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

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September 26, 2017

**Abstract**

In this paper, we exploit quasi-experimental variation in the assignment of rent control due to a 1994 ballot initiative to study the welfare impacts of rent control on its tenant beneficiaries as well as the impact on landlords' responses and the rental market as a whole. Leveraging new micro data which tracks an individual's migration over time, we find that rent control increased the probability a renter stayed at their 1994 address by close to 20 percent. At the same time, using data on the history of individual parcels in San Francisco, we find that treated landlords reduced their supply of available rental housing by 15%, by either converting to condos/TICs, selling to owner occupied, or redeveloping buildings. This led to a city-wide rent increase of 7% and caused $5 billion of welfare losses to all renters. We develop a dynamic, structural model of neighborhood choice to evaluate the welfare impacts of our reduced form effects. We find that rent control offered large benefits to impacted tenants during the 1995-2012 period, averaging between $3100 and $5900 per person each year, with aggregate benefits totaling over $423 million annually. The substantial welfare losses due to decreased housing supply could be mitigated if insurance against large rent increases was provided as a form of government social insurance, instead of a regulated mandate on landlords.

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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. State lawmakers in Illinois, Oregon, and California are considering repealing laws that limit cities' ability to pass or expand rent control. Already extremely popular around the San Francisco Bay Area, with seven cities having imposed rent control regulations, five additional Bay Area cities placed rent control measures on the November 2016 ballot, with two passing.

A substantial body of economic research has warned about potential negative efficiency consequences to limiting rent increases below market rates, including over-consumption of housing by tenants of rent controlled apartments (Olsen (1972), Gyourko and Linneman (1989)), mis-allocation of heterogeneous housing to heterogeneous tenants (Glaeser and Luttmer (2003), Sims (2011)), negative spillovers onto neighboring housing (Sims (2007), Autor et al. (2014)) and, in particular, under-investment 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. A variety of affordable housing advocates have argued that tenants greatly value these insurance benefits, allowing them to stay in neighborhoods in which they have spent many years and feel invested in.

Due to a lack of detailed data and natural experiments, we have little well-identified empirical evidence evaluating the relative importance of these competing effects.[1] In this paper, we bring to bear new micro data, exploit quasi-experimental variation in the assignment of rent control provided by unique 1994 local San Francisco ballot initiative, and employ structural modeling to fill this gap. We find tenants covered by rent control do place a substantial value on the benefit, as revealed by their migration patterns. However, landlords of properties impacted by the law change respond over the long term by substituting to other types

[1] A notable exception to this is Sims (2007) and Autor et al. (2014) which use the repeal of rent control in Cambridge, MA to study it's spillover effects onto nearby property values and building maintenance.

of real estate, in particular by converting to condos and redeveloping buildings so as to exempt them from rent control. This substitution toward owner occupied and high-end new construction rental housing likely fueled the gentrification of San Francisco, as these types of properties cater to a higher income individuals.

The 1994 San Francisco ballot initiative created rent control protections for small multi-family housing built prior to 1980. This led to quasi-experimental rent control expansion in 1994 based on whether the multifamily 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 sources which provide the address-level migration decisions and housing characteristics for close to the universe of adults living in San Francisco in the early 1990s. This allows us to define our treatment group as renters who lived in small apartment buildings built prior to 1980 and our control group as renters living in small multifamily 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 to.

On average, we find that in the medium to long term, the beneficiaries of rent control are between 10 and 20 percent more likely to remain at their 1994 address relative to the control group. These effects are significantly stronger among older households and among households that have already spent a number of years at their current address. This is consistent with the fact both of these populations are less mobile in general, allowing the accrue greater insurance benefits.

On the other hand, for households with only a few years at their current address, the impact of rent control can be negative. Perhaps even more surprisingly, though, the impact is only negative in census tracts which had the *highest* rate of rent appreciation. This evidence suggests that landlords actively try to remove their tenants in those areas where the reward for resetting to market rents is greatest. In practice, landlords have a few possible ways of removing tenants. First, landlords could move into the property themselves, known as move-in eviction. The Ellis Act also allow 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 quite common and can be quite large. Moreover, consistent with the empirical evidence, it seems likely that landlords would be most successful at removing tenants with the least built-up neighborhood capital, i.e. those tenants who have not lived in the neighborhood for long.

To understand the reduced form impact of rent control on rental supply, we merge in historical parcel history data from the SF Assessor's Office, which allows us to observe parcel splits and condo conversions. We find that the owners of exogenously rent controlled properties substitute toward other types of real estate that are not regulated by rent control. In particular, we find that rent-controlled buildings were almost 10 percent more likely to convert to a condo or a Tenancy in Common (TIC) than buildings in the control group, representing a substantial reduction in the supply of rental housing. Consistent with these findings, we moreover find that, compared to the control group, there is a 15 percent decline in the number of renters living in these buildings and a 25 percent reduction in the number of renters living in rent-controlled units, relative to 1994 levels.

In order to evaluate the welfare impacts of these reduced form effects, we construct and estimate a dynamic discrete choice model of neighborhood choice. Motivated by our reduced form evidence, we allow for household preferences to depend on neighborhood tenure and age, and allow for monetary transfers between landlords of rent-controlled properties and their tenants. The model features fixed moving costs and moving costs variable with distance. A key contribution of the paper, relative to the existing dynamic discrete choice literature, is to show how such models can be identified in a GMM framework using quasi-experimental evidence.

We find that rent control offered large benefits to impacted tenants during the 1995-2012 period, averaging between \$3100 and \$5900 per person each year, with aggregate benefits totaling over \$423 million annually. These effects are counterbalanced by landlords reducing supply in response to the introduction of the law. We conclude that this led to a city-wide rent increase of 7% and caused \$5 billion of welfare losses to all renters. We discuss how the substantial welfare losses due to decreased housing supply could be mitigated if insurance against large rent increases was provided as a form of government social insurance, instead of a regulated mandate on landlords.

Our paper is most related to the literature on rent control. Recent work by Autor et al. (2014) and Sims (2007) leverages policy variation in rent control laws in Cambridge, Massachusetts to study the property and neighborhood effects of removing rent control regulations. Our paper studies the effects of enacting rent control laws, which could have very different effects than decontrol. De-control studies the effects of removing rent control on buildings which remain covered. Indeed, we find a large share of landlords substitute away from supply of rent controlled housing. Further, we are able to quantify how tenants use and benefit from rent control, a previously unstudied topic due to the lack of the combination of appropriate data, natural experiments and estimation methods.

There also exists an older literature on rent control primarily using cross-sectional methods, making it hard to empirically 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)).

Our estimation methods build on the dynamic discrete choice literature. Previous work using dynamic demand for housing and neighborhoods has required strong assumptions about how agents form expectations and how all neighborhood characteristics evolve over time (Bishop and Murphy (2011), **?**, **?**, Davis et al. (2017), Murphy (2017)). We relax these assumptions by building on Scott (2013). His key insight is to use realized values of agents' future expected utility as a noisy measure of agents' expectations. This allows many of the assumptions typically used in dynamic discrete choice estimation to be removed. He leverages "renewal" actions in tenants' choice sets which allows estimation to focus on specific actions in agents' choice sets which exhibit finite dynamic dependence, greatly simplifying the dynamic problem (Arcidiacono and Miller (2011), Arcidiacono and Ellickson (2011)). Our contribution is to show how Scott's method can be generalized to a set of difference-in-difference style linear and instrumental variable regressions that can be used in combination with a natural experiment to identify the model parameters.

Finally, our paper is related to a separate strand of literature on community attachment in sociology. Kasarda and Janowitz (1974) provide survey evidence that length of residence is correlated with various self-reported indicators of neighborhood attachment. We estimate households' attachments to their neighborhoods, as revealed by their migration decisions. Consistent with survey evidence, we find community attachment grows with years living in one's neighborhood, but it accumulates quite slowly over time. One additional year of residence increases one's community attachment by the equivalent of $300.

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 reduced form results. Section 5 develops and estimates a dynamic discrete choice structural model. Section 6 discusses the welfare impacts of rent control. Section 7 concludes.

## 2 A History of Rent Control 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.[2] 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.[3] These "mom and pop" landlords were cast as less profit driven

[2]Proposition 13, passed in 1978, limited annual property tax increases for owners. Tenants felt they were entitled to similar benefits by limiting their annual rent increases.

[3]Annual allowable rent increase was cut to 4% in 1944 and later to 60% of the CPI in 1992, where is remains today.

than the large scale, corporate landlords, and more similar to the tenants who were the ones being protected. These small multi-family structures made up about 30% 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, small multi-families were increasingly purchased by larger businesses who would sell a small share of the building to a live-in owner, 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. Beginning in 1995, 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.

## 3 Data

We bring together data from multiple sources to enable us to observe property characteristics, determine treatment and control groups, track migration decisions of tenants, and observe the property decisions of landlords. Our first dataset is from Infutor, which provides the entire address history of individuals who resided in San Francisco at some point between the years of 1980 and 2016.[4] 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 at which the individual lived at that particular location, the name of the individual, and some demographic information including age and gender.

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

[4] Infutor is a data aggregator of address data using many sources including sources such as phonebooks, magazine subscriptions, and credit header files.

and compare these San Francisco census tract population counts to those reported in the 1990 and 2000 population counts for adults 18 years old and old. A regression of the Infutor populations on census population are shown in Figures A.1 and A.2[5] Figure A.1 shows that for each additional person recorded in the 1990 Census, Infutor contains an additional 0.45 people, suggesting we have a 45% 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 70% of the census tract variation in population measured from Infutor. Our data is even better in the year 2000. Figure A.2 shows that we appear to have 1.2 people in Infutor for each person observed in the 2000 US census. We likely over count the number of people in each tract in Infutor since we are not conditioning on year of death in the Infutor data, leading to over counting of alive people. However, the Infutor data still track population well, as the census tract population in the 2000 Census can explain 90% of the census tract variation in population measured from Infutor. Now, Infutor matches well the level of the San Francisco population and generates an even higher $R^2$ of 89.9%.

We merge these data with public records information provided by DataQuick about the particular property located at a given address. 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. Again, 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. Figures A.3 and A.4 show the age distribution of the occupied stock by census tract. In both of the years 1990 and 2000, our R-squareds are high and we often cannot reject a slope of one. [6] This

[5] We 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.

[6] Since year built comes from the Census long form, these data are based only on a 20% sample of the true distribution of building ages in each tract, creating measurement error that is likely worse in the census

highlights the extremely high quality of the linked Infutor-DataQuick data, as the addresses are clean enough to merge the outside data source DataQuick and still manage to recover the same distribution of building ages as reported in both 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 under-classify residents as owners. Figures A.5 and A.6 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 is leads to a 0.43 (0.56) percentage point increase in the ownership rate measured in Infutor. Despite the under counting, 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.

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 also merge with 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 into renovations and changes in building use type over time based on the quantity and type of permit issued to each building over time.

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.

Historical data on annual San Francisco wide market rents are from a dataset pro-

than in the merged Infutor-DataQuick data.

duced by Eric Fisher, who collected historical apartment advertisements dating back to the 1950s. See https://experimental-geography.blogspot.ca/2016/05/employment-construction-and-cost-of-san.html for further details on the construction. Figure 1 shows the time series of SF rental rates generated by this data. We use an imputation procedure to construct annual rents at the zipcode level. Specifically, using census data we construct a relationship between zipcode house price deviations from the SF mean and zipcode rent deviations from the SF mean. We then use this relationship to construct zipcode level rent measures in the years we don't have census data.[7]

Summary statistics are provided in Table 1 and Table 2.

# 4 Reduced Form Effects

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 particular institutional history of the expansion of rent control in San Francisco. Specifically, we exploit the successful 1994 ballot initiative which removed the original 1979 exemption for small multifamily 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 multifamily housing built prior to 1980 were, all of a sudden, protected by statute against rent increases. Tenants who lived in small multifamily 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 multifamily 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 multifamily 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 multifamily

[7]Census data reports rents paid by tenants, not asking rents. We therefore use a level adjustment to ensure that the average imputed market SF rent is equal to that reported by Eric Fisher. See the appendix for the exact details of the imputation procedure.

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 the 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 multifamily 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 once again exclude buildings constructed in the early 1990s to remove any differential effects of new construction. Figure 2 shows the geographic distribution of treated buildings and control buildings in San Francisco.

## 4.1 Tenant Effects

We begin our analysis by studying the impact of rent control provisions on its tenant beneficiaries. We use a differences-in-differences design described above, with the following exact specification:

$$Y_{it} = \delta_{xt} + \alpha_i + \beta_t * T_i + \gamma_{st} + \epsilon_{it}. \tag{1}$$

Here, $Y_{it}$ are outcome variables equal to one if, in year $t$, the tenant $i$ is still living at either the same address, in the same zipcode $z$, or in San Francisco as they were at the end of 1993. The variables $\delta_{zt}$ and $\alpha_i$ denote zipcode by year fixed effects and individual tenant fixed effects, respectively. The variable $T_i$ denotes treatment, equal to one if, on December 31, 1993, the tenant is living in a multifamily building with less than or equal to four units built between the years 1900 and 1979.

We include fixed effects $\gamma_{st}$ denoting the interaction of dummies for the year the tenant moved into the apartment $s$ 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. 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.[8,9] Our coefficient of interest, quantifying the effect of rent control on future residency, is denoted by $\beta_t$.

Our estimated effects are shown in Figure 3, along with 90% confidence intervals. 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. Not only that, but they 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. We would expect that our results would be particularly strong in those years when the outside option is worse due to quickly rising rents. 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 out estimated SF effects and median rents.

In Table 3, we collapse our estimated effects into a short-term 1994-1999 effect, a medium-

[8] We have also ran our regressions with census tract by year fixed effects and our results are robust to this even finer neighborhood classification. Further, dropping the zip-year fixed effects also produces similar results.

[9] While there may be some sorting into older buildings based on personal characteristics, it seems likely that once neighborhood characteristics have been controlled for, as well as the number of years lived in the apartment as of December 31, 1993, these characteristics would not lead to differential trends in migration decisions which could contaminate our estimates. As a robustness test, 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 very similar results.

term 2000-2004 effect, and a long-term post-2005 effect. We find that in the short-run, tenants in rent-controlled housing are 2.18 percentage points more likely to remain at the same address. This estimate reflects a 4.03 percent increase relative to the 1994-1999 control group mean of 54.10 percent. In the medium term, rent-controlled tenants are 3.54 percentage points more likely to remain at the same address, reflecting a 19.38 percent increase over the 2000-2004 control group mean of 18.27 percent. Finally, in the long-term, rent-controlled tenants are 1.47 percentage points more likely to remain at the same address. This is a 12.95 percent increase over the control group mean of 11.35 percent. These effects are intuitive since we expect the utility benefits of staying in a rent controlled apartment to grow over time as the wedge between controlled and market rents widen.

Tenants who benefit from rent control are 2.00 percentage points more likely to remain in San Francisco in the short-term, 4.51 percentage points more likely in the medium-term, and 3.66 percentage points more likely in the long-term. Relative to the control group means, these estimates reflect increases of 2.62 percent, 8.78 percent, and 8.42 percent respectively. Since these numbers are of the same magnitude as the treatment effects of stay at one's exact 1994 apartment, we find that absent rent control essentially all of those incentivized to stay in their apartments would have otherwise moved out of San Francisco.

These estimated overall effects mask interesting heterogeneity. We begin by cutting the data on two dimensions. First, 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 "low turnover" group of individuals who had been living at their address for greater than or equal to four years and a "high turnover" group of individuals who had been living at their address for between four and fourteen years. Finally, we form four subsamples by taking the $2 \times 2$ cross across each of these two dimensions and re-estimate our effects for each subsample.

The results are reported in Figure 4. 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 make 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 *negative* 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 do another cut of the data, sorting individuals based on the 1990-2000 rent appreciation of their 1993 zipcode. Individuals are then sorted into two groups based on whether their zipcode experienced above or below median rent appreciation. We now estimate our effects by age, turnover, and zipcode rent appreciation. The results are in Figure 5 and Figure 6. Among older, lower turnover individuals, we find that the effects are always positive and strongest in those areas which experienced the most rent 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* rent appreciation. They are positive in the areas which experienced below median rent appreciation.[10]

This result suggests that landlords are likely actively trying to remove tenants in those areas where rent control is affording the most benefits, i.e. high rent 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

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

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. Finally, landlords are legally allowed to negotiate with tenants over a monetary transfer convincing them to leave. Such transfers are, in fact, quite prevalent in San Francisco. Moreover, it is likely that those individuals who have not lived in the neighborhood long, and thus not developed an attachment to the area, could be more readily convinced to accept such payments or are worse at fighting eviction. Indeed, since landlord can evict or pay tenants to move out, rent control need not inefficiently distort renters' decisions to remain in their rent controlled apartments. 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.

These considerations help to rationalize some additional, final findings. In Figure 7 and Figure 8, we examine the impact that rent control has on the types of neighborhoods tenants live in in a given year. We find that treated individuals, i.e. those who received rent control, ultimately live in census tracts with lower house prices, lower median incomes, and lower college shares than the control group. As Figure 9 and Figure 10 show, 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 the 1993 addresses, they would have lived in census tracts which had significantly higher college shares and higher house prices than 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.

## 4.2 Parcel and Landlord Effects

We continue our analysis by studying the impact of rent control on the structures themselves. In particular, we examine how rent control impacts the nature of the tenants who live in the buildings, as well as its impact on investments that landlords choose to make in the properties. We run a similar specification to that above:

$$Y_{kt} = \delta_{zt} + \lambda_k + \beta_t * T_k + \epsilon_{kt}, \tag{2}$$

where $k$ now denotes the individual parcel and $\lambda_k$ represent parcel fixed effects. The variable $T_k$ denotes treatment, equal to one if, on December 31, 1993, the parcel is a multifamily building with less than or equal to four units built between the years 1900 and 1979. The $\delta_{zt}$ variables once again reflect zipcode by year fixed effects. Our outcome variables $Y_{kt}$ now include the number of renters and owners living in the building, whether the building sits vacant, the number of renovation permits associated with the building, and whether the building is ever converted to a condo. 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 percentage of the average number of individuals living at that parcel between the years 1990-1994. We estimate a decline of approximately 10 percent over the long-run, although this effect is not statistically significant.

We next decompose this effect into the impact on the number of renters and the number of owners living at the treated buildings. As shown in Figure 11b, we find that there is a significant decline in the number of renters living at a parcel, approximately equal to 20 percent in the late 2000s, relative to the 1990-1994 level. Figure 11c shows that the decline in renters was counterbalanced by an increase of approximately 10 percent in the number of owners in the late 2000s. This is our first evidence suggestive of the idea that landlords redeveloped or converted their properties so as to exempt them from the new rent control regulations.

We now look more closely at the decline in renters. In Figure 12b, we see that there is an eventual decline of almost 30 percent in the number of renters living in rent-controlled apartments, relative to the 1990-1994 average.[11] 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 multifamily housing or simply converting the existing structure to condos. These redeveloped buildings replaced about 10 percent of the initial rental housing stock treated by rent control, as shown in Figure 12a.

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 multifamily 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 multifamily 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.

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, as shown in Figure 12d. Of course, conversions of multifamily housing to condos undoubtedly require

[11]Note 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.

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.

Moreover, under the San Francisco rent control regulations, capital improvements can be passed onto tenants in the form of higher rents. If the existing tenants are unable to afford the higher rents, capital improvements could be one way to get new tenants in the property and reset to market rents. It is important to note that this evidence contradicts the traditional view of rent control, that landlords will be disincentivized from investing in the property. On the contrary, we find that landlords appear to make significant investments in their properties.

Taken together, we see rent controlled increased property investment, demolition and reconstruction of new buildings, conversion to owner occupied housing and a decline of the number of renters per building. All of these responses lead to a housing stock which caters to higher income individuals. Rent control has actually fueled the gentrification of San Francisco, the exact opposite of the policy's intended goal.

