treated buildings. As shown in panel B, we find that there is a significant decline in the number of renters living at a parcel, equal to 14.5 percent in the late 2000s, relative to the 1990–1994 level. Panel C shows that the decline in renters was counterbalanced by an increase of 8.1 percent in the number of owners in the late 2000s. This is our first evidence suggestive of the idea that landlords redeveloped or converted their properties so as to exempt them from the new rent control regulations.

We now look more closely at the decline in renters. In panel A of Figure 8, we see that there is an eventual decline of 24.6 percent in the number of renters living in rent-controlled apartments, relative to the 1990–1994 average.¹⁹ This decline is significantly larger than the overall decline in renters. This is because a number of buildings which were subject to rent control status in 1994 were redeveloped in such way so as to no longer be subject to it. These redevelopment activities include tearing down the existing structure and putting up new single family, condominium, or multi-family housing or simply converting the existing structure to condos. These redeveloped buildings replaced 7.2 percent of the initial rental housing stock treated by rent control, as shown in panel B of Figure 8.

To further investigate this mechanism, we check directly whether a multi-family property which fell under the rent control regulations in 1994 is more likely to have converted to condominium housing or a tenancy in common, relative to a multi-family property which did become subject to rent control. In panel C of Figure 8, we show that treated buildings are 8 percentage points likely to convert to condo or TIC in response to the rent control law. This represents a significant loss in the supply of rent-controlled housing.

As a final test of whether landlords actively respond to the imposition of rent control, we examine whether the landlords of rent-controlled properties disproportionately take out addition/alteration (i.e., renovation) permits. We find this to strongly be the case, with treated buildings receiving 4.6 percent more addition/alteration permits per unit as shown in panel D of Figure 8. Of course, conversions

¹⁹Note here that we mean relative to the number of individuals who lived at parcels which received rent control status due to the 1994 law change.

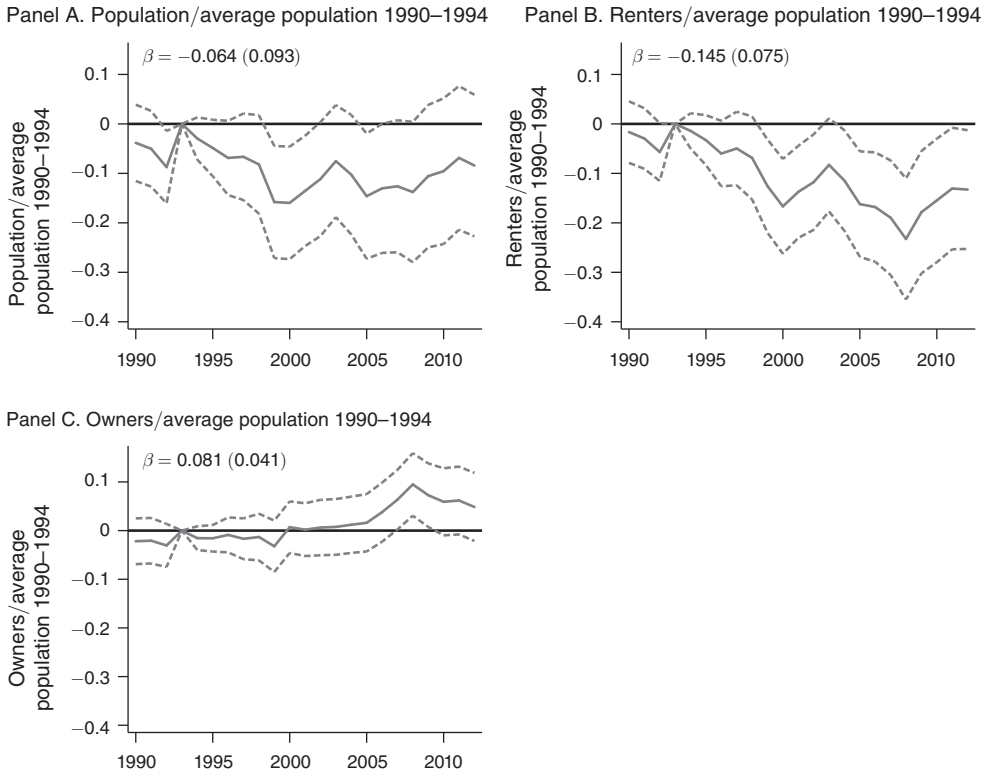


FIGURE 7. TREATMENT EFFECT FOR MULTI-FAMILY RESIDENCE (2–4 UNITS)

Notes: Sample consists of all multi-family residences with 2–4 units in San Francisco that were built during 1900–1990. The treatment effects along with 90 percent CI are plotted. Standard errors are clustered at the parcel level. The average treatment effects in the post-2006 period and their standard errors are reported in the upper-left corner.

of multi-family housing to condos undoubtedly require significant alteration to the structural properties of the building and thus would require such a permit to be taken out. These results are thus consistent with our results regarding condo conversion.

