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STATEMENT OF WILLIAM A. BIRDWELL PRESENTED TO THE
OREGON STATE SENATE COMMITTEE ON HOUSING IN
OPPOSITION TO SB 608 RELATING TO RESIDENTIAL TENANCIES
INTRODUCED IN JANUARY 2019

Introduction

My name is William A. Birdwell. I am a native Oregonian, having been born in Portland and raised mainly in Gresham. I graduated from Gresham High School and Oregon State University.

I have lived and practiced intellectual property law in Portland most of my life, though I have lived in other states and out of the country for significant periods, mainly for educational purposes. I now practice law part time and live in West Linn.

I am presenting this Statement mainly in opposition to one feature of SB 608. The specific feature that I oppose is embodied in those portions of the Bill that prohibit no-cause evictions of a tenant after twelve months occupancy. That effectively converts a leasehold interest into a life estate, particularly when coupled with the requirement that to evict a tenant for cause the landlord must show three documented

breaches of the lease agreement by the tenant within a 12-month period, regardless of the seriousness of any one breach.

Background

Late last year I purchased two rental properties in Portland after selling two out-of-state properties to invest in Oregon. This was a difficult, time-consuming and expensive set of real estate transactions that occupied most of my time for about six months. The most important property I sold was a rental property in Los Angeles that I sold specifically to escape LA rent control. Even though the property was not highly leveraged, I could not increase the rent more than 3% per year so I could not realize an acceptable return on my investment given the expenses associated with the property. The other property was a rental house in Tucson that I had purchased many years ago as my residence but converted to a rental property after I moved back to Oregon.

In deciding to invest in Portland I was aware of, but felt I could live with, the rent control ordinance in Portland. However, I was taken aback when I found out about SB 608 introduced in early January 2019. SB 608 is very similar in salient respects to the LA ordinance that I found unacceptable, particularly in the limited conditions required for a no cause eviction.

I think the properties I purchased in Portland are quality properties. One is a fully rented 7-plex and the other is a recently-vacated duplex to which I have made some improvements and which I am about to put on the rental market. However, if SB 608 becomes law I will again be saddled with a troublesome no cause eviction prohibition.

Critique of SB 608

Structure of SB 608

As I understand the circumstances that gave rise to this Bill, the Committee on Housing has determined that an emergency exists in Oregon due to a lack of affordable housing for a significant portion of the State's population.

Generally, SB 608 basically appears to do two things: (1) control rent increases and evictions by owners of rental properties over 15 years old, and (2) relieve the owners of properties 15 years or less years old from the rent control and eviction controls applied to owners of such properties. But it is hard to see how these steps would encourage development of affordable housing any more than would otherwise occur naturally under the current law or, for a specific example, under the rent control ordinance in the City of Portland. Indeed, credible studies have concluded that rent control laws

do not produce new affordable housing.¹ That being so, there is no rational relationship between the declaration of an emergency and the effect of the Bill.

What most likely would incent developers to build affordable housing are things like tax breaks, subsidies, and condemnation of land that is not in use then selling it to a developer at a favorable price. The benefits of these incentives could be passed onto modest means tenants in the form of lower rents. It would also increase the supply of housing and thereby reduce rents naturally.

Moreover, the burden of State action would be borne by all the citizens, not just those who managed to save enough money to own real property.

Rent Increase Limit

As a practical matter I think rental property owners should be able to live with annual increases up to 7% plus the consumer price index above the existing rent. I believe this because, like the Portland rent regulation, the legally-allowed

¹ R. Diamond et al., The Effects of Rent Control Expansion on Tenants, Landlords, and Inequality: Evidence from San Francisco, December 21, 2018; <https://web.stanford.edu/~diamondr/DMQ.pdf>; Email: diamondr@stanford.edu; R. Diamond, What does economic evidence tell us about the effects of rent control?, Brookings Report October 18, 2018, <https://www.brookings.edu/research/what-does-economic-evidence-tell-us-about-the-effects-of-rent-control/>; L. Sturtevant, The Impacts of Rent Control: A Research Review and Synthesis, MHC Research Foundation, 1775 Eye Street, N.W., suite 1100, Wash. D/C. 20006,

increase will probably be within rent increases that typical market forces allow anyway

Prohibition of No-cause Evictions After Twelve Months Occupancy and the Requirements to Evict

This prohibition of no-cause evictions after twelve months occupancy effectively converts a tenant's lease to a life estate after twelve months, thereby transferring a property right from the owner of the property to a tenant who, unlike the owner, has made no capital investment in the property. It is made worse by the provision of the proposed law that to evict a tenant the landlord must prove by documentation that the tenant has breached the lease agreement at least three times during the preceding twelve months, regardless of the nature of the breach. That is an incredibly high burden because it does not take into consideration the severity of the breach, that the tenant knows there must be three breaches, that the likelihood of three breaches by the same tenant over twelve month period is low, that the landlord has to be made aware of the breach, and that the tenant is likely to try to evade detection or acknowledgment of the breach to the landlord.

Generally, the tenants I had in LA, now have and expect to have in the future tend to be intelligent and educated people. But, that does not mean they are benign; indeed, I think smart tenants are more likely to game the system. The

landlord needs the option of simply not renewing their lease.

For example, in LA one of my tenants started running an Airbnb out of his unit! The other tenant started using a garage to which she had no rental rights as a business studio without first asking me, then proceeded to get paint on the new concrete floor. She also took on a subtenant without permission and by the time I became aware of that my legal rights with respect to that subtenant became problematic under the LA landlord tenant law.

In the Tucson rental house I had numerous problems with various tenants over the years, for example, abusing the property, loud partying and doing illegal drugs. Fortunately, Tucson does not have rent control so I was able to get rid of these people at the end of their leases without having to take legal action.

Sometimes, a tenant may simply habitually disturb other tenants and the only way to remedy that situation is not to renew the lease of the offending tenant; indeed, that may be the best remedy in many situations. For example, even when there is a breach of the lease it may be better for both parties to do a no-cause eviction rather than for the tenant to have the stigma of a cause eviction on their record and for both parties to engage in expensive litigation.

Bear in mind that landlords want their tenants to stay because (a) even when a tenant chooses to leave there is expense associated with the turnover, and (b) evicting a tenant is expensive, particularly if the tenant disputes the eviction. The idea that tenants who pay their rent and comply with the terms of their lease need the protection of a prohibition of no-cause evictions lacks merit, and there is no apparent rational basis for the prohibition against a non-cause eviction.

To the extent the proposed restriction on no-cause evictions after twelve months occupancy is to prevent a landlord from evicting a tenant so as to raise the rent more than the 7% plus the cost of living index, SB 608 could simply be amended, for example, to prohibit the landlord from doing so during the following year plus that amount of time that passed before a new tenant began renting the same unit. And, if a tenant has occupied the property for more than a year an extended notice period for termination could be provided.

The Tone and Methodology of SB 608

Generally, SB 608 is disturbing in its tone and methodology in that it seems grounded on the assumption that most rental property owners are bad actors who will only respond to punitive measures. Yet, at least I have not heard of any evidence that the Housing committee has compiled to justify that conclusion, nor am I aware of any investigation of

the behavior of tenants. It seems to me that this proposed legislation should be grounded on evidence rather than supposition.

Conclusion

Last week, on January 28, several persons with apparent relevant experience and expertise testified before this Senate Committee that there is a lack of affordable housing in Oregon and the consensus seemed to be that there is an emergency in that respect. The only part of SB 608 that seems intended to respond to that emergency is that part which relieves real estate developers from the rent control provisions of the rest of the Bill. Making owners of existing properties subject to rent control does not appear to promote development of more affordable housing. But, imposing a new burden on owners of rental property while excluding new developers from that burden simply maintains the status quo for developers, which fails to respond to the emergency.

In my opinion what SB 608 will do if passed into law is impair the contracts that landlords have with their tenants, encourage breaches of rental agreements by tenants, promote discord and litigation between tenants and landlords, and lead to the effective conversion of mere tenant leases to life estates in what was exclusively the landlord's property. In my opinion

none of these results promotes the construction of more affordable rental housing.

Instead, what the State should do to promote more affordable housing in response to the emergency is provide positive incentives, like tax breaks to developers to build new rental housing, without creating the unrelated burden of eliminating no cause evictions after 12 months occupancy. Increased rental supply should also have the effect of driving rents down. Similarly, the State could provide financial incentives to existing property owners to improve their properties, perhaps even in exchange for rent limitations.

In my view it is almost always better to use the carrot rather than the stick, especially when there no evidence that the stick works.

Respectfully submitted,

William A. Birdwell

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

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Abstract

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% and lowers displacement from San Francisco. Landlords treated by rent control reduce rental housing supplies by 15% 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.

*We are grateful for comments from Ed Glaeser, Christopher Palmer, Paul Scott, and seminar and conference participants.

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1 Introduction

Steadily rising housing rents in many of the US'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 over-consumption 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 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 micro data 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

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

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 towards 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 5 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 provides address-level migration decisions 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% 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. 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

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 the Data Section for more details.

³In practice, landlords 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.

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% higher income, relative to control group buildings in the same zipcode.

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 provides property-level information on renovations, condo conversions, 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 2 discusses the history of rent control in San Francisco. Section 3 discusses the data used for the analysis. Section 4 presents our empirical results. Section 5 concludes.

2 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% and covered all rental units built before June 13th, 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% 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.

3 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

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.

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

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 details 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 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% (0.74/0.9) renters and 18% owner occupants prior to 1994.

3.1 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

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.

on census population are shown in Figures 1 and 2.⁷ Figure 1 shows that for each additional person recorded in the 1990 Census, Infutor contains an additional 0.44 people, suggesting we have a 44% 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% of the census tract variation in population measured from Infutor. Our data is even better in the year 2000. Figure 2 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% 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-squareds are also the highest in this age range at 65% – 76%. 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-squareds range from 0.61-0.85. It is clear the data disproportionately undersamples 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. 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 R-squareds range from 67% to 91% and we often cannot reject a slope of one.⁸ This

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 that lived in San Francisco at some point in their life.

⁸Since year built comes from the Census long form, these data are based only on a 20% sample of the

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. Figures 3 and 4 plot 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 one 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-squareds over 90% in both decades. This further highlights the quality of the Infutor data.

3.2 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 31st, 1993. In the first step, we use “NamePrism”, a 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 6 ethnic categories defined by the U.S. 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 data set 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

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.

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.

basis for its ethnicity classifier.¹⁰ In this step, each tenant is assigned a probability, ranging from 0% to 100%, of belonging to each of the 6 ethnic groups, and the 6 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 6 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 6 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 6 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 6 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.

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 U.S. 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

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% of our sample. For the rest of sample, we use racial and ethnic composition at the census block group (4%), census tract (0.2%), and 5-digit zipcode levels (1%), 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% of our sample.

possible, whereas CFPB uses racial distribution at the census block group level, which is a larger geographic unit, and thus less refined.

3.2.1 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 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. The table also 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 over-represents whites in San Francisco and under-represents the number of minorities. This is because we only assign a race to an individual if the probability of that race is above 80%. In practice, this means 8,009 tenants are not assigned a race, equal to 17.27% 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

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 Table 4. 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 Table 5, 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.

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

4 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 2.

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 4 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 4 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 5 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 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 zipcode of the building, which we will always condition on in our analysis. Panel A1 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 4.2 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.

4.1 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 differences-in-differences design described above, with the following exact specification:

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

Here, Y_{iszt} are outcome variables equal to one 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 one 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 zipcode-by-year fixed effects, δ_{zt} . In this way, we control for any differences in the geographic distribution of treated buildings vs. control buildings, ensuring that our identification is based off of individuals who live in the same neighborhood, as measured by zipcode. 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 6, along with 90% 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 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% between our estimated same address effects and median rents, and a correlation of 78.4% between our estimated SF effects and median rents.

In Table 6, 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% increase relative to the 1994-1999 control group mean of 54.10%. In the medium term, rent-controlled tenants are 3.54 percentage points more likely to remain at the same address, reflecting a 19.38% increase over the 2000-2004 control group mean of 18.27%. 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% increase over

¹³Annual 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 this data is based on actual listings, this is likely the most accurate measure of true *market* rate rents, among all possible data sources.

the control group mean of 11.35%. Whether these effects should widen 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%, 8.78%, and 8.42% 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.

4.1.1 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 Table 7, 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% to 63% larger across the six point estimates.

As further robustness, we redefine the neighborhood more finely, using census tracts instead of zipcodes. Table 7 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% and 28% smaller across the six point estimates. Dropping the zipcode-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 five and ten 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 A of Table 8 reports the treatment effect using this alternative control group. The effects are statistically indistinguishable from our main effects. Panel B of Table 8 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.

4.1.2 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 7 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.

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

¹⁵As 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 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.

We next examine treatment effect heterogeneity across neighborhoods, duration of tenancy, and age.¹⁶ The goal of this exercise is twofold. 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 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 “high-turnover” group of individuals who had been living at their address for less than four years and a “low-turnover” group of individuals who had been living at their address for between four and fourteen years. Finally, we cut the sample of zipcodes 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 Figures 8 and 9. 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 turnover. Older, low-turnover households have a strong, positive response to rent control. That is, they are more likely to remain at their 1993 address relative to the control group. In contrast, older, high-turnover 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

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

based on the 1990-2000 price appreciation of their 1993 zipcodes. Among older, lower turnover 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, high-turnover 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 course, if landlords predominantly use evictions, tenants are not compensated for their loss of rent protection, weakening the insurance value of rent control.

