**Does Rent Control Cause Higher Eviction Rates? New Evidence from San Francisco**

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**1. Introduction**

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 showcasing 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 [[11]–[13]](https://www.zotero.org/google-docs/?SfoOYX). 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. So while theory dominated the academic debate (to the extent that it can be said a debate existed), a combination of ideology and first-hand experience shaped the conversation among those who actually had skin in the game.

The publication of [[14]](https://www.zotero.org/google-docs/?JcnU7a) by Arnott 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 hardline rent *freezes* imposed by their first generation precursors -- 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” [[15]](https://www.zotero.org/google-docs/?IwEheS) 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 [[9]](https://www.zotero.org/google-docs/?hSRCv7), or how the supply of controlled rental housing changes in response to local demand shocks in San Francisco [[16]](https://www.zotero.org/google-docs/?jnEqZG). This paper adopts that same ethos in asking a question that similarly seeks to tease out one important dimension of rent regulation: does rent control contribute to higher rates of eviction in San Francisco? Despite this narrowness of the question, the findings presented here, which suggest that San Francisco renters are more than *2x* as likely to be evicted if they have rent control, have potentially far-reaching ramifications for how we think about a policy designed specifically to *increase* housing stability for these same tenants.

The 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 its diffuse, market-wide effects (e.g. housing quality and supply, rental prices, etc. [[10], [16]–[18]](https://www.zotero.org/google-docs/?3DUvx1)). By focusing on eviction rates, this paper re-centers tenant outcomes, providing the first estimates of the causal effect of rent control 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 -- 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 other jurisdictions where rent control status is determined by a property built-year cutoff.

Literature Review

There are two recent papers in the rent control literature, that bear a close resemblance to this one and are therefore worth briefly mentioning. Beyond the fact that both of these papers center their quasi-experimental studies of rent control in the San Francisco market, [[19]](https://www.zotero.org/google-docs/?De4199)stands out as the first paper to do so with the express purpose of measuring tenant outcomes.[[2]](#footnote-2) That study convincingly demonstrates that rent control in San Francisco has led to a significant reduction in displacement for incumbent tenants, especially tenants of color. However, the authors wade into muddier waters with their conclusion that on net, the goals of the San Francisco rent control ordinances were undermined by an accompanying decrease in the supply of affordable rental housing which in their view has actually made gentrification *worse*. Although the empirical findings of [[19]](https://www.zotero.org/google-docs/?xBnydJ) appear to be sound, its summary conclusion about the effectiveness of the policy hinges on: a) a very specific definition of the word gentrification; and b) the tenuous assumption that it is the goal of rent control to curb that gentrification in the first place. In contrast, this paper approaches rent control solely as a housing stability measure, and offers causal evidence of additional, tenant-centered effects that were not considered in that research. Another significant difference is that our study covers the entire stock of rent controlled properties in the city of San Francisco, whereas [[19]](https://www.zotero.org/google-docs/?ibUlFh) only includes buildings with four or fewer residential units, making our findings more general.

