**The Effect of Rent Control on Eviction Rates: Causal Evidence from San Francisco**

Max Gardner

Department of Civil and Environmental Engineering, University of California, Berkeley, USA

mgardner@berkeley.edu

Max Gardner is a Ph.D. Candidate in Civil Systems Engineering in the Department of Civil and Environmental Engineering at UC Berkeley. His research interests include housing affordability, behavioural modelling of location choice processes in the context of intra-urban migration, and microsimulation for regional transportation and land use forecasting.

The Effect of Rent Control on Eviction Rates: Causal Evidence from San Francisco

This paper presents causal evidence of a significant positive effect of rent control on eviction rates in San Francisco, CA. Using a publicly available dataset of eviction notices (n=21,806) and property tax records (n=1,978,687) filed between 2007 and 2016, I am able to estimate a local average treatment effect of ~1.3% evictions per residential unit per year conditioned on rent control status. Compared to the baseline rate of eviction notices over this same time period, the findings suggest that for a given tenant, positive rent control status (i.e., living in a rent-controlled unit) increases the likelihood of eviction by approximately 240%per year.

Keywords: rent control; eviction; causal inference

# Introduction

## Literature Review

Few policies in the realm of housing and urban economics occupy as prominent a position in the popular consciousness as rent control. Yet until very recently, the number of new, empirical findings on its effectiveness as a regulatory tool have been few and far between[[1]](#footnote-1). Theoretical models proving rent control’s many inefficiencies formed the basis of a decades-long consensus among economists who treated the science as not merely settled but self-evident (Hazlett, 1982; Navarro, 1985; Jenkins, 2009). At the same time, and perhaps paradoxically, a lack of detailed data on tenant and landlord outcomes made it very difficult to disentangle any of the empirical effects of rent control, good or bad, from the other market forces operating in complex urban systems.

The publication of (Arnott, 1995) seems to mark a turning point in the academic literature on rent control. In that paper Arnott argued that modern, “second-generation” rent controls were so nuanced and malleable -- compared to the hard-line rent *freezes* imposed by their first-generation predecessors -- that they defied *a priori* characterization as either good or bad policy. Arnott instead advocated for the use of empirical evidence to evaluate the effects of rent control on a case-by-case basis. Recently, as concerns over gentrification and displacement in America’s “superstar cities” (Gyourko et al., 2013) have sparked renewed interest in the topic of rent control, a new body of empirical research has emerged which takes Arnott’s call as a common point of reference. Instead of asking whether or not rent control “works”, this new literature is focused on more targeted assessments like how rent control affects commute times in New Jersey (Krol & Svorny, 2005), or how the supply of controlled rental housing changes in response to local demand shocks in San Francisco (Asquith, 2019). This paper asks a similarly targeted question about one important aspect of rent regulation: does rent control contribute to higher rates of eviction in San Francisco? The answer to this question, and the findings presented here, have potentially far-reaching ramifications for how we think about a policy designed specifically to *increase* housing stability for incumbent tenants.

This paper makes two significant contributions to the literature. First, despite the fact that tenants are the presumptive beneficiaries of rent control, most quantitative studies of the policy have tended to focus on diffuse, market-wide effects like housing quality, supply, and affordability (Asquith, 2019; Autor et al., 2014; Mense et al., 2018; Sims, 2007). Instead, this intent of this paper is to re-centre tenants and tenant outcomes in the debate over rent control by providing the first causal estimate in the peer-reviewed literature of its effect on eviction rates. Secondly, this paper presents a novel approach for studying the direct effects of rent control by employing a popular causal inference method -- the regression discontinuity (RD) -- in an entirely new setting. Although the results of this particular study are specific to eviction rates in San Francisco, the same methodology can be used to investigate other effects of rent control in the many other jurisdictions where rent control eligibility is determined by similar criteria.

The work presented here builds on two other recent studies of rent control in San Francisco, both of which employ quasi-experimental designs. The first is (Diamond et al., 2019), whichstands out as one the first observational studies to focus explicitly on measuring outcomes for existing tenants under rent control.[[2]](#footnote-2) Despite the authors’ conclusion that rent control has likely worsened the effects of gentrification by reducing the supply of affordable housing available to future residents, they do find that incumbent tenants “benefit on net” as a result of reduced rates of displacement and below-market rents. This latter finding, however, does not directly test for differential rates of forced displacement among controlled and uncontrolled tenants, nor does it account for the potentially greater costs of a forced relocation relative to one that is voluntary or economically induced. In contrast, this paper approaches rent control solely as a housing stability measure, and in doing so offers strong evidence that any attempt to quantify the benefit of rent control must also take into account the effect of rent control-induced eviction on incumbent tenants. Another significant difference is that this study covers the entire stock of rent controlled properties in the city of San Francisco, whereas the identification strategy employed by (Diamond et al., 2019) restricts their analysis to small multifamily properties (<= 4 units). As such, the findings presented here are more general.