4.1.3 Effects on Neighborhood Quality

The results from the previous subsection help to rationalize some additional, final findings. In Figure 10a, 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 Figure 10b 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

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

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

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. 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.

4.2 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 similar specification to that above:

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

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 zipcode-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 Figure 11a 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% 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 Figure 11b, we find that there is a significant decline in the number of renters living at a parcel, equal to 14.5% in the late 2000s, relative to the 1990-1994 level. Figure 11c shows that the decline in renters was counterbalanced by an increase of 8.1% 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 Figure 12a, we see that there is an eventual decline of 24.6% 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% of the initial rental housing stock treated by rent control, as shown in Figure 12b.

A natural question is whether this redevelopment activity was a response of landlords to the imposition of rent control or, instead, if such activity was also taking place within the control group and thus reflected other trends. Since we have the entire parcel history for a property, we can 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 Figure 12c, 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.

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.

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% more addition/alteration permits per unit as shown in Figure 12d. Of course, conversions 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.

4.2.1 Treatment Effect Heterogeneity

We now explore the heterogeneity in these effects between high and low house price appreciation zipcodes. 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 zipcodes. Here, we investigate whether landlords of rent-controlled apartments also disproportionately converted to condo or redeveloped buildings in high appreciation areas. Table 9 reports the average treatment effects within high and low appreciation zipcodes. We find a 21% decline in the renter population and a 12% increase in the owner population within the high appreciation zipcodes, versus a 11% renter decline and 6% owner increase in low appreciation areas. Further, we find condo conversions increase by 10% in high appreciation zipcodes versus 5.8% in low appreciation areas. The conversion to owner-occupied housing may be especially lucrative in these high appreciation zipcodes as they likely have higher income residents. In contrast, we find a larger effect (9.3% vs 3.2%) 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.

4.2.2 Gentrification Effects

The previous section shows that rent control incentivized landlords to substitute away from an older rental housing stock towards new construction rentals and owner-occupied condos. Combining our estimates of rent control's effect on the number of owner occupants (8.1%) and renters living in rent control exempt housing (7.2%) suggests that 15.3% of the treated properties engaged in renovations to evade rent control. Since these types of renovations create housing that likely caters to high income tastes, rent control may have fueled the

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

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.

Table 10 shows that properties treated by rent control have tenants who came from neighborhoods with \$1292 higher per capita incomes, representing a 2.8% increase, relative to residents of control group buildings located in the same zipcode. This 2.8% increase represents the average income increase across *all* properties treated by rent control. Since only 15.3% of these properties upgraded their housing stock, we would expect these high income residents to only be drawn into this 15.3%. Indeed, the other 85% 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 the non-renovated housing stock have incomes similar to that of the control group. Under this assumption, our estimate of a 2.8% increase in residents' incomes suggests that the renovated buildings attracted residents with *at least* 18% ($2.8/0.153$) higher incomes than residents of control group buildings in the same zipcode. In this way, rent control appears to have brought higher income residents into San Francisco, fueling gentrification.

4.3 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 towards 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.

5 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 micro data 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% 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%. Further, we find that there was a 25% 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 towards 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.

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Table 1: Sample Characteristics of Multi-Family Properties (2 – 4 Units) and their Tenants

A. Tenants Living in Multi-Family Residence (2 – 4 Units)						
	1990–1993			1994–2016		
	Treat	Control	Difference	Treat	Control	Difference
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 SF	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)
No. Persons	44502	1861	46363	44502	1861	46363
B. Multi-Family Properties (2 – 4 Units)						
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						
Population/Avg Pop 90-94	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 Pop 90-94	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 Pop 90-94	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 Pop 90-94	0 (0)	0 (0)	0 (0)	0.129 (0.740)	0.060 (0.541)	0.069 (0.005)
Owners/Avg Pop 90-94	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)
No. Parcels	25925	892	26817	25925	892	26817

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.

Table 2: Representativeness of Infutor Data: Population by Age Groups and Age of Occupied Housing Stocks

A. Population by Age Group							
1990				2000			
Age Group	Slope	S.E.	R^2	Age Group	Slope	S.E.	R^2
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

B. Age of Occupied Housing							
Year Built	Slope	S.E.	R^2	Year Built	Slope	S.E.	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 R-squareds 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 R-squareds of regressing the fraction of buildings built in various time periods in each census tract in 1990 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.

Table 3: Race of Tenants in Multi-Family Residence (2 – 4 Units)

	(1)	(2)	(3)	(4)
				Percent of SF Tenants Aged 20 – 65 in Small-Multi Family Residences – 1990 Census
Predicted Race	Freq.	Percent	Avg. Racial Probability	
white	28771	75.01%	0.95	57.36%
black	537	1.40%	0.93	7.72%
hispanic	3144	8.20%	0.95	14.18%
asian	5902	15.39%	0.98	20.16%
Other	.	.	.	0.59%
Total	38354	100%	0.95	100%

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 shows the racial distribution for the 38354 tenants with a classified race/ethnicity. In addition, 8009 tenants are not assigned a race, corresponding to 17.27% of our sample of tenants. They are not assigned a predicted race because their maximum racial probability from the set of predicted racial probabilities for all ethnic categories is below 0.8, following the procedure detailed in section 3.2. Columns 1 and 2 report the number of tenants and the share of the sample by predicted race. Column 3 reports the average final racial probability by predicted racial categories. Column 4 reports 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. The data source is 1990 U.S. Census. The category “Other” refers to all other racial/ethnic categories from the Census which include non-hispanic American Indian and Alaska Native, and non-hispanic Multi-racial.

Table 4: 2010 Census Block Racial Distribution by Race of Tenants of the 1994 Rent Control Cohort

Predicted Race	Avg. Share in Census Block of 2010 Address			
	white	black	hispanic	asian
white	63.4%	4.2%	12.1%	16.4%
black	24.8%	24.0%	24.4%	22.8%
hispanic	33.7%	6.3%	31.4%	24.9%
asian	38.1%	4.1%	13.2%	40.8%

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. The columns 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.

Table 5: Prediction of 2010 Census Block Racial Distribution using Racial Classification

	(1) Share White	(2) Share Black	(3) Share Hispanic	(4) Share Asian
White	0.385 (0.010)	-0.199 (0.004)	-0.123 (0.006)	-0.064 (0.008)
Hispanic	0.089 (0.011)	-0.178 (0.004)	0.071 (0.007)	0.021 (0.009)
Asian	0.133 (0.011)	-0.199 (0.004)	-0.111 (0.007)	0.180 (0.008)
R^2	0.212	0.062	0.129	0.189
Observations	36656	36656	36656	36656

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. The dependent variable is share of white, black, hispanic or asian population in the census block that contains a tenant’s 2010 address. The independent variable is a tenant’s racial classification. black is the omitted category.

Table 6: Treatment Effect for Tenants of Multi-Family Residence (2 – 4 Units)

	(1)	(2)
	In SF	Same Address
<i>Treat</i> × <i>Period</i>		
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	1251801	1251801

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.

Table 7: Robustness Check of Treatment Effect for Tenants of Multi-Family Residence (2 – 4 Units)

A. Treatment Group Lives in Buildings Built between 1960 & 1979		
	(1)	(2)
	In SF	Same Address
Treat \times Period		
1994 – 1999	0.0326 (0.0105)	0.0289 (0.011)
2000 – 2004	0.0642 (0.0151)	0.0370 (0.0118)
Post 2005	0.0531 (0.0145)	0.0164 (0.0084)
Control Mean 1994 – 1999	0.7641	0.541
Control Mean 2000 – 2004	0.5138	0.1827
Control Mean Post 2005	0.4346	0.1135
Adjusted R^2	0.584	0.609
Observations	135594	135594
B. Census Tract Fixed Effects		
	(1)	(2)
	In SF	Same Address
Treat \times Period		
1994 – 1999	0.0175 (0.0084)	0.0157 (0.0087)
2000 – 2004	0.0426 (0.012)	0.0284 (0.0092)
Post 2005	0.0364 (0.0114)	0.0113 (0.0066)
Control Mean 1994 – 1999	0.7641	0.541
Control Mean 2000 – 2004	0.5138	0.1827
Control Mean Post 2005	0.4346	0.1135
Adjusted R^2	0.588	0.609
Observations	1243242	1243242

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 zipcode-by-year fixed effects in our baseline regressions, we use census tract by year fixed effects. 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.

Table 8: Robustness Check of Treatment Effect for Tenants of Multi-Family Residence (2–4 Units)

A. Control Group Lives in Buildings with 5–10 Units		
	(1)	(2)
	In SF	Same Address
Treat × Period		
1994 – 1999	0.0319 (0.0096)	0.0162 (0.0094)
2000 – 2004	0.0424 (0.0132)	0.0291 (0.0099)
Post 2005	0.0400 (0.0124)	0.0167 (0.0071)
Control Mean 1994 – 1999	0.7356	0.541
Control Mean 2000 – 2004	0.4935	0.178
Control Mean Post 2005	0.4092	0.1064
Adjusted R^2	0.587	0.608
Observations	1246023	1246023
B. Control Group Lives in Buildings with 2–10 Units		
	(1)	(2)
	In SF	Same Address
Treat × Period		
1994 – 1999	0.0256 (0.0063)	0.0201 (0.0064)
2000 – 2004	0.0452 (0.0089)	0.0340 (0.0067)
Post 2005	0.0387 (0.0084)	0.01575 (0.0048)
Control Mean 1994 – 1999	0.7507	0.541
Control Mean 2000 – 2004	0.5043	0.1805
Control Mean Post 2005	0.4227	0.1101
Adjusted R^2	0.587	0.608
Observations	1296270	1296270

Notes: In Panel A, 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 B, 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.

Table 9: Treatment Effect Heterogeneity for Multi-Family Parcels by House Price Appreciation

	(1) High Appreciation	(2) Low Appreciation
Population/Avg Pop 90-94	-0.092 (0.176)	-0.050 (0.108)
Renters/Avg Pop 90-94	-0.207 (0.144)	-0.112 (0.085)
Renters in Rent-Controlled Buildings/Avg Pop 90-94	-0.284 (0.148)	-0.225 (0.088)
Renters in Redeveloped Buildings/Avg Pop 90-94	0.032 (0.058)	0.093 (0.016)
Owners/Avg Pop 90-94	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)

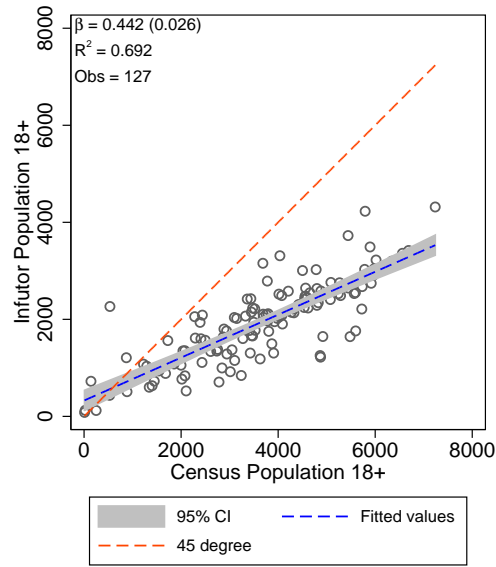
Notes: Sample consists of all multi-family residences with 2 – 4 units in San Francisco that were built during 1900 – 1990. We divide tenants into two groups by whether their 1993 zipcode experienced above or below median house price appreciation during 1990 – 2000. Columns 1 and 2 report the average 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.

Table 10: Treatment Effect on Parcel Level Average Income for Multi-Family Residence (2-4 Units)

	(1) Per Capita Income
Treat	1292 (522)
Constant	53084 (514)
Control Mean	45703
Control S.D.	22071
R^2	0.398
Observations	24271

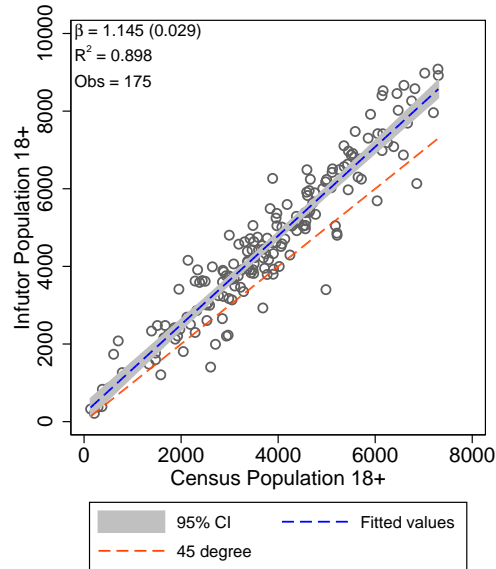
Notes: Table reports parcel level regression for the 2015 cross-section of parcels in San Francisco that we can match people living there. We further restricts to parcels that we can match someone living there before 1994. The dependent variable is the average per capita income across individuals living in each parcel. Per capita income is measured in 2010 dollars in the census block group of each individual's 2010 address. Regression includes zipcode fixed effects. Robust standard errors are reported.