The paper most closely resembling this one methodologically is [[16]](https://www.zotero.org/google-docs/?0vmv5T) by Asquith. Asquith leverages a very similar dataset of San Francisco eviction notices and tax assessor property records, as well as an instrumental variables (IV) design, to show that landlords decrease the supply of rent-controlled housing via evictions in response to local demand shocks. The contributions of my study are distinct from those of [[16]](https://www.zotero.org/google-docs/?OsYTAi) in a few significant ways. First, my results serve as a check on those of [[16]](https://www.zotero.org/google-docs/?711jDv), many of which are not statistically significant, and are further tempered by the author’s concession that the price shocks used in his model are implausibly large[[3]](#footnote-3). In contrast, my results indicate a statistically significant increase in the annual probability of eviction for residential units under rent control under status quo market conditions. The second key difference, not unrelated to the first, is that the findings of [[16]](https://www.zotero.org/google-docs/?a3wfY9) are only applicable in the context of local demand shocks, whereas my results are more broadly relevant. Although Asquith does include a rent control status in his IV design, the actual treatment variable of interest is a local demand shock, rather than rent control, making it impossible to disentangle the direct effect of rent control itself. This design is also much less interpretable because the author relies on a secondary model that estimates the hedonic price effects of the introduction of transit amenities targeted towards high-income knowledge workers to proxy for the local demand shocks. In contrast, I use a regression discontinuity (RD) design with rent control status as the treatment. The RD design has the benefit of directly estimating the treatment effect of rent control on eviction rates, while also making the identification of that treatment extremely transparent and easily understood. In the context of an extremely contentious and highly visible policy like rent control, the importance of these points cannot be overstated. Lastly, by treating evictions as a channel through which the supply of controlled rental housing can be manipulated, the findings of [[16]](https://www.zotero.org/google-docs/?gCVX0f) are not that dissimilar from previous research (e.g. [[10], [17], [19]](https://www.zotero.org/google-docs/?QNIQ34), etc.) that has demonstrated a depressive effect of rent control on housing supply. Although the use of eviction data to study this phenomenon is certainly a novel approach, it is also problematic for two reasons: 1) the types of evictions used in that study do not result in permanent decontrol and as such should be treated as only temporary reductions in supply[[4]](#footnote-4); and 2) it assumes that the cost of evictions is one borne by the market, ignoring the severe ramifications of being evicted for the tenant [[21]](https://www.zotero.org/google-docs/?3ATGkW). By centering tenant outcomes instead of market effects, my findings are more relevant to an evaluation of rent control as effective policy for promoting housing stability among incumbent tenants, putting aside for the moment the question of downstream costs to future residents.

**2. 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=47,763) 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[[5]](#footnote-5), but a geographically anonymized version of this dataset can be downloaded directly from the city’s open data portal[[6]](#footnote-6). Most of the eviction records include the reason for the eviction but many are not specified (see Tables 1 and 2 for a summary of the eviction data). Although eviction notices do not necessarily result in an eviction, it is common for tenants faced with a notice to pre-emptively vacate their residences in order to avoid the legal ramifications associated with an actual eviction. In this way, eviction notices are in many ways a better measure of the pressures faced by tenants than a similar dataset of unlawful detainers or writs of restitution would be. The terms eviction and eviction notice will be used interchangeably to refer to the Rent Board data.

The outcome variable of interest in this study is eviction notices per residential unit per year across the universe of San Francisco parcels with two or more residential units.[[7]](#footnote-7) The full dataset was assembled by matching eviction records against annual parcel-level tax assessor records published by the City and County of San Francisco between 2007 and 2016. After cleaning and standardizing the assessor data, 2,182,926 parcel records are aggregated by year and street address to arrive at a total population of 1,711,141 unique year/address combinations. After dropping 43 eviction records due to incomplete or malformed data, I am able to find an exact match in the assessor records for 94.9% of the eviction notices (n=45,273).

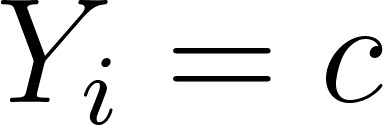
|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| M034102 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 11/6/2003 | POINT (-122.37332 37.83162) |
| M040862 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 5/19/2004 | POINT (-122.37332 37.83162) |
| M040863 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 5/19/2004 | POINT (-122.37332 37.83162) |
| M041095 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 6/18/2004 | POINT (-122.37332 37.83162) |
| M041096 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 6/18/2004 | POINT (-122.37332 37.83162) |
| M041097 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 6/18/2004 | POINT (-122.37332 37.83162) |
| M041098 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 6/18/2004 | POINT (-122.37332 37.83162) |
| M041099 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 6/18/2004 | POINT (-122.37332 37.83162) |
| M041351 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 7/23/2004 | POINT (-122.37332 37.83162) |
| M050558 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 3/31/2005 | POINT (-122.37332 37.83162) |
| M050872 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 5/17/2005 | POINT (-122.37332 37.83162) |
| M081924 | 1200 Block Of North Point Street | San Francisco | CA | 94130 | 12/5/2008 | POINT (-122.37332 37.83162) |
| M100991 | 1200 Block Of North Point Street | San Francisco | CA | 94123 | 7/23/2010 | POINT (-122.37332 37.83162) |