The paper most closely resembling this one methodologically is (Asquith, 2019). Asquith leverages very similar datasets of eviction notices and tax assessor records, along with a selection-on-unobservables design to show that landlords decrease the supply of rent-controlled housing via evictions in response to local demand shocks. Rather than observing these shocks, however, the author relies on a secondary model to estimate the hedonic price effects of newly sited transit amenities targeted towards high-income knowledge workers. This two-stage design makes the results of the primary instrumental variables (IV) model difficult to interpret and limits their relevance to the context of local demand shocks. The results of the IV model also depend heavily on the validity of the estimated shocks, which the author concedes are implausibly large. The biggest factor distinguishing this study from Asquith’s, however, is that the actual treatment variable of interest in Asquith’s study is the demand shock, from which it impossible to disentangle the direct effect of rent control. This design works well for the purposes of that study, which is primarily concerned with evictions as a channel through which landlords can manipulate the supply of controlled rental housing. In this sense, the findings of Asquith are not that dissimilar from previous research demonstrating a depressive effect of rent control on housing supply (Autor et al., 2014; Diamond et al., 2019; Sims, 2007). In contrast, this paper treats eviction as a cost that is primarily borne by incumbent tenants rather than a housing market. By centring tenant outcomes instead of market effects, my findings are more relevant to an evaluation of rent control as tool for promoting housing stability.

## Rent Control in San Francisco

Rent control in San Francisco was established in 1979 with the passage of the Residential Rent Stabilization and Arbitration Ordinance, also known as the Rent Ordinance[[3]](#footnote-3). The provisions of the ordinance are typical of second-generationcontrols, which are often (and more accurately) described as rent *stabilization* rather than rent control because they include explicit mechanisms by which rents can be increased over time.[[4]](#footnote-4) Annual allowable rent increases are often pegged to a macroeconomic indicator like the Consumer Price Index (CPI), as is the case for San Francisco.

Beginning in 1995, in accordance with California state law, the SF Rent Ordinance was amended to include a vacancy decontrol provision. Vacancy decontrol is another common feature of second generation rent regulations that allows owners of controlled properties to return their rents to market rate at the start of each new tenancy. Since the inception of vacancy decontrol in California, renters there have worried about the incentive it creates for rent controlled landlords to keep tenant durations short and tenant turnover high (Herscher, 1995). Asquith (2019) and others have shown evidence to suggest that this incentive may in fact be impacting displacement rates for rent controlled tenants, but studies so far have been unable to disentangle the effect of rent control on forced or induced relocation from other regional dynamics.

# Materials and Methods

## Data

The main data source is a database of eviction notices filed with the San Francisco Rent Board between 2007 and 2016. These data (n=21,806) represent the full universe of eviction notices filed by landlords against tenants in San Francisco during that ten-year period. The detailed data used for this study are made available by written request from the San Francisco Rent Board, but a geographically anonymized version of this dataset can be downloaded directly from the city’s open data portal.[[5]](#footnote-5) Most of the eviction records include the eviction type (e.g. failure to pay rent, owner move-in, etc.) but many are not specified (see Table 1 for a summary of the eviction data). Although eviction notices do not necessarily result in an eviction, the notice itself and the threat of eviction can be enough to cause tenants to pre-emptively vacate their residences (Levin, 2020). In this way, eviction notice rates might actually be a better measure of forced displacement pressure than unlawful detainers or writs of restitution.[[6]](#footnote-6)

The dependent variable of interest in this study is eviction notices per residential unit per year across the entire population of San Francisco properties with two or more residential units.[[7]](#footnote-7) The full dataset was assembled by matching eviction notice records against annual parcel-level tax assessor records published by the City and County of San Francisco over the same time period as the eviction notices. After cleaning and standardizing the assessor data, 1,978,687 parcel records are aggregated by year and street address to arrive at a total population of 1,553,397 unique year/address combinations. Unit/apartment numbers are dropped (e.g. “123 Main St #4” becomes “123 Main St”), and the total units are aggregated along with the eviction counts, square footage, and assessed value. After dropping four eviction records due to incomplete or malformed data, I am able to find an exact match in the assessor records for 92.4% of the eviction notices (n=20,136).

Next, the sample is further restricted to include only those addresses that can be reasonably identified as “rent control eligible” according to their assessor designated building class codes. Table 4 describes these class codes and their inferred rent control eligibility, but in general I deem any property with 2+ residential units to be eligible. Of these (n=349,607) I drop an additional 5,094 parcel records (and their associated 296 evictions) due to inconsistent unit counts in the assessor records (e.g. 0 units for a parcel with a multifamily class code). In the end, the sample of observations used for analysis is comprised of 344,513 annual address records, which in total account for 13,963 eviction notices. Table 2 provides descriptive statistics of this dataset.

After restricting the sample to properties with rent control eligible use codes, rent control status can be inferred solely by the year in which a property was built. Given that the Rent Ordinance was passed on June 16, 1979 and that it applied only to structures in existence at that time, rent control eligibility was (and continues to be) extended only to properties built on or before that date.[[8]](#footnote-8) In the following analysis, this arbitrary but well-known delineation between “treated” (i.e. rent controlled) and “control” (i.e. market rate) groups of properties forms the basis for a pseudo-natural experiment in which a treatment effect (i.e. change in eviction rates) can be estimated and causality can be inferred.