Figure 1: Population Age 18 and above: 1990 Census



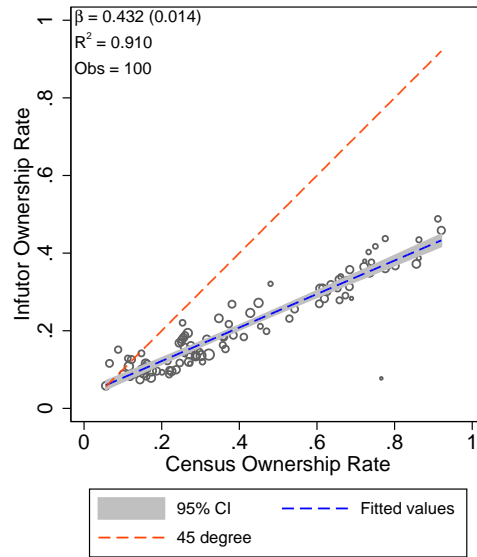
Notes: Plot shows the population of 18 and over in each census tract in 1990 from Infutor data against that from 1990 Census.

Figure 2: Population Age 18 and above: 2000 Census



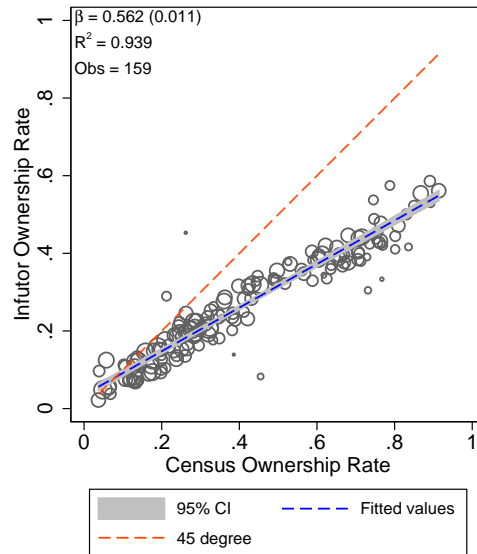
Notes: Plot shows the population of 18 and over in each census tract in 2000 from Infutor data against that from 2000 Census.

Figure 3: Ownership Rate at Individual Level: 1990 Census



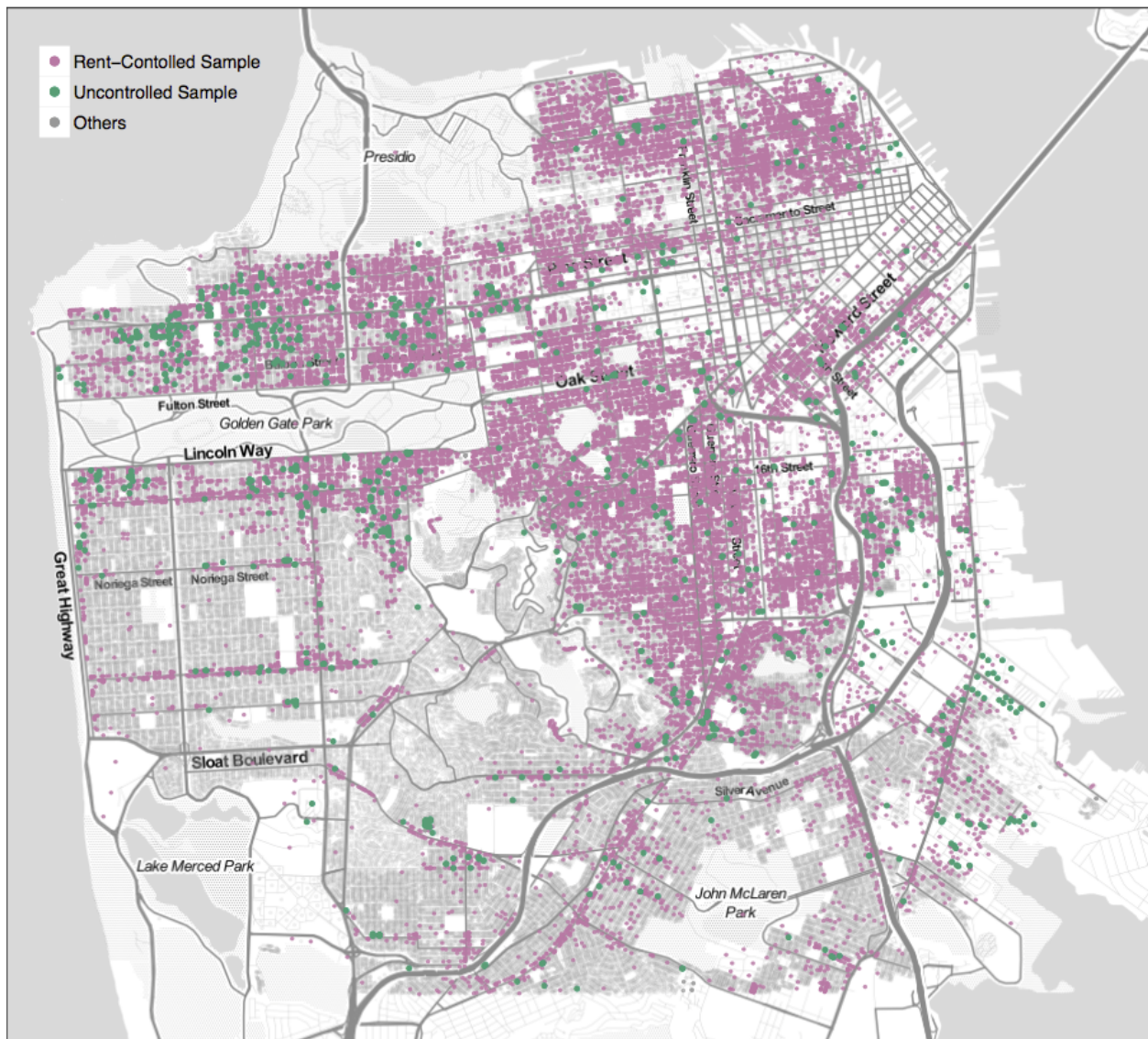
Notes: Plot shows the ownership rate at the individual level in 1990 from Infutor data against the ownership rate of occupied housing units from 1990 Census. The size of marker is proportional to the number of occupied housing units in each census tract. The fitted line is by weighted least square using the number of occupied housing units as weights.

Figure 4: Ownership Rate at Individual Level: 2000 Census



Notes: Plot shows the ownership rate at the individual level in 2000 from Infutor data against the ownership rate of occupied housing units from 2000 Census. The size of marker is proportional to the number of occupied housing units in each census tract. The fitted line is by weighted least square using the number of occupied housing units as weights.

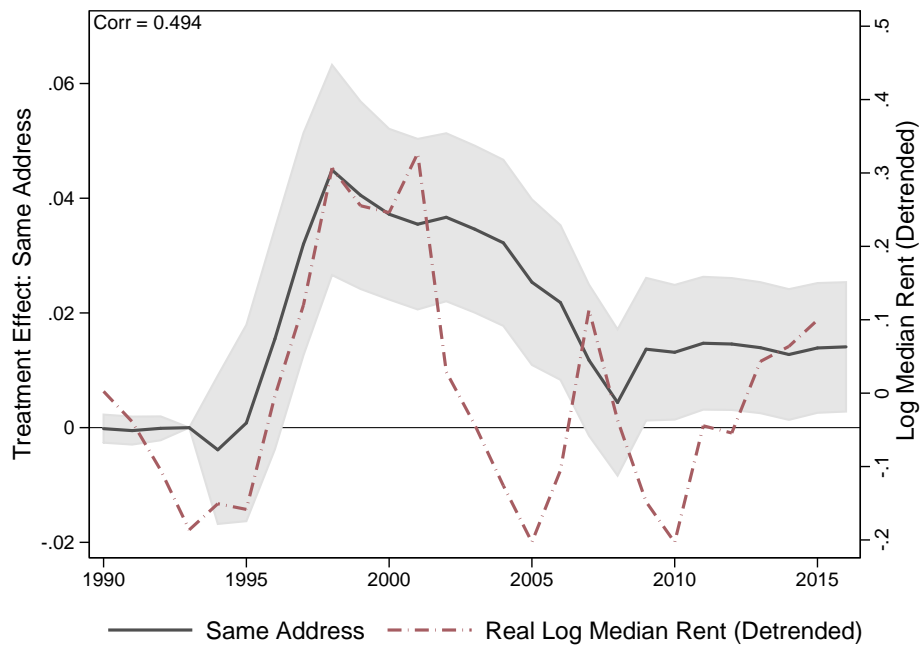
Figure 5: Geographic Distribution of Treated and Control Buildings in San Francisco



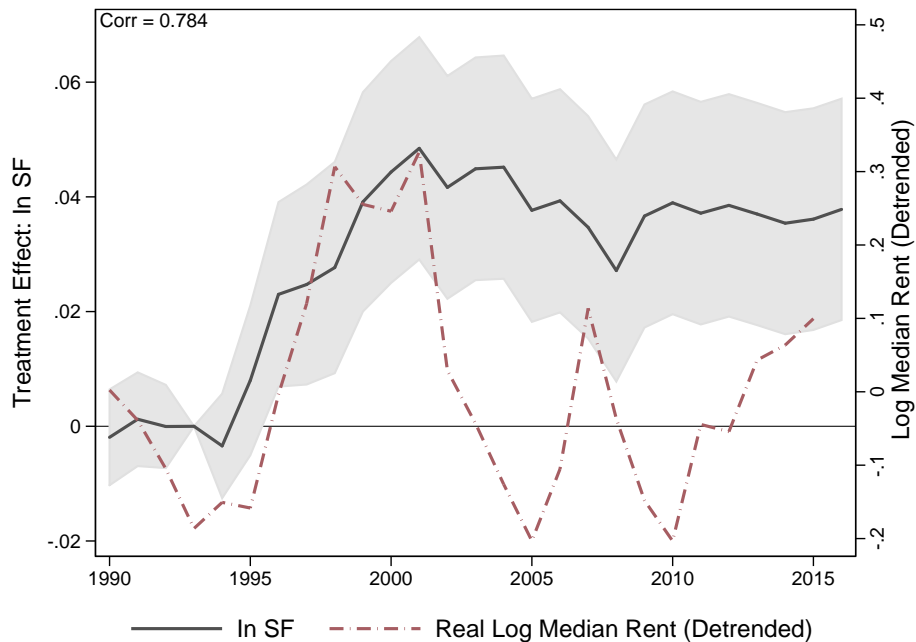
Notes: The purple dots represent parcels in the treatment group, which are parcels corresponding to multi-family residences with 2 – 4 units in San Francisco that were built between 1900-1979. The green dots represent parcels in the control group, which are parcels corresponding to multi-family residences 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 5 or more units.

Figure 6: Treatment Effect for Tenants in Multi-Family Residence (2 – 4 Units)

(a) Staying at Same Address



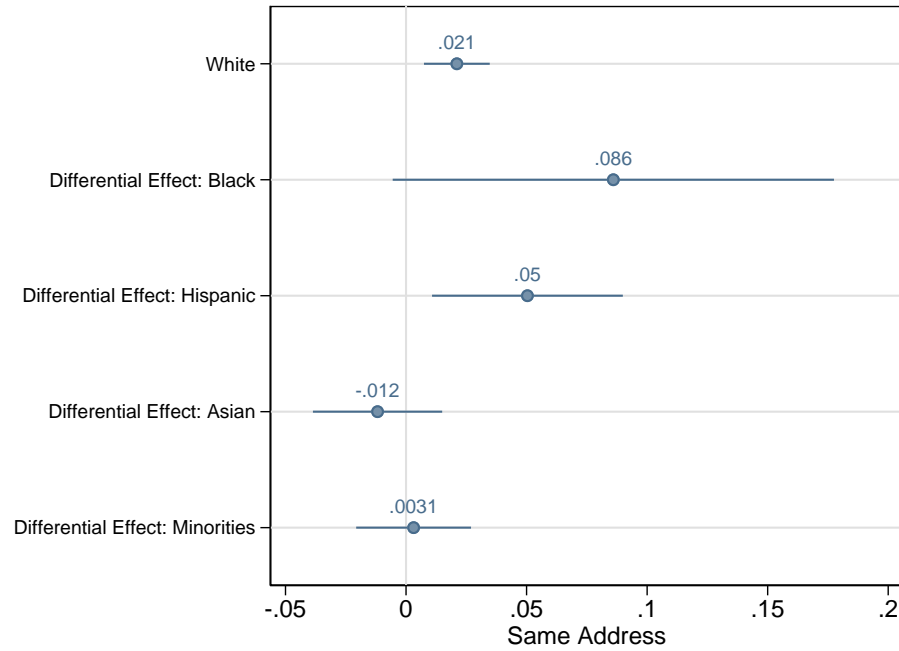
(b) Staying in San Francisco



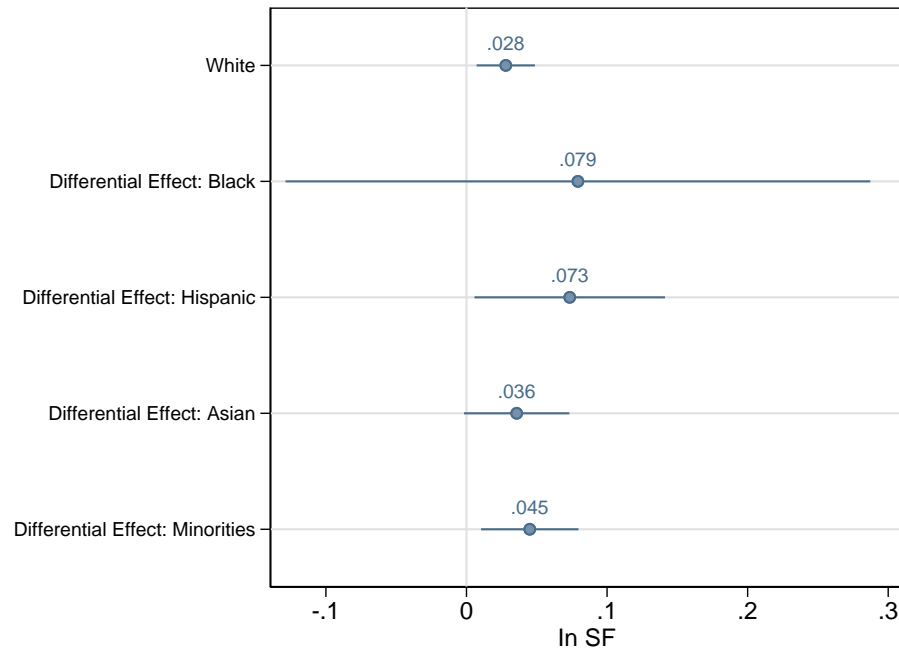
Notes: Sample consists of all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2 – 4 units that were built during 1900 – 1990. The solid line plots the treatment effects for staying at the same address in Panel (a) and staying in San Francisco in Panel (b) along with 90% 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.