This sample is then further restricted to include only those addresses that can be reasonably identified as “rent control eligible” according to their assessor designated building class codes (see Appendix A for a description of these class codes). In general, “rent control eligible” properties are those with 2+ residential units whose rent control status otherwise depends only on whether or not it was constructed prior to 1980 in accordance with the San Francisco Rent Control Ordinance. Of these, I drop an additional 5,680 parcel records (and their associated 434 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 includes 29,749 eviction notices across 378,380 annual address records. See Table 3 for descriptive statistics of this dataset.

Beyond the count of residential units, the San Francisco rent control ordinance uses a built-year cutoff to determine rent control status. Being that the law took effect in 1980 and applied only to structures in existence at that time, rent control eligibility was (and continues to be) extended only to properties built in 1979 and earlier. This somewhat arbitrary but well-known delineation between “treated” (i.e. rent controlled) and “control” (i.e. market rate) groups of properties forms the basis for framing this analysis as a pseudo-natural experiment in which a treatment effect (i.e. change in eviction rates) can be estimated and causality can be inferred.

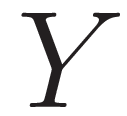
**3. Methodology**

The particular quasi-experimental design exploited in this study is known as regression discontinuity (RD). The use of RD dates back to 1960 [[22]](https://www.zotero.org/google-docs/?fPK60X), 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*” [[23]](https://www.zotero.org/google-docs/?5yt97V). RD also benefits from an extremely transparent identification mechanism relative to other selection on unobservables designs like IV. Standard methods of graphical analysis like those shown in the following section make interpretation of both the design and its results easily understood by a wide variety of audiences [[24]](https://www.zotero.org/google-docs/?nOLBjI). Compared to other causal inference methods, its most significant shortcoming is perhaps the limited set of circumstances in which the RD design is appropriate. In particular, RD requires a treatment assignment mechanism that depends wholly or in-part by a characteristic threshold value that a participant either exceeds or does not.

The basic premise of RD is that for observational data where “treatment” [](https://www.codecogs.com/eqnedit.php?latex=R_i#0) can be predicted based on whether a variable [](https://www.codecogs.com/eqnedit.php?latex=Y_i#0) lies above or below a threshold cutoff value [](https://www.codecogs.com/eqnedit.php?latex=c#0), the effect of [](https://www.codecogs.com/eqnedit.php?latex=R_i#0) on the outcome [](https://www.codecogs.com/eqnedit.php?latex=E_i#0) can be identified as long as the relationship between [](https://www.codecogs.com/eqnedit.php?latex=Y_i#0) and [](https://www.codecogs.com/eqnedit.php?latex=E_i#0) is smooth and continuous for values of [](https://www.codecogs.com/eqnedit.php?latex=Y_i#0) above and below [](https://www.codecogs.com/eqnedit.php?latex=c#0). If this latter assumption holds, then the causal effect of [](https://www.codecogs.com/eqnedit.php?latex=R_i#0) on [](https://www.codecogs.com/eqnedit.php?latex=E_i#0) can be estimated by measuring the size of the “jump” or discontinuity in [](https://www.codecogs.com/eqnedit.php?latex=E_i#0) at [](https://www.codecogs.com/eqnedit.php?latex=Y_i%20%3D%20c#0). In this study I implement the “sharp” RD design [[24]](https://www.zotero.org/google-docs/?qu9OOV), where treatment assignment is completely deterministic based on the threshold. The basic functional form is

[](https://www.codecogs.com/eqnedit.php?latex=%20E_i%20%3D%20%5Calpha%20%2B%20%5Cbeta%20R_i%2B%20%5Cgamma%20(Y_i%20-%20c)%20%2B%20%5Clambda(Y_i%20-%20c)%20%5Ccdot%20R_i#0)