## Methodology

To estimate the treatment effect of rent control, I use a regression discontinuity (RD) design, exploiting the 1979 built-year cut-off eligibility requirement described above. The RD design has the dual benefit of directly estimating the treatment effect of rent control on eviction rates, and also making the identification of that treatment extremely transparent and easily understood. The use of RD dates back to 1960 (Thistlethwaite & Campbell, 1960), but its popularity as a causal inference method has gained significantly since the 1990s. Numerous studies comparing the statistical power of the RD against randomized controlled trial (RCT) experimental designs have served to bolster its reputation as an effective substitute in cases where true RCT designs are infeasible, as is often the case in policy analysis. A 2018 meta-analysis of 15 “within-study comparisons”, each of which compared causal estimates obtained from both RD and RCT analysis conducted within the same study, found that the bias of the RD estimates was distributed tightly and symmetrically around zero (within 0.07 standard deviations of the RCT values in a given study on average), concluding that RD is “*robustly internally valid in research practice*” (Chaplin et al., 2018). RD also benefits from an extremely transparent identification mechanism relative to other selection-on-unobservables designs like IV. Standard methods of graphical analysis make interpretation of both the design and its results easily understood by a wide variety of audiences (Imbens & Lemieux, 2008).

The most significant shortcoming of RD relative to other causal inference methods is perhaps the limited set of circumstances in which the design is appropriate. In particular, RD requires a treatment assignment mechanism that depends either wholly or partially on a characteristic threshold value that a participant either exceeds or does not. If treatment *Ri* can be predicted based on whether a variable *Yi* lies above or below a threshold value *c*, then the effect of *Ri* on the outcome *Ei* can be identified given that the relationship between *Yi* and *Ei* is smooth and continuous for values of *Yi* above and below *c*. The premise of RD is that if this latter assumption holds, then the causal effect of *Ri* on *Ei* can be estimated by measuring the size of the “jump” or discontinuity in *Ei* at *Yi – c*.

In this study I implement the “sharp” RD design (Imbens & Lemieux, 2008), where treatment assignment is completely deterministic based on the threshold. The basic functional form is

|  |  |  |
| --- | --- | --- |
|  | *Ei = α + βRi + γ(Yi – c)+ λ(Yi – c) ∙ Ri*  given *Ri* = **1**{*Yi* < *c*}, *c* – *h* < *Yi* < *c* + *h* | (1) |

where the dependent variable *E* is the annual evictions per unit for property *i*, *Y* is the built-year of the property, also known as the “running variable” in RD parlance, *c* is the threshold value (1980 in our case) along the dimension of the running variable, and *R* is a rent-control “treatment” indicator that evaluates to one for properties built prior to 1980 and zero otherwise. The bandwidth parameter *h* identifies the maximum distance between the running variable and the cut-off threshold, beyond which observations are excluded from the sample. Many methods exist to identify an optimal bandwidth, but in repeated tests I found that my estimates of the treatment effect were robust to variation of this value. I ultimately settled on a bandwidth of 27 because this limits the sample to buildings constructed between 1953 and 2007, which ensures that only buildings with a full 10 years of history over the period of observation (2007-2016) are included in the analysis.

# Results

## Empirical Analysis

### Eviction Notices

Table 1 summarizes the eviction records (n=13,963) used in the main analysis after segmenting by eviction type category and their built-year relative to the 1980 cut-off. ***[Table 1 near here]***. The left two columns represent eviction notices filed in rent-controlled properties, while the right two columns represent those filed in uncontrolled properties. The first thing that stands out is that there are nearly two orders of magnitude (~73x) more notices filed in rent-controlled properties. More than anything this number reflects the fact that of the 344,513 property records used in the analysis only 14,132 (4.1%) were for properties built after 1979, a fact which itself is explained by the diminishing construction rate of multifamily housing in San Francisco over time (Figure 1). ***[Figure 1 near here]***

More interesting, however, is the fact that No-fault evictions constitute a much higher percentage of eviction notices at rent-controlled properties compared to the uncontrolled sector.[[9]](#footnote-9) These findings are consistent with Asquith (2019), and support the idea that San Francisco’s rent control laws may be incentivizing controlled landlords to evict law-abiding tenants. Also of interest is the fact that Breach of Lease or At-fault evictions constitute a smaller portion of evictions in rent controlled properties, which may suggest that rent control is actually achieving one of its primary objectives of keeping tenants from failing to pay their rent. Unfortunately, there are too many uncategorized eviction notices in the dataset to say anything more conclusive on the subject.

### Mean Differences

Table 2 compares the counts and averages of the aggregate assessor records by rent control eligibility and built-year threshold. By normalizing the data according to the number of observations in each category, average outcomes can be compared between the treatment and control groups to obtain a reliable first indication of what a more detailed model might reveal. The results show a mean difference of +0.77% in the rate of eviction notices for rent controlled addresses relative to their uncontrolled counterparts. Though small in magnitude, this difference corresponds to an eviction rate that is 2.4x higher for rent controlled addresses on an annual, per-unit basis. ***[Table 2 near here]***

### Graphical Analysis

Figure 2 shows the traditional RD plot, comparing average observed outcomes on the Y-axis across binned values of the running variable (built-year) on the X axis for the range defined by the bandwidth parameter *h*. The data points on either side of the built-year threshold are used to fit two linear models that highlight the discontinuous nature of this relationship. Visual inspection shows the size of the discontinuity in Figure 2 closely approximates not only the mean difference computed from Table 1 (~0.77%), but also the size of treatment effects estimated by the RD models below (0.09% - 1.41%).