Figure 7: Heterogeneity by Tenant's Race in Treatment Effect for Tenants

(a) Staying at Same Address

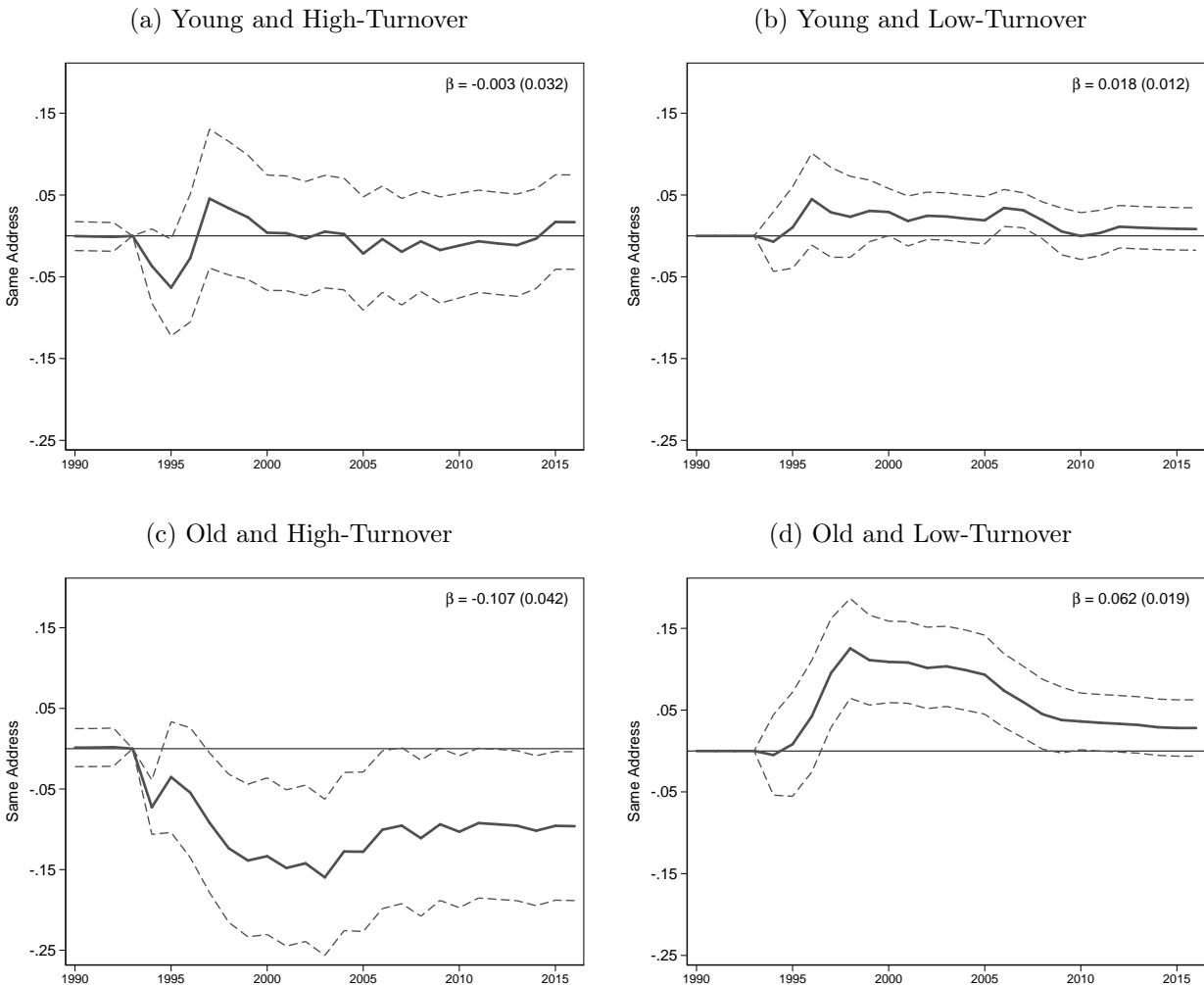


(b) Staying in San Francisco



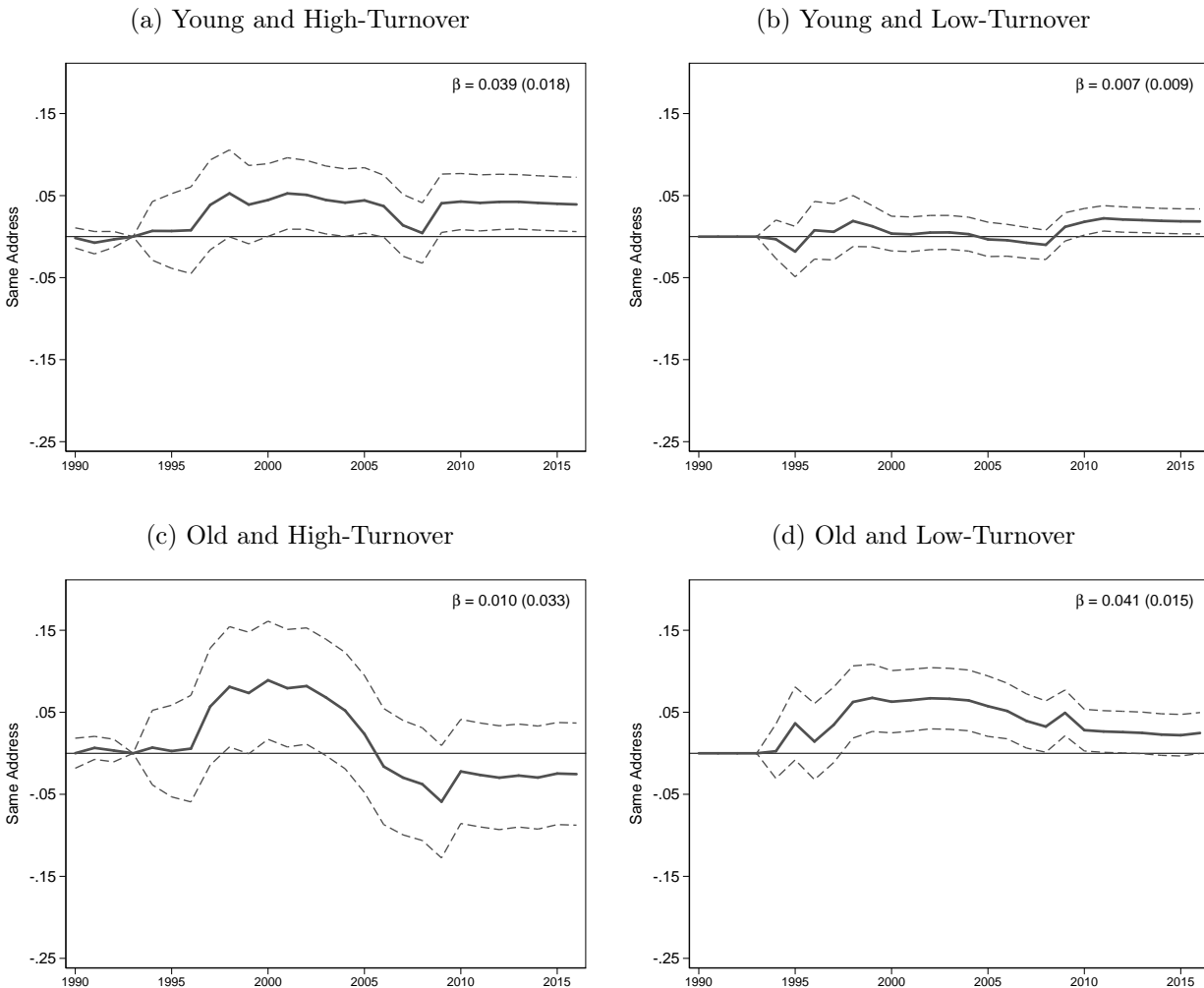
Notes: Sample consists of all tenants with a classified race/ethnicity between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2 – 4 units that were built during 1900 – 1990. For white tenants, we report the average treatment effect in the post-1994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

Figure 8: Heterogeneity by Age and Tenure in Treatment Effect of Staying at Same Address within Neighborhoods with High House Price Appreciation



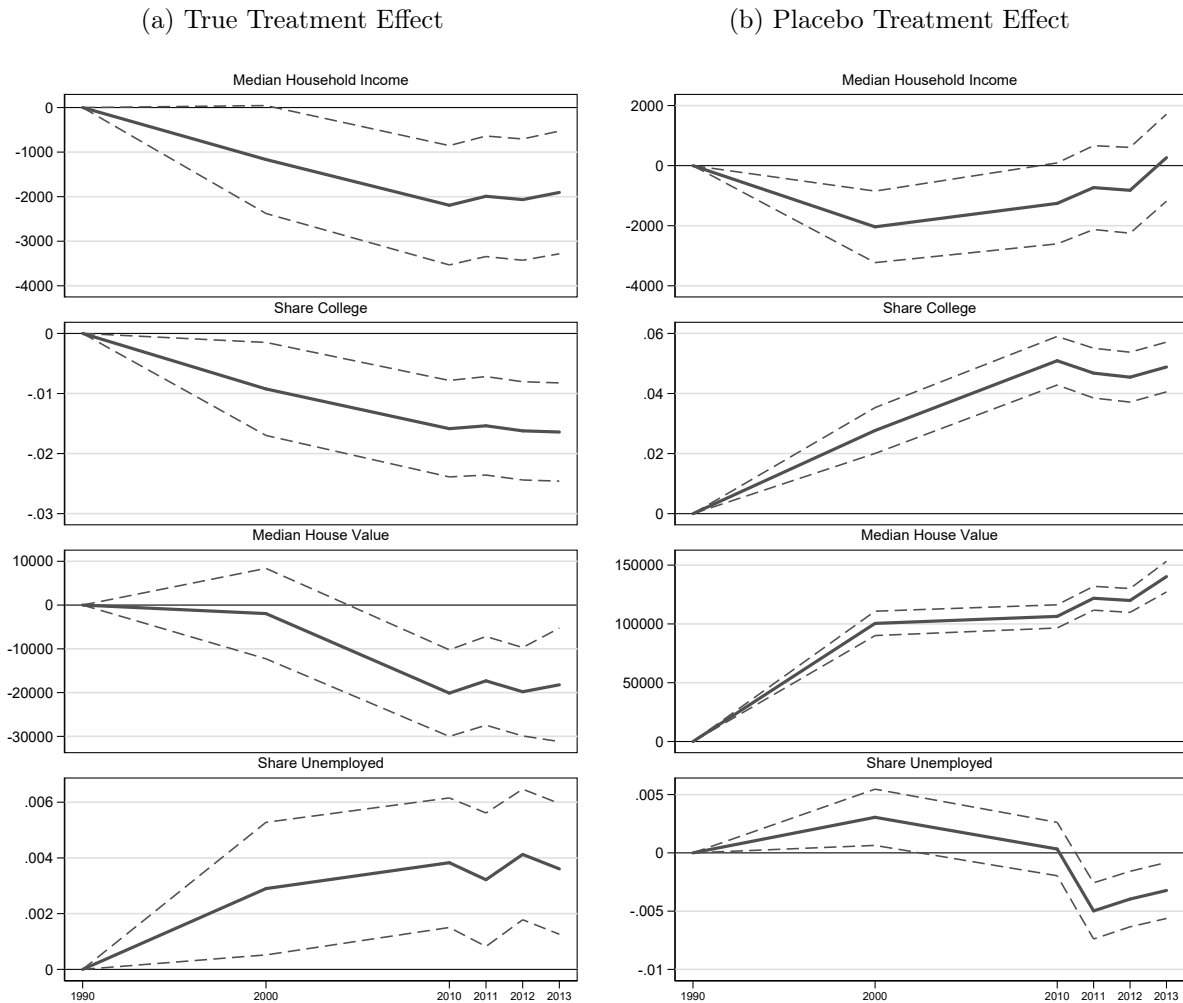
Notes: Sample consists of all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2 – 4 units that were built during 1900 – 1990. We first divide individuals into two groups by whether their 1993 zipcode experienced above or below median house price appreciation during 1990 – 2000, and restrict our sample to individuals living in zipcodes that experienced high appreciation. 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 “low-turnover” group of individuals who had been living at their 1993 address for greater than or equal to four years and a “high-turnover” group of individuals who had been living at their address for less than four years. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the person level. The average treatment effects in the post-1994 period and their standard errors are reported in the upper-right corner.