[](https://www.codecogs.com/eqnedit.php?latex=%20%5Ctext%7Bgiven%7D%20%5Cquad%20R_i%20%3D%201%5C%7BY_i%20%3C%20c%5C%7D%20%5Cquad%20%5Ctext%7Band%7D%20%5Cquad%20%20c%20-%20h%20%3C%20Y_i%20%3C%20c%20%2B%20h#0)

where the dependent variable[](http://www.sciweavers.org/tex2img.php?bc=Transparent&fc=Black&im=jpg&fs=100&ff=modern&edit=0&eq=E#0) is the annual evictions per unit *i*, [](http://www.sciweavers.org/tex2img.php?bc=Transparent&fc=Black&im=jpg&fs=100&ff=modern&edit=0&eq=Y#0) is the built-year of the property, also known as the “running variable” in RD parlance, [](http://www.sciweavers.org/tex2img.php?bc=Transparent&fc=Black&im=jpg&fs=100&ff=modern&edit=0&eq=c#0) is the threshold value (1980 in our case) along the dimension of the running variable, and [](http://www.sciweavers.org/tex2img.php?bc=Transparent&fc=Black&im=jpg&fs=100&ff=modern&edit=0&eq=R#0)is a rent-control “treatment” indicator that evaluates to 1 for properties built prior to 1980. The bandwidth parameter [](http://www.sciweavers.org/tex2img.php?bc=Transparent&fc=Black&im=jpg&fs=100&ff=modern&edit=0&eq=h#0)identifies the maximum distance between the running variable and the cutoff 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 not sensitive to the bandwidth. 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.

**5. Empirical Results**

*Eviction Types*

Table 1 summarizes the eviction records (n=30,183) used in the main analysis after segmenting by eviction type category and their built-year relative to the 1980 cutoff.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | **built before 1980 (n=29,769)** | | **built after 1980 (n=414)** | |
| **eviction category** | *count* | *%* | *count* | *%* |
| breach of lease | 12,239 | 41.3 | 211 | 52.9 |
| no-fault | 6,848 | 24.1 | 48 | 11.6 |
| unknown/Other | 10,258 | 34.6 | 145 | 35.5 |

Table 1. Eviction frequency (1994-2017) by category and built-year cutoff in rent control eligible addresses

The left two columns therefore 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 (~72x) more notices filed in rent controlled properties. More than anything this number reflects the fact that of the 384,060 property records used in the analysis only 16,125 (4.2%) were for properties built after 1980, which itself is explained by the diminishing construction rate of multifamily housing in San Francisco over time (Figure 1).

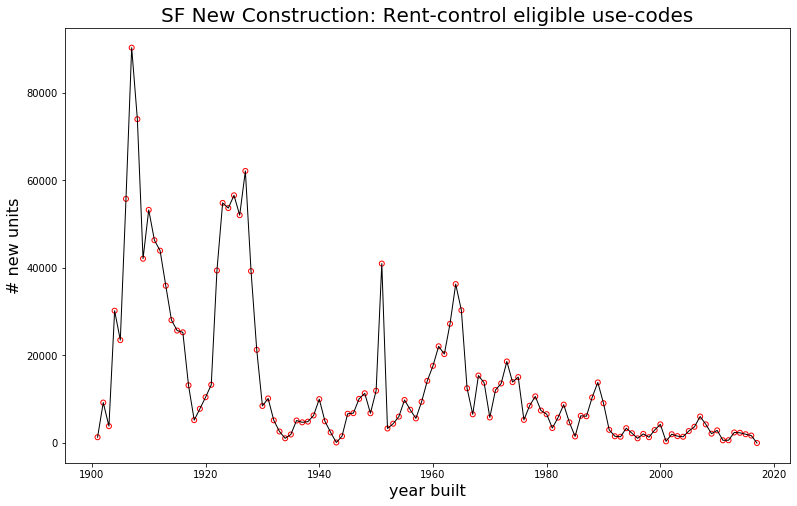
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Figure 1. Histogram of unit counts in rent control eligible buildings by year built. Construction of multi-family has slowed significantly since the 1960s, but there is no apparent

More interesting, however, is the fact that evictions at controlled properties are more likely to be no-fault evictions like condo conversions or owner move-ins[[8]](#footnote-8). These findings are in line with the idea that San Francisco’s rent control laws may be incentivizing controlled landlords to evict unproblematic, law-abiding tenants. And while the results in Table 1 seems to support that theory, they also show that breach of lease or at-fault evictions constitute a smaller portion of evictions in rent control units, a fact that might suggest rent control is actually achieving one of its primary objectives of keeping tenants from failing to pay their rent.