The data presented in Figure 2 also clearly show that a significant and unique discontinuity exists at the threshold, adding substantive, visual evidence of a real causal effect. They also suggest that apart from the discontinuity itself, there exists almost no correlation between eviction rates and built-year. This lends additional credibility to a causal interpretation because it increases the likelihood of treatment assignment (i.e. rent control status) being the only channel through which systematic variation in eviction rates explains variation in the property built-year. ***[Figure 2 near here]***

## Model Results

Four RD models are fit according to the approach described above. Each of the four models estimates a positive treatment effect on rent control significant at the 0.002 level or below. After applying the bandwidth parameter and filtering out records with malformed or incomplete covariate data, a total of 53,493 observations are used to estimate these models. The results are summarized in Table 3. ***[Table 3 near here]***

Using the specification described in Equation 1, Model 1 estimates an average treatment effect of ~0.9% (p=0.002). This estimate is slightly larger than the mean difference-based estimate (0.77%), but still very much in line with the size of the discontinuity observed in Figure 2.

Model 2 adds two property-level characteristics derived from the assessor records: 1) an interaction term between the log of the assessed value and the log of the assessed square footage of the property; and 2) the log of the total number of units at the property. The value-per-square-foot interaction term is found to positively correlate with eviction rates, while the coefficient on total units is negative, suggesting eviction rates are higher in more valuable properties with fewer units. This result makes intuitive sense, as more valuable properties can likely fetch higher market rate rents, and in buildings with fewer units the potential benefit of evicting one tenant represents a higher proportion of a property’s total value to the landlord. Both of these variables are found to be significant at the level of <0.001. Their inclusion in the model produces a larger treatment effect estimate of 1.36% and improves the significance level from 0.002 to <0.001.

Model 3 adds Census tract-based demographic characteristics from the 2009-2013 5-year American Community Survey. A positive coefficient (p=<0.001) on the percent of occupied units that are rented rather (non-owner occupied) suggests that eviction rates are higher in areas with lower rates of home ownership. Percent Latino also has a positive coefficient (p=<0.001), indicating a greater probability of eviction in areas with greater concentrations of Latinos. The addition of these sociodemographic characteristics did not significantly affect the estimated coefficient on rent control (1.35%) which remained significant at the p < 0.001 level. Alternative model specifications were tested using other Census-based covariates, including median household income and median move-in year. The latter term was included in order to test for the potential effect of tenancy duration, which in the context of vacancy decontrol means more heavily discounted rents and thus a greater incentive to evict. I found no evidence to suggest that such an effect exists, although it is possible that tract-level Census data is simply not granular enough to capture this relationship. It is also possible that landlords may be less willing to initiate economically motivated evictions against tenants with whom they have long-standing relationships.

Model 4 adds neighbourhood fixed effects to the equation according to the 72 assessor-designated neighbourhoods that appear in the assessor records. These terms are a blunt instrument designed to account for any other geographic variation not captured by the Census-based sociodemographic variables. The addition of neighbourhood fixed effects in a slightly higher estimated treatment effect of ~1.41%, still significant at the < 0.001 level.

# Discussion

## Internal Validity

One of the biggest limitations of the RD design is that it is applicable under only a very limited set of experimental conditions. Fortunately, because the identification strategy is so transparent, there are many well-documented methods available for investigating the internal validity of a given RD design. I will now briefly mention these, which in addition to the small standard errors and p-values reported in the previous section suggest that this study has avoided some of the biggest pitfalls of the RD method.

First, RD assumes that the treatment effect is the only discontinuity in an otherwise smooth functional form describing the relationship between the running variable and the outcome. Figure 2 demonstrates this fact visually. The strength of this assumption is further supported by the fact that Figure 2 shows a nearly flat response in the y-axis, and also that each of the four RD models fails to reject the null hypothesis that the coefficient on year-built is zero. Together, these results suggest that apart from treatment effect itself, the relationship between built-year and eviction rates is effectively random. Although RD does not *require* that the running variable be uncorrelated with the outcome, the fact that in this study the treatment assignment itself seems to be the only channel through which variation in eviction rates is related to the property built-year significantly strengthens the case for a causal interpretation.

Another common source of bias in RD designs occurs when treatment assignment can be manipulated by study participants. This occurs when subjects are aware of the cut-off threshold that determines treatment assignment (e.g. policy eligibility), and are able to nudge themselves past that threshold in the dimension of the running variable in order to qualify for treatment. Manipulation of this kind would introduce a structural imbalance in the sample population immediately above and below the threshold value, invalidating the experimental design. In the case of this study, despite the fact that both landlord and tenant are typically aware of both the age of their property and the threshold for rent control eligibility, neither party has the means to change the construction date as recorded by the county assessor’s office.