Figure 9: Heterogeneity by Age and Tenure in Treatment Effect of Staying at Same Address within Neighborhoods with Low House Price Appreciation



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 zipcode experienced above or below median house price appreciation during 1990 – 2000, and restrict our sample to individuals living in zipcodes that experienced low appreciation. 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 “low-turnover” group of individuals who had been living at their 1993 address for greater than or equal to four years and a “high-turnover” group of individuals who had been living at their address for less than four years. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the person level. The average treatment effects in the post-1994 period and their standard errors are reported in the upper-right corner.

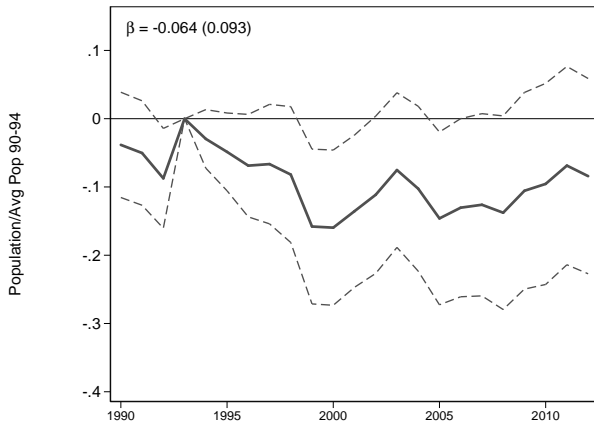
Figure 10: Treatment Effect on Neighborhood Quality for Tenants of Multi-Family Residence (2 – 4 Units)



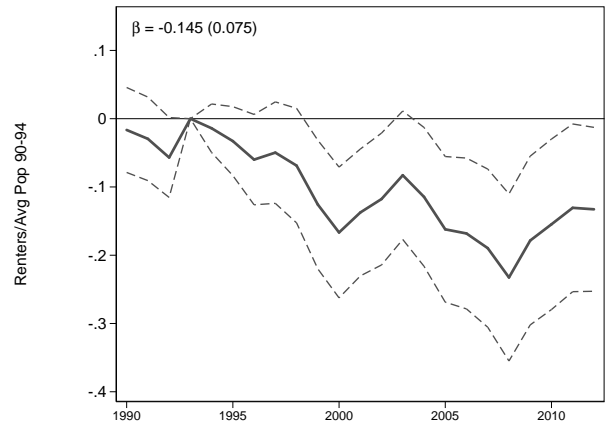
Notes: Sample consists of all tenants between 20 and 65 years old living in San Francisco as of December 31, 1993 and in multi-family residences with 2 – 4 units that were built during 1900 – 1990. Median household income, share of residents with college education and above, median house value, and share 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% CI are plotted. Standard errors are clustered at the person level.

Figure 11: Treatment Effect for Multi-Family Residence (2 – 4 Units)

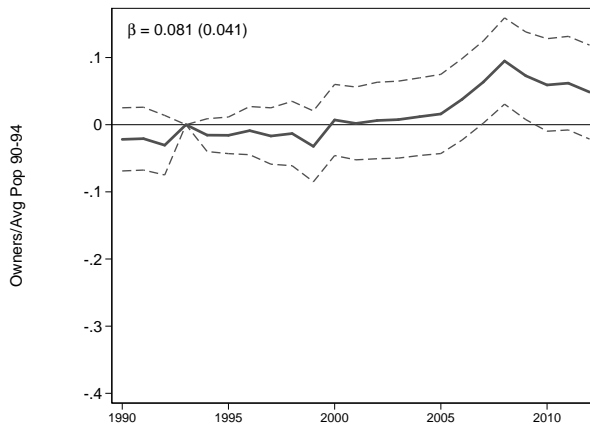
(a) Population/Average Population 1990-1994



(b) Renters/Average Population 1990-1994



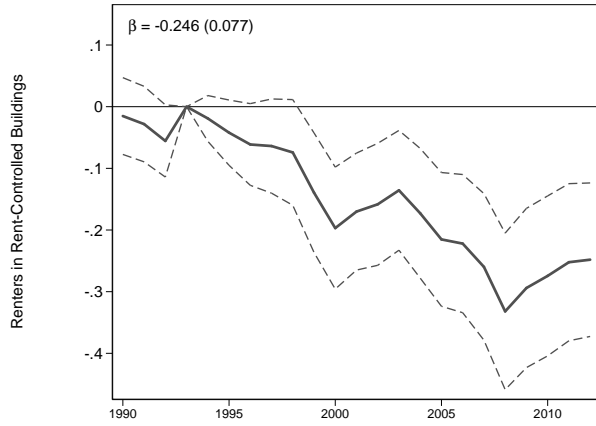
(c) Owners/Average Population 1990-1994



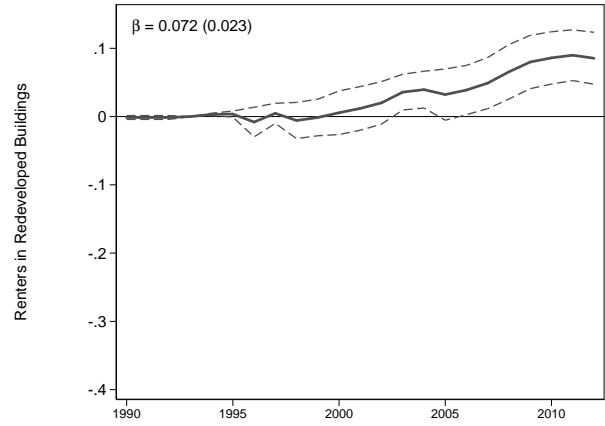
Notes: Sample consists of all multi-family residences with 2 – 4 units in San Francisco that were built during 1900 – 1990. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the parcel level. The average treatment effects in the post-2006 period and their standard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

Figure 12: Treatment Effect for Multi-Family Residence (2 – 4 Units)

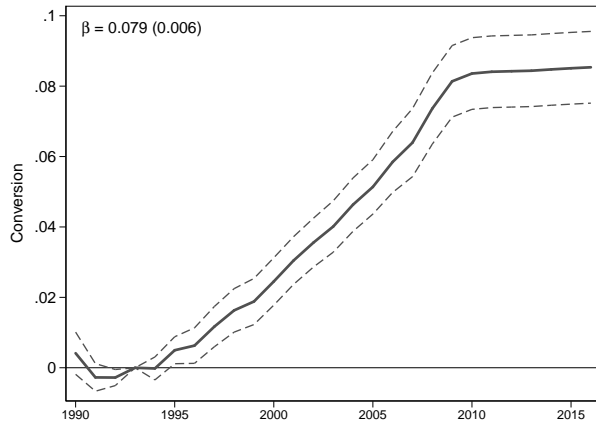
(a) Renters in Rent-Controlled Buildings/Average Population 1990-1994



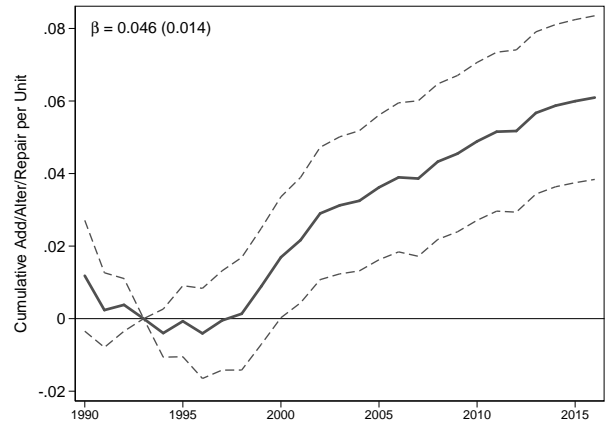
(b) Renters in Redeveloped Buildings/Average Population 1990-1994



(c) Conversion



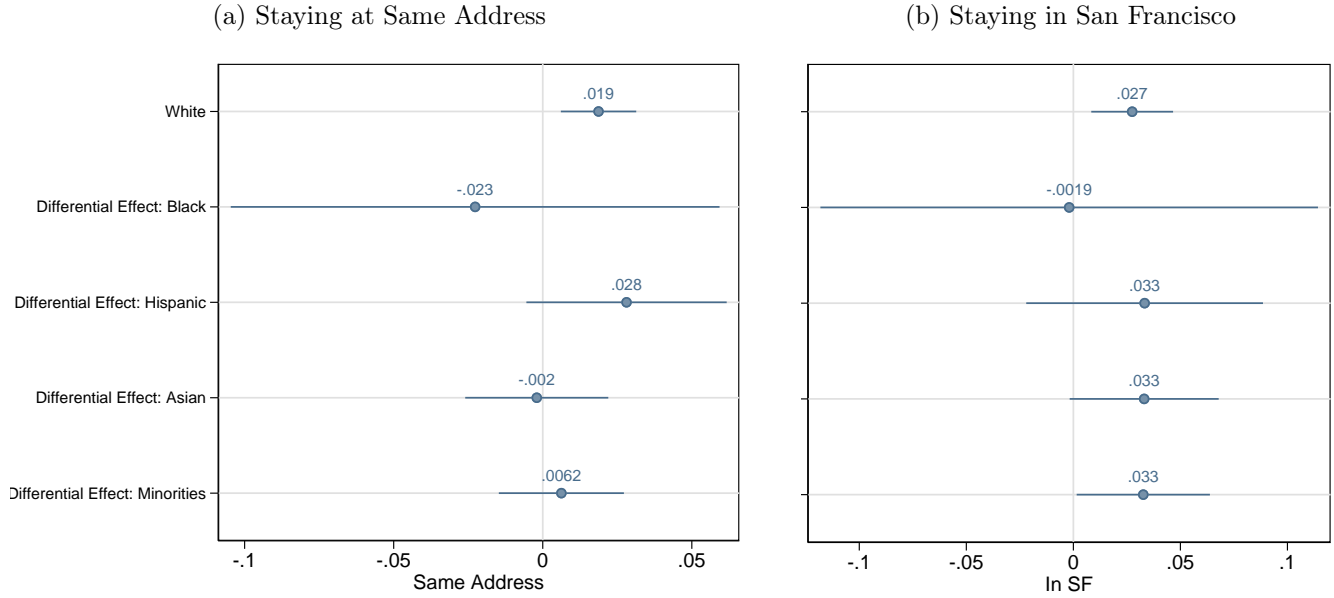
(d) Cumulative Add/Alter/Repair per Unit



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% 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.

Appendix for Online Publication

Figure A1: Heterogeneity by Tenant's Race in Tenant Treatment Effect, Full Renter Sample



Notes: Sample consists of all tenants with 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 each tenant, we assign a racial/ethnic category that has the maximum racial probability from the set of predicted racial probabilities. For white tenants, we report the average treatment effect in the post-1994 period along with 90% 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% CI. Minorities consist of all ethnic groups other than white. Standard errors are clustered at the person level.

BROOKINGS

Report

What does economic evidence tell us about the effects of rent control?

Rebecca Diamond Thursday, October 18, 2018

Steadily rising housing rents in many of the US's large, productive cities have reignited the discussion whether to expand or enact rent control provisions. Under pressure to fight rising rents, state lawmakers in Illinois, Oregon, and California are considering repealing laws that limit cities' abilities to pass or expand rent control. While rules and regulations of rent control vary from place to place, most rent control consists of caps on price increases within the duration of a tenancy, and sometimes beyond the duration of a tenancy, as well as restrictions on eviction.

New research examining how rent control affects tenants and housing markets offers insight into how rent control affects markets. While rent control appears to help current tenants in the short run, in the long run it decreases affordability, fuels gentrification, and creates negative spillovers on the surrounding neighborhood.

A substantial body of economic research has used theoretical arguments to highlight the potential negative efficiency consequences to keeping rents below market rates, going back to Friedman and Stigler (1946). They argued that a cap on rents would lead landlords to sell their rental properties to owner occupants so that landlords could still earn the market price for their real estate. Rent control can also lead to "mis-match" between tenants and rental units. Once a tenant has secured a rent-controlled apartment, he may not choose to move in the future and give up his rent control, even if his housing needs change (Suen 1980, Glaeser

and Luttmer 2003, Sims 2011, Bulow and Klemperer 2012). This mis-allocation can lead to empty-nest households living in family-sized apartments and young families crammed into small studios, clearly an inefficient allocation. Similarly, if rental rates are below market rates, renters may choose to consume excessive quantities of housing (Olsen 1972, Gyourko and Linneman 1989). Rent control can also lead to decay of the rental housing stock; landlords may not invest in maintenance because they can't recoup these investment by raising rents. (Downs 1988, Sims 2007).

Of course, rent control also offered potential benefits for tenants. For example, rent control provides insurance against rent increases, potentially limiting displacement. Affordable housing advocates argue that these insurance benefits are valuable to tenants. For instance, if long-term tenants have developed neighborhood-specific capital, such as a network of friends and family, proximity to a job, or children enrolled in local schools, then tenants face large risks from rent appreciation. In contrast, individuals who have little connection to any specific area can easily insure themselves against local rental price appreciation by moving to a cheaper location. Those invested in the local community are not able to use this type of "self-insurance" as easily, since they must give up some or all of their neighborhood specific capital. Rent control can provide these tenants with this type of insurance.

Until recently, there was little data or natural experiments with which to assess the importance of these competing arguments, and to assess how rent controls affects tenants, landlords, or the broader housing market. But newly-available housing-market data spanning periods of dramatic change in rent control laws in Cambridge, MA and in San Francisco, CA have allowed economists to examine these questions empirically. While these studies do find support for the idea that existing tenants benefit from the insurance provided by rent control, they also find the overall cost of providing that insurance is very large.