*Mean Differences*

Table 2 compares the counts and averages of the San Francisco annual parcel records by rent control eligibility and built-year threshold. Only the bottom two rows represent the observations included in the RD analysis, with the bottom most row representing rent controlled units. By normalizing the data by the number of observations in each category, average outcomes can be compared between the assignment groups to obtain a first indication of what a causal model might reveal. The results show a difference of +1.3% in the rate of eviction notices for rent controlled addresses compared to their uncontrolled counterparts, which corresponds to a ~2.3x increase in the probability of eviction on an annual, per-unit basis.

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **rent control eligible** | **built before 1980** | **total addresses** | **total units** | **avg. units per address** | **total evictions** | **prob. 1+ eviction** | **avg. evictions per address** | **avg. evictions per unit** |
| **N** | **N** | 76,864 | 364,153 | 4.737627 | 485 | 0.004215 | 0.006310 | 0.003296 |
| **Y** | 1,120,987 | 1,588,711 | 1.417243 | 12,308 | 0.006608 | 0.010980 | 0.008609 |
| **Y** | **N** | 15,645 | 134,008 | 8.565548 | 404 | 0.014062 | 0.025823 | **0.009969** |
| **Y** | 362,735 | 1,770,103 | 4.879879 | 29,345 | 0.036341 | 0.080899 | **0.023403** |

Table 2. A comparison of the eviction rates by rent control eligibility and built-year cutoff

*Graphical Analysis*

Figure 2 shows the traditional RD plot, comparing average observed outcomes on the Y-axis across binned values of the running variable on the X axis for the range defined by the bandwidth parameter [](http://www.sciweavers.org/tex2img.php?bc=Transparent&fc=Black&im=jpg&fs=100&ff=modern&edit=0&eq=h#0). The data points on either side of the built-year threshold are then 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 (~1.2%), but also the size of treatment effects estimated by the RD models below (0.09% - 1.6%). The simplicity of the RD approach is one of its greatest assets as a policy analysis tool.

The data presented in Figure 2 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) being the only channel through which systematic variation in eviction rates is explained by variation in the property built-year.

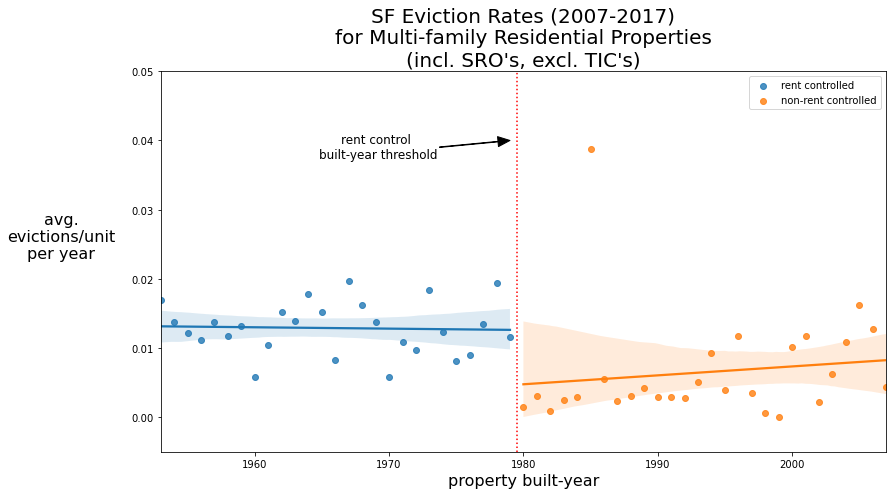
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Figure 2. Annual eviction rate per unit by built-year with local averages and linear fit. The magnitude of the discontinuity at the built-year threshold (1980) should be comparable to the average treatment effect estimated using RD.