Sensitivity to bandwidth selection is another common specification test used in RD analysis. Whereas overly narrow bandwidths might overestimate the significance of the variation observed at the discontinuity, too-wide of a bandwidth might bias the results by including observations that are irrelevant to behaviour at the discontinuity. Accordingly, RD estimates that are robust to this somewhat-arbitrarily chosen parameter are much more credible than those that are heavily dependent upon it (Imbens & Lemieux, 2008). In Section 3 it was briefly mentioned that treatment effect estimates in this study were not sensitive to variation in bandwidth.

In the context of an extremely contentious and highly visible policy like rent control, the importance of having a transparent identification strategy and easily interpreted experimental results cannot be overstated.

## Policy Implications (External Validity)

To the best of this author’s knowledge, the findings presented in this paper constitute the first rigorous estimate of the causal effect of rent control on eviction rates in the peer-reviewed literature. That being said, it is important to understand the context in which these findings are most relevant.

The treatment effect estimated via RD is a *local* average treatment effect (LATE), rather than the more general average treatment effect (ATE) associated with a true RCT design. In other words, the RD estimate is not guaranteed to be unbiased for observations outside of the subpopulation where the treatment effect is measured (*Yi = c*). In the present study that means one must be careful in extending the validity of the model results to properties built in the 1920s, for example, which depends on the degree to which a homogenous treatment effect can be assumed. However, given the seemingly stochastic nature of the relationship between built-year and eviction rates in this study, this does not seem like an unreasonable assumption to make.

Additionally, recent research indicates that certain precautions can reduce the likelihood that RD estimates are biased, many of which -- including large sample sizes, the use of nonparametric tests and estimators, and careful bandwidth selection -- were implemented in this study (Chaplin et al., 2018; Gelman & Imbens, 2019). It is also worth noting that regardless of whether or not the results are relevant to very old or very new buildings in San Francisco, the estimated treatment effect is clearly only valid for the city of San Francisco. That being said, the implications of these findings are highly relevant to current and future policy in San Francisco and beyond.

In 2020 California voters had the chance to repeal the 1995 law restricting municipalities from enacting rent control on residential units constructed after February 1995.[[10]](#footnote-10) Had it passed, Proposition 21 would have been replaced the 1995 restriction with a rolling 15-year window, allowing San Francisco legislators to expand rent control eligibility to buildings built before 2006 by 2021, 2007 by 2022, and so on. Even though Prop 21 was ultimately rejected by voters, the pressure to expand rent control in California does not appear to be dissipating, as indicated most recently by the passage of AB 1482, the Tenant Protection Act of 2019.[[11]](#footnote-11) If and when the time comes for San Francisco to reassess its rent control eligibility requirements, the results presented here will be directly applicable for both policymakers, residents, and property owners wishing to evaluate the impact of such a policy change.

Increased housing stability is one area in which experts typically agree that rent control offers real benefits (Diamond et al., 2019; Glaeser & Luttmer, 2003). In fact, a recent review of the rent control literature published by the USC Equity Research Institute (ERI) found that “nearly every academic study finds that rent stabilization [...] increases housing stability for rent-stabilized residents” (Pastor et al., 2018). However, the findings presented here show that the effects of rent control on housing stability are more heterogenous than the current academic literature suggests. While the ERI review offers strong evidence in favour of re-evaluating rent control as an anti-*displacement* measure rather than anti-gentrification, the reality is that even under this more appropriately defined rubric its effectiveness in San Francisco is unclear.

As we have seen, Diamond et al. (2019) were able to estimate a net benefit of $393 million per year for incumbent rent controlled tenants in San Francisco, but failed to account for the cost of rent control-induced evictions. And while it may seem unlikely that a 1-2% increase in the rate of evictions would do much to offset such a large benefit, the severity of the impact of an eviction must not be underestimated. Recent data from the City of San Francisco itself suggests that eviction is the third-leading cause of homelessness there, representing 13% of survey respondents (n=1,039), up from just 4% in 2011 (Applied Survey Research, 2019). Additionally, recent work by Desmond and others has demonstrated the deleterious, sometimes trans-generational effects of eviction on health outcomes, homelessness, and job retention (Desmond, 2012; Desmond & Gershenson, 2016; Desmond & Kimbro, 2015). Long term effects like these make it a very difficult and fraught task to estimate the true cost of rent control-induced eviction to rent controlled tenants.

Whatever those costs may be, it would be a mistake to interpret them as a failure that is inherent to Rent Control as a policy. Rather the causal effect of rent control on eviction rates as measured in this study represents a failure of San Francisco’s Rent Ordinance to adequately protect tenants from the incentive to evict that exists under vacancy decontrol. Fortunately, a solution to this problem exists that does not require abolishing vacancy decontrol, a move that would almost certainly be politically unfeasible. Instead, the city must enact stronger eviction protections for rent controlled tenants. For example, eviction could be treated as a disqualifying event, preventing a property owner from taking advantage of vacancy decontrol at the start of their next lease. San Francisco policymakers have the ability to close these eviction-based rent control loopholes, and have previously made strides in this direction with respect to No-fault evictions in particular (Riley, 2013). With local and state-wide COVID-19 eviction moratoriums expiring soon, it is now more important than ever that City officials re-examine the role that evictions play in undermining the success of rent stabilization.