Treatment Effect Heterogeneity.—We now explore the heterogeneity in these effects between high and low house price appreciation zip codes. This analysis is motivated by our previous tenant regressions in which we found that landlords of rent-controlled buildings appear to have actively removed tenants in high appreciation zip codes. Here, we investigate whether landlords of rent-controlled apartments also disproportionately converted to condo or redeveloped buildings in high appreciation areas. Table 7 reports the average treatment effects within high and low appreciation zip codes. We find a 21 percent decline in the renter population and a 12 percent increase in the owner population within the high appreciation zip codes, versus a 11 percent renter decline and 6 percent owner increase in low appreciation areas. Further, we find condo conversions increase by 10 percent in high appreciation zip codes versus 5.8 percent in low appreciation areas. The conversion to owner-occupied housing may be especially lucrative in these high appreciation zip

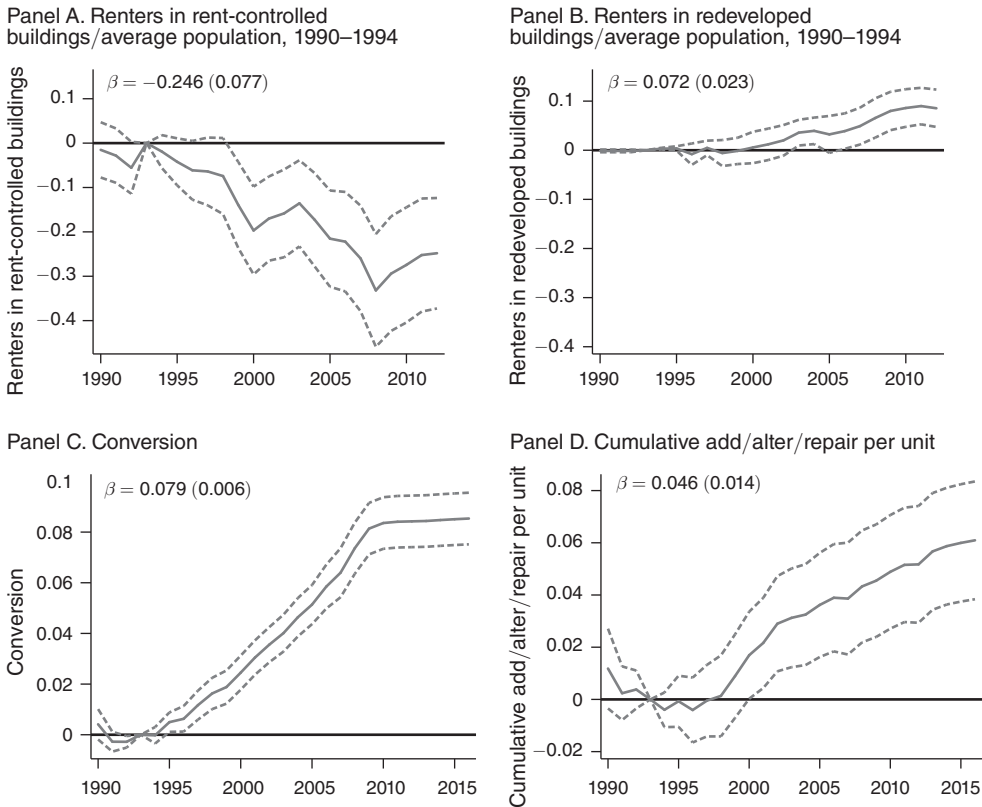


FIGURE 8. TREATMENT EFFECT FOR MULTI-FAMILY RESIDENCE (2–4 UNITS)

Notes: Sample consists of all multi-family residences with 2–4 units in San Francisco that were built during 1900–1990. The treatment effects along with 90 percent CI are plotted. Standard errors are clustered at the parcel level. The average treatment effects in the post-2006 period and their standard errors are reported in the upper-left corner.

codes as they likely have higher income residents. In contrast, we find a larger effect (9.3 percent versus 3.2 percent) of properties being knocked down and rebuilt in low appreciation areas than high priced areas. This effect is possibly driven by land use regulations making it very hard to build new construction in high-end areas of San Francisco.²⁰ Overall, these effects reaffirm that the landlords remove rental housing stock in those areas where it is most profitable to do so.