# 5 A Structural Spatial Equilibrium Model

The reduced form shows that rent control can either increase or decreases tenancy durations depending on whether the tenant receives a buyout or eviction or instead remains at their residence at below market rents. To quantify how tenants trade off these decisions and to quantify the welfare impact of rent control to covered tenants, we estimate a dynamic discrete choice model of neighborhood choice.

## 5.1 Model Setup

Each year $t$, a household decides whether to remain in its current home, a choice which we denote as $\mathcal{S}$, or to move, in which case the households chooses a neighborhood $j \in \mathcal{J}$ to live in. We denote the household's choice as $x \in \{\mathcal{S}\} \cup \mathcal{J}$. The relevant state variables for the household's decision problem are the current neighborhood $j_{t-1} \in \mathcal{J}$, the number of years lived in the current neighborhood $\tau_{n,t-1} \in \mathbb{N} \cup \{0\}$, the number of years lived in the current house $\tau_{h,t-1} \in \mathbb{N} \cup \{0\}$, and whether the residence is rent-controlled $d_{t-1} \in \{0,1\}$. We also have a state variable $a_{t-1} \in \{Y, M\}$ denoting whether the household is in a young $(Y)$ or mature $(M)$ state of life. We let $\theta_{t-1} = (j_{t-1}, \tau_{n,t-1}, \tau_{h,t-1}, d_{t-1}, a_{t-1})$ denote the household's current state variable. The transition dynamics of the state variable are straightforward. We have $j_t = j(x_t)$, where:

$$
\begin{aligned}
j(x_t) &= j_{t-1} \text{ if } x_t = \mathcal{S} \\
j(x_t) &= x_t \text{ otherwise.}
\end{aligned}
$$

This equation simply says that the neighborhood remains the same if the household decides to remain in its current home. Otherwise, the new neighborhood is given by the household's choice. The implications for years in the current neighborhood and years in the current house are clearly similar, with:

$$
\begin{aligned}
\tau_n(x_t) &= \tau_{n,t-1} + 1 \text{ if } x_t \in \{\mathcal{S}, j_{t-1}\} \\
\tau_n(x_t) &= 0 \text{ otherwise.}
\end{aligned}
$$

and

$$
\begin{aligned}
\tau_h(x_t) &= \tau_{h,t-1} + 1 \text{ if } x_t = \mathcal{S} \\
\tau_n(x_t) &= 0 \text{ otherwise.}
\end{aligned}
$$

Finally, we assume that each period young households transition to mature households with exogenous probability $\xi$. This is clearly a simplification, made due to limitations of the data, but captures the idea that households experience certain life events such as marriage and having children at different ages.[12] Mature households do not transition back into young households. We denote the (probabilistic) transition function as $\theta_t = \Theta(x_t, \theta_{t-1})$. We identify the set of neighborhood locations $\mathcal{J}$ as the San Francisco zipcodes, the counties (other than San Francisco County) in the Bay Area, and an outside option denoting any location outside of the Bay Area.

We assume that a household $i$ has the following per-period utility from their housing decision:

$$\begin{aligned} u(x, \omega_t, \varepsilon_{it}, \theta_{t-1}) &= \gamma_a \exp R_t(j, d, \tau_h) + \alpha_a \tau_n + \varphi_a(x, j_{t-1}, \tau_{n,t-1}) \\ &\quad + \Lambda(x, d_{t-1}) + \omega_{jt} + \varepsilon_{ixt}, \end{aligned} \tag{3}$$

where $R_t(j, d, \tau_h)$ denotes the rent paid at the chosen location, $\varphi_a(x, j_{t-1}, \tau_{n,t-1})$ are moving costs, $\Delta_t(x, d_{t-1})$ are possible monetary transfers from landlords to tenants, $\omega_{jt}$ is an unobservable neighborhood taste shock, and $\varepsilon_{ixt}$ is an idiosyncratic logit error taste shock over the possible choices which is specific to household $i$.[13] Note that we are suppressing the dependence of $(j, \tau, d)$ on $x$. If a tenant does not live in a rent-controlled property, she pays market rents, given by $R_t(j, 0)$. Thus, there is no dependence on $\tau_h$. In contrast, the rent paid by tenants in rent-controlled properties $R_t(j, 1, \tau_h)$ is a function of the number of years lived in the property. Crucially, note that the household has utility over *exponential* rents, with coefficient $\gamma_a$. We, of course, expect this coefficient to be negative. This assumption ensures, due to the effects of Jensen's inequality, that rent control offers real insurance value to tenants. We moreover allow for utility to depend on how long a household has lived in the current neighborhood, as measured by parameter $\alpha_a$. Intuitively, households may build up neighborhood capital over time which makes that location more attractive. For instance, over time people form meaningful friendships with their neighbors and acquire valuable local

[12] In principle, we could tract the exact age as a stage variable, but this makes the state space very large.

[13] We measure rents as monthly rents divided by 3000, measured in 2010 dollars. We divide by 3000 for computational convenience.

knowledge, such as that regarding local amenities. We allow both the rent utility parameter and neighborhood capital parameter to depend on whether the household is in the young or mature stage of life.

Households incur moving costs when they switch homes. We assume that there is a fixed moving cost $\varphi_{0,a} > 0$, as well as a cost $\varphi_{d,a} > 0$ that is variable with distance. We allow the variable moving cost parameter to depend on current neighborhood capital $\tau_{n,t-1}$, with the interaction effect measured by $\varphi_{\tau,a}$. This allows for the possibility that the desirability of nearby neighborhoods changes as one accrues neighborhood capital. In particular,

$$\begin{aligned}\varphi_a\left(x, j_{t-1}\right) &= 0 \text{ if } x_t = \mathcal{S} \\ \varphi_a\left(x, j_{t-1}\right) &= \varphi_{0,a} + \varphi_{d,a} d\left(j_t, j_{t-1}\right) + \varphi_{\tau,a}\left(d\left(j_t, j_{t-1}\right) \times \tau_{n,t-1}\right) \text{ otherwise,}\end{aligned}$$

where $d\left(j_t, j_{t-1}\right)$ denotes the distance between the old and new neighborhoods. We allow the moving costs to vary with age. For example, it seems likely that households with children will find moving more costly than households without children, since changing schools could prove disruptive.

We also allow for possible monetary transfers from landlords of rent-controlled properties to tenants incentivizing them to move. These may represent true tenant buyouts or the amount of buyout that would have been required to rationalize the tenant out-migration, even if in reality the migration was due to eviction. In practice, the city of San Francisco allows for such negotiations and these payments are, in practice, quite prevalent. We do not explicitly model the bargaining game between landlords and tenants. Instead, we proceed in more reduced form fashion and parameterize the transfers as:

$$\begin{aligned}\Lambda_t\left(x, d_{t-1}, a_{t-1}\right) &= 0 \text{ if } x_t = \mathcal{S} \text{ or } d_{t-1} = 0 \\ \Lambda_t\left(x, d_{t-1}, a_{t-1}\right) &= \lambda_1\left[R_t\left(j, 0\right) - R_t\left(j, 1, \tau_h\right)\right] + \lambda_2 \tau_n + \lambda_Y 1\left[a_{t-1} = Y\right] \text{ otherwise.}\end{aligned}$$

The first equation simply says that, if the tenant does not move or does not live in rent-controlled housing, he receives no transfers. The first term in the second equation denotes the difference between market rents and rent-controlled rents. We would expect the coefficient on this term, $\lambda_1$, to be weakly positive. Intuitively, the greater the current difference between market rents and rent-controlled rents, the greater the incentive for landlords to remove tenants and thus the more landlords should be willing to pay to convince tenants to leave. We also allow for the outcome of the bargaining to depend on neighborhood tenure $\tau_n$, with the impact measured by the coefficient $\lambda_2$. This allows for more invested tenants to receive a larger payment, since their outside option, i.e. choosing to stay, is likely better than that of a short term tenant who has not built up a large stock of neighborhood capital. Finally, we allow the level difference in transfers to differ between young and mature households, measured by $\lambda_Y$.

We decompose the unobservable neighborhood amenity value $\omega_{jt}$ into

$$\omega_{jt} = \omega_j + \tilde{\omega}_{jt},$$

where $\omega_j$ is a time-invariant fixed effect and $\tilde{\omega}_{jt}$ is a per-period neighborhood specific shock. We impose no structure on the distribution of $\tilde{\omega}_{jt}$ beyond requiring that $F(\tilde{\omega}_{j,t+1}|\tilde{\omega}_{jt}, x_{it}) = F(\tilde{\omega}_{j,t+1}|\tilde{\omega}_{jt})$. That is, the decision of any individual agent has no impact on the distribution of the neighborhood amenity value next period.

Letting $\beta$ denote the common discount factor, the household's dynamic optimization problem at time $t$ is given by:

$$V\left(\theta_{i,t-1}, \omega_t, \varepsilon_{it}\right) = \max_{x^*} E\left(\sum_{s \geq t}^{\infty} \beta^{s-t} u\left(x^*, \omega_t, \varepsilon_{it}, \theta_{i,t-1}\right) | \theta_{i,t-1}, \omega_t, \varepsilon_{it}\right).$$

We next define the ex-ante value function $\overline{V}\left(\theta_{it}, \omega_t\right)$ by integrating over the idiosyncratic errors:

$$\overline{V}_t\left(\theta_{t-1}\right) = \int \cdots \int V\left(\theta_{t-1}, \omega_t, \left(\varepsilon_1, ..., \varepsilon_{J+1}\right)\right) dF\left(\varepsilon_1\right) ... dF\left(\varepsilon_{J+1}\right),$$

where $J$ is the number of neighborhoods and $\varepsilon_{J+1}$ it the logit error associated with staying in the current home. From this we can define the value function conditional on actions:

$$v_t(x, \theta_{t-1}) = \overline{u}_t(x, \theta_{t-1}) + \beta E_t\left[\overline{V}_{t+1}(\Theta(x, \theta_{t-1}))\right],$$

where $\overline{u}_t(x, \theta_{t-1}) = u(x, \omega_t, 0, \theta_{t-1})$, $\Theta(x, \theta_{t-1})$ denotes the state transition function, and $E_t[\cdot]$ denotes expectations conditional on time $t$ information.

Since the idiosyncratic taste shocks follow a logit specification, we get the standard results (see e.g. Hotz and Miller (1993)) relating conditional value functions to conditional choice probabilities $p_t(x|\theta_{t-1})$:

$$p_t(x|\theta_{t-1}) = \frac{\exp(v_t(x, \theta_{t-1}))}{\sum_{x'} \exp(v_t(x', \theta_{t-1}))}. \tag{4}$$

In what follows, we denote the log of the denominator of this expression as:

$$I_t(\theta_{t-1}) = \ln\left(\sum_{x'} \exp(v_t(x', \theta_{t-1}))\right)$$

We also have that the ex-ante value function is given by:

$$\overline{V}_t(\theta_{t-1}, \omega_t) = I_t(\theta_{t-1}) + \Gamma, \tag{5}$$

where $\Gamma$ is Euler's gamma.

## 5.2 Renewal Actions

The key challenge in identifying dynamic discrete choice models is dealing with the expected continuation values in the Bellman equation. To be able to calculate the expected continuation values, one generally must make assumptions about exactly how agents form expectations, including exactly what information is known to the agent and how they expect market-level state variables to evolve. This normally requires assuming all market state variables (e.g. rents and amenities) are observed and follow assumed transition dynamics. We build on Scott (2013) and make no assumptions about how amenities evolve. We also do not assume how agents form expectations about future market states, other than that they are on average rational. Following work by Arcidiacono and Ellickson (2011) and Arcidiacono and Miller (2011), we make extensive use of renewal actions, or action(s) which, given current states $\theta_{t-1}$ and $\theta'_{t-1}$, lead to the same state in the next period. This will allow us to difference out much of the long-term continuation values in the Bellman equation, which are impossible to estimate without strong assumptions.

### 5.2.1 Immediate Renewals

Suppose we have two households in states $\theta_{t-1}$ and $\theta'_{t-1}$. In period $t$, these two households take the actions $x$ and $x'$ respectively. Using equation (4) and differencing we find that:

$$v_t\left(x,\theta_{t-1}\right)-v_t\left(x',\theta'_{t-1}\right)=\ln\left(\frac{p_t\left(x|\theta_{t-1}\right)}{p_t\left(x'|\theta'_{t-1}\right)}\right)+I_t\left(\theta_{t-1}\right)-I_t\left(\theta'_{t-1}\right)$$

Substituting in for the conditional value functions, we get:

$$\begin{aligned} & \overline{u}_t\left(x,\theta_{t-1}\right)-\overline{u}_t\left(x',\theta'_{t-1}\right)+\beta E_t\left[\overline{V}_{t+1}\left(\Theta\left(x,\theta_{t-1}\right)\right)\right]-\beta E_t\left[\overline{V}_{t+1}\left(\Theta\left(x',\theta'_{t-1}\right)\right)\right] \\ = \; & \ln\left(\frac{p_t\left(x|\theta_{t-1}\right)}{p_t\left(x'|\theta'_{t-1}\right)}\right)+I_t\left(\theta_{t-1}\right)-I_t\left(\theta'_{t-1}\right). \end{aligned} \tag{6}$$

Now assume $x$ and $x'$ are renewal actions in the sense that $\Theta\left(x,\theta_{t-1}\right)=\Theta\left(x',\theta'_{t-1}\right)$. Note that we do not require $x=x'$, although this will often be the case. For example, if two households in non-rent controlled housing are living in the same neighborhood $j$ and have the same level of neighborhood tenure, then $x=\mathcal{S}$ and $x'=j$, i.e. one household choosing to stay in the current home and the other moving to another house in the same neighborhood, constitute renewal actions. The key implication is that the future continuation values difference out, leaving:

$$\overline{u}_t(x,\theta_{t-1}) - \overline{u}_t(x',\theta'_{t-1}) = \ln\left(\frac{p_t(x|\theta_{t-1})}{p_t(x'|\theta'_{t-1})}\right) + I_t(\theta_{t-1}) - I_t(\theta'_{t-1}). \tag{7}$$

If $\theta_{t-1} \neq \theta'_{t-1}$, we also need to remove the difference of log sums, which implicitly involves future continuation values as well.

To do so, suppose the households *move* to some neighborhood $j^* \in \mathcal{J}$, with $j^* \neq x$ and $j^* \neq x'$. This always constitutes a renewal action, so we get equation (7) again with $x$ and $x'$ replaced with $j^*$:

$$\overline{u}_t(j^*,\theta_{t-1}) - \overline{u}_t(j^*,\theta'_{t-1}) = \ln\left(\frac{p_t(j^*|\theta_{t-1})}{p_t(j^*|\theta'_{t-1})}\right) + I_t(\theta_{t-1}) - I_t(\theta'_{t-1}). \tag{8}$$

Differencing equations (7) and (8) yields:

$$\begin{aligned} \ln\left(\frac{p_t(x|\theta_{t-1})}{p_t(x'|\theta'_{t-1})}\right) - \ln\left(\frac{p_t(j^*|\theta_{t-1})}{p_t(j^*|\theta'_{t-1})}\right) \quad &= \quad \left[\overline{u}_t(x,\theta_{t-1}) - \overline{u}_t(x',\theta'_{t-1})\right] \\ &\quad - \left[\overline{u}_t(j^*,\theta_{t-1}) - \overline{u}_t(j^*,\theta'_{t-1})\right], \end{aligned} \tag{9}$$

which removes the log sums. Intuitively, equation (9) compares the difference in utility between two different actions a household in state $\theta_{t-1}$ could take versus a household in state $\theta'_{t-1}$. This "differences-in-differences" approach removes all long-term utility differences since actions are selected to create renewals.

#### 5.2.2 One Period Ahead Renewals

Now suppose that $x$ and $x'$ are not renewal actions in period $t$. Following Scott (2013), we substitute the expected difference in continuation values in equation (6) with its realization and expectational errors:

$$
\begin{aligned}
& \overline{u}_t\left(x,\theta_{t-1}\right)-\overline{u}_t\left(x',\theta'_{t-1}\right)-\ln\left(\frac{p_t\left(j|\theta_{t-1}\right)}{p_t\left(j'|\theta'_{t-1}\right)}\right)-\left[I_t\left(\theta_{t-1}\right)-I_t\left(\theta'_{t-1}\right)\right] \\
= \;& \beta\left(\overline{V}_{t+1}\left(\Theta\left(x',\theta'_{t-1}\right)\right)-\overline{V}_{t+1}\left(\Theta\left(x,\theta_{t-1}\right)\right)\right)+\xi_t^V\left(x',\theta'_{t-1}\right)-\xi_t^V\left(x,\theta_{t-1}\right)
\end{aligned}
$$

where

$$
\xi_t^V\left(x,\theta_{t-1}\right)=\beta\left(E_t\left[\overline{V}_{t+1}\left(\Theta\left(x,\theta_{t-1}\right)\right)\right]-\overline{V}_{t+1}\left(\Theta\left(x,\theta_{t-1}\right)\right)\right)
$$

is the expectational error.

We now again make use of renewals. Suppose that at time $t+1$, both households move to the same neighborhood, that is $x_{t+1}=x'_{t+1}=j^*\in\mathcal{J}$. To see the effects of this, first substitute out the realized ex-ante value functions using equations (4) and (5). We have:

$$
\begin{aligned}
& \overline{u}_t\left(x,\theta_{t-1}\right)-\overline{u}_t\left(x',\theta'_{t-1}\right)-\ln\left(\frac{p_t\left(j|\theta_{t-1}\right)}{p_t\left(j'|\theta'_{t-1}\right)}\right)-\left[I_t\left(\theta_{t-1}\right)-I_t\left(\theta'_{t-1}\right)\right] \\
= \;& \beta\left(v_{t+1}\left(j^*,\Theta\left(x',\theta'_{t-1}\right)\right)-v_{t+1}\left(j^*,\Theta\left(x,\theta_{t-1}\right)\right)\right) \\
& -\beta\ln\left(\frac{p_{t+1}\left(j^*,\Theta\left(x'|\theta'_{t-1}\right)\right)}{p_{t+1}\left(j^*,\Theta\left(x|\theta_{t-1}\right)\right)}\right)+\xi_t^V\left(x',\theta'_{t-1}\right)-\xi_t^V\left(x,\theta_{t-1}\right).
\end{aligned}
$$

Since $j^*$ is a renewal action, the time $t+2$ expected value functions difference out and this equation becomes:

$$
\begin{aligned}
& \overline{u}_t\left(x,\theta_{t-1}\right)-\overline{u}_t\left(x',\theta'_{t-1}\right)-\ln\left(\frac{p_t\left(j|\theta_{t-1}\right)}{p_t\left(j'|\theta'_{t-1}\right)}\right)-\left[I_t\left(\theta_{t-1}\right)-I_t\left(\theta'_{t-1}\right)\right] \\
= \;& \beta\left(\overline{u}_{t+1}\left(j^*,\Theta\left(x',\theta'_{t-1}\right)\right)-\overline{u}_{t+1}\left(j^*,\Theta\left(x,\theta_{t-1}\right)\right)\right) \\
& -\beta\ln\left(\frac{p_{t+1}\left(j^*,\Theta\left(x'|\theta'_{t-1}\right)\right)}{p_{t+1}\left(j^*,\Theta\left(x|\theta_{t-1}\right)\right)}\right)+\xi_t^V\left(x',\theta'_{t-1}\right)-\xi_t^V\left(x,\theta_{t-1}\right).
\end{aligned}
\tag{10}
$$

To fully remove the conditional value functions, we once again must remove the difference in log sums $I_t\left(\theta_{t-1}\right)-I_t\left(\theta'_{t-1}\right)$.