# Acknowledgements

I would like to thank Professors Paul Waddell and Carolina Reid, as well as Timothy Thomas, Ph.D. for their guidance and encouragement, as well as Erin McElroy and Dan Sakaguchi at AEMP for their help assembling the eviction notification data.

**Declaration of Interest Statement**

There are no relevant financial or non-financial competing interests to report.

# References

Ambrosius, J. D., Gilderbloom, J. I., Steele, W. J., Meares, W. L., & Keating, D. (2015). Forty years of rent control: Reexamining New Jersey’s moderate local policies after the great recession. *Cities*, *49*, 121–133.

Applied Survey Research. (2019). *San Francisco Homeless Count & Survey* (San Francisco Homeless Point-in-Time Count & Survey). San Francisco Department of Homelessness and Supportive Housing. https://hsh.sfgov.org/wp-content/uploads/2020/01/2019HIRDReport\_SanFrancisco\_FinalDraft-1.pdf

Arnott, R. (1995). Time for revisionism on rent control? *Journal of Economic Perspectives*, *9*(1), 99–120.

Asquith, B. (2019). *Do Rent Increases Reduce the Housing Supply under Rent Control? Evidence from Evictions in San Francisco*.

Autor, D. H., Palmer, C. J., & Pathak, P. A. (2014). Housing market spillovers: Evidence from the end of rent control in Cambridge, Massachusetts. *Journal of Political Economy*, *122*(3), 661–717.

Chaplin, D. D., Cook, T. D., Zurovac, J., Coopersmith, J. S., Finucane, M. M., Vollmer, L. N., & Morris, R. E. (2018). The internal and external validity of the regression discontinuity design: A meta-analysis of 15 within-study comparisons. *Journal of Policy Analysis and Management*, *37*(2), 403–429.

Desmond, M. (2012). Eviction and the reproduction of urban poverty. *American Journal of Sociology*, *118*(1), 88–133.

Desmond, M., & Gershenson, C. (2016). Housing and employment insecurity among the working poor. *Social Problems*, *63*(1), 46–67.

Desmond, M., & Kimbro, R. T. (2015). Eviction’s fallout: Housing, hardship, and health. *Social Forces*, *94*(1), 295–324.

Diamond, R., McQuade, T., & Qian, F. (2019). *The Effects of Rent Control Expansion on Tenants, Landlords, and Inequality: Evidence from San Francisco*.

Early, D. W., & Olsen, E. O. (1998). Rent control and homelessness. *Regional Science and Urban Economics*, *28*(6), 797–816.

Gelman, A., & Imbens, G. (2019). Why high-order polynomials should not be used in regression discontinuity designs. *Journal of Business & Economic Statistics*, *37*(3), 447–456.

Glaeser, E. L., & Luttmer, E. F. (2003). The misallocation of housing under rent control. *American Economic Review*, *93*(4), 1027–1046.

Gyourko, J., & Linneman, P. (1990). Rent controls and rental housing quality: A note on the effects of New York City’s old controls. *Journal of Urban Economics*, *27*(3), 398–409.

Gyourko, J., Mayer, C., & Sinai, T. (2013). Superstar cities. *American Economic Journal: Economic Policy*, *5*(4), 167–199.

Hazlett, T. (1982). Rent Controls and the housing Crisis. *Resolving the Housing Crisis: Government Policy, Decontrol and the Public Interest*, *277*.

Herscher, E. (1995, July 27). *Berkeley Renters Vow to Resist End of Controls*. SFGATE. https://www.sfgate.com/news/article/Berkeley-Renters-Vow-to-Resist-End-of-Controls-3027981.php

Heskin, A. D., Levine, N., & Garrett, M. (2000). The effects of vacancy control: A spatial analysis of four California cities. *Journal of the American Planning Association*, *66*(2), 162–176.

Imbens, G. W., & Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of Econometrics*, *142*(2), 615–635.

Jenkins, B. (2009). Rent control: Do economists agree? *Econ Journal Watch*, *6*(1).

Krol, R., & Svorny, S. (2005). The effect of rent control on commute times. *Journal of Urban Economics*, *58*(3), 421–436.

Levin, S. (2020, April 17). *Sick, elderly, pregnant: The California renters being evicted even during the pandemic*. The Guardian. http://www.theguardian.com/world/2020/apr/17/sick-elderly-pregnant-the-california-renters-being-evicted-even-during-the-pandemic

Maharawal, M. M., & McElroy, E. (2018). The anti-eviction mapping project: Counter mapping and oral history toward bay area housing justice. *Annals of the American Association of Geographers*, *108*(2), 380–389.

Mense, A., Michelsen, C., & Kholodilin, K. (2018). *Empirics on the causal effects of rent control in Germany*.

Moon, C.-G., & Stotsky, J. G. (1993). The effect of rent control on housing quality change: A longitudinal analysis. *Journal of Political Economy*, *101*(6), 1114–1148.

Murray, M. P., Rydell, C. P., Barnett, C. L., Hillestad, C. E., & Neels, K. (1991). Analyzing rent control: The case of Los Angeles. *Economic Inquiry*, *29*(4), 601–625.