Gentrification Effects.—The previous section shows that rent control incentivized landlords to substitute away from an older rental housing stock toward new construction rentals and owner-occupied condos. Combining our estimates of rent control’s effect on the number of owner occupants (8.1 percent) and renters living in rent control exempt housing (7.2 percent) suggests that 15.3 percent of the treated properties engaged in renovations to evade rent control. Since these types of

²⁰Most new construction in San Francisco has occurred in neighborhoods that historically were dominated by industry and warehouses.

TABLE 7—TREATMENT EFFECT HETEROGENEITY FOR MULTI-FAMILY PARCELS
BY HOUSE PRICE APPRECIATION

	High appreciation (1)	Low appreciation (2)
Population/average population 1990–1994	−0.092 (0.176)	−0.050 (0.108)
Renters/average population 1990–1994	−0.207 (0.144)	−0.112 (0.085)
Renters in rent-controlled buildings/average population 1990–1994	−0.284 (0.148)	−0.225 (0.088)
Renters in redeveloped buildings/average population 1990–1994	0.032 (0.058)	0.093 (0.016)
Owners/average population 1990–1994	0.116 (0.066)	0.063 (0.052)
Conversion	0.100 (0.011)	0.058 (0.006)
Cumulative Add/alter/repair per unit	0.016 (0.03)	0.061 (0.015)

Notes: Sample consists of all multi-family residences with 2–4 units in San Francisco that were built during 1900–1990. We divide tenants into two groups by whether their 1993 zip code experienced above- or below-median house price appreciation during 1990–2000. Columns 1 and 2 report the average treatment effects for various parcel level outcomes in the post-2006 period for residences in the high and low appreciation areas, respectively. Standard errors in parentheses are clustered at the parcel level.

renovations create housing that likely caters to high income tastes, rent control may have fueled the gentrification of San Francisco. To assess this, we compare the 2015 residents living in properties treated by rent control to those living in the control buildings in 2015. While we do not have data directly on the income levels of the 2015 residents of these properties, we can use the historical neighborhood choices of these tenants as a proxy for their income. Intuitively, if residents of treated buildings used to live in high-end neighborhoods, while residents of control buildings used to live in low-end neighborhoods, we can infer that the residents of treated buildings are likely to be higher income. Specifically, we take all residents in the treatment and control buildings as of 2015. We then look at their addresses as of 2010, five years prior. We geocode these 2010 addresses to census block groups and measure the block group per capita income of their 2010 address, from the ACS.

We find that properties treated by rent control have tenants who came from neighborhoods with \$1,292 higher per capita incomes (standard error of 522), representing a 2.8 percent increase, relative to residents of control group buildings located in the same zip code.²¹ This 2.8 percent increase represents the average income increase across *all* properties treated by rent control. Since only 15.3 percent of these properties upgraded their housing stock, we would expect these high income residents to only be drawn into this 15.3 percent. Indeed, the other 85 percent of the treated housing stock that did not renovate may have lower income residents due to the direct effect of rent control on tenant mobility. To construct a lower bound estimate of the effect of rent control on gentrification, we will assume that residents of

²¹ The full regression details are reported in online Appendix Table A3.

the non-renovated housing stock have incomes similar to that of the control group. Under this assumption, our estimate of a 2.8 percent increase in residents' incomes suggests that the renovated buildings attracted residents with *at least* 18 percent ($2.8/0.153$) higher incomes than residents of control group buildings in the same zip code. In this way, rent control appears to have brought higher income residents into San Francisco, fueling gentrification.

C. Impacts on Inequality

Taking our results all together, it appears rent control has substantively different impacts on income inequality in the short versus long run. In the short run, rent control prevents displacement of the initial 1994 tenants from San Francisco, especially among racial minorities. To the extent that these 1994 tenants are of lower income than those moving into San Francisco over the following years, rent control increases income inequality. However, this short-term effect decays over time. Eight years after the law change, 4.5 percent of the tenants treated by rent control were able to remain in San Francisco because of rent control. However, five years later, this effect had decayed to 3.7 percent, and will likely continue to decline in the future.