We follow the same procedure as previously, subtracting equation (8) from equation (10):

$$\begin{aligned}
& \ln\left(\frac{p_t\left(j|\theta_{t-1}\right)}{p_t\left(j'|\theta'_{t-1}\right)}\right)-\ln\left(\frac{p_t\left(j^*|\theta_{t-1}\right)}{p_t\left(j^*|\theta'_{t-1}\right)}\right)+\beta\ln\left(\frac{p_{t+1}\left(j^*,\Theta\left(x|\theta_{t-1}\right)\right)}{p_{t+1}\left(j^*,\Theta\left(x'|\theta'_{t-1}\right)\right)}\right) \qquad (11)\\
= & \left[\overline{u}_t\left(x,\theta_{t-1}\right)-\overline{u}_t\left(x',\theta'_{t-1}\right)\right]-\left[\overline{u}_t\left(j^*,\theta_{t-1}\right)-\overline{u}_t\left(j^*,\theta'_{t-1}\right)\right]\\
& +\beta\left(\overline{u}_{t+1}\left(j^*,\Theta\left(x,\theta_{t-1}\right)\right)-\overline{u}_{t+1}\left(j^*,\Theta\left(x,\theta_{t-1}\right)\right)\right)\\
& +\xi_t^V\left(x',\theta'_{t-1}\right)-\xi_t^V\left(x,\theta_{t-1}\right).
\end{aligned}$$

Equations (9) and (11) provide a linear regression framework which we can use to fully identify and estimate the parameters of the model.

## 5.3 Empirical Framework

We now discuss how to empirically operationalize the preceding considerations.

### 5.3.1 Constructing Conditional Choice Probabilities

We first need to construct empirical estimates of the conditional choice probabilities, $p_t\left(x|\theta_{t-1}\right)$. In a given year $t$, we focus on those households who were part of the 1994 treatment and control groups described in the previous section and who have not moved away from their 1994 residence. Given the latter restriction, we do not need to keep track of $\tau_h$ and we therefore suppress the dependence of $\theta_{t-1}$ on this state variable in what follows.

With a large enough dataset, we could simply compute empirical frequencies for all conditional choice probabilities. However, since there are many states, not all CCPs in our data are measured precisely. We therefore use kernel smoothing on the empirical frequencies to improve the prediction error. We smooth over distance, neighborhood tenure, and age. We use a Gaussian kernel. Distance is measured between the midpoints of zipcodes. Neighborhood tenure equals the number of years the renter has lived in that zipcode. Young renters are those under the age of 40, while mature/old renters are those 40 and older. We use $k$-fold cross validation to set the optimal bandwidths with $k$=5.

#### 5.3.2 Identifying the Parameters of the Model

We set $\beta = .85$.[14] We estimate the various parameters of the model by estimating equation (9) and (11) for appropriately chosen values of $\left(\theta_{t-1}, \theta'_{t-1}\right)$ and $(x, x')$. Intuitively, by examining the differential behavior of individuals in certain states of the world and following certain types of deviations, we can isolate the impact of the different parameters of the model. We begin by constructing a regression equation for $\gamma_M, \lambda_1$, and $\lambda_2$. These are the (mature) rent utility parameter and the parameters of the transfer function. Normally, we would be confronted with a significant endogeneity problem in estimating these parameters since market rents $R_t(j, 0)$ in neighborhood $j$ are likely correlated with the amenity value $\omega_{jt}$ unobservable to the econometrician.

We overcome this essential endogeneity problem by exploiting the quasi-experimental nature of the 1994 San Francisco rent control ballot measure. This law change quasi-randomly assigned renters *within* a given neighborhood $j$ to rent control status. As mentioned, we focus exclusively on this population for our regressions.

Now let $\theta_{t-1} = (j, \tau_n, 1, M)$ and $\theta'_{t-1} = (j, \tau_n, 0, M)$ for some $j \in \mathcal{J}$. We furthermore set $x = x' = \mathcal{S}$ and let $j^*$ be any element of $\mathcal{J}$. In words, we consider two mature households who both lived in neighborhood $j$ in 1994 and have not moved as of year $t$. The two households are of equal tenure $\tau_n$. One was assigned to rent control status in 1994 and the other was not. We examine the relative probabilities of these individuals staying in neighborhood $j$ in year $t$, using neighborhood $j^*$ as the renewal choice in the manner described in the previous

[14]This choice is consistent with the evidence provided in De Groote and Verboven (2016), who estimate a household discount factor of .87.

section. Under these assumptions, equation (11) gives the regression:

$$
\begin{aligned}
Y_{j,j^*}^t &= \gamma_M \left[\exp R_t(j,1) - \exp R_t(j,0)\right] + \\
&\quad +\lambda_1 \left[\left(\beta \ln R_{t+1}(j,0) - \ln R_t(j,0)\right) - \left(\beta \ln R_{t+1}(j,1) - \ln R_t(j,1)\right)\right] \\
&\quad +\lambda_2 \left[\beta(t+\tau_n+1) - (t+\tau_n)\right] \\
&\quad +\xi_t^V\left(x', \theta_{t-1}'\right) - \xi_t^V\left(x, \theta_{t-1}\right) + \chi_{j,j^*}^t \\
Y_{j,j^*}^t &= \ln\left(\frac{p_t(\mathcal{S}|j,1,\tau_n)}{p_t(\mathcal{S}|j,0,\tau_n)}\right) - \ln\left(\frac{p_t(j^*|j,1,\tau_n)}{p_t(j^*|j,0,\tau_n)}\right) + \beta \ln\left(\frac{p_{t+1}(j^*|j,1,\tau_n)}{p_{t+1}(j^*|j,0,\tau_n)}\right)
\end{aligned}
$$

Intuitively, this regression compares the probability of staying in the neighborhood for one more year and then moving to $j^*$ versus moving to $j^*$ this year. This difference in probabilities is then differenced between treatment and control, which differences out all the utility impacts of living in $j$ vs $j^*$ other than those which are impacted by rent control.

Note that we have included an additional error term $\chi_{j,j^*}^t$, reflecting measurement error in our constructed conditional choice probabilities.The key for identification is that the unobserved amenity value $\omega_{jt}$ differences out. We furthermore know that:

$$E_t\left[\left(R_t(j,1) - R_t(j,0)\right)\left(\xi_t^V\left(x', \theta_{t-1}'\right) - \xi_t^V\left(x, \theta_{t-1}\right)\right)\right] = 0$$

due to rational expectations. That is, the expectational error is uncorrelated with any time $t$ information. In general, however, we do *not* have:

$$E_t\left[\left(R_{t+1}(j,1) - R_{t+1}(j,0)\right)\left(\xi_t^V\left(x', \theta_{t-1}'\right) - \xi_t^V\left(x, \theta_{t-1}\right)\right)\right] = 0.$$

The time $t+1$ rent difference may be correlated with the expectational error. This is intuitive. For instance, neighborhood $j$ may be better at date $t+1$ than was expected since market rents are lower than anticipated. We, therefore, instrument for the time $t+1$ rent difference $R_{t+1}(j,1) - R_{t+1}(j,0)$ with $Z_t$, equal to the one-period lagged value $R_t(j,1) - R_{t-1}(j,0)$.

Since $Z_t$ is in the time $t$ information set, we have:

$$E_t\left[Z_t\left(\xi_t^V\left(x',\theta'_{t-1}\right)-\xi_t^V\left(x,\theta_{t-1}\right)\right)\right]=0.$$

Thus, our exclusion restrictions are satisfied and the parameters are identified.

To identify the impact of tenure on utility $\alpha_M$, consider two mature households living in non-rent controlled housing in neighborhood $j$, with different levels of initial tenure, $\tau_n$ and $\tau'_n$.Suppose both households move to $j^*$ after one year. We thus have $\theta_{t-1}=(j,\tau_n,0,M)$ and $\theta'_{t-1}=(j,\tau'_n,0,M)$ for some $j\in\mathcal{J}$ and $x=x'=\mathcal{S}$. Then equation (11) becomes:

$$\begin{aligned}
Y_{j,j^*}^t &= \alpha_M\left(\tau_n-\tau'_n\right)+\xi_t^V\left(x',\theta'_{t-1}\right)-\xi_t^V\left(x,\theta_{t-1}\right)+\chi_{j,j^*}^t\\
Y_{j,j^*}^t &= \ln\left(\frac{p_t\left(\mathcal{S}|j,0,\tau_n\right)}{p_t\left(\mathcal{S}|j,0,\tau'_n\right)}\right)-\ln\left(\frac{p_t\left(j^*|j,0,\tau_n\right)}{p_t\left(j^*|j,0,\tau'_n\right)}\right)+\beta\ln\left(\frac{p_{t+1}\left(j^*|j,0,\tau_n\right)}{p_{t+1}\left(j^*|j,0,\tau'_n\right)}\right)
\end{aligned}$$

Since both households live in non-rent controlled housing in the same neighborhood, they pay the same rents and receive the same unobserved amenity value. Indeed, the only payoff-relevant difference between the two populations is the number of years they have lived in the neighborhood. Thus, appropriately examining the relative probabilities of staying in the neighborhood is informative of the importance of tenure on utility or, in other words, of the magnitude of $\alpha_M$. Intuitively, as one builds up more neighborhood capital, the benefits of staying in the neighborhood an additional year. Thus, the relative probability of staying one more year versus moving away should grow if neighborhood capital is accruing.

To estimate moving costs, we consider two mature households of equal tenure $\tau_n$ living in non-rent controlled housing in neighborhood $j$. Suppose that one household immediately moves to another house in the same zipcode and one household stays in the same home. Formally, $\theta_{t-1}=\theta'_{t-1}=(j,\tau_n,0,M)$, $x=\mathcal{S}$, and $x'=j$. As was discussed in Section 5.2.1, this constitutes an immediate renewal since rents do not change and neighborhood tenure does not change. Since one is only changing the house they live in due to the logit error and the moving costs, we can identify the fixed cost of moving. If people move houses a lot within a zipcode, moving costs must be low. If they do it rarely, moving costs must be high. Equation (9) gives the regression:

$$\begin{aligned} Y_j^t &= -\varphi_{0,M} + \chi_j^t \\ Y_j^t &= \ln\left(\frac{p_t\left(\mathcal{S}|j,0,\tau_n\right)}{p_t\left(j|j,0,\tau_n\right)}\right), \end{aligned}$$

which identifies the fixed moving cost parameter $\varphi_{0,M}$. Note that there is only one log difference instead of two since the households begin in the same state.

We also need the variable moving cost parameter, $\varphi_{d,M}$. Consider two mature households of equal tenure $\tau_n$, both living in non-rent controlled housing, one living in neighborhood $j$ and the other in neighborhood $j'$. Suppose they immediately move to either neighborhood $j^*$ or $j^{**}$. Both of these are choices constitute immediate renewals. Therefore, Equation (9) gives the specification:

$$\begin{aligned} Y_{j,j',j^*,j^{**}}^t &= \varphi_{d,M}\left(d_{j,j^*} - d_{j',j^*}\right) - \varphi_{d,M}\left(d_{j,j^{**}} - d_{j',j^{**}}\right) + \chi_{j,j',j^*,j^{**}}^t \\ Y_{j,j',j^*,j^{**}}^t &= \ln\left(\frac{p_t\left(j^*|j,0,\tau_n\right)}{p_t\left(j^*|j',0,\tau_n\right)}\right) - \ln\left(\frac{p_t\left(j^{**}|j,0,\tau_n\right)}{p_t\left(j^{**}|j',0,\tau_n\right)}\right). \end{aligned}$$

Intuitively, this compares the relative probabilities of moving to $j^*$ vs $j^{**}$ depending on whether one starts in $j$ or $j'$. If $j$ is very close to $j^*$, but far from $j^{**}$, then the difference in moving costs between the moves in large. However, if $j'$ is equidistant between the two, the moving costs between the two locations are the same. The relationship between these differences in distances and differences in migration probabilities identifies the marginal cost of moving with respect to distance. Using similar considerations, one can estimate the interaction term parameter $\varphi_{\tau,M}$. The equation is detailed in the appendix.

As one would expect, the equations for young households are very similar to the ones described above, but the probability of transitioning to a mature household must be taken into account. Furthermore, one can use the treatment group as well as the control group to estimate the neighborhood tenure parameters and the variable moving cost parameters. All of these additional equations are detailed in the appendix. The model is then estimated via GMM.

Finally, it remains to estimate the permanent component of amenities $\omega_j$.[15] We do so after estimating the full GMM system detailed above. We once again consider two mature households of equal tenure $\tau_n$, living in neighborhoods $j$ and $j'$ respectively and suppose that both households move to some neighborhood $j^*$ after one year. We thus have, $\theta_{t-1} = (j, \tau_n, 0, M)$, $\theta'_{t-1} = (j', \tau_n, 0, M)$, and $x = x' = \mathcal{S}$. These choices yield the equation:

$$
\begin{aligned}
Y^t_{j,j',j^*} &= \omega_j - \omega_{j'} + \tilde{\omega}_{jt} - \tilde{\omega}_{j't} + \xi^V_t\left(x', \theta'_{t-1}\right) - \xi^V_t\left(x, \theta_{t-1}\right) + \chi^t_{j,j',j^*} \\
Y^t_{j,j',j^*} &= \ln\left(\frac{p_t\left(\mathcal{S}|j,0,\tau_n\right)}{p_t\left(\mathcal{S}|j',0,\tau_n\right)}\right) - \ln\left(\frac{p_t\left(j^*|j,0,\tau_n\right)}{p_t\left(j^*|j',0,\tau_n\right)}\right) + \beta\ln\left(\frac{p_{t+1}\left(j^*|j,0,\tau_n\right)}{p_{t+1}\left(j^*|j',0,\tau_n\right)}\right) \\
&\quad - (\beta - 1)\,\varphi_{d,M}\left(d_{j,j^*} - d_{j',j^*}\right) - \gamma_M\left[R_t(j,0) - R_t(j',0)\right]
\end{aligned}
$$

Identification comes from the fact that, averaging over time, we average out the per-period neighborhood amenity shocks and expectational error shocks. Moreover, note that we do not have an endogeneity problem since we have already estimated $\gamma_M$ and can therefore move the utility impact of the rent difference to the left hand side of the equation. We also account for the differential moving costs related to distance on the left hand side of the equation. Finally, note that we can only identify fixed amenity value differences between neighborhoods. We therefore choose a normalization, letting zipcode 94110, representing the Mission District and Bernal Heights, be our baseline zipcode. We set its amenity value fixed effect to zero.

## 5.4 Model Estimates

Table 4 shows the parameter estimates of the model. Panel A reports the parameters measured in rent equivalent dollar units, with the exception of the transfer payments, which

[15] We cannot identify amenities of the outside options, i.e. the rest of the Bay Area and the rest of the country, as no one in our 1994 cohorts started off living in those locations.

are measured in actual dollar amounts.[16] Panel B reports the estimates in units of migration elasticities. We will focus on the estimates in Panel A. Normalizing the coefficient on exponential rents to 1, we identify the standard deviation of tenants' idiosyncratic shocks to their location preferences. We find that young renters have annual location taste shocks with a standard deviation equivalent to $7,411. Mature renters face location shocks with a 12.7% lower standard deviation. These estimates are consistent with our previously discussed hypothesis that young renters' migration decisions are more driven by idiosyncratic shocks than older households.

Turning to the magnitudes of the tenant buyouts, we find young renters receive $1.631 more dollars from their landlords for each additional $1 below market their rent is. Mature renters face similar impact of $1.404. We also find buyout offers are larger as tenants live in their zipcodes longer. For each additional year a young (mature) tenant lives in their zipcode, they receive $164 ($141) additional dollars in the buyout offer from their landlord. Finally, we find mature tenants receive larger buyout offers overall by $70,702. This may reflect that landlords expect older tenants to remain in their apartments for the very long term. Along the same lines, to the extent that these transfers reflect evictions, landlords would be more incentivized to evict older renters. To get a better sense of the magnitudes of these buyout payments, Figure 14 plots the average buyout to young tenants offered in each year in the data, across all tenants and neighborhoods. By 2010, the average offer to tenants who still remain at their 1994 address is just over $30,000. Figure 15 plots the heterogeneity across zipcodes in mean buyout offers, highlighting that some zipcodes experience much large rent increases than others over this time period. In the most expensive zipcode, the average buyout in 2010 is just about $40,000, while in the cheapest zipcode the mean buyout offer is around $25,000. These numbers seem very much in line with popular press anecdotes about tenant buyouts in San Francisco.

Moving along to our estimates of moving costs, we find the fixed cost of moving is

[16] These are measured at the mean rent paid by rent-controlled households, $2350.

equivalent, in rent-equivalent dollars, to $42,626 for young renters and $38,988 for old renters. These estimates seem quite reasonable and actually quite below what is typically found in the literature. A main driver of the magnitude of this estimate are the short-run migration elasticities with respect to a one-year temporary change. It is often quite hard to find a high quality instrument for rents that does not effect other omitted variables such as amenities. Likely, many instruments for rent also impact the supply and quality of amenities, leading to rent elasticities being biased towards 0. Our rent control policy experiment only affects rents and cannot effect amenities in our regressions, as we are comparing migration decisions between market rent and rent controlled households in the same neighborhood consuming the same amenities.

In addition to the fixed costs of moving, we find that the moving costs increase with the distance of the move. A 1 percent increase in move distance is equivalent to $114 for the young and $101 for the old. Finally, we also consider whether these variable moving costs change as households live in their zipcodes longer. One might think that the longer a household has lived in the area the more familiar they are with further and further away neighborhoods, lowering those marginal moving costs. Indeed, we find this is the case, with each additional year a tenant has lived in their zipcode lowering the moving cost by $415 for the young and $357 for the old.

Lastly, we turn to our neighborhood capital estimates. Proponents of rent control often argue that long-term residents are the ones in the most need of rent control as migrating away from their community forces them to lose many of the connections and investments they have been in the neighborhoods over time. We do find very statistically significant effects of neighborhood capital accumulation. However, the economic magnitude is small. Young (mature) households increasingly value living in their zipcode by $266 ($292) in dollar rent equivalent terms. However, these effects can add up to a sizable effect over a lifetime.

# 6 Welfare Effects of Rent Control

## 6.1 Welfare Decomposition: 1994-2012

We begin our investigation of the welfare effects of rent control by decomposing the impacts of the 1994 ballot initiative on its beneficiaries, relative to the control group. We discuss here mature households. The expressions for young households are exactly analogous.

### 6.1.1 Derivations

In any given year $t$ between the years of 1994 and 2012, the average utility difference between the treatment group and the control group is given by:

$$\begin{aligned} \Delta U_t^M &= \sum_{\theta_{t-1}} \sum_x \left(\overline{u}_t\left(x, \theta_{t-1}\right) + E_t\left[\varepsilon_{ixt} | x, \theta_{t-1}\right]\right) p_t\left(x|\theta_{t-1}\right)\left(p_t^T\left(\theta_{t-1}\right) - p_t^C\left(\theta_{t-1}\right)\right) \quad (12) \\ &= \sum_{\theta_{t-1}} \sum_x \left(\overline{u}_t\left(x, \theta_{t-1}\right) + E_t\left[\varepsilon_{ixt} | x, \theta_{t-1}\right]\right)\left(p_t^T\left(x, \theta_{t-1}\right) - p_t^C\left(x, \theta_{t-1}\right)\right) \end{aligned}$$

where recall $\overline{u}_t\left(x, \theta_{t-1}\right) = u\left(x, \omega_t, 0, \theta_{t-1}\right)$ and the utility function is defined in equation (3). The expression $p_t\left(x|\theta_{t-1}\right)$ again denotes the conditional probability of choosing $x \in \{\mathcal{S}\} \cup \mathcal{J}$, given that the current state is $\theta_{t-1}$, $p_t^T\left(\theta_{t-1}\right), p_t^C\left(\theta_{t-1}\right)$ denote the probabilities of being in state $\theta_{t-1}$ for the treatment group and control group respectively, and $p_t^T\left(x, \theta_{t-1}\right), p_t^C\left(x, \theta_{t-1}\right)$ denote the joint probabilities. The conditional expectation $E_t\left[\varepsilon_{it} | x, \theta_{t-1}\right]$ denotes the expected logit error conditional on choosing $x$ from state $\theta_{t-1}$. Of course, equation (12) simply says that the average utility difference is the weighted average utility received by the treatment group minus the weighted average utility received by the control group.