Nagy, J. (1995). Increased Duration and Sample Attrition in New York City′ s Rent Controlled Sector. *Journal of Urban Economics*, *38*(2), 127–137.

Nagy, J. (1997). Do vacancy decontrol provisions undo rent control? *Journal of Urban Economics*, *42*(1), 64–78.

Navarro, P. (1985). Rent control in cambridge, mass. *The Public Interest*, *78*, 83.

Pastor, M., Carter, V., & Abood, M. (2018). Rent Matters: What are the Impacts of Rent Stabilization Measures? *Los Angeles: USC Dornsife Program for Environmental and Regional Equity*.

*Reference: Assessor-Recorder Property Class Codes | DataSF | City and County of San Francisco*. (n.d.). Retrieved June 26, 2021, from https://data.sfgov.org/Housing-and-Buildings/Reference-Assessor-Recorder-Property-Class-Codes/pa56-ek2h

Riley, N. J. (2013, June 12). *Condo conversion law OKd by S.F. board*. SFGATE. https://www.sfgate.com/bayarea/article/Condo-conversion-law-OKd-by-S-F-board-4594985.php

Sims, D. P. (2007). Out of control: What can we learn from the end of Massachusetts rent control? *Journal of Urban Economics*, *61*(1), 129–151.

Thistlethwaite, D. L., & Campbell, D. T. (1960). Regression-discontinuity analysis: An alternative to the ex post facto experiment. *Journal of Educational Psychology*, *51*(6), 309.

# Appendix A.

Eviction Frequency by Type and Built-Year Cut-off

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  |  | **built year <= 1979** | | **built year > 1979** | |
| **eviction type** | **eviction category** | *count* | *%* | *count* | *%* |
| unknown | unknown/other | 5066 | 36.8 | 44 | 23.3 |
| Breach of Lease Agreement | Breach of lease | 3031 | 22.0 | 66 | 34.9 |
| Nuisance | Breach of lease | 1621 | 11.8 | 28 | 14.8 |
| Owner Move-in (OMI) | No-fault | 1177 | 8.5 | 3 | 1.6 |
| Capital Improvement | No-fault | 518 | 3.8 | -- | -- |
| Non-payment of Rent | Breach of lease | 487 | 3.5 | 11 | 5.8 |
| Habitual Late Payment of Rent | Breach of lease | 399 | 2.9 | 4 | 2.1 |
| ELLIS | No fault | 346 | 2.5 | -- | -- |
| Illegal Use of Unit | Breach of lease | 199 | 1.4 | 1 | 0.5 |
| Breach of Lease Agreement Nuisance | Breach of lease | 182 | 1.3 | 3 | 1.6 |
| Roommate Living in Same Unit | Breach of lease | 119 | 0.9 | 3 | 1.6 |
| Unapproved Subtenant | Breach of lease | 110 | 0.8 | 1 | 0.5 |
| Other | unknown/other | 108 | 0.8 | 18 | 9.5 |
| Demolition | No fault | 94 | 0.7 | -- | -- |
| Denial of Access to Unit | Breach of lease | 54 | 0.4 | 2 | 1.1 |
| Nuisance Illegal Use of Unit | Breach of lease | 46 | 0.3 | -- | -- |
| Breach of Lease Agreement Illegal Use of Unit | Breach of lease | 34 | 0.2 | -- | -- |
| Breach of Lease Agreement Nuisance Illegal Use of Unit | Breach of lease | 24 | 0.2 | -- | -- |
| Non-payment of Rent Breach of Lease Agreement | Breach of lease | 14 | 0.1 | -- | -- |
| Failure to Sign Lease Renewal | Breach of lease | 19 | 0.1 | -- | -- |
| Denial of Access to Unit Breach of Lease Agreement | Breach of lease | 10 | 0.1 | -- | -- |
| Condo Conversion | No fault | 17 | 0.1 | -- | -- |
| Habitual Late Payment of Rent Breach of Lease Agreement | Breach of lease | 11 | 0.1 | -- | -- |
| Non-payment of Rent Habitual Late Payment of Rent | Breach of lease | 10 | 0.1 | 1 | 0.5 |
| Unapproved Subtenant Breach of Lease Agreement | Breach of lease | 8 | 0.1 | -- | -- |
| Substantial Rehabilitation | No fault | 7 | 0.1 | -- | -- |
| Breach of Lease Agreement Roommate Living in Same Unit | Breach of lease | 1 | 0.0 | -- | -- |
| Breach of Lease Agreement Other | Breach of lease | 5 | 0.0 | 1 | 0.5 |
| Roommate Living in Same Unit Nuisance | Breach of lease | 6 | 0.0 | -- | -- |
| Development Agreement | No fault | 1 | 0.0 | -- | -- |
| Denial of Access to Unit Breach of Lease Agreement Nuisance | Breach of lease | 4 | 0.0 | -- | -- |
| Unapproved Subtenant Breach of Lease Agreement Nuisance | Breach of lease | 2 | 0.0 | -- | -- |
| Unapproved Subtenant Breach of Lease Agreement Nuisance Illegal Use of Unit | Breach of lease | 1 | 0.0 | -- | -- |
| Unapproved Subtenant Illegal Use of Unit | Breach of lease | 1 | 0.0 | -- | -- |
| Unapproved Subtenant Nuisance | Breach of lease | 1 | 0.0 | -- | -- |
| Unapproved Subtenant Nuisance Illegal Use of Unit | Breach of lease | 2 | 0.0 | -- | -- |
| Nuisance Capital Improvement | Breach of lease | 1 | 0.0 | -- | -- |
| Non-payment of Rent Nuisance | Breach of lease | 2 | 0.0 | -- | -- |
| Non-payment of Rent Unapproved Subtenant | Breach of lease | 1 | 0.0 | -- | -- |
| Non-payment of Rent Other | Breach of lease | 1 | 0.0 | -- | -- |
| Denial of Access to Unit Other | Breach of lease | 2 | 0.0 | -- | -- |
| Non-payment of Rent Habitual Late Payment of Rent Nuisance | Breach of lease | 1 | 0.0 | -- | -- |
| Non-payment of Rent Habitual Late Payment of Rent Breach of Lease Agreement Nuisance | Breach of lease | 1 | 0.0 | -- | -- |
| Non-payment of Rent Denial of Access to Unit | Breach of lease | 1 | 0.0 | -- | -- |
| Non-payment of Rent Breach of Lease Agreement Nuisance | Breach of lease | 1 | 0.0 | -- | -- |
| Denial of Access to Unit Breach of Lease Agreement Nuisance Illegal Use of Unit | Breach of lease | 3 | 0.0 | -- | -- |
| Breach of Lease Agreement Failure to Sign Lease Renewal | Breach of lease | 3 | 0.0 | -- | -- |
| Denial of Access to Unit Breach of Lease Agreement Other | Breach of lease | 1 | 0.0 | -- | -- |
| Habitual Late Payment of Rent Nuisance | Breach of lease | 4 | 0.0 | -- | -- |
| Habitual Late Payment of Rent Breach of Lease Agreement Nuisance | Breach of lease | 6 | 0.0 | -- | -- |
| Habitual Late Payment of Rent Breach of Lease Agreement Failure to Sign Lease Renewal | Breach of lease | 1 | 0.0 | -- | -- |
| Denial of Access to Unit Nuisance | Breach of lease | 2 | 0.0 | -- | -- |
| Good Samaritan Tenancy Ends | No fault | 2 | 0.0 | -- | -- |
| Lead Remediation | No fault | 6 | 0.0 | -- | -- |
| Habitual Late Payment of Rent Other | Breach of lease | -- | -- | 1 | 0.5 |
| Habitual Late Payment of Rent Roommate Living in Same Unit Nuisance | Breach of lease | -- | -- | 1 | 0.5 |
| Nuisance Other | Breach of lease | -- | -- | 1 | 0.5 |