In the long run, on the other hand, landlords are able to respond to the rent control policy change by substituting toward types of housing exempt from rent control price caps, upgrading the housing stock, and lowering the supply of rent-controlled housing. Indeed, the prior section showed that as of 2015, the average property treated by rent control has *higher income* residents than similar market rate properties. The long-term landlord response thus offsets rent control's initial effect of keeping lower income tenants in the city by replacing them with residents of above-average income. In this way, rent control works to increase income inequality in both the short run and in the long run, but through different means. Rent control's short-term effects increases the left tail of the income distribution, while the long-term effects increase the right tail.

In addition to widening income inequality, rent control has unequal effects on tenants living in San Francisco at the time of the law change and future tenants of the city. Incumbent tenants already living in San Francisco who get access to rent control as part of the law change are clearly made better off as indicated by their preference to remain in their rent-controlled apartment. However, this comes at the expense of future renters in San Francisco, who must bear higher rents due to the endogenous reductions in rental supply. In this way, the law served as a transfer from future renters in the city to renters in 1994, creating economic well-being inequality between incumbent and future renters of San Francisco. Our companion paper (Diamond, McQuade, and Qian 2018) performs a fully quantitative analysis of these welfare gains and losses through the lens of a dynamic discrete choice model of tenant migration and performs general equilibrium counterfactual analyses.

Since incumbent renters are made better off, it is not surprising that popular votes to expand rent control often pass in cities with high renter populations. The beneficiaries are the ones who are able to vote, while future renters who pay the costs of rent control do not get a say in these elections. Local popular votes thus appear to be an inefficient way to set rent control policies.

IV. Conclusion

In this paper, we have studied the impact of rent control on its tenant beneficiaries as well as the landlord response. To answer this question, we exploit a unique rent control expansion in San Francisco in 1994 that suddenly provided rent control protections for small multi-family housing built prior to 1980. By combining new panel microdata on individual migration decisions with detailed assessor data on individual parcels in San Francisco, we get quasi-experimental variation in the assignment of rent control at both the individual tenant level and at the parcel level.

We find that, on average, in the medium to long term the beneficiaries of rent control are between 10 and 20 percent more likely to remain at their 1994 address relative to the control group and, moreover, are more likely to remain in San Francisco. Further, we find the effects of rent control on tenants are stronger for racial minorities, suggesting rent control helped prevent minority displacement from San Francisco. All our estimated effects are significantly stronger among older households and among households that have already spent a number of years at their current address. On the other hand, individuals in areas with quickly rising house prices and with few years at their 1994 address are less likely to remain at their current address, consistent with the idea that landlords try to remove tenants when the reward is high, through either eviction or negotiated payments.

We find that landlords actively respond to the imposition of rent control by converting their properties to condos and TICs or by redeveloping the building in such a way as to exempt it from the regulations. In sum, we find that impacted landlords reduced the supply of available rental housing by 15 percent. Further, we find that there was a 25 percent decline in the number of renters living in units protected by rent control, as many buildings were converted to new construction or condos that are exempt from rent control.

This reduction in rental supply likely increased rents in the long run, leading to a transfer between future San Francisco renters and renters living in San Francisco in 1994. In addition, the conversion of existing rental properties to higher-end, owner-occupied condominium housing ultimately led to a housing stock increasingly directed toward higher income individuals. In this way, rent control contributed to the gentrification of San Francisco, contrary to the stated policy goal. Rent control appears to have increased income inequality in the city by both limiting displacement of minorities and attracting higher income residents.

These results highlight that forcing landlords to provide insurance against rent increases can ultimately be counterproductive. If society desires to provide social insurance against rent increases, it may be less distortionary to offer this subsidy in the form of government subsidies or tax credits. This would remove landlords' incentives to decrease the housing supply and could provide households with the insurance they desire. A point of future research would be to design an optimal social insurance program to insure renters against large rent increases.