We can decompose this average utility difference by substituting in for the utility function from equation (3). We find that:

$$\begin{aligned} \Delta U_t^M &= \Delta U_t^{M,\mathrm{Rent}} + \Delta U_t^{M,Payoff} + \Delta U_t^{M,NC} \\ &\quad + \Delta U_t^{M,MC} + \Delta U_t^{M,Miles} + \Delta U_t^{M,Amenity} + \Delta U_t^{M,Logit}. \end{aligned} \tag{13}$$

That is, the average utility difference between the treatment group and the control arises from differences in average rent paid $\Delta U_t^{M,\mathrm{Rent}}$, in transfers received from landlords $\Delta U_t^{M,Payoff}$, in accumulated neighborhood capital $\Delta U_t^{M,NC}$, in fixed costs $\Delta U_t^{M,MC}$, in variable moving costs $\Delta U_t^{M,Miles}$, in neighborhood amenity values $\Delta U_t^{M,Amenity}$, and in idiosyncratic valuations $\Delta U_t^{M,Logit}$. Suppressing the dependence of $j$ and $\tau$ on $x$, we can formally write these terms as:

$$\begin{aligned} \Delta U_t^{\mathrm{Rent}} &= \sum_{\theta_{t-1}} \sum_x \gamma_M \exp\left(R_t\left(j, d, \tau_h\right)\right)\left(p_t^T\left(x, \theta_{t-1}\right) - p_t^C\left(x, \theta_{t-1}\right)\right) \\ \Delta U_t^{Payoff} &= \sum_{\theta_{t-1}} \sum_x \Lambda_t\left(x, d_{t-1}, M\right)\left(p_t^T\left(x, \theta_{t-1}\right) - p_t^C\left(x, \theta_{t-1}\right)\right) \\ \Delta U_t^{M,NC} &= \sum_{\theta_{t-1}} \sum_x \alpha_M \tau_n \left(p_t^T\left(x, \theta_{t-1}\right) - p_t^C\left(x, \theta_{t-1}\right)\right) \\ \Delta U_t^{M,MC} &= \sum_{\theta_{t-1}} \sum_x \varphi_{0,M} 1\left[x \neq \mathcal{S}\right]\left(p_t^T\left(x, \theta_{t-1}\right) - p_t^C\left(x, \theta_{t-1}\right)\right) \\ \Delta U_t^{M,Miles} &= \sum_{\theta_{t-1}} \sum_x \varphi_{d,M} d_{j,j_{t-1}} 1\left[x \neq \mathcal{S}\right]\left(p_t^T\left(x, \theta_{t-1}\right) - p_t^C\left(x, \theta_{t-1}\right)\right) \\ \Delta U_t^{M,Amenity} &= \sum_{\theta_{t-1}} \sum_x \omega_{jt}\left(p_t^T\left(x, \theta_{t-1}\right) - p_t^C\left(x, \theta_{t-1}\right)\right). \end{aligned}$$

We can measure each of these terms.[17] We recover estimates of $\gamma_M, \Lambda_M, \alpha_M, \varphi_{0,M}, \varphi_{d,M}$, and $\omega_{jt}$ from our structural model. We can then recover the other needed quantities from standard reduced form differences-in-differences analysis. For example, $\sum_{\theta_{t-1}} \sum_x \exp\left(R_t\left(j, d, \tau_h\right)\right)\left(p_t^T\left(x, \theta_{t-1}\right) - p_t^C\left(x, \theta_{t-1}\right)\right)$ is simply the average difference in rents paid between treatment and control in year $t$, $\sum_{\theta_{t-1}} \sum_x \tau_n\left(p_t^T\left(x, \theta_{t-1}\right) - p_t^C\left(x, \theta_{t-1}\right)\right)$

[17] Since we measure rents as monthly rents/3000, we multiply by 36,000 to convert to an annual rent number.

is the average difference in accumulated neighborhood capital between treatment and control, $\sum_{\theta_{t-1}}\sum_x 1\left[x\neq\mathcal{S}\right]\left(p_t^T\left(x,\theta_{t-1}\right)-p_t^C\left(x,\theta_{t-1}\right)\right)$ is the average difference in number of moves between treatment and control, and
$\sum_{\theta_{t-1}}\sum_x d_{j,j_{t-1}} 1\left[x\neq\mathcal{S}\right]\left(p_t^T\left(x,\theta_{t-1}\right)-p_t^C\left(x,\theta_{t-1}\right)\right)$ is the average difference in distance moved between treatment and control. Each of these can be readily calculated using the reduced form methodology described in Section 4. The average utility difference due to transfers and the average utility difference due to amenities can be similarly calculated by combining our structural estimates with reduced form differences-in-differences analysis.

Deriving an expression for the utility difference due to idiosyncratic valuations $\Delta U_t^{M,Logit}$ is a bit more complicated. We have that:

$$\Delta U_t^{M,Logit}=\sum_{\theta_{t-1}}\sum_x E_t\left[\varepsilon_{it}|x,\theta_{t-1}\right]\left(p_t^T\left(x,\theta_{t-1}\right)-p_t^C\left(x,\theta_{t-1}\right)\right). \tag{14}$$

We therefore need an expression for the conditional expectation $E_t\left[\varepsilon_{ixt}|x,\theta_{t-1}\right]$. Using Bayes' rule, we get:

$$\begin{aligned}E_t\left[\varepsilon_{ixt}|x,\theta_{t-1}\right] &= \frac{\int\varepsilon_{ixt}\left(\prod_{x'\neq x}e^{-e^{-\left(\varepsilon_{ixt}+v_t\left(x,\theta_{t-1}\right)-v_t\left(x',\theta_{t-1}\right)\right)}}\right)e^{-\varepsilon_{ixt}}e^{-e^{-\varepsilon_{ixt}}}d\varepsilon_{ixt}}{p_t\left(x|\theta_{t-1}\right)}\\ &= \frac{\int\varepsilon_{ixt}\left(\prod_{x'\neq x}e^{-e^{-\left(\varepsilon_{ixt}+\ln p_t\left(x|\theta_{t-1}\right)-\ln p_t\left(x'|\theta_{t-1}\right)\right)}}\right)e^{-\varepsilon_{ixt}}e^{-e^{-\varepsilon_{ixt}}}d\varepsilon_{ixt}}{p_t\left(x|\theta_{t-1}\right)},\end{aligned}$$

where in the second equality we used the Hotz and Miller (1993) inversion $v_t\left(x,\theta_{t-1}\right)-v_t\left(x',\theta_{t-1}\right)=\ln p_t\left(x|\theta_{t-1}\right)-\ln p_t\left(x'|\theta_{t-1}\right)$. Substituting into equation (14), we derive:

$$\begin{aligned}\Delta U_t^{M,Logit}=&\sum_{\theta_{t-1}}\sum_x\left\{\int\varepsilon_{ixt}\left(\prod_{x'\neq x}e^{-e^{-\left(\varepsilon_{ixt}+\ln p_t\left(x|\theta_{t-1}\right)-\ln p_t\left(x'|\theta_{t-1}\right)\right)}}\right)e^{-\varepsilon_{ixt}}e^{-e^{-\varepsilon_{ixt}}}d\varepsilon_{ixt}\right\}\times\\ &\left(p_t^T\left(\theta_{t-1}\right)-p_t^C\left(\theta_{t-1}\right)\right).\end{aligned} \tag{15}$$

Since we have empirical estimates of each of the probabilities, we can estimate this utility difference.

We finally convert our estimated utility differences into rent equivalent dollar amounts. Consider an individual in the control group who pays the average San Francisco rent in year $t$, which we denote as $\overline{R}_t$. We now proceed iteratively. The dollar rent equivalent $\Delta W_t^{\mathrm{Re}nt}$ of the utility difference $\Delta U_t^{\mathrm{Re}nt}$ in year $t$ due to rent differences can be calculated as the solution to :

$$\gamma_M \exp\left(\overline{R}_t + \Delta W_t^{\mathrm{Re}nt}\right) - \gamma_M \exp\left(\overline{R}_t\right) = \Delta U_t^{\mathrm{Re}nt},$$

which gives:

$$\Delta W_t^{\mathrm{Re}nt} = \ln\left(\frac{\Delta U_t^{\mathrm{Re}nt}}{\gamma_M} + \exp\left(\overline{R}_t\right)\right) - \overline{R}_t.$$

The dollar rent equivalent incremental impact of transfers can then be calculated as:

$$\Delta W_t^{Payoff} = \ln\left(\frac{\Delta U_t^{Payoff}}{\gamma_M} + \exp\left(\overline{R}_t + \Delta W_t^{\mathrm{Re}nt}\right)\right) - \left(\overline{R}_t + \Delta W_t^{\mathrm{Re}nt}\right).$$

Now let $\Delta U_t^{M,\iota}$ denote the utility differences, with $\iota \in \{1, ..., 7\}$ corresponding to the ordering in equation (13). Iterating on our procedure gives the dollar rent equivalent incremental impacts of each element of the decomposition:

$$\Delta W_t^{\iota} = \ln\left(\frac{\Delta U_t^{Payoff}}{\gamma_M} + \exp\left(\overline{R}_t + \sum_{\iota'<\iota} \Delta W_t^{\iota'}\right)\right) - \left(\overline{R}_t + \sum_{\iota'<\iota} \Delta W_t^{\iota'}\right).$$

#### 6.1.2 Results

The results of this decomposition are reported in Table 5. Note that we still need to compute the welfare effects due to allocative efficiency, $\Delta U_t^{M,Logit}$, which are currently all set to zero. This will be coming soon in a future draft. We find that the beneficiaries of the 1994 rent control law received large welfare benefits between the 1994-2012 period. Older households received a total rent-equivalent dollar benefit of \$100,644, reflecting an annual benefit of \$5,925. These benefits were front loaded, with households earning a cumulative benefit of \$68,705 and average annual benefit of \$7,634 during the 1995-2003 period Cumulative benefits equaled \$37,939 during the 2004-2012 period, reflecting an annual average of \$4,215.

In terms of decomposition, most of the benefits from the rent control law came from protection against rent increases and transfers.[18] Respectively, protection against rent increases constituted 49.6% of the total benefit and transfers constituted 33.8% of the total. Lower moving costs, both fixed and variable, were 15.2% of the total. Increased neighborhood capital constituted only small fraction of the total benefit at 1.3%. The welfare benefits from increased amenity values were negligible.

The benefits of the rent control expansion were smaller for younger households, although still large. That they are smaller is consistent with our estimate that younger households receive larger idiosyncratic shocks, which leads to more frequent moving and thus smaller benefits from rent control protections. Younger households are also estimated to receive smaller transfers. Cumulative welfare benefits for these households totaled \$56,953, reflecting an annual average of \$3,164. Similar to older households, the benefits were front loaded. Younger households received cumulative benefits of \$39,525 during the 1995-2003 period and cumulative benefits of \$17,427 during the 2004-2012 period. Annual averages were \$4,392 and \$1,936 respectively.

Also similar to older households, most of the benefits came from protection against rent increases and transfers, constituting 57.5% and 32.7% respectively over the total period. The fraction due to moving costs is much smaller for younger households, at only 4.9%. Note this reinforces the idea that, due to a higher variance if idiosyncratic shocks, younger households optimally choose to move more often. The fraction due to neighborhood capital is once again small, constituting just 2.1% for the average. Welfare benefits due to increased amenity values now reflect a small, but non-negligible, fraction of the total benefit at 2.8%.

[18] The model assumes that all observed moves are rational choices. The transfers we estimate are those which rationalize the observed empirical frequencies. It is possible that some of the moves we see in the data are forced evictions, rather than the result of negotiations between landlords and tenants over monetary compensation. To the extent that this is the case, our welfare benefits from transfer payments over overstated. However, even in the extreme case where the welfare benefits from transfers are zero, the benefits from protection against rent increases would still be large.

We aggregate these numbers over the entire population of renters impacted by the rent control law. The aggregate welfare benefits are very large. Older households received a cumulative benefit of \$3.960 billion dollars over the entire period, while younger households received a cumulative benefit of \$3.661 billion dollars. Across the entire population, the aggregate benefit was \$7.621 billion dollars, reflecting an annual average of \$423.383 million dollars. Note also that these welfare numbers are only for the 1994 population impacted by the rent control expansion. It does not take into account the welfare benefits for renters who moved into the impacted properties in later years, which presumably were also quite large.

## 6.2 General Equilibrium Welfare Impact of Reduced Supply

We finally turn to evaluating the GE welfare impact of the landlord supply response. Intuitively, since landlords reduced supply in response to the 1994 law, as was shown in Section 4.2, average San Francisco rents were higher than they otherwise would have been. Using our structural framework, we quantify the magnitude of this cost.

### 6.2.1 Derivations

We evaluate the welfare impact relative to the 1993 steady state, prior to the introduction of the law change. Aggregate welfare in this steady state is given by:

$$\sum_j N_j \ln \left( \sum_{x \in \{\mathcal{S}\} \cup \mathcal{J}} \exp\left(v\left(x, j\right)\right) \right),$$

where $N_j$ is the number of people living in neighborhood $j$. Note that the state variable now does not include rent control status since we are consider the pre-law steady state. Suppose that the law raises rents in zipcode $j$ by San Francisco by a proportional amount equal to $d \ln R_j$. Using standard calculations, we find that the local welfare impact of a change in rents is given by:

$$\sum_j N_j \sum_x p\left(x|j\right) \sum_k \frac{\partial v\left(x, j\right)}{\partial \ln R_k} d \ln R_k, \tag{16}$$

where $p(x|j)$ are the pre-law conditional choice probabilities To compute this quantity we thus need to calculate $\partial v(x,j)/d\ln R_k$ for all $j$, $x$, and $k \in \mathcal{J}$ and we need to determine the zipcode level rent response to the measured reduced form supply reduction.

Steady-state in the model is characterized by the equation:

$$\forall j : N_j \left(1 - p(\mathcal{S}|j) - p(j|j)\right) = \sum_{j' \neq j} N_{j'} p(j|j'). \tag{17}$$

This simply says that, in steady state, the number of renters flowing out of neighborhood $j$ must be equal to the number of renters flowing into neighborhood $j$. We now assume that the supply decrease is the same proportionally in each zipcode. Since small multifamily housing constituted 44% of 1994 non rent-controlled housing stock, our reduced form results indicate that rental supply in San Francisco decreased by 6 percent. Letting $d\ln N_j/d\Phi$ denote the supply response, where $\Phi$ is simply a convenient notation indicating the impact of the law, we have

$$\frac{d\ln N_j}{d\Phi} = \frac{d\ln N_{SF}}{d\Phi} = -.06 \text{ for all } j \text{ in SF}$$

We determine how much rents have to change by in the new long-run steady state given this supply response. Taking a derivative of equation (17) with respect to $\Phi$ gives:

$$\frac{d\ln N_j}{d\Phi} N_j \left(1 - \sum_{x \in \{\mathcal{S}, j\}} p(x|j)\right) - N_j \sum_{x \in \{\mathcal{S}, j\}} \frac{dp(x|j)}{d\Phi} = \sum_{j' \neq j'} \left[\frac{d\ln N_{j'}}{d\Phi} N_{j'} p(j|j') + N_{j'} \frac{dp(j|j')}{d\Phi}\right], \tag{18}$$

for all $j$.

Now, in steady state, the conditional probabilities are given by:

$$p(x|j) = \frac{\exp(v(x,j))}{\sum_{x'} \exp(v(x',j))}.$$

So:

$$\frac{dp(x|j)}{d\Phi} = \sum_k \frac{\partial p(x|j)}{\partial \ln R_k}\frac{d\ln R_k}{d\Phi} \tag{19}$$
$$= \sum_k p(x|j)\left(\frac{\partial v(x,j)}{\partial \ln R_k} - \sum_{x'} p(x'|j)\frac{\partial v(x',j)}{\partial \ln R_k}\right)\frac{d\ln R_k}{d\Phi}.$$

To finish the calculation, we therefore need to determine $\partial v(x,j)/\partial \ln R_k$. With these in place, we can plug equation (19) into equation (18) and solve the resulting system of equations for the rent responses $d\ln R_k/d\Phi$.

We note that in steady-state:

$$v(x,j) = \overline{u}(x,j) + \beta \ln\left(\sum_{x'} \exp\left(v\left(x', j(x)\right)\right)\right)$$

Taking derivatives with respect to log rents, we get:

$$\frac{\partial v(x,j)}{\partial \ln R_k} = \gamma \exp(R_j) R_j 1\left[j(x) = k\right] + \beta \sum_{x'} p\left(x'|j(x)\right)\frac{\partial v\left(x', j(x)\right)}{\partial \ln R_k}.$$

This is a system of equations which can be numerically solved for the partial derivatives. The system for young renters is similar, but takes into account the possibility of transitioning to a mature renter.

### 6.2.2 Results

We find that 6% decrease in housing supply led to 7% increase in rental prices. These caused an aggregate welfare loss to renters of $5 Billion. This is almost as large as the benefits accrued by the lucky beneficiaries of rent control. These GE welfare losses only account for the increased rents due to the decreased supply of housing. We also found that rent control incentivized landlords to invest in their properties by renovating and building new housing, as well as converting to owner occupancy. These effects likely attached higher income tenants to San Francisco and further raised rents. It appears that the GE losses from the landlords' response to rent control essentially completely undoes the gains accrued to the households that were lucky enough to receive rent control in 1994.

# 7 Conclusion

In this paper, we study the welfare impacts of rent control on its tenant beneficiaries as well as the welfare impacts of landlords' responses. To answer this question, we exploit a unique rent control expansion in San Francisco in 1994 that suddenly provided rent control protections for small multifamily housing built prior to 1980. By combining new panel micro data on individual migration decisions with detailed assessor data on individual SF parcels we get quasi-experimental variation in the assignment of rent control at both the individual tenant level and at the parcel level.

We find that, on average, in the medium to long term the beneficiaries of rent control are between 10 and 20 percent more likely to remain at their 1994 address relative to the control group. These 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 rents 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 as a way as to exempt it from the regulations. In sum, we find that impacted landlords reduced the supply the available rental housing by 15 percent. Consistent with this evidence, we find that there was a 20 percent decline in the number of renters living in impacted buildings, relative to 1990-1994 levels, and a 30 percent decline in the number of renters living in units protected by rent control.

We develop a dynamic, structural model of neighborhood choice to translate our reduced form impacts into welfare impacts. A key contribution of the paper is to show how quasi-experimental evidence can be leveraged to estimate to dynamic discrete choice model. We find that rent control offered large benefits to impacted tenants during the 1995-2012 period, averaging between \$3100 and \$5900 per person each year, with aggregate benefits totaling over \$423 million annually. Over the entire period, tenants received cumulative benefits of around \$7.6 billion. We find that most of these benefits came from protection against rent increases and transfer payments from landlords. However, we find losses to all renters of \$5 billion due to rent control's effect on decreasing the rental housing and raising market rents.

These results highlight that forcing landlords to provided insurance against rent increases leads to large losses to tenants. If society desires to provide social insurance against rent increases, it would be more desirable 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 household 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 for Individual Regressions for Multi-Family Residence (2-4 Units)

| | **Mean** | **S.D** |
|---|---|---|
| *Demographics* | | |
| Age in 1993 | 38.584 | 10.707 |
| Male | 0.504 | 0.500 |
| Landlord | 0.144 | 0.352 |
| *Residency* | | |
| In SF | 0.643 | 0.479 |
| Same Zip | 0.443 | 0.497 |
| same_street | 0.391 | 0.488 |
| Same Address | 0.375 | 0.484 |
| Years at 1993 Address | 6.761 | 4.911 |
| Years at Address | 1.659 | 0.597 |
| Observations | 1508247 | |

*Notes*: Sample consists of all tenants and landlords between 20 and 65 years old living in SF in 1993 and small multi-family residences that were built during $1900 - 1990$. If a building is constructed post 1993 and we observe a landlord living there in 1993, we include it into the treatment group for rent control. Table reports the mean, standard deviation and median of demographic characteristics and various dependent variables during $1990 - 2016$.