**4. 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. The results are summarized in Table 3. See Appendix B for detailed summaries of each model.

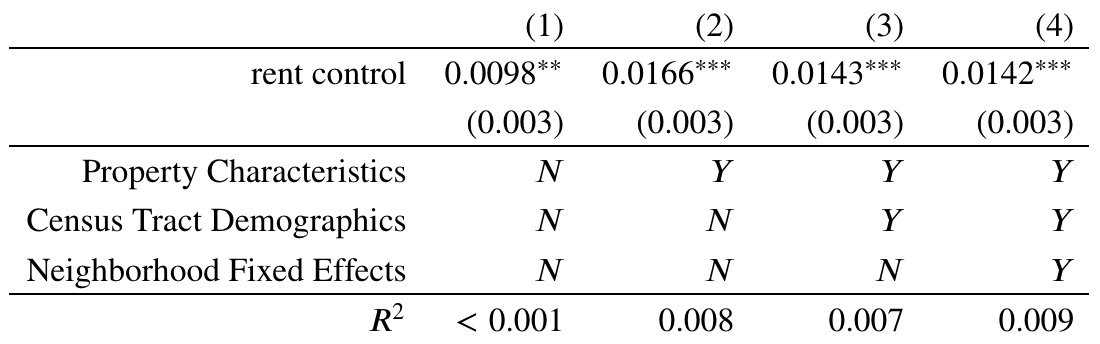


Table 3. Coefficient on rent control for four RD models (n=54,975) with standard errors in parentheses. Significance codes indicate p-values as follows: ‘\*\*\*’ < 0.001 < ‘\*\*’ < 0.01 < ‘\*’ < 0.05

Using the specification described in Equation 1, Model 1 estimates an average treatment effect of ~1% with a p-value of 0.002. This estimate is slightly smaller than the mean difference-based estimate, but still very much in line with the size of the discontinuity in Figure 2.

Model 2 adds two property-level characteristics derived from the assessor records: 1) the log of the assessed value divided by the log of the assessed square footage of the property; and 2) the log of the total units at the property. The value-per-square-foot 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 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 1.6% and improves the statistical significance of the treatment effect from 0.002 to < 0.001.

Model 3 adds Census tract-based demographic characteristics from the 2009-2013 5-year American Community Survey [citation needed]. A negative coefficient (p=0.013) on the log of the median household income suggests that eviction rates are higher in lower income areas, while a positive coefficient (p=0.07) on the percent of occupied units that are rented rather than owned suggests that eviction rates are higher in areas with lower rates of home ownership. Two other terms, percent minority (non-white) and percent of rental properties with tenants who moved in prior to the year 2000 both have negative but statistically insignificant coefficients. The latter term was included in order to account for the potential effect of tenancy duration on eviction rates. In the context of vacancy decontrol, longer tenancies should equate to more heavily discounted rents, thereby increasing the potential value of an eviction for a landlord wishing to return their property to market rate. I found no evidence to suggest that this is the case, although it is possible that tract-level Census data is not granular enough to capture this relationship. It is also possible that landlords may be less willing to initiate economically-incentivized evictions against tenants with whom they have long-standing relationships. The addition of these sociodemographic characteristics resulted in a slightly smaller estimated treatment effect of ~1.4%. The statistical significance was unchanged as < 0.001.

Finally, Model 4 adds neighborhood fixed effects to the equation according to the 72 assessor-designated neighborhoods that appear in the assessor records. These terms are a somewhat blunt instrument designed to account for any other geographic variation not captured by the Census-based sociodemographic variables. The addition of neighborhood fixed effects has very little impact on the treatment effect estimate, which remains steady at ~1.4% and significant at the < 0.001 level.

**6. Discussion**

*Internal Validity*

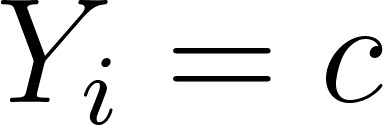
One of the biggest limitations of the RD design is that it is applicable under only a very narrow set of (quasi-)experimental conditions.