REFERENCES

- Arnott, Richard.** 1995. "Time for Revisionism on Rent Control?" *Journal of Economic Perspectives* 9 (1): 99–120.
- Asquith, Brian.** 2018. "Do Rent Increases Reduce the Housing Supply under Rent Control? Evidence from Evictions in San Francisco." Unpublished.
- Autor, David H., Christopher J. Palmer, and Parag A. Pathak.** 2014. "Housing Market Spillovers: Evidence from the End of Rent Control in Cambridge, Massachusetts." *Journal of Political Economy* 122 (3): 661–717.
- Bulow, Jeremy, and Paul Klemperer.** 2012. "Regulated Prices, Rent Seeking, and Consumer Surplus." *Journal of Political Economy* 120 (1): 160–86.
- Comenetz, Joshua.** 2016. "Frequently Occurring Surnames in the 2010 Census." <https://www2.census.gov/topics/genealogy/2010surnames/surnames.pdf>.
- Consumer Financial Protection Bureau.** 2014. "Using Publicly Available Information to Proxy for Unidentified Race and Ethnicity: A Methodology and Assessment." Washington, DC: Consumer Financial Protection Bureau. https://files.consumerfinance.gov/f/201409_cfpb_report_proxy-methodology.pdf.
- Diamond, Rebecca, Tim McQuade, and Franklin Qian.** 2018. "Who Benefits from Rent Control? The Equilibrium Consequences of San Francisco's Rent Control Expansion." Unpublished.
- Diamond, Rebecca, Tim McQuade, and Franklin Qian.** 2019. "The Effects of Rent Control Expansion on Tenants, Landlords, and Inequality: Evidence from San Francisco: Dataset." *American Economic Review*. <https://doi.org/10.1257/aer.20181289>.
- Downs, Anthony.** 1988. *Residential Rent Controls: An Evaluation*. Washington, DC: Urban Land Institute.
- Early, Dirk W.** 2000. "Rent Control, Rental Housing Supply, and the Distribution of Tenant Benefits." *Journal of Urban Economics* 48 (2): 185–204.
- Elliott, Marc N., Peter A. Morrison, Allen Fremont, Daniel F. McCaffrey, Philip Pantoja, and Nicole Lurie.** 2009. "Using the Census Bureau's Surname List to Improve Estimates of Race/Ethnicity and Associated Disparities." *Health Services and Outcomes Research Methodology* 9 (2): 69.
- Fetter, Daniel K.** 2016. "The Home Front: Rent Control and the Rapid Wartime Increase in Home Ownership." *Journal of Economic History* 76 (4): 1001–43.
- Glaeser, Edward L., and Erzo F. P. Luttmer.** 2003. "The Misallocation of Housing under Rent Control." *American Economic Review* 93 (4): 1027–46.
- Gyourko, Joseph, and Edward Glaeser.** 2008. *Rethinking Federal Housing Policy: How to Make Housing Plentiful and Affordable*. Washington, DC: American Enterprise Institute.
- Gyourko, Joseph, and Peter Linneman.** 1989. "Equity and Efficiency Aspects of Rent Control: An Empirical Study of New York City." *Journal of Urban Economics* 26 (1): 54–74.
- Gyourko, Joseph, and Peter Linneman.** 1990. "Rent Controls and Rental Housing Quality: A Note on the Effects of New York City's Old Controls." *Journal of Urban Economics* 27 (3): 398–409.
- Moon, Choon-Geol, and Janet G. Stotsky.** 1993. "The Effect of Rent Control on Housing Quality Change: A Longitudinal Analysis." *Journal of Political Economy* 101 (6): 1114–48.
- Olsen, Edgar O.** 1972. "An Econometric Analysis of Rent Control." *Journal of Political Economy* 80 (6): 1081–1100.
- Sims, David P.** 2007. "Out of Control: What Can We Learn from the End of Massachusetts Rent Control?" *Journal of Urban Economics* 61 (1): 129–51.
- Sims, David P.** 2011. "Rent Control Rationing and Community Composition: Evidence from Massachusetts." *B.E. Journal of Economic Analysis & Policy* 11 (1): Article 25.
- Stigler, George, and Milton Friedman.** 1946. *Roofs or Ceilings? The Current Housing Problem*. Irvington-on-Hudson, NY: The Foundation for Economic Education, Inc.
- Suen, Wing.** 1989. "Rationing and Rent Dissipation in the Presence of Heterogeneous Individuals." *Journal of Political Economy* 97 (6): 1384–94.
- Ye, Junting, Shuchu Han, Yifan Hu, Baris Coskun, Meizhu Liu, Hong Qin, and Steven Skiena.** 2017. "Nationality Classification Using Name Embeddings." *Proceedings of the 2017 ACM on Conference on Information and Knowledge Management*: 1897–906.