Table 2: Sample Characteristics for Parcel Regressions for Multi-Family Residence (2-4 Units)

| | **Mean** | **S.D** |
|---|---|---|
| *Residency* | | |
| Permantly Vacant | 0.052 | 0.222 |
| Vacant | 0.103 | 0.304 |
| *Population* | | |
| Population/Avg Pop 90-94 | 2.076 | 3.728 |
| Renters/Avg Pop 90-94 | 1.541 | 3.276 |
| Renters in Rent-Controlled Buildings/Avg Pop 90-94 | 1.311 | 1.808 |
| Renters in Redeveloped Buildings/Avg Pop 90-94 | 0.108 | 0.679 |
| Owners/Avg Pop 90-94 | 0.535 | 1.467 |
| *Permits* | | |
| Accumulative Add/Alter/Repair per Unit | 0.256 | 0.487 |
| Ever Received Add/Alter/Repair | 0.336 | 0.472 |
| Observations | 724037 | |

*Notes*: Sample consists of all parcels that are multi-family residence with fewer than four units in SF that were built during $1900-1990$. If a building associated with a parcel is constructed post 1993 and we observe someone living there before 1993, we include it into the treatment group for rent control. Table reports the mean, standard deviation and median of various dependent variables during $1990-2016$.

Table 3: Treatment Effect for Tenants of Multi-Family Residence (2-4 Units)

| | (1)<br>In SF | (2)<br>Same Zip | (3)<br>Same Address |
|---|---|---|---|
| *Treat*$\times$*Period* | | | |
| 1994-1999 | 0.0200** | 0.0226*** | 0.0218*** |
| | (0.0081) | (0.0087) | (0.0083) |
| 2000-2004 | 0.0451*** | 0.0355*** | 0.0354*** |
| | (0.0115) | (0.0104) | (0.0088) |
| Post 2005 | 0.0366*** | 0.0302*** | 0.0147** |
| | (0.0109) | (0.0084) | (0.0063) |
| Control Mean $1994-1999$ | 0.7641 | 0.5971 | 0.5410 |
| Control Mean $2000-2004$ | 0.5138 | 0.2672 | 0.1827 |
| Control Mean Post 2005 | 0.4346 | 0.1801 | 0.1135 |
| Adjusted $R^2$ | 0.600 | 0.630 | 0.655 |
| Observations | 1251747 | 1251747 | 1251747 |

*Notes*: Sample consists of all tenants between 20 and 65 years old living in SF in 1993 and in small multi-family residences that were built during $1900-1990$. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. 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. Significance levels: * 10%, ** 5%, *** 1%.

## Table 4: Model Estimates

### A. Parameter Estimates in 2010 Dollars

| St Dev of Logit Shocks | | Tenant Buyouts | | Moving Costs | | Neighborhood Capital | |
|---|---|---|---|---|---|---|---|
| Young Renters | 7441.178*** | Log Below Market Rent | 1.631*** | Fixed Cost | 42626.11*** | Young Renters | 265.795*** |
| | (1278.596) | (Young Renters) | (0.092) | (Young Renters) | (4776.017) | | (52.9889) |
| Old Renters | 6496.264*** | Log Below Market Rent | 1.404*** | Fixed Cost | 38987.65*** | Old Renters | 291.781*** |
| | (995.629) | (Old Renters) | (0.101) | (Old Renters) | (4005.167) | | (47.4294) |
| | | Years in Zipcode | 164.222*** | MC per Ln Mile | 11426.11*** | | |
| | | (Young Renters) | (107.696) | (Young Renters) | (1816.79) | | |
| | | Years in Zipcode | 141.439*** | MC per Ln Mile | 10066.49*** | | |
| | | (Old Renters) | (90.99) | (Old Renters) | (1437.622) | | |
| | | Old Renter-Direct effect | 70702.05*** | $\Delta$MC wrt Yrs in Zip | -415.607*** | | |
| | | | (12339.43) | (Young Renters) | (74.24046) | | |
| | | | | $\Delta$MC wrt Yrs in Zip | -357.6618*** | | |
| | | | | (Old Renters) | (57.12502) | | |

### B.Demand Semi-Elasticities to Remain in Home with respect to 1 year Temporary Changes

| Log Rent | | Tenant Buyouts | | Moving Costs | | Neighborhood Capital | |
|---|---|---|---|---|---|---|---|
| Young Renters | -0.210*** | Log Below Market Rent | -0.327*** | Fixed Cost | 0.580*** | Young Renters | 0.0019*** |
| | (0.040) | (Young Renters) | (0.068) | (Young Renters) | (0.003) | | (0.00007) |
| Old Renters | -0.244*** | Log Below Market Rent | -0.327*** | Fixed Cost | 0.580*** | Old Renters | 0.0024*** |
| | (0.041) | (Old Renters) | (0.068) | (Old Renters) | (0.003) | | (0.00008) |
| | | Years in Zipcode | -0.0012 | MC per Mile | 0.095*** | | |
| | | (Young Renters) | (0.0008) | (Young Renters) | (0.005) | | |
| | | Years in Zipcode | -0.0012 | MC per Mile | 0.096*** | | |
| | | (Old Renters) | (0.0008) | (Old Renters) | (0.005) | | |
| | | Old Renter-Direct effect | -0.583*** | $\Delta$MC wrt Yrs in Zip | -0.003*** | | |
| | | | (0.0082) | (Young Renters) | (-0.0002) | | |
| | | | | $\Delta$MC wrt Yrs in Zip | -0.003*** | | |
| | | | | (Old Renters) | (-0.0002) | | |

Table 5: Welfare Effects of 1994 Rent-Controlled Cohort in 2010 Dollars

**A. Old Residents (Age 40+)**

| | 1995-2003 | | | 2004-2012 | | | 1995-2012 | | |
|---|---|---|---|---|---|---|---|---|---|
| | Cumulative | Per Year | Share | Cumulative | Per Year | Share | Cumulative | Per Year | Share |
| Rent | 30,285 | 3,365 | 40.6% | 22,644 | 2,516 | 50.2% | 52,929 | 2,940 | 44.2% |
| Payoff | 25,560 | 2,840 | 34.3% | 10,511 | 1,168 | 23.3% | 36,071 | 2,004 | 30.2% |
| Neighborhood Capital | 812 | 90 | 1.1% | 583 | 65 | 1.3% | 1,395 | 77 | 1.2% |
| Fixed Moving Cost | 8,125 | 903 | 10.9% | 1,352 | 150 | 3.0% | 9,477 | 526 | 7.9% |
| Distance of Moves | 3,857 | 429 | 5.2% | 2,827 | 314 | 6.3% | 6,684 | 371 | 5.6% |
| Amenity | -42 | -5 | -0.1% | -276 | -31 | -0.6% | -318 | -18 | -0.3% |
| Logit Shock | 5,918 | 658 | 7.9% | 7,470 | 830 | 16.6% | 13,388 | 744 | 11.2% |
| Total per Person | 74,514 | 8,279 | | 45,111 | 5,012 | | 119,625 | 6,646 | |

**B. Young Residents (Age 20-39)**

| | 1995-2003 | | | 2004-2012 | | | 1995-2012 | | |
|---|---|---|---|---|---|---|---|---|---|
| | Cumulative | Per Year | Share | Cumulative | Per Year | Share | Cumulative | Per Year | Share |
| Rent | 20,782 | 2,309 | 63.1% | 11,940 | 1,327 | 146.3% | 32,722 | 1,818 | 79.6% |
| Payoff | 12,537 | 1,393 | 38.0% | 6,113 | 679 | 74.9% | 18,650 | 1,036 | 45.4% |
| Neighborhood Capital | 431 | 48 | 1.3% | 750 | 83 | 9.2% | 1,181 | 66 | 2.9% |
| Fixed Moving Cost | 3,741 | 416 | 11.3% | -1,643 | -183 | -20.1% | 2,098 | 117 | 5.1% |
| Distance of Moves | 1,655 | 184 | 5.0% | -949 | -105 | -11.6% | 706 | 39 | 1.7% |
| Amenity | 243 | 27 | 0.7% | 829 | 92 | 10.2% | 1,073 | 60 | 2.6% |
| Logit Shock | -6,428 | -714 | -19.5% | -8,879 | -987 | -108.8% | -15,308 | -850 | -37.2% |
| Total per Person | 32,960 | 3,662 | | 8,162 | 907 | | 41,121 | 2,285 | |

**C. SF Aggregate**

| | 1995-2003 | | | 2004-2012 | | | 1995-2012 | | |
|---|---|---|---|---|---|---|---|---|---|
| | Cumulative | Per Year | Share | Cumulative | Per Year | Share | Cumulative | Per Year | Share |
| Old | 2,766,989,545 | 307,443,283 | 56.6% | 1,675,156,256 | 186,128,473 | 76.2% | 4,442,145,875 | 246,785,882 | 62.7% |
| Young | 2,118,588,225 | 235,398,692 | 43.4% | 524,611,003 | 58,290,111 | 23.8% | 2,643,199,099 | 146,844,394 | 37.3% |
| All | 4,885,577,770 | 542,841,975 | | 2,199,767,259 | 244,418,584 | | 7,085,344,974 | 393,630,276 | |

Figure 1: Historical Trend of Nominal Median Rent

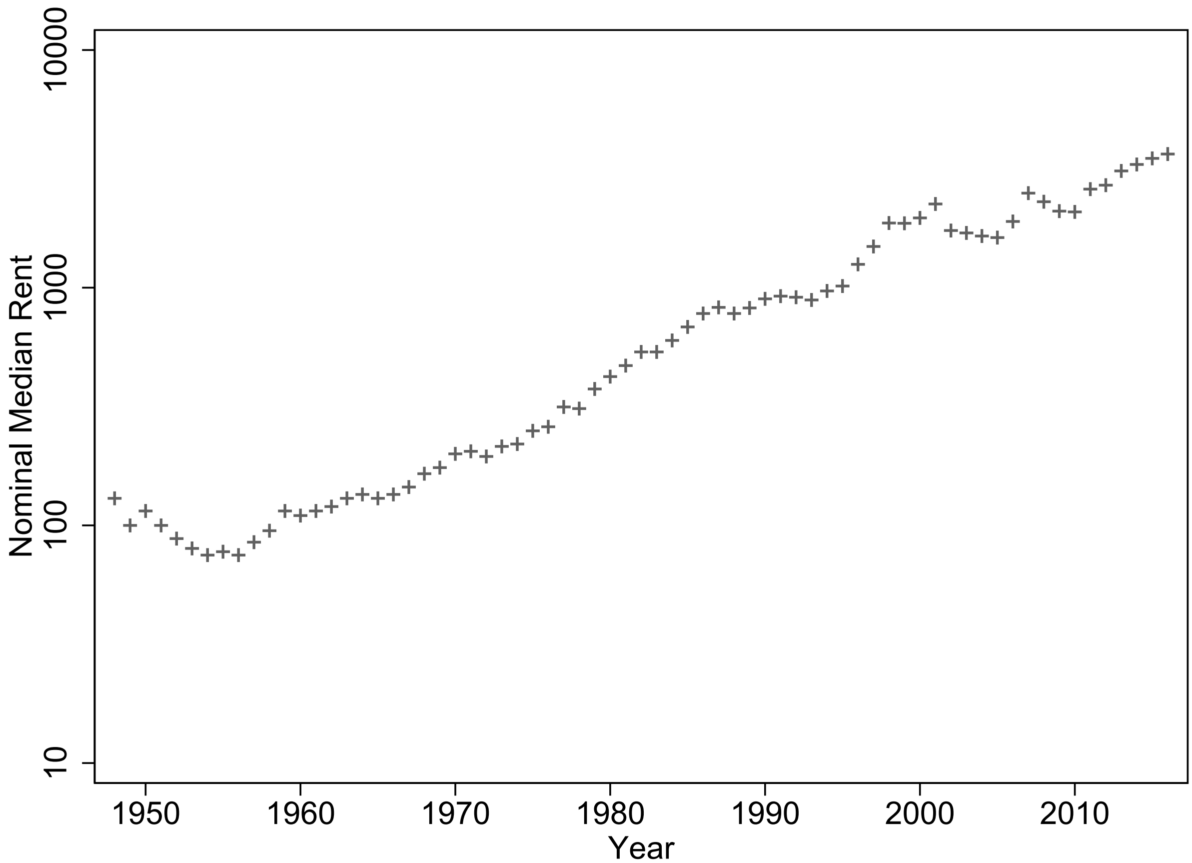


Figure 2: Geographic Distribution of Treated and Control Buildings in San Francisco

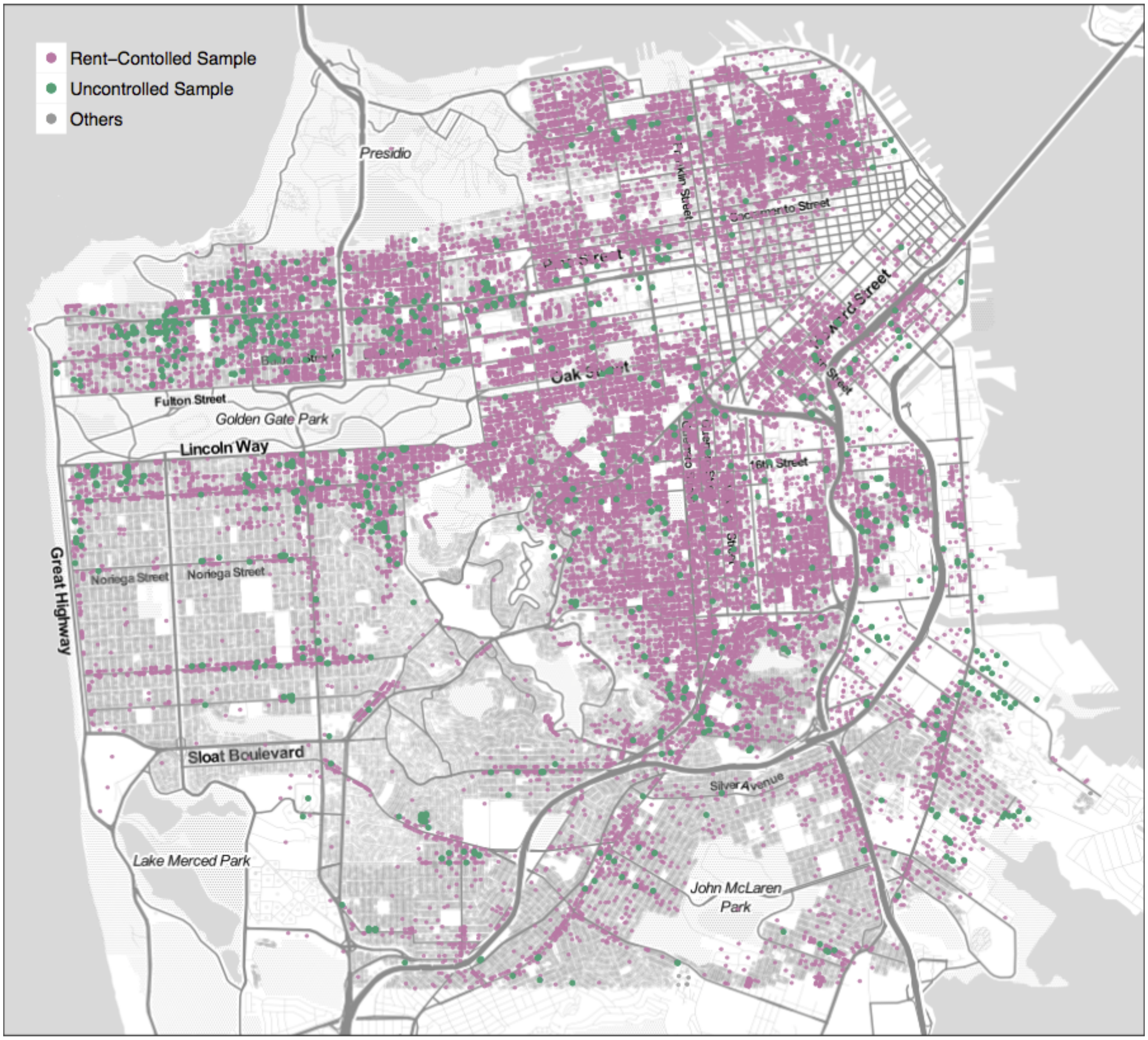
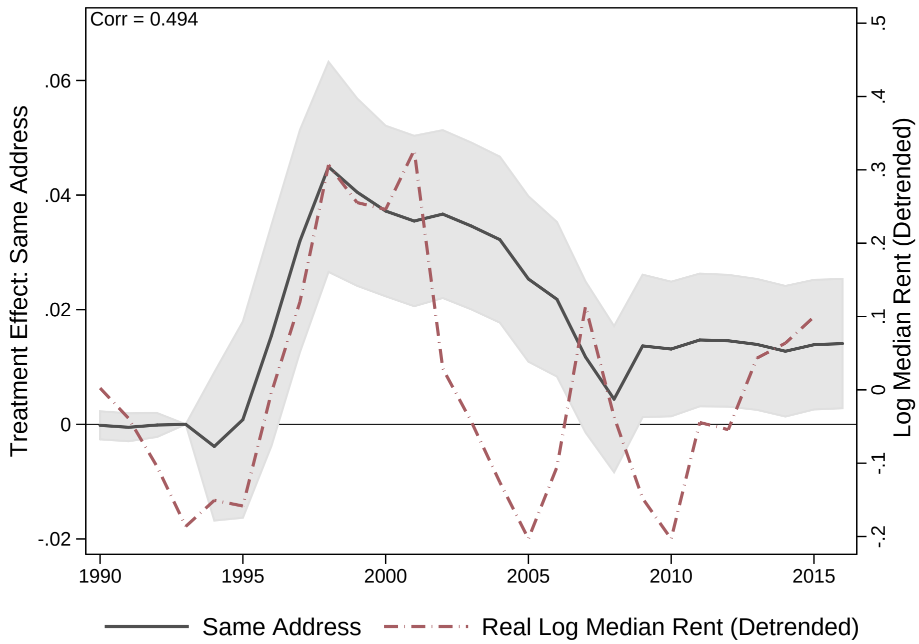
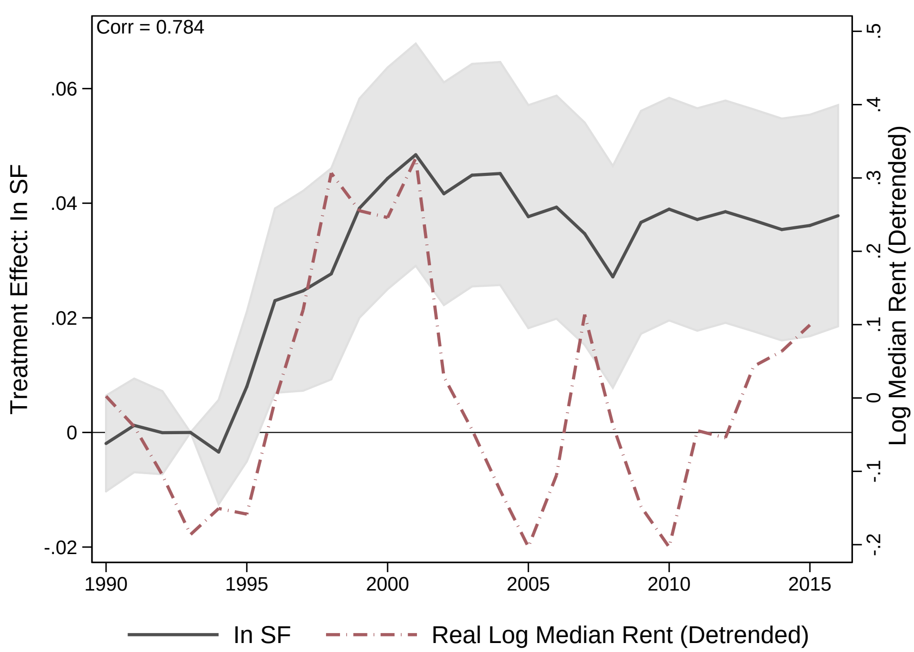