From December 1970 through 1994, all rental units in Cambridge built prior to 1969 were regulated by a rent control ordinance that placed strict caps on rent increases and tightly restricted the removal of units from the rental stock. The legislative intent of the rent control ordinance was to provide affordable rental housing, and at the eve of rent control's elimination in 1994, controlled units typically rented at 40-plus percent below the price of nearby non-controlled properties. In November 1994, the Massachusetts electorate passed a referendum to eliminate rent control by a narrow 51–49 percent margin, with nearly 60 percent of Cambridge residents voting to retain the rent control ordinance. This law change directly impacted properties previously subject to rent control, enabling landlords to begin to charge market rents.

Autor, Palmer, and Pathak (2014) (APP), studies the impact of this unexpected change and find that newly decontrolled properties' market values increased by 45 percent. In addition to these direct effects of rent decontrol, APP find removing rent control has substantial indirect effects on neighboring properties, boosting their values too. Post-decontrol price appreciation was significantly greater at properties that had a larger fraction of formerly controlled neighbors: residential properties at the 75th percentile of rent control exposure gained approximately 13 percent more in property value following decontrol than did properties at the 25th percentile of exposure. This differential appreciation of properties in rent control-intensive locations was equally pronounced among decontrolled and never-controlled units, suggesting that the effect of rent control had been to reduce the whole neighborhood's desirability.

The economic magnitude of the effect of rent control removal on the value of Cambridge's housing stock is large, boosting property values by \$2.0 billion between 1994 and 2004. Of this total effect, only \$300 million is accounted for by the direct effect of decontrol on formerly controlled units, while \$1.7 billion is due to the indirect effect. These estimates imply that more than half of the capitalized cost of rent control was borne by owners of never-controlled

properties. Rent controlled properties create substantial negative externalities on the nearby housing market, lowering the amenity value of these neighborhoods and making them less desirable places to live. In short, the policy imposed \$2.0 billion in costs to local property owners, but only \$300 million of that cost was transferred to renters in rent-controlled apartments.

Diamond, McQuade, and Qian (2018) (DMQ) examine the consequences of an expansion of rent control on renters, landlords, and the housing market that resulted from a unique 1994 local San Francisco ballot initiative. In 1979, San Francisco imposed rent control on all standing buildings with five or more apartments. Rent control in San Francisco consists of regulated rent increases, linked to the CPI, within a tenancy, but no price regulation between tenants. New construction was exempt from rent control, since legislators did not want to discourage new development. Smaller multi-family buildings were exempt from this 1979 law change since they were viewed as more “mom and pop” ventures, and did not have market power over rents.

This exemption was lifted by a 1994 San Francisco ballot initiative. Proponents of the initiative argued that small multi-family housing was now primarily owned by large businesses and should face the same rent control 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 a differential expansion in rent control in 1994 based on whether the small multi-family housing was built prior to or post 1980—a policy experiment where otherwise similar housing was treated differently by the law.

To examine rent control’s effects on tenant migration and neighborhood choices, DMQ examine panel data that provides address-level migration decisions and housing characteristics for the majority of adults living in San Francisco in the early 1990s. This allows them to define a treatment group of renters who lived in

small multi-family apartment buildings built prior to 1980 and a control group of renters living in small multi-family housing built between 1980 and 1990. Their data allows them to follow each of these groups over time up until the present, regardless of where they migrate.

Between five and ten years after the law change, the beneficiaries of rent control are 19 percent less likely to have moved to a new address, relative to the control group's migration rate. Further, impact on the likelihood of remaining in San Francisco as whole was the same, indicating a large share of the renters that rent control caused to remain at their 1994 address would have left San Francisco had they not been covered by rent control.

These 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 fact that both of these populations are likely to be less mobile. Renters who don't need to move very often are more likely to find it worthwhile to remain in their rent controlled apartment for a long time, enabling them to accrue larger rent savings. Finally, DMQ find these effects are especially large for racial minorities, likely indicating that minorities faced greater displacement pressures in San Francisco than whites.

While expansion of rent control did prevent some displacement among tenants living in San Francisco in 1994, the landlords of these properties responded to mitigate their rental losses in a number of ways. In practice, landlords have a few possible ways of removing tenants. First, landlords could move into the property themselves, known as move-in eviction. Second, the Ellis Act allows landlords to evict tenants if they intend to remove the property from the rental market, for instance, in order to convert the units to condos. Finally, landlords are legally allowed to offer their tenants monetary compensation for leaving. In practice, these transfer payments from landlords are common and can be quite large.

DMQ find that rent-controlled buildings were 8 percentage points more likely to convert to a condo than buildings in the control group. Consistent with these findings, they 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 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 that caters to higher income individuals. DMQ find that this high-end housing, developed in response to rent control, attracted residents with at least 18 percent higher income. 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.

It may seem surprising that the expansion of rent control in San Francisco led to an upgraded housing stock, catering to high-income tastes, while the removal of rent control in Cambridge also led to upgrading and value appreciation. To reconcile these effects, it is useful to think about which types of landlords would respond to a rent control expansion versus a rent control removal. In the case of rent control expansion, some landlords will choose to recoup some of their losses

by converting to condo or redeveloping their building to exempt it from rent control. However, other landlords may choose to accept the rent control regulation, and no longer perform maintenance on the building and allow it to decay. In the rent control expansion case, one would see an increase in condo conversions and upgrades, driven by the landlords that chose to respond in this way. However, when rent control is removed, the landlords who own the rent controlled buildings are the ones who didn't choose to convert to condo or redevelop in response to the initial passage of rent control. Indeed, one would expect this subset of landlords to choose to upgrade and invest in their properties once the rent control regulation is removed.

Rent control appears to help affordability in the short run for current tenants, but in the long-run decreases affordability, fuels gentrification, and creates negative externalities on the surrounding neighborhood. These results highlight that forcing landlords to provide insurance to tenants 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 a government subsidy or tax credit. 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.

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GUIDANCE | MAY 2018

The Impacts of Rent Control: A Research Review and Synthesis

By: Lisa Sturtevant, Ph.D

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About NMHC

Based in Washington, DC, the National Multifamily Housing Council (NMHC) is a national association representing the interests of the larger and most prominent apartment firms in the U.S. NMHC's members are the principal officers of firms engaged in all aspects of the apartment industry, including ownership, development, management and financing. NMHC advocates on behalf of rental housing, conducts apartment related research, encourages the exchange of strategic business information and promotes the desirability of apartment living. Nearly one-third of Americans rent their housing, and almost 15 percent live in an apartment (buildings with five or more units). For more information, contact NMHC at 202/974-2300, e-mail the Council at info@nmhc.org, or visit NMHC's Web site at www.nmhc.org

About the NMHC Research Foundation

In 2016, NMHC formed a non-profit (501(c)(3)) Research Foundation to produce research that will further support the apartment industry's business interests. The work supported by the NMHC Research Foundation raises the industry's standard of performance and encourage worldwide investment in the sector. The NMHC Research Foundation funds unique and original research on a wide range of topics, including issues related to development and redevelopment activity, affordable and workforce housing, demographics, tax policy, regulatory environment and zoning and land use, among others. For more information, visit www.nmhc.org/Research-Foundation.

About the Author

Dr. Lisa Sturtevant has been involved in research and analysis on local economic, demographic and housing market conditions for more than 15 years. As president of Lisa Sturtevant & Associates, LLC, she leads housing needs assessments and planning projects for local communities throughout the country. In addition, she conducts special studies on housing issues for local and national organizations.

Introduction

Rent control laws limit the amount of rent a landlord can charge, either by setting a rent ceiling and/or by limiting rent increases. The latter approach is sometimes referred to as a rent stabilization policy. Most rent control or rent stabilization policies also set rules for the conditions under which a landlord can evict a tenant.

Many policies allow landlords to petition for greater rent increases if they make significant improvements to the property. New York City's rent control and rent stabilization laws are well known, but rent control has also been adopted in cities in California, New Jersey, Massachusetts and Maryland and in the District of Columbia. There are initiatives underway presently to expand rent control in California (where rent control was restricted in 1995 by the Costa-Hawkins Rental Housing Act) as well as Illinois, Washington and Oregon.

Renewed interest in rent control is a reaction to growing housing affordability challenges across the country and in high-cost coastal markets, in particular. As rents continue to rise, rent control is being advocated by some as a mechanism to help mitigate the rental affordability challenge and make it easier for lower-income individuals and families to find housing they can afford in high-cost regions.

Imposing limits on rents would seem to be a logical way to keep housing costs low for people who need affordable housing. However, there are significant problems associated with rent control programs. Economists nearly universally agree that rent ceilings reduce the quantity and quality of housing and that even more moderate forms of rent stabilization have efficiency challenges and negative housing market impacts.¹

This report synthesizes the empirical research on the effects of rent control and rent stabilization on individual renters and communities, building on prior evaluations of the rent control literature.² This report does not include a review of every rent control study. Rather, the research included in this review includes only empirical studies of rent control and rent stabilization programs in the U.S. Theoretical studies were excluded, as were studies that simply provided a descriptive analysis of a rent control program. Non-U.S. studies were excluded with the presumption that housing markets and housing policy are substantially different in other countries that have implemented rent control. The vast majority of the studies included in this synthesis were published in peer-reviewed journals, though other studies (e.g., consulting reports) were included if they met the other criteria.

The earliest study included in this synthesis was published in 1972 and the latest was released in 2017. The reviewed research includes case studies of programs in a single market—New York, Boston, Los Angeles, San Francisco, Santa Monica, Washington, D.C.—as well as fewer studies that take a cross-sectional approach across markets. Most of the research employed various

¹ Jenkins, Blair. 2009. Rent Control: Do Economists Agree? *Econ Journal Watch* 6(1): 73-112.

² In addition to Blair (2009), see also Turner, Bengt and Stephen Malpezzi. 2003. A Review of Empirical Evidence on the Costs and Benefits of Rent Control. *Swedish Economic Policy Review* 10: 11-56.

multivariate statistical techniques, while a small handful of studies were able to take advantage of a policy change that created a natural experiment (see Table 1).

Table 1. Empirical Studies of Rent Control and Rent Stabilization (By Publication Date)

Authors (Date)	Geographical Areas
Olsen (1972)	NYC
Rydell et al. (1981)	Los Angeles
Fallis and Smith (1984)	Los Angeles
Mengle (1985)	Multiple
Navarro (1985)	Cambridge, MA
Linneman (1987)	NYC
Peat Marwick (1988)	NYC
Gyourko and Linneman (1989)	NYC
Ault and Saba (1990)	NYC
Gyourko and Linneman (1990)	NYC
Levine, Grisby and Heskin (1990)	Santa Monica
Turner (1990)	Washington, DC
Rappaport (1992)	NYC
Caudill (1993)	NYC
Honig and Filer (1993)	NYC
Moon and Stotsky (1993)	NYC
Ault, Jackson and Saba (1994)	NYC
Nagy (1995)	NYC
Malpezzi (1996)	Multiple
Gissy (1997)	Multiple
Grimes and Chressanthis (1997)	Multiple
Nagy (1997)	NYC
Early and Phelps (1999)	Multiple
Early (2000)	NYC
Glaeser (2002)	California, New Jersey
Glaeser and Luttmer (2003)	NYC
Krol and Svorny (2005)	New Jersey
Sims (2007)	Boston
Sims (2011)	Boston
Diamond, McQuade and Qian (2017)	San Francisco

What Is Rent Control?

Rent control often refers to laws that set caps on rents, while rent stabilization generally refers to policies that regulate how often and how fast rent levels can increase.³ Generally adopted at the municipal level, rent control laws often are coupled with rules related to tenant eviction and to exceptions to the rent levels or increases under certain circumstances. New York City has the most established rent control laws, but there are currently rent control policies in place in communities in California, New Jersey, Maryland and the District of Columbia.

Local rent control or rent stabilization policies can vary on different dimensions:

- Regulation of rent *level* or rates of rent *increases* and how these levels or rates are set;
- Types of residential buildings or units subject to rent control, based on the age or size of the building, and, consequently, what share of the locality's rental stock is subject to rent control;
- Rules on rent changes upon a tenant vacating a rent-controlled unit (i.e., vacancy allowances/vacancy decontrol policies); and
- Eviction rules that outline the circumstances under which landlords of rent-controlled buildings can turn out a tenant.

The variation in rent control and rent stabilization policies has important implications for understanding findings from the research on policy impacts and for generalizing specific findings to other existing and potential rent control policies. Market conditions also matter when measuring the effects of rent control or rent stabilization, as does the length of time the law has been in place. Furthermore, rent control is one of many different forms of regulation that can impact housing supply and price, and sometimes it can be challenging to isolate rent control's impact.

Key Findings

Even with these caveats, there are several clear and consistent findings about how rent control laws impact residents, landlords and local housing markets:

1. Rent control and rent stabilization policies do a **poor job at targeting benefits**. While some low-income families do benefit from rent control, so, too, do higher-income households. There are more efficient and effective ways to provide assistance to lower-income individuals and families who have trouble finding housing they can afford.
2. Residents of rent-controlled units move less often than do residents of uncontrolled housing units, which can mean that rent control causes **renters to continue to live in**

³ Policies are sometimes referred to as “first-generation” and “second-generation” rent control to distinguish between stricter programs with rent caps and more moderate programs that regulate rent increases and provide tenant protections. In this paper, the term “rent control” is generally used to refer to both types of programs unless otherwise specified.

units that are too small, too large or not in the right locations to best meet their housing needs.