1. Notable exceptions include (Gyourko & Linneman, 1990; Murray et al., 1991; Moon & Stotsky, 1993; Nagy, 1995, 1997; Early & Olsen, 1998; Heskin et al., 2000; Glaeser & Luttmer, 2003; Krol & Svorny, 2005; Sims, 2007). [↑](#footnote-ref-1)
2. Early & Olsen (1998), Krol & Svorny (2005), and Ambrosius et al. (2015) could be considered exceptions, but these rely on census data rather than disaggregate tenant observations. [↑](#footnote-ref-2)
3. San Francisco Administrative Code Chapter 37 [↑](#footnote-ref-3)
4. In the context of this paper, I will use both terms (“stabilization” and “control”) in reference to the San Francisco Rent Ordinance. [↑](#footnote-ref-4)
5. In this case, the eviction records were graciously provided to the author, unaltered, courtesy of the Anti-Eviction Mapping Project (Maharawal & McElroy, 2018). [↑](#footnote-ref-5)
6. This paper will use both “eviction” and “eviction notice” in reference to the Rent Board data. [↑](#footnote-ref-6)
7. Properties with a tenancy-in-common (TIC) use code are excluded from the analysis. [↑](#footnote-ref-7)
8. Since the assessor records only provide the property built year, and not month or day, I consider all properties built in 1979 to be rent controlled. By potentially including uncontrolled properties in the sample of controlled properties, it is possible that the estimated treatment effect is conservative in magnitude. However, repeated tests of the models with and without properties built in 1979 did not significantly alter the results. [↑](#footnote-ref-8)
9. Here I use “No-fault” to describe any of the following nine eviction types: owner move-in (OMI), capital improvement, Ellis Act, condo conversion, substantial rehabilitation, lead remediation, good Samaritan tenancy ends, development agreement, and demolition. All other evictions, except for those where the eviction type was not indicated, are considered “At-fault” or “Breach of lease” evictions. See Appendix A For a full enumeration of eviction types, categories, and observations. [↑](#footnote-ref-9)
10. See [California Civil Code 1954.50-1954.535](https://leginfo.legislature.ca.gov/faces/codes_displayText.xhtml?lawCode=CIV&division=3.&title=5.&part=4.&chapter=2.7.&article) [↑](#footnote-ref-10)
11. See <https://leginfo.legislature.ca.gov/faces/billTextClient.xhtml?bill_id=201920200AB1482> [↑](#footnote-ref-11)