Figure 3: Treatment Effect for Tenants in Multi-Family Residence (2-4 Units)

(a) Staying at Same Address

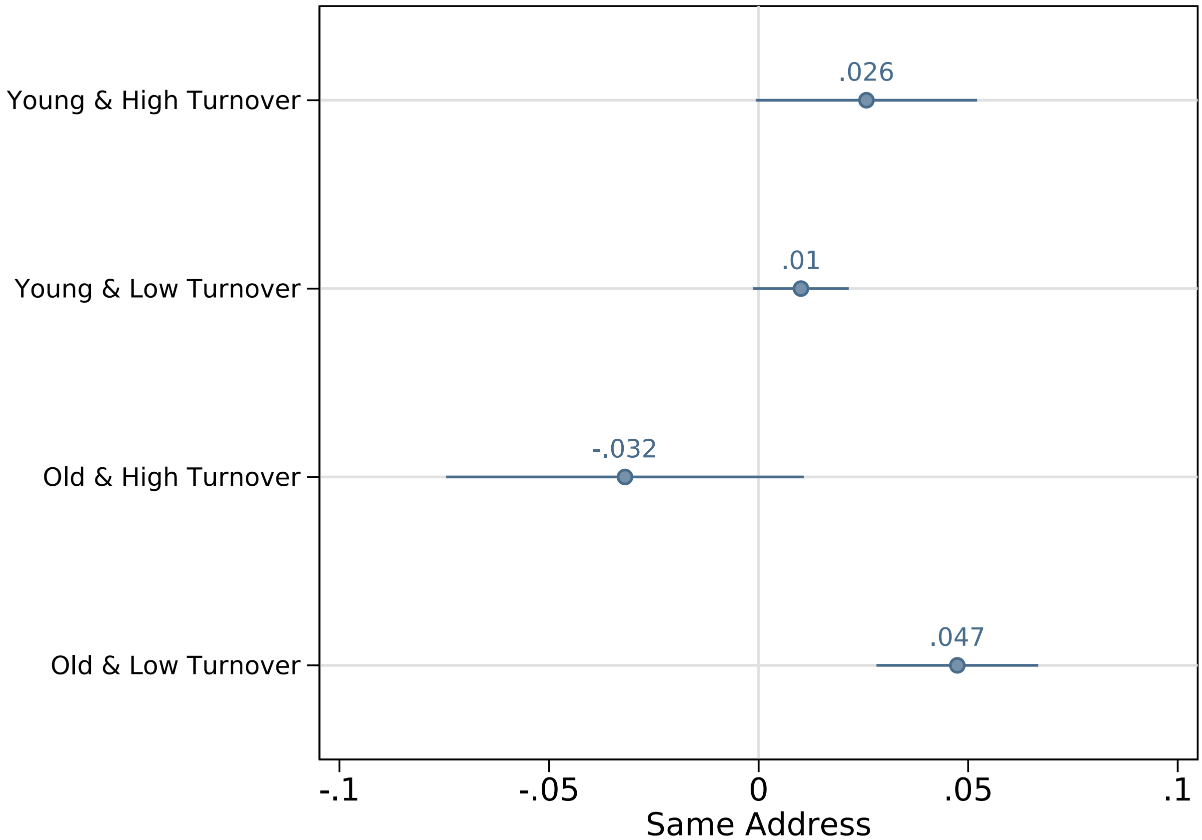



(b) Staying in San Francisco




*Notes*: Sample consists of all tenants between 20 and 65 years old living in SF in 1993 and in small multi-family residences that were built during 1900 – 1990. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. Standard errors are clustered at the person level. Significance levels: * 10%, ** 5%, *** 1%.

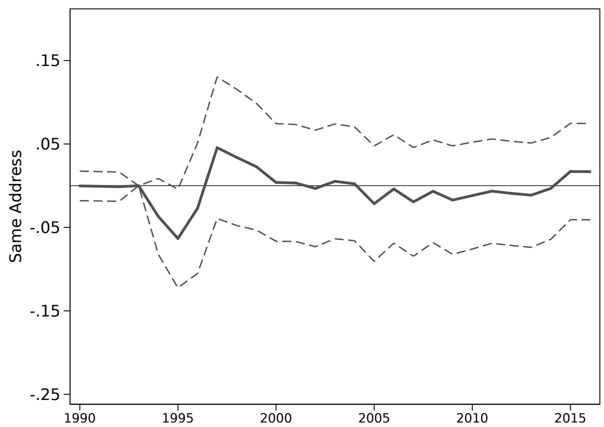
Figure 4: Heterogeneity by Age and Tenure in Treatment Effect for Tenants of Multi-Family Residence (2-4 Units)



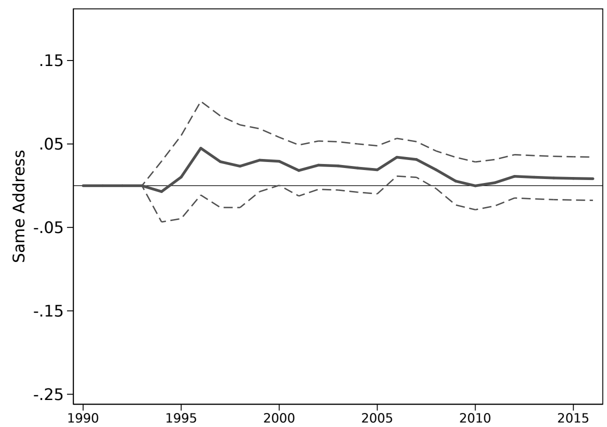

*Notes*: Sample consists of all tenants between 20 and 65 years old living in SF in 1993 and in small multi-family residences that were built during $1900-1990$. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. We 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. We also 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 average treatment effects in the post-1994 period along with 90% CI are plotted. Standard errors are clustered at the person level.

Figure 5: Heterogeneity by Rent Appreciation, Age and Tenure in Treatment Effect for Tenants of Multi-Family Residence (2-4 Units)

(a) High Rent Appreciation, Young and High Turnover

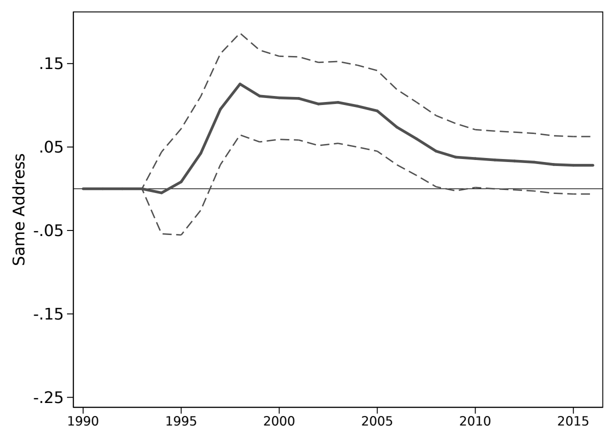



(b) High Rent Appreciation, Young and Low Turnover




(c) High Rent Appreciation, Old and High Turnover

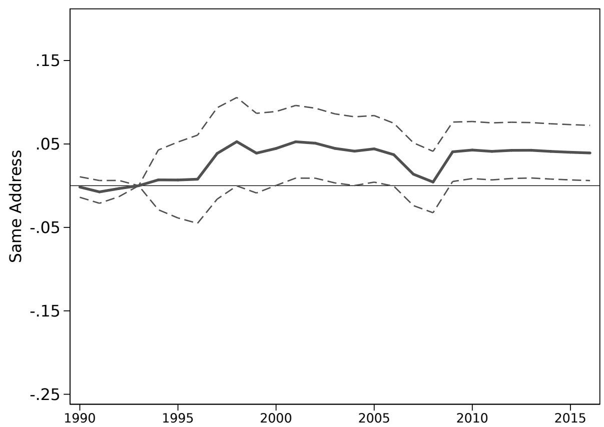

(d) High Rent Appreciation, Old and Low Turnover



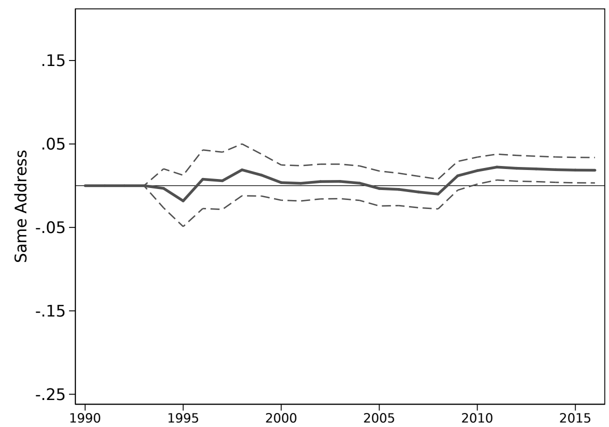

*Notes*: Sample consists of all tenants between 20 and 65 years old living in SF in 1993 and in small multi-family residences that were built during $1900-1990$. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. We first individuals into two groups by whether their 1993 census tract experienced above or below median rent appreciation during 1990-2000. We further sort the sample by age group and tenure following the same definitions as in Figure 4. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the person level.

Figure 6: Heterogeneity by Rent Appreciation, Age and Tenure in Treatment Effect for Tenants of Multi-Family Residence (2-4 Units)

(a) Low Rent Appreciation, Young and High Turnover

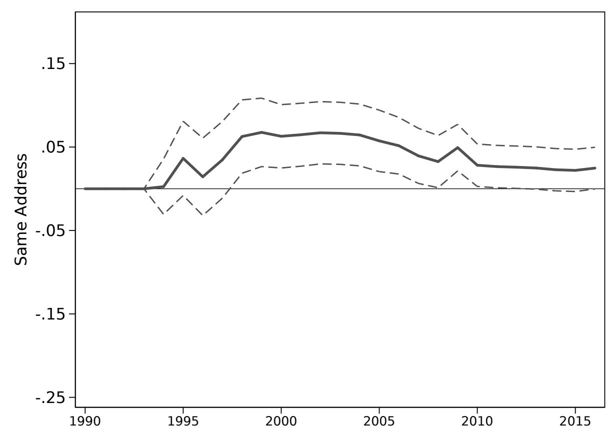



(b) Low Rent Appreciation, Young and Low Turnover



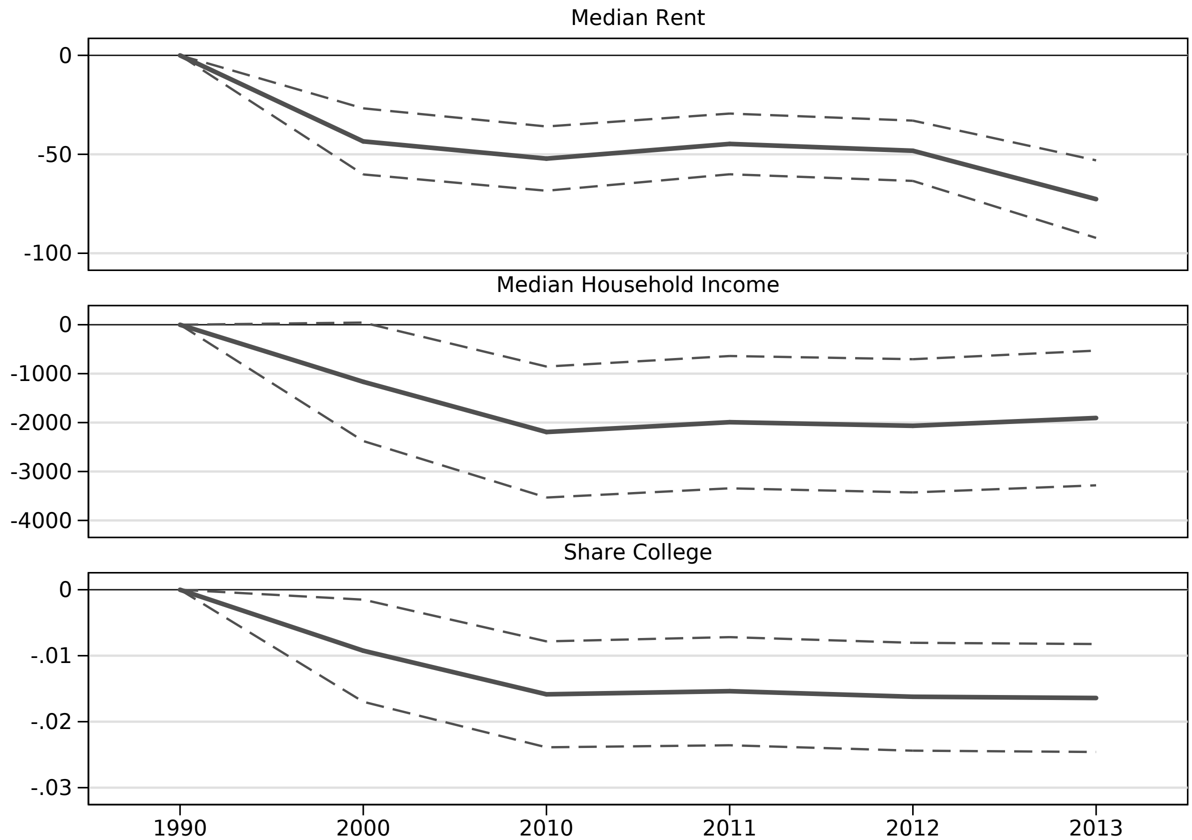

(c) Low Rent Appreciation, Old and High Turnover


(d) Low Rent Appreciation, Old and Low Turnover



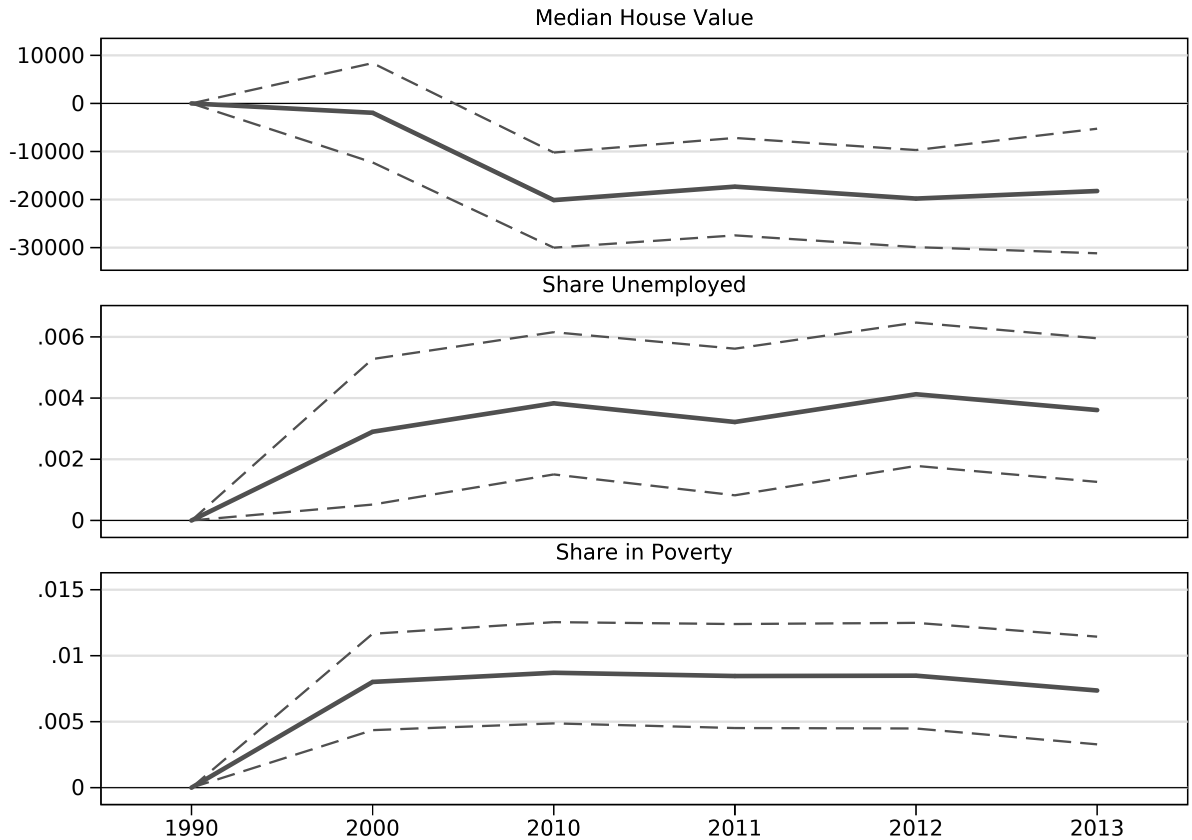

*Notes*: Sample consists of all tenants between 20 and 65 years old living in SF in 1993 and in small multi-family residences that were built during $1900-1990$. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. We first individuals into two groups by whether their 1993 census tract experienced above or below median rent appreciation during 1990-2000. We further sort the sample by age group and tenure following the same definitions as in Figure 4. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the person level.

Figure 7: Treatment Effect at the Census Tracts level for Tenants of Multi-Family Residence (2-4 Units) – Dynamic Version



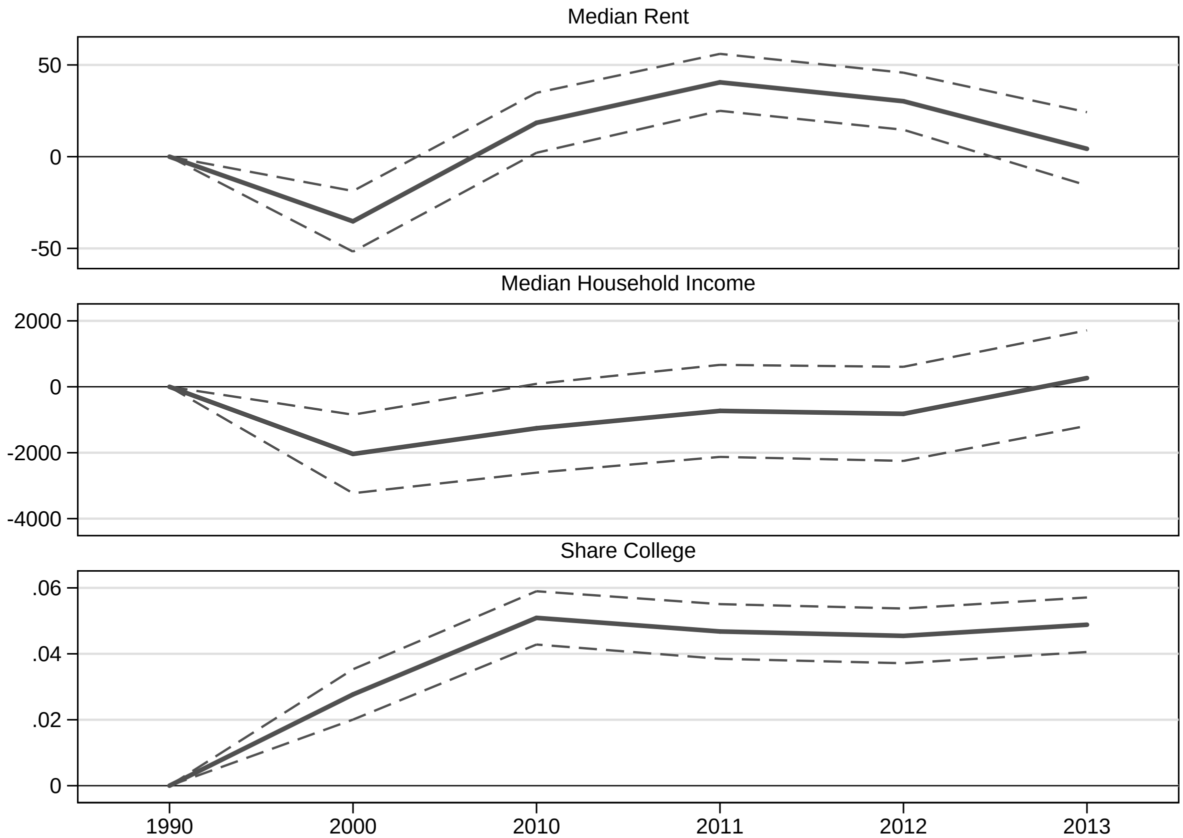

*Notes*: Sample consists of all tenants between 20 and 65 years old living in SF in 1993 and in small multi-family residences that were built during 1900 – 1990. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. Median rent, median household income and share of residents with college education and above are measured in the census tract that an individual is living in a given year. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the person level.