3. **Rent-controlled buildings potentially can suffer from deterioration or lack of investment**, but the risk is minimized when there are effective local requirements and/or incentives for building maintenance and improvements.
4. Rent control and rent stabilization laws lead to a **reduction in the available supply of rental housing** in a community, particularly through the conversion to ownership of controlled buildings.
5. Rent control policies **can hold rents of controlled units at lower levels** but not under all circumstances.
6. Rent control policies **generally lead to higher rents in the uncontrolled market**, with rents sometimes substantially higher than would be expected without rent control.
7. There are **significant fiscal costs** associated with implementing a rent control program.

Impacts of Rent Control

The research on rent control and rent stabilization programs has examined the effects of those regulations in several different areas:

- **Targeting Housing Benefits:** How well do rent control policies assist the individuals and families most in need of affordable housing?
- **Allocation of Existing Housing Units:** Do rent control policies lengthen tenancy duration? Do they create a mismatch between units and households?
- **Maintenance and Building Quality:** Does rent control lead to a decline in building maintenance and lower building quality?
- **Housing Availability:** Does rent control reduce the overall supply of rental housing?
- **Rent Levels:** Are rents of controlled units lower than market-rate rents? Does a shortage in housing supply caused by rent control lead to higher rents in the uncontrolled market?
- **Fiscal Impacts:** Do rent control policies lead to lower levels of property tax revenue collected by the municipality? How substantial are administrative costs associated with rent control laws?
- **Homelessness:** Does rent control increase homelessness as a result of reduced housing supply?

The following review assesses the research evidence on each of these issues.

Targeting Housing Benefits

Hypothesis: Limiting rent levels or rent increases under a rent control law allows lower-income individuals and families to gain access to housing they can afford in high-cost housing markets. Depending on how it is implemented, a rent control strategy can create and preserve mixed-income neighborhoods and can help promote racial and economic integration.

Alternative: Because rent control creates a limited pool of below-market units, the law creates a system where landlords are incentivized to exercise greater control over tenant selection. Landlords of rent-controlled buildings could make units more readily available to households with particular characteristics (e.g., higher-income households, households without children) or prospective tenants who can pay a fee to apply for rent-controlled units. As a result, rent control may not meet the needs of individuals and families most in need of affordable housing.

Overview of Findings: Rent control and rent stabilization policies do a poor job at targeting benefits. While some low-income families do benefit from rent control, those most in need of housing assistance are not disproportionately the beneficiaries of rent control. Furthermore, rent control generally does not lead to more economically and/or racially integrated neighborhoods.

Implications: Rent control/rent stabilization is not an efficient mechanism for helping lower-income households access affordable housing. There are more effective ways to provide assistance to lower-income individuals and families who have trouble finding housing they can afford. For example, researchers point to increasing the number of federal housing vouchers and expanding the Low-Income Housing Tax Credit (LIHTC) program as more promising ways to create more affordable housing options.

Research Findings on Targeting Housing Benefits

Research demonstrates that New York City's rent control and rent stabilization laws are administered indiscriminately and benefits from the programs tend to be quite small and poorly targeted. Based on the research reviewed, the inefficiency in targeting benefits in New York has increased over time.

Using data from 1968⁴, Gyourko and Linneman (1989) found some poorer individuals were benefiting from New York City's rent control program; however, there was no evidence that the program successfully targeted those most in need, so benefits of rent control were also enjoyed by higher-income households. Using the same data, Olsen (1972) came to a slightly different

⁴ The New York Housing Vacancy Survey (NYHVS) was administered in 1968 and 1981 and provided detailed data on residents of rent-controlled, rent-stabilized and uncontrolled units. Therefore, a number of studies of NYC's rent control programs use 1968 or 1981 data. The NYHVS currently is administered by the U.S. Census Bureau every three years to comply with New York state and New York City rent regulation laws.

interpretation, demonstrating that renters who lived in rent-controlled apartments had significantly lower average incomes than those in uncontrolled units. However, Olsen (1972) concluded that there was significant variation in the distribution of the benefits of rent control and that New York City's program was "a poorly focused redistribution device."

Looking at data between 1965 and 1968, Ault and Saba (1990) found that residents of rent-controlled apartments tended to be more likely minority and elderly—two groups a rent control policy could want to target. However, over time, they found that the benefit of the rent control subsidy in New York City was greater for higher-income households than for lower-income or minority households. Furthermore, renters with similar needs or characteristics were very unlikely to receive similar levels of benefits under New York City's rent control law, indicating that the program did a poor job of targeting assistance.

These early studies of New York generally provide evidence from the city's rent control law, adopted in the 1940s, rather than the later-implemented rent stabilization law. Using 1981 data, Linneman (1987) also concluded that both the city's rent control and rent stabilization programs were targeted haphazardly, benefitting some low-income households, particularly seniors, but generally doing a poor job at directing housing benefits to those most in need. Early (2000) used data from 1996 and confirmed not only that rent control and rent stabilization in New York City were poorly targeted, but also that the city's laws induced landlords to change the way they recruited tenants, giving preference to older and smaller households. This observation that older households (i.e., seniors) and smaller households (i.e., households without children) were preferred by landlords of rent-controlled properties was also made by Linneman (1987), Gyourko and Linneman (1989) and Ault and Saba (1990).

While New York City's rent control history is unique in many ways, the lack of efficient targeting of the potential benefits of a rent control program is observed in other communities. In the Boston metropolitan area, Sims (2007) found that in the late 1980s and early 1990s, lower-income households were not well served by rent control programs. Specifically, he found that about a quarter (26 percent) of rent-controlled units were occupied by tenants with incomes in the bottom quartile while 30 percent of rent-controlled units were occupied by tenants in the top half of the income distribution. Navarro (1985) had come to a similar conclusion about rent control programs in the Boston area, examining data from the 1970s.

In her study of Washington, D.C.'s rent control policy, where about two-thirds of the stock was under control during the 1980s, Turner (1990) concluded that D.C.'s rent control policy did not benefit renters efficiently or equitably. In particular, the policy benefited renters who remained in their units a long time (including affluent renters) and did not provide assistance to recent or frequent movers (including poor individuals and families at risk of homelessness) who were unable to pay the above-market rents landlords could charge upon a tenant's departure.

A benefit of a rent control policy could potentially be an increase in economic and racial integration if lower-income households are able to access housing in higher-income neighborhoods. However, there is scant evidence of this benefit in the empirical research. Glaeser (2002) examined whether rent control increased residential integration in eight cities with rent control in California and in seven cities with rent control in New Jersey. In rising cost regions, such as those in California, rent control did increase lower-rent housing options; however, the occupants of rent-controlled units in California and the beneficiaries of living in higher-opportunity neighborhoods tended to be seniors rather than families with children. In New Jersey, where housing

markets were on the decline, rent control was actually associated with *increased* economic segregation in municipalities.

Using earlier data from 1979 and 1987 to evaluate Santa Monica’s rent control policy, Levine, Grigsby and Heskin (1990) found the city’s rent control program did provide benefits to low-income households in the city, finding no evidence that the city’s rent control policy provided a disproportionate benefit to middle- and higher-income households. However, there was no impact on economic or racial integration in the community as a result of rent control.

Sims (2011) examined whether rent control in Cambridge, Mass., increased economic and racial segregation and found that rent control appeared to increase the share of minority residents in the city, but it was associated with a decrease in the proportion of very low-income residents. Sims (2011) concluded that the modest impacts on racial integration in Cambridge were overshadowed by the increases in economic segregation in the community.

Allocation of Existing Housing Units

Hypothesis: Rent control acts as a price control, which limits the overall supply of housing and does not allow units to be allocated to the residents who would benefit most since price cannot be used to sort renters into different units. Renters who gain access to rent-controlled apartments stay in those units longer than they would in a market-rate unit, even if the unit is no longer appropriate for their household (e.g., too big or too small, based on changes in household composition).

Overview of Findings: Residents of rent-controlled units are less mobile than residents of uncontrolled housing units, and the benefit of living in a rent-controlled unit causes tenants to remain in their units longer than they would without rent control, leading to a mismatch in unit type or size and the need of the household.

Implications: Reduced mobility caused by rent control may limit the availability of so-called “family-sized” units (i.e., units with three or more bedrooms) if tenants of rent-controlled units remain in units after children have moved out on their own. There could also be instances of housing overcrowding if residents of rent-controlled units stay in homes that are too small as their family grows (e.g., after the birth of a child or when a relative moves in). Reduced mobility associated with rent control could have other impacts.

Tenants of rent-controlled units may be less likely to change jobs or may be more likely to endure long commutes because they do not want to move into a new unit. Rent control may induce people to put off homeownership and to remain renters longer to take advantage of below-market rents. Finally, when residents of rent-controlled buildings stay in their units as their incomes increase, rather than move into units with higher rents, the result is a reduction in the supply of affordable housing available to those with lower incomes.

Research Findings on Allocation of Existing Housing Units

Studies on rent control and rent stabilization in New York City consistently demonstrate that these policies have been associated with reduced residential mobility and a significant “mismatch” between tenants’ housing needs and the characteristics of the units. Gyourko and Linneman (1989) used 1968 data to examine the distributional consequences of rent control in New York City, demonstrating that New York City’s rent control policy led to longer tenures and lower likelihoods of homeownership among tenants in rent-controlled units. Ault, Jackson and Saba (1994) also examined 1968 data and estimated that the typical rent control tenant remained in his or her unit about 18 years longer than an otherwise identical tenant in an uncontrolled unit.

Other research in New York City attempted to differentiate the residential mobility impacts of rent control versus the more moderate rental stabilization program by using later data. Using data from 1981, Linneman (1987) compared length of tenancy among residents living in uncontrolled units with tenancy among those living in units under the city’s rent control law and units subject to the city’s later-enacted rent stabilization law.

Residents of strictly controlled rental units received significant rental subsidies relative to those of the stabilized and uncontrolled sectors, and that subsidy led to these renters remaining in their units significantly longer than they would otherwise be expected. But Linneman noted that there was no significant difference in the tenancy durations of residents of rent-stabilized units and uncontrolled units. At the time of the data collection (in 1981), New York City’s rent stabilization policy had only been fully implemented for seven years (since 1974), which may partially explain why no difference between tenants of uncontrolled and rent-stabilized units was found.

Nagy (1995) also compared residential mobility between renters living in rent-controlled units and rent-stabilized units using data from 1978 and 1987 (between four and 13 years after full adoption of rent stabilization in New York City). He found that tenants of rent-controlled units were, in fact, significantly less mobile than those living in rent-stabilized units; however, the differences were not so clear after accounting for the differences in the socioeconomic characteristics of the two types of renters. For example, renters living in rent-controlled units were more likely to be white and older compared to rent-stabilized renters, and those characteristics themselves are associated with reduced mobility.

Rappaport (1992) examined the effects rent control had on the probability of rental vacancies and occupant turnovers in New York City, which is a slightly different way of looking at residential mobility and length of tenure. She found that compared to uncontrolled units, a rent-controlled apartment was about eight percent less likely to turn over in a three-year period.

Lower levels of residential mobility and longer tenures in current units suggest that tenants of rent-controlled units would be more likely to live in housing that is not optimal either in terms of size or amenities. Using data from the 1990 Census and comparing the New York City apartment market to comparable areas that had not adopted rent control, Glaeser and Luttmer (2003) found that between 15 and 21 percent of New York City apartment renters lived in larger or smaller units than they would if they were living in a city without rent control or rent stabilization (with the range depending on whether unit size was measured by rooms or bedrooms). The authors cautioned that in New York City there also is a “housing misallocation” among both renters in uncontrolled buildings and owners when compared to other markets, suggesting that the New York City market may be generally inducing misallocation of units.

In 1994, San Francisco adopted rent control protections for small multifamily housing (four units or less) built prior to 1980. Diamond, McQuade and Qian (2017) compared outcomes for tenants and landlords of small pre-1980 buildings and post-1980 buildings to estimate the impact of rent control on residential mobility. The authors found that residents of rent-controlled buildings were between 10 and 20 percent more likely to stay in their current unit over the study period, with the effects significantly stronger among older households and among households that already had a long tenure in their current unit, two groups that have lower levels of residential mobility even absent having access to a rent-controlled unit.

Washington D.C.'s rent control law was adopted in 1985 and applied limits on rent increases to all units in buildings built before 1975. In an evaluation of rent control in D.C., Turner (1990) found that tenants of rent-controlled units moved less frequently than tenants of other units, concluding that the city's rent control program contributed to the very low rates of residential mobility observed in the city. Turner (1990) did not find evidence of reduced probabilities of homeownership associated with rent control in Washington D.C.

Krol and Svorney (2005) examined the impacts on residential mobility of rent control programs in New Jersey more implicitly by examining the links between length of commute and presence of rent control. Using data from the 1980, 1990 and 2000 censuses, the authors found a positive and statistically significant relationship between the presence of rent control in a city and commute times for workers who lived in those cities. The municipalities with the most restrictive regulations were associated with the longest average commute times. Krol and Svorney (2005) suggested that in New Jersey, a lack of household mobility was the driver of the longer commute times and concluded that the relative immobility of the population was the factor underlying the relationship they observed between rent control and commute times.

Maintenance and Building Quality

Hypothesis: Limiting rents through a rent control or rent stabilization law reduces the return on investments in rental housing and will cause landlords to choose to undermaintain their properties until the output of housing services (as measured by housing quality and amenities) declines to the level that is supported by the below-market rents.