Figure 8: Treatment Effect at the Census Tracts level for Tenants of Multi-Family Residence (2-4 Units) – Dynamic Version



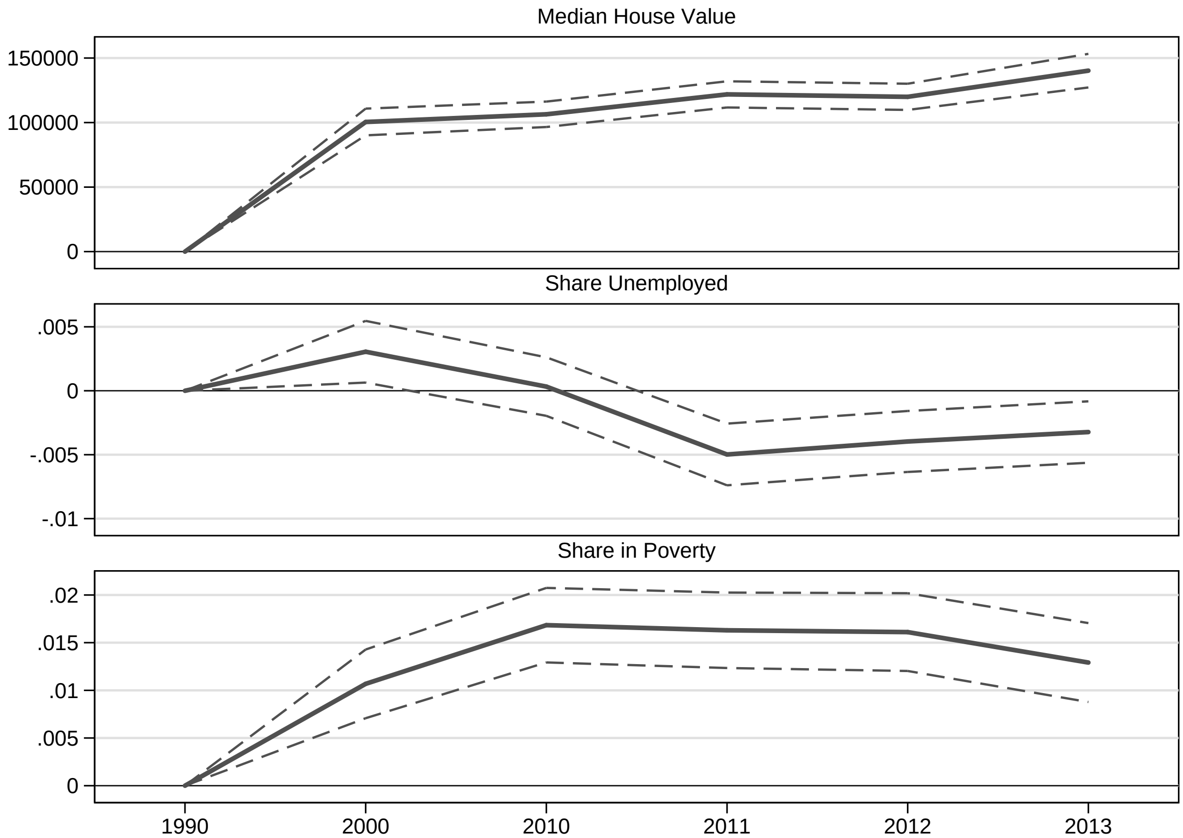

*Notes*: Sample consists of all tenants between 20 and 65 years old living in SF in 1993 and in small multi-family residences that were built during 1900 – 1990. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. Median house value, share of unemployed and share of residents below poverty line are measured in the census tract that an individual is living in a given year. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the person level.

Figure 9: Treatment Effect at the Census Tracts level for Tenants of Multi-Family Residence (2-4 Units) – Static Version



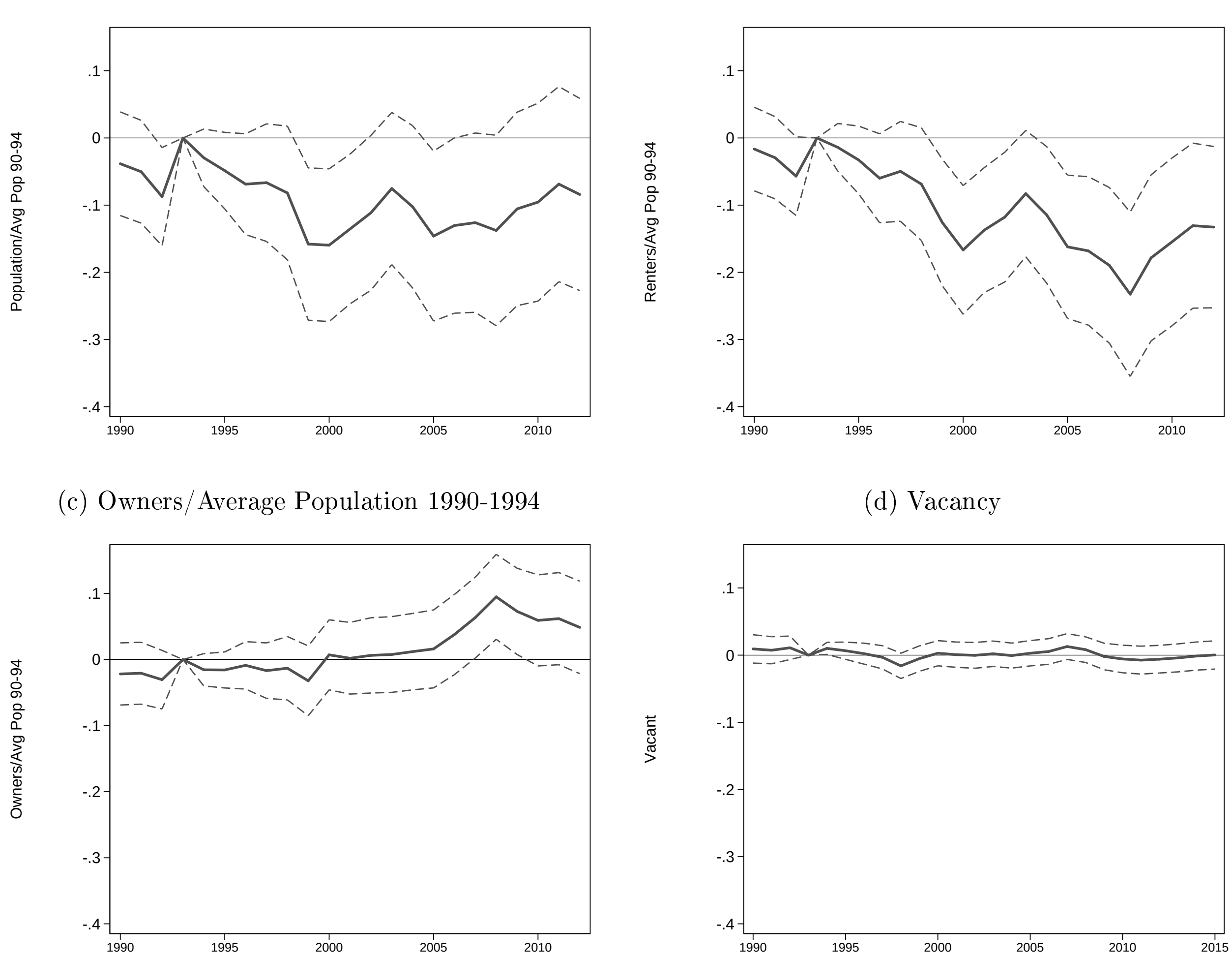

*Notes*: Sample consists of all tenants between 20 and 65 years old living in SF in 1993 and in small multi-family residences that were built during 1900 – 1990. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. Median rent, median household income and share of residents with college education and above are measured in the census tract that an individual is living in a given year for the control group, and are measured in their 1993 census tract for the treated group. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the person level.

Figure 10: Treatment Effect at the Census Tracts level for Tenants of Multi-Family Residence (2-4 Units) – Static Version




*Notes*: Sample consists of all tenants between 20 and 65 years old living in SF in 1993 and in small multi-family residences that were built during $1900-1990$. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. Median house value, share of unemployed and share of residents below poverty line are measured in the census tract that an individual is living in a given year for the control group, and are measured in their 1993 census tract for the treated group. 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

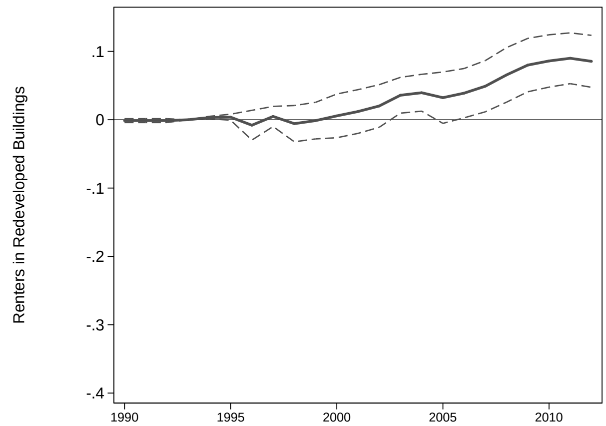
(d) Vacancy

*Notes*: Sample consists of all small multi-family residences that were built during $1900 - 1990$. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the parcel level.

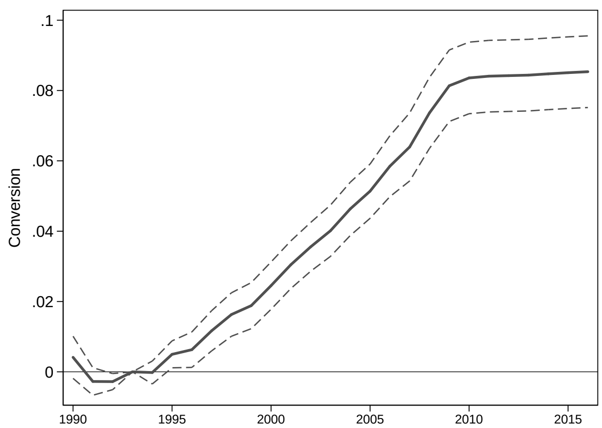
Figure 12: Treatment Effect for Multi-Family Residence (2-4 Units)

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

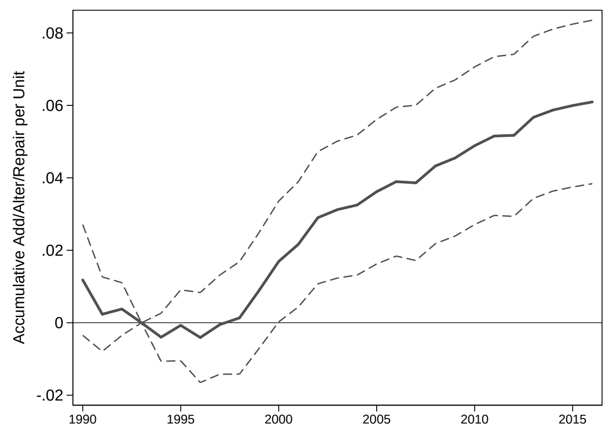

(b) Renters in Redeveloped Buildings/Average Population 1990-1994




(c) Conversion



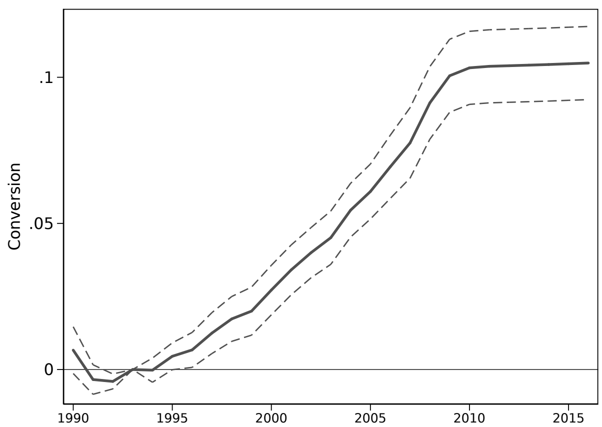

(d) Accumulative Add/Alter/Repair per Unit



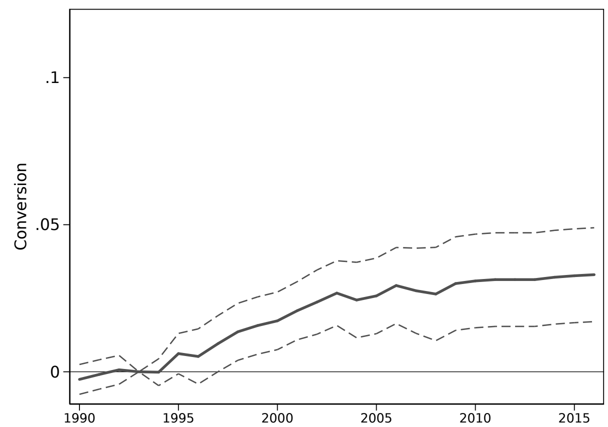

*Notes*: Sample consists of all small multi-family residences that were built during $1900-1990$. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the parcel level.

Figure 13: Heterogeneity by Rent Appreciation in Treatment Effect for Multi-Family Residence (2-4 Units)

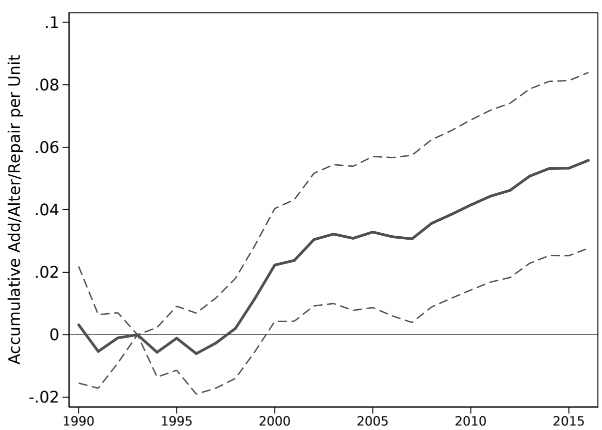
(a) Conversion, High Rent Appreciation



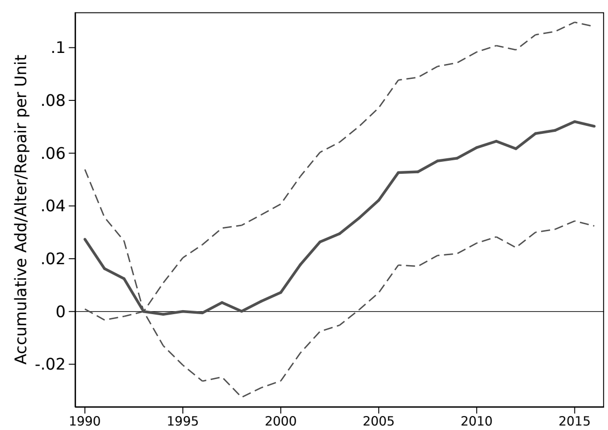

(b) Conversion, Low Rent Appreciation




(c) Accumulative Add/Alter/Repair per Unit, High Rent Appreciation




(d) Accumulative Add/Alter/Repair per Unit, Low Rent Appreciation




*Notes*: Sample consists of all small multi-family residences that were built during $1900 - 1990$. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. We sort our sample by whether their 1993 census tract experienced above or below median rent appreciation during 1990-2000. The treatment effects along with 90% CI are plotted. Standard errors are clustered at the parcel level.

Figure 14: Average Annual Tenant Buyouts

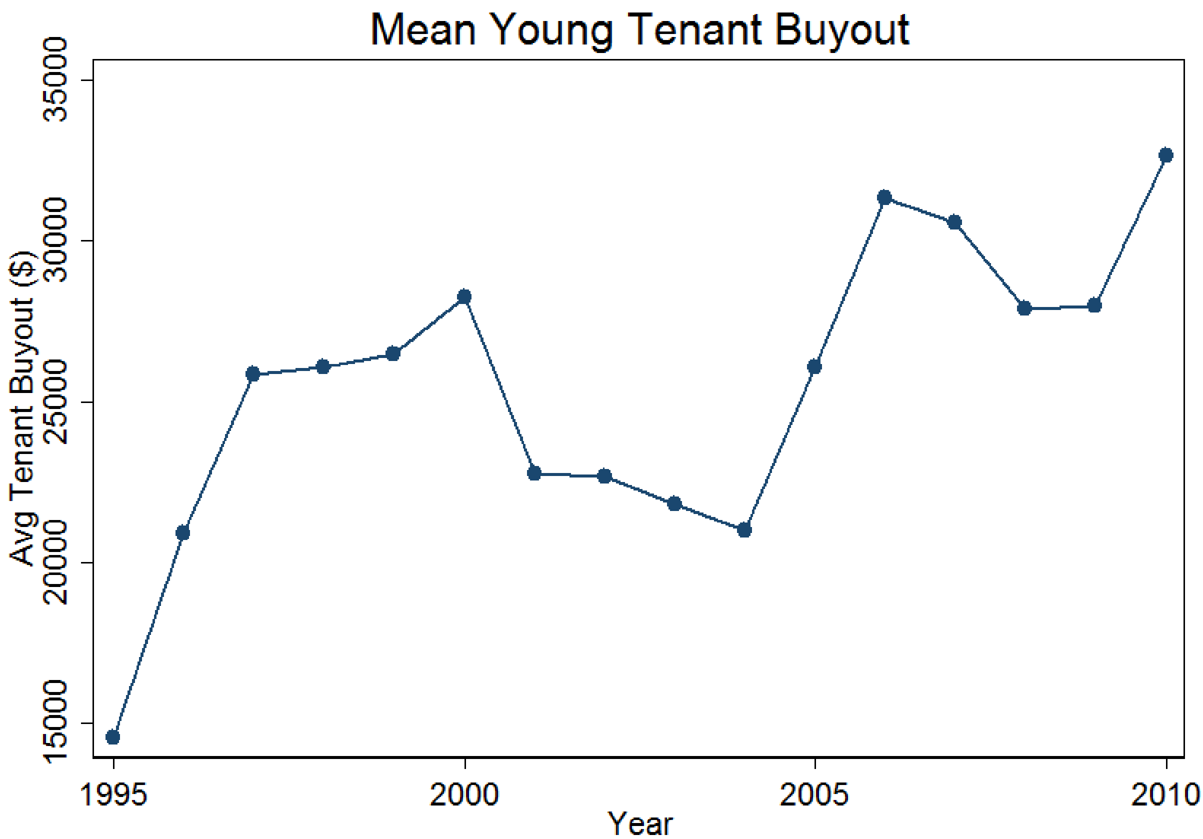
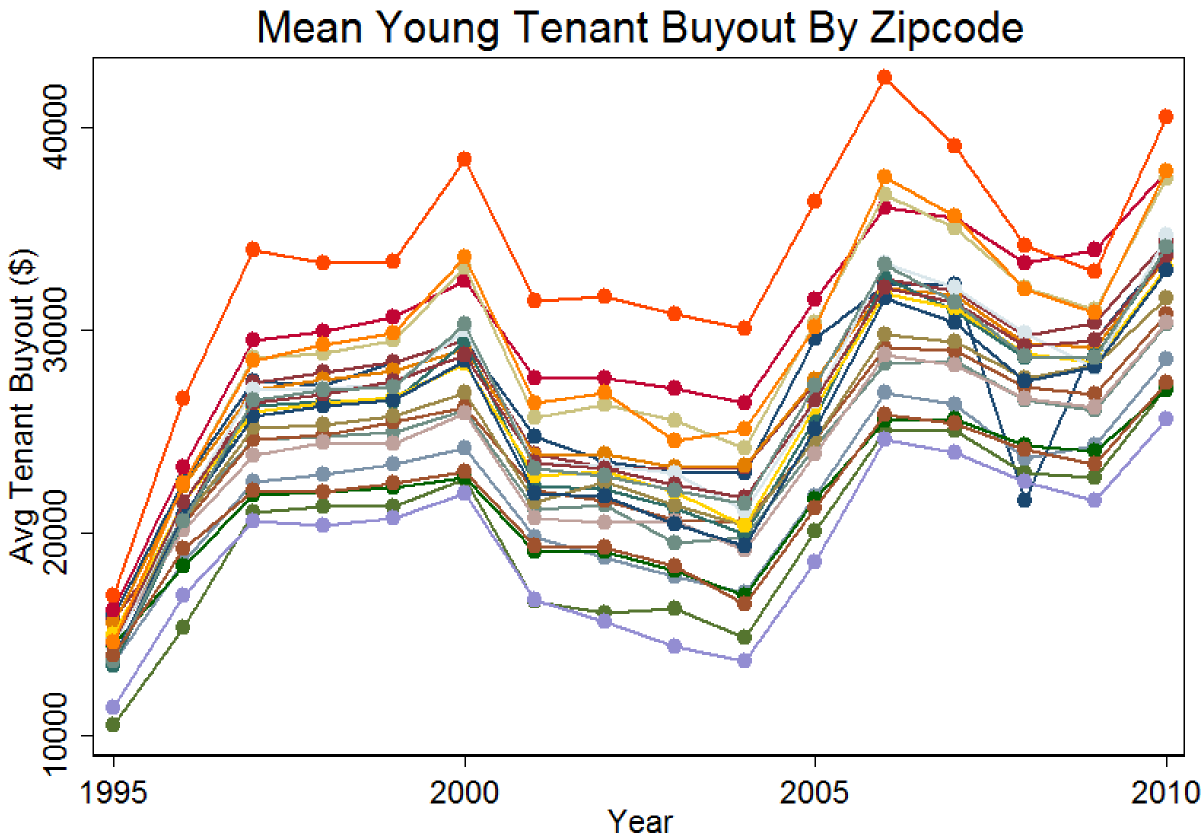


Figure 15: Annual Tenant Buyouts by Zipcode




# A Appendix Tables

## Table A.1: Treatment Effect for Multi-Family Residence (2-4 Units)

| | (1) Permanently Vacant | (2) Vacant | (3) Population/ Avg Pop 90-94 | (4) Renters/ Avg Pop 90-94 | (5) Renters in Rent-Controlled Buildings/ Avg Pop 90-94 | (6) Renters in Redeveloped Buildings/ Avg Pop 90-94 | (7) Owners/ Avg Pop 90-94 | (8) Conversion | (9) Accumulative Add/Alter/Repair per Unit | (10) Ever Received Add/Alter/Repair |
|---|---|---|---|---|---|---|---|---|---|---|
| *Treat*×*Period* | | | | | | | | | | |
| 1994-1999 | -0.0024 | -0.0085 | -0.0329 | -0.0342 | -0.0434 | 0.0005 | 0.0013 | 0.0100*** | -0.0043 | 0.0140* |
| | (0.0046) | (0.0095) | (0.0604) | (0.0472) | (0.0481) | (0.0084) | (0.0299) | (0.0032) | (0.0085) | (0.0081) |
| 2000-2005 | 0.0048 | -0.0064 | -0.0791 | -0.1059* | -0.1516** | 0.0253 | 0.0269 | 0.0384*** | 0.0234* | 0.0413*** |
| | (0.0067) | (0.0114) | (0.0814) | (0.0638) | (0.0654) | (0.0176) | (0.0394) | (0.0045) | (0.0123) | (0.0111) |
| Post 2006 | 0.0084 | -0.0076 | -0.0642 | -0.1453* | -0.2457*** | 0.0717*** | 0.0811** | 0.0793*** | 0.0459*** | 0.0552*** |
| | (0.0094) | (0.0116) | (0.0933) | (0.0747) | (0.0773) | (0.0225) | (0.0409) | (0.0061) | (0.0144) | (0.0130) |
| Control Mean 1994 – 1999 | 0.0293 | 0.0800 | 1.8291 | 1.3540 | 1.3395 | 0.0232 | 0.4752 | 0.3360 | 0.1825 | 0.2352 |
| Control Mean 2000 – 2005 | 0.0469 | 0.0968 | 2.1917 | 1.6278 | 1.5978 | 0.0502 | 0.5639 | 0.3460 | 0.2473 | 0.3066 |
| Control Mean Post 2006 | 0.1035 | 0.1137 | 2.4287 | 1.8338 | 1.7659 | 0.0965 | 0.5949 | 0.3667 | 0.2976 | 0.3624 |
| Adjusted $R^2$ | 0.565 | 0.354 | 0.600 | 0.569 | 0.555 | 0.404 | 0.603 | 0.747 | 0.803 | 0.763 |
| Observations | 724037 | 724037 | 643589 | 643589 | 643589 | 643589 | 643589 | 769181 | 706633 | 724037 |

*Notes*: Sample consists of all small multi-family residences that were built during 1900 – 1990. If a building is constructed post 1993 and we observe a tenant living there in 1993, we include it into the treatment group for rent control. Table reports the mean of dependent variables for the control group during 1994 – 1999, 2000 – 2005 and post-2006. Standard errors are clustered at the parcel level. Significance levels: * 10%, ** 5%, *** 1%.

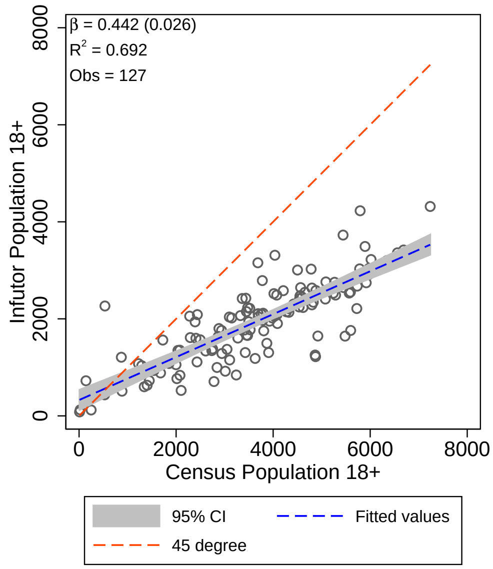
## Table A.2: Model Estimates

A. Model Parameter Estimates in Exp(Rent) Units

| St Dev of Logit Shocks | | Tenant Buyouts | | Moving Costs | | Neighborhood Capital | |
|---|---|---|---|---|---|---|---|
| Young Renters | 2.55*** | Log Below Market Rent | -17.581*** | Fixed Cost | 25.146*** | Young Renters | 0.1292*** |
| | (0.562) | (Young Renters) | (1.398) | (Young Renters) | (5.551) | | (0.0298) |
| Old Renters | 2.271*** | Log Below Market Rent | -15.658*** | Fixed Cost | 22.377*** | Old Renters | 0.1329*** |
| | (0.453) | (Old Renters) | (1.283) | (Old Renters) | (4.444) | | (0.0258) |
| | | Years in Zipcode | 0.671*** | MC per Mile | 4.129*** | | |
| | | (Young Renters) | (0.152) | (Young Renters) | (0.874) | | |
| | | Years in Zipcode | 0.597*** | MC per Mile | 3.685*** | | |
| | | (Old Renters) | (0.122) | (Old Renters) | (0.706) | | |
| | | Old Renter-Direct effect | 4.625*** | $\Delta$MC wrt Yrs in Zip | -0.1258*** | | |
| | | | (0.927) | (Young Renters) | (0.02515) | | |
| | | | | $\Delta$MC wrt Yrs in Zip | -0.112*** | | |
| | | | | (Old Renters) | (0.02029) | | |

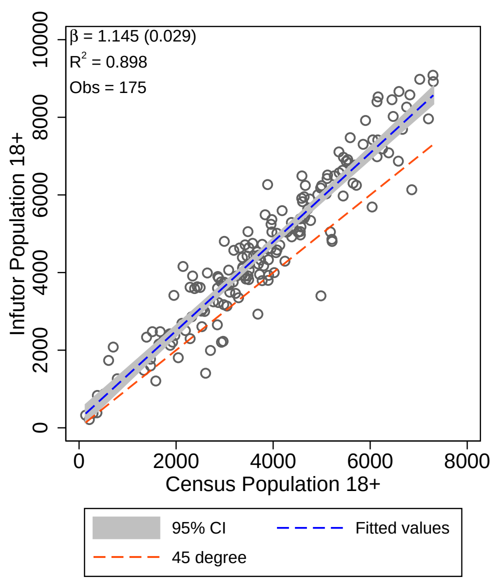
*Notes*: Model parameter estimates in the units of exponential value of monthly rent divided by $\$1,500$.

Figure A.1: Population Age 18 and above: 1990 Census




*Notes*: The size of marker is proportional to the population of 18 and over in the Census in each census tract. The fitted line is by weighted least square using the population of 18 and over in the Census as weights.

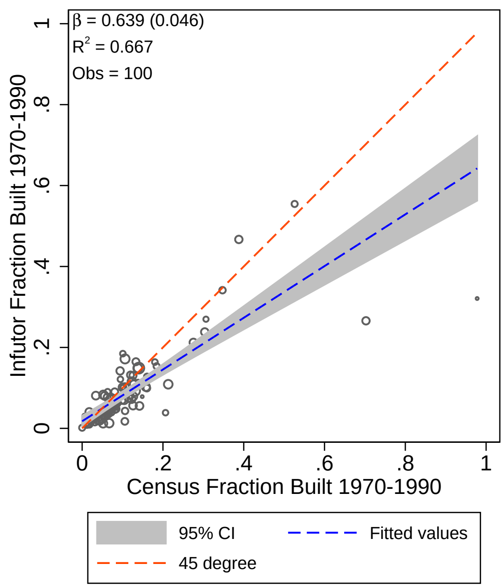
Figure A.2: Population Age 18 and above: 2000 Census



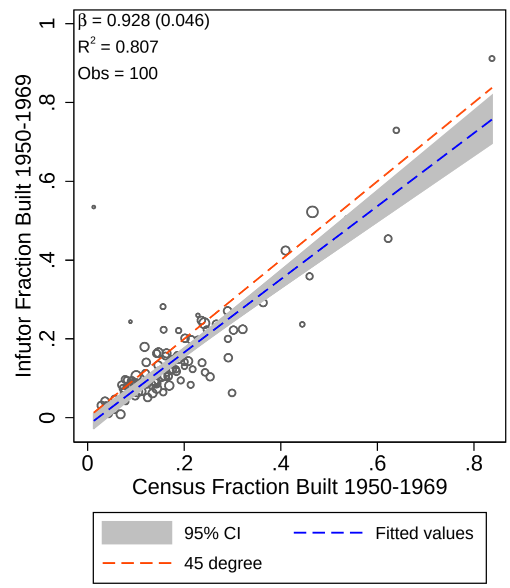

*Notes*: The size of marker is proportional to the population of 18 and over in the Census in each census tract. The fitted line is by weighted least square using the population of 18 and over in the Census as weights.

Figure A.3: Age of Occupied Housing: 1990 Census

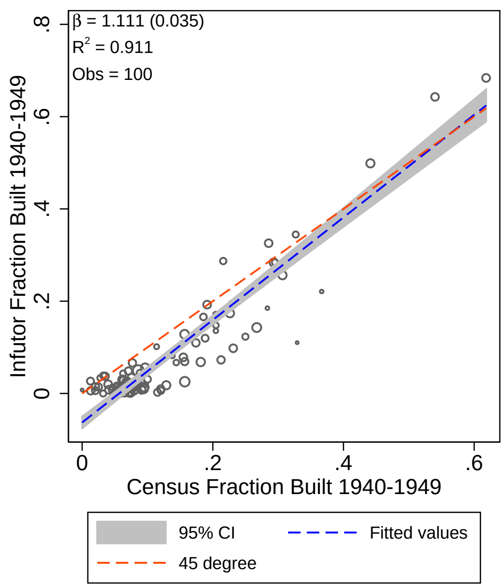
(a) Built 1970 to 1990: 1990 Census



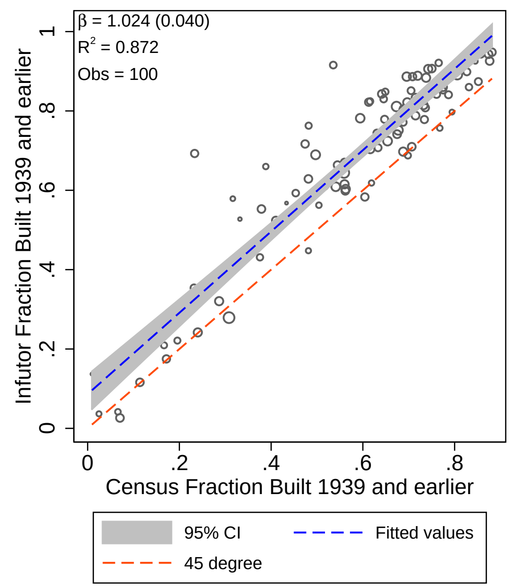

(b) Built 1950 to 1969: 1990 Census




(c) Built 1940 to 1949: 1990 Census



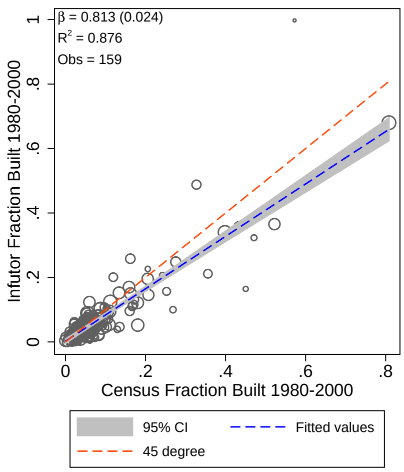

(d) Built 1939 or earlier: 1990 Census



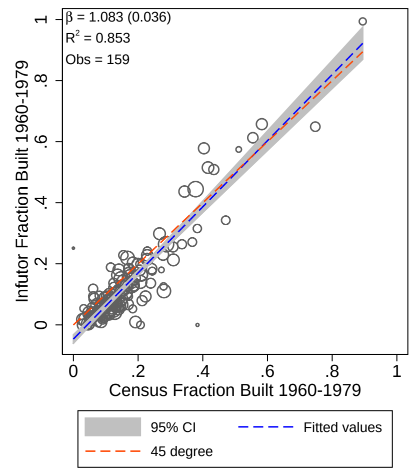

*Notes*: The size of marker is proportional to the number of occupied housing units in each census tract. The fitted line is by weighted least square using the number of occupied housing units as weights.

Figure A.4: Age of Occupied Housing: 2000 Census

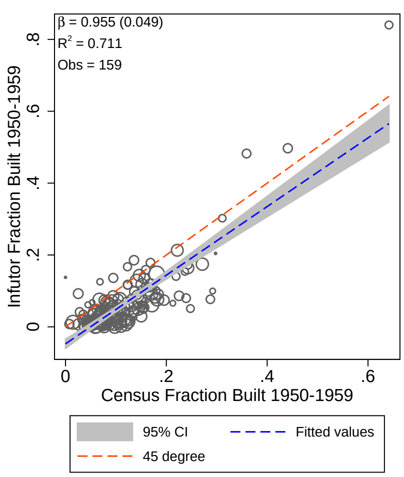
(a) Built 1980 to 2000: 2000 Census



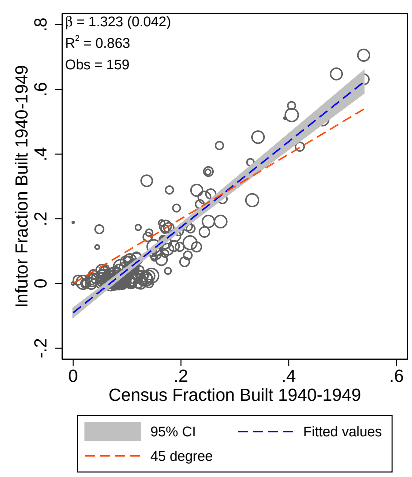

(b) Built 1960 to 1979: 2000 Census



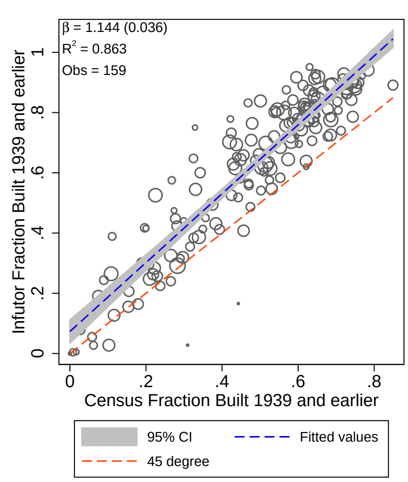

(c) Built 1950 to 1959: 2000 Census




(d) Built 1940 to 1949: 2000 Census

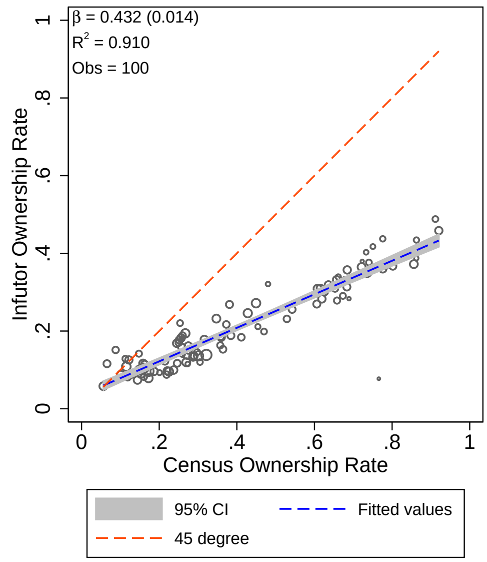



(e) Built 1930 or earlier: 2000 Census



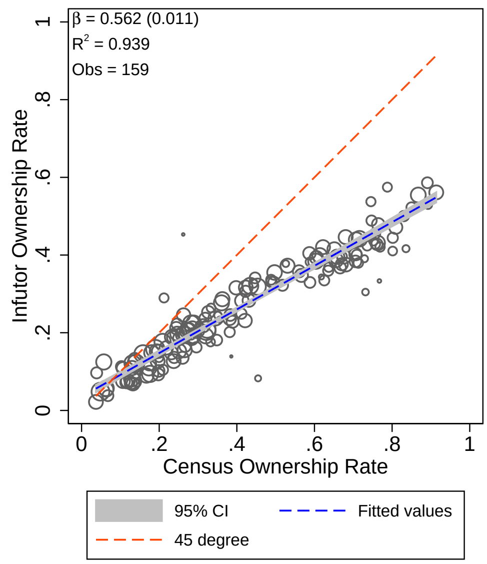

*Notes*: The size of marker is proportional to the number of occupied housing units in each census tract. The fitted line is by weighted least square using the number of occupied housing units as weights.

Figure A.5: Ownership Rate at Person Level: 1990 Census




*Notes*: Plot shows the ownership rate at the person level from our Infutor sample in 1990 against the ownership rate of occupied housing units in 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 A.6: Ownership Rate at Person Level: 2000 Census




*Notes*: Plot shows the ownership rate at the person level from our Infutor sample in 1990 against the ownership rate of occupied housing units in 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.