Alternative: Even as rent control may limit landlords' desire to maintain their properties, other local ordinances that require landlords to maintain units to certain standards, to repay tenant maintenance expenditures, and/or to permit rent increases for well-maintained or upgraded units could counteract the potential negative impacts on housing quality associated with rent control.

In addition, a rental subsidy in the form of rent control could encourage tenants to contribute to maintenance and upkeep, counteracting any possible deferred maintenance on the part of the landlord.

Overview of Findings: There is no clear association documented in the empirical research between rent control and building quality, particularly if other ordinances, requirements or incentives are present to have landlords maintain buildings.

Implications: Rent control laws in communities that have comprehensive requirements related to building maintenance may mitigate any potential negative impacts on housing quality. However, without appropriate incentives or requirements, landlords of rent-controlled buildings will be unlikely to make improvements to buildings. Furthermore, tenants of rent-controlled buildings may feel obligated to take on more maintenance responsibilities and costs, which ends up reducing the benefits of the rent control subsidy.

Research Findings on Maintenance and Building Quality

Earlier studies of rent control in New York City did find a negative relationship between rent control and building quality, but researchers noted that it is difficult to isolate the rent control impacts since the law applied to older and often lower-quality buildings. Using data from 1968, Gyourko and Linneman (1990) found that New York City's rent control law had a large and significant negative impact on the quality of rental buildings, but the impact was primarily for non-high-rise buildings and buildings that were already in a state of disrepair when rent control was adopted. In the same study, Gyourko and Linneman (1990) provided modest evidence that rent control in New York City induced maintenance and upkeep on the part of individual residents of controlled units.

The relationship between rent control/stabilization and building quality in New York City is even less clear cut in later years. Moon and Stotsky (1993) offered mixed evidence that rent control led to a decline in housing quality in New York City over the 1978 to 1987 period. While there was a possibility of the quality of rent-controlled units to decline over time, Moon and Stotsky (1993) found that maintenance and improvements occurred in rent-controlled buildings when other economic conditions were favorable to induce landlords to renovate. For example, if landlords were able to offer a payout to existing tenants and/or were able to capitalize on higher rents when existing tenants moved out, they could be incentivized to maintain their buildings. This research also found that longer-term tenants of rent-controlled units in New York City were incentivized to provide maintenance and upkeep of their property even if the landlord did not.

Rent control was ended in Massachusetts in 1995, and the termination of rent control laws in cities in the Boston metropolitan area provided a type of natural experiment to examine the impacts of rent control. While Sims (2007) primarily focused on impacts of rent control on housing supply, the findings from this research also suggested that in the late 1980s and early 1990s, rent control in Boston was associated with modest deterioration in the quality of the rental housing stock without a counteracting tenant-supplied maintenance and upkeep. Similarly, while not the primary outcome analyzed in their evaluation of rent control in Los Angeles, researchers at RAND (Rydell et al. 1981) found evidence of deferred maintenance and upkeep in rent-controlled buildings.

However, after 15 years of rent control in Washington D.C., Turner (1990), found no evidence of a decline in housing quality and, in fact, documented that units exempt from rent control in D.C. had more maintenance issues than rent-controlled buildings. She concluded that other local policies helped to ensure building quality in the city.

Looking across the country at other communities with rent control, Mengle (1985) attempted to examine the relationship between housing quality and rent control across eight metropolitan areas in the mid-1970s, with a dataset that included four metropolitan areas where rent control was present and four where rent control was not present. While Mengle (1985) found evidence of reduced residential building quality in the metropolitan areas with rent control, it was noted that rent control laws varied considerably across municipalities in the study, but the model did not explicitly attempt to account for those policy differences or attempt to ascertain other regulations or ordinances that could incentivize building maintenance.

Housing Availability

Hypothesis: Regulations that limit rents could reduce the availability of rental housing in both the short- and long-term. Rent control could induce landlords of properties covered by the law to convert their buildings to condominium ownership, thereby taking rental units off the market. The supply of affordable rental housing could also be diminished if landlords redevelop their properties such that the new units are not subject to rent-controlled laws. Longer-term, while rent control regulations almost always exclude new construction, if rent control puts downward pressure on market rents or if developers fear that in the future new units will be subject to controls, they may not build new housing.

Overview of Findings: Rent control and rent stabilization laws generally led to a reduction in the available supply of rental housing because landlords were induced to convert their properties into condominiums or to redevelop into housing not subject to rent control regulations. The impact of rent control on new construction is less clear cut in the empirical research.

Implications: Rent control incentivizes landlords to convert their rental properties into condominiums, particularly when there is strong demand for and a lack of homeownership opportunities in a community. New housing construction may be negatively impacted if developers are uncertain about future applications of rent control and rent stabilization policies.

Research Findings on Housing Availability

While there is a substantial body of theoretical work on the relationship between rent controls and housing supply, there is surprisingly little empirical research on the impacts.

In a study of San Francisco over the 1995 to 2012 period, Diamond, McQuade and Qian (2017) found that, overall, landlords with properties covered by rent control were more likely than other property owners to convert their units to condominiums or to redevelop their buildings, which reduced the supply of available rental housing in the city. Diamond, McQuade and Qian (2017) estimated that the reduced supply of rental housing—in just this one segment of the multifamily housing stock; that is, properties with four or fewer units—led to a seven percent increase in city-wide rents.

Sims (2007) came to a similar conclusion in Boston. While he found no significant relationship between the presence of rent control and the level of new housing construction in the Boston area, rent control was associated with a shift from units offered as rentals to those offered as

ownership, with landlords of rent-controlled buildings converting their units to condominium. In the Boston region, therefore, it was estimated that rent control kept thousands of rental units off the market.

In California and New Jersey, over the 1970 to 1990 period, Glaeser (2002) found mixed results on the relationship between rent control and housing supply. In California, the supply of housing in cities with rent control increased more slowly than it did in cities without rent control; however, the difference was not statistically significant until Glaeser (2002) controlled by initial city size. In New Jersey, there was a significant impact of rent control on the housing stock, with the overall supply of housing actually declining in cities that had adopted rent control. However, that effect disappeared when city size was controlled for. Thus, these findings suggest no clear-cut relationship between rent control and housing construction but also suggest that rent control impacts housing availability differently in different kinds of markets.

Rent Levels

Hypothesis: Rent control will keep rents of units in controlled buildings lower than market rents, but if rent control results in a decline in the rental housing supply, then rents for uncontrolled units will be higher than what they would be without a rent control program in place.

Alternative: Depending on the vacancy decontrol policy, landlords of rent-controlled buildings could set initial rents higher than market rents to compensate for lower future rents allowed under rent control. Furthermore, tenants would be willing to pay higher rents initially if they knew that rent increases would be capped over time.

Overview of Findings: Units subject to rent control usually have rents that are lower than market rents, which provide a benefit to tenants of those units, often inducing them to stay longer than they would otherwise. However, when a rent control policy includes a vacancy decontrol or vacancy allowance condition, new residents of controlled units could actually pay more than market rates initially.

Rent control policies generally lead to higher rents in the uncontrolled market, with rents sometimes substantially higher than would be expected without rent control (i.e., between 10 to 25 percent higher). Over time, if rent control does not apply to new construction, there is some evidence that the impact on uncontrolled rents diminishes.

Implications: Rent control policies can hold rents of controlled units at lower levels and benefit some tenants, particularly those who do not move often. However, adopting a rent control law will lead to rent increases in the unregulated market, which reduces the well-being of residents of uncontrolled units and can actually diminish the overall welfare of all residents. Attempting to moderate a rent control policy by implementing a vacancy decontrol provision could actually increase the rents of controlled units in the short-term.

Research Findings on Rent Levels

There is general consensus that rent control policies without vacancy allowances do lead to lower rents for units in rent-controlled buildings. Gyourko and Linneman (1989) found that rent

control resulted in significantly lower rents for controlled units. In fact, they estimated that the reduced rents created a substantial subsidy for beneficiaries of rent control, possibly as high as 27 percent of renters' total annual income. In a study of Los Angeles rent control over the 1969 to 1978 period, Fallis and Smith (1997) documented that rent control was, indeed, responsible for keeping the rents of controlled units low.

Newer rent control and rent stabilization policies typically have a vacancy decontrol or vacancy allowance policy that makes it more difficult to be certain that units in buildings subject to rent control will have below-market rents. Under New York City's rent stabilization program, when a tenant vacates a unit, the landlord can reset the rent level, increasing it by up to 20 percent. Rent increases as allowed under the rent stabilization policy then apply to this new, higher rent.

This vacancy allowance feature could result in some tenants of rent-stabilized apartments paying higher-than-market rents, at least initially. Nagy (1997) found that in 1981, tenants of rent-stabilized units paid higher rents than did tenants of similar, uncontrolled apartments. However, tracking tenants over time, Nagy (1997) found that rent-stabilized tenants had below-market rents six years later (assuming they remained in the rent-stabilized unit.)

A bigger concern among policymakers has been whether adopting rent control will lead to increases of rents in the unregulated market, typically through a reduction in the supply of rental housing. Caudill (1993) analyzed the New York City rental market in 1968 and concluded that rents of uncontrolled units in the city were between 22 and 25 percent higher than they would have been had rent control not been in place.

Early (2000) used New York City data from 1996 to demonstrate that rent control drove up rents of uncontrolled units in the city and that the increase in rents in the overall rental market actually made tenants of rent-controlled units worse off than they would have been if rent control had not been in place. Lower rents in the broader uncontrolled market would have given rent control tenants more opportunities to live in a unit that was a better fit for their needs, and their overall utility would have been increased.

Fallis and Smith (1997) found that two years after the adoption of rent control in Los Angeles, uncontrolled rents increased more than three times faster than rent-controlled units, and the researchers concluded that it was the rent control policy itself—and not other characteristics of the uncontrolled units or market—that led to the significantly greater rent increases.

In Boston, Sims (2007) documented a more complicated relationship among rent control, building quality and rent levels to find that rent levels of unregulated units declined in buildings near rent-controlled buildings that were of lower quality.

There have been attempts to examine the link between rent control and market rents in cross-sectional studies. While there are unanswered issues related to the endogeneity between rising rents/rent levels and adoption of rent control, Malpezzi (1996) used data for 54 metro areas to demonstrate a positive, significant relationship between the presence of a rent control policy and median rent in the metropolitan area. Early and Phelps (1999) examined the impact of rent control on uncontrolled rents in 49 metropolitan areas over the 1984 to 1996 period.

Results from this analysis suggested a positive and statistically significant relationship between the presence of rent control and uncontrolled rents—specifically, the introduction of rent control increased median uncontrolled rent levels by more than 13 percent. However, if rent control did

not apply to new construction, the effect diminished over time, and between two and three decades after adoption of rent control, there is no significant relationship between the presence of rent control and rent levels in the uncontrolled market. (Like other cross-sectional studies, there was no attempt in either of these two studies to account for the variability in rent control programs and the fact that policies are often only in place in select municipalities within a region.)

Fiscal Impacts

Hypothesis: Because rent control limits investment return, and potentially building quality, it can also reduce the value of the property and, as a result, lower the amount of property tax revenue collected from the property owner. In addition, rent control increases public expenditures through administrative procedures to implement and oversee the rent control program.

Overview of Findings: While there is very little empirical research on the topics, rent control laws can reduce the amount of property tax revenue collected on rental properties and can be associated with fairly high administrative costs.

Implications: There can be significant costs to the municipality (and potentially to the state) of implementing rent control.

Research Findings on Fiscal Impacts

There is very little empirical research on the relationship between rent control and local property tax revenue. In his analysis of rent control in Cambridge, Mass., Navarro (1985) concluded that the city lost out on between five and 10 million dollars per year in property tax revenue as a result of lower property values induced by rent control. In New York City, a study conducted by the accounting firm Peat Marwick (1988) estimated that there was a loss in taxable assessed property values attributable to rent control at approximately \$4 billion in the late 1980s, which meant that the city lost out on an estimated \$370 million annually in property tax revenue.

Rent control regulations create administrative processes that would not be required without the law. As a result, there is a cost to the municipality (and potentially to a state) to implement, administer and enforce a rent control program. In Cambridge, Navarro (1985) estimated that the public costs to administer the city’s rent control program totaled about \$40 per unit. Linneman (1987) concluded that the administrative costs associated with New York City’s rent stabilization policy were “substantial and inefficient.”

Homelessness

Hypothesis: Rent control reduces the availability of housing and, therefore, can increase homelessness in a community.

Overview of Findings: There is no consistent relationship observed between rent control and the prevalence of homelessness.

Implications: Given the complex causes of homelessness, a rent control policy has an unknown effect on a community's homeless population, but if it reduces the availability of affordable housing in that community, the homeless population will be better served by programs that are more specifically targeted.

Research Findings on Homelessness

In a review of the relationship between homelessness and rent control in metro areas across the country, Honig and Filer (1993) found no significant association either between the presence of a rent control policy and homelessness or between rent control and incidences of overcrowding. Early and Olsen (1998) found that rent control was associated with decreased rental vacancy and increased rents, which could increase homelessness; however, they found that those potential effects were more than offset by other effects that decreased homelessness. Using 1990 data, Grimes and Chressanthis (1997) found a significant though very small impact of rent control on the level of homelessness, though as Gissy (1997) also concluded, it is possible that underlying factors related to both the likelihood of adopting rent control and level of homelessness could be an intervening factor explaining the relationship.

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