In addition to the small standard errors and p-values (< 0.002) reported in Section 4, a number of other indicators suggest this study has avoided the biggest potential pitfalls of the RD method. First, the main assumption of RD is 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. These results suggest that apart from treatment itself, there is hardly any quantifiable relationship at all between the property built-year and eviction rates. It appears that apart from the 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 (just that the relationship is smooth and continuous), 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 the subjects. This occurs when subjects are aware of the cutoff threshold that determines treatment assignment (i.e. policy eligibility), and are able to nudge their own values of the running variable in order to qualify for a policy or 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 this case, despite the fact that both landlord and tenant are typically aware of both the age of their property/residence and the 1980 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 a bandwidth might bias the results by including observations that are too far removed from the discontinuity to be relevant. Accordingly RD estimates that are robust to this somewhat-arbitrarily chosen parameter are much more credible than those that are heavily dependent upon it [[24]](https://www.zotero.org/google-docs/?W0WpFt). In Section 3 it was briefly mentioned that treatment effect estimates in this study were found to be robust to variation in bandwidth.

*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. However, the treatment effect estimated via RD is a *local* average treatment effect (LATE), rather than the more general average treatment effect typically associated with an 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 ([](https://www.codecogs.com/eqnedit.php?latex=Y_i%20%3D%20c#0)). 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. Such a claim depends on the degree to which a homogenous treatment effect can be assumed. Given the seemingly stochastic nature of the relationship between built-year and eviction rates, such an assumption is not altogether implausible. Additionally, recent research suggests 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 [[23], [25]](https://www.zotero.org/google-docs/?65YMdP). It is also worth noting that regardless of whether or not the results are relevant to San Francisco’s oldest and youngest residential properties, they are clearly only relevant to properties located within the city and county of San Francisco.

In 2020 California voters had the chance to repeal the 1995 law[[9]](#footnote-9) restricting municipalities from enacting rent control on residential units constructed after February 1995. If the ballot measure, Proposition 21, had passed, the 1995 restriction would have been replaced with a rolling 15-year window that would have allowed 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[[10]](#footnote-10). If and when the time comes for San Francisco to reassess its rent control built-year 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.

*Policy Implications*

Increased housing stability is one area in which experts agree that rent control offers real benefits. A recent review of the rent control literature published by the USC Equity Research Institute found that “nearly every academic study finds that rent stabilization [...] increases housing stability for rent-stabilized residents” [[26]](https://www.zotero.org/google-docs/?o9arlU).

However, I find that rent control also xxxx.

It is certainly true that if rent control is reframed as less of a broad economic policy and more of a displacement prevention measure, then much of the existing evidence against it becomes somewhat moot. For example, the key argument of [[19]](https://www.zotero.org/google-docs/?Z2XJTO) is that even though rent control yields real economic benefit to incumbent tenants in San Francisco, it also makes housing less affordable for newcomers to the city and therefore actually worsens the very problem it was meant to solve, which according to the authors is either city-wide economic inequality or gentrification or both. This conclusion of course depends on the somewhat tenuous assumption that rent control is meant to ameliorate either of these broader economic conditions. Viewed solely through the lens of housing stability, that study’s findings paint a much more favorable picture in which rent control yields a net benefit of $393 million per year for rent controlled tenants. All of these papers, however, fail to account for the findings presented here, however, which indicated a significant *destabilizing* effect of rent control due to higher rates of eviction.

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[[11]](#footnote-11). Recent work by Desmond and others has also demonstrated the deleterious, sometimes trans-generational effects of eviction on health outcomes, homelessness, and job retention [[21], [27], [28]](https://www.zotero.org/google-docs/?Yk9ceT).

For the purposes of this study, however, a more useful question to ask of these new findings is first *why* rent-controlled tenants are more likely to be evicted, and secondly whether or not that “why” is inherent to Rent Control as an economic concept or just rent control as legislated by the City and County of San Francisco. There is much evidence to suggest that it is the latter. The data in Table 1 (and Appendix A) for example showed that evictions in rent controlled units are less likely to be issued for a breached lease (e.g. late rent, failure to pay, nuisance, etc.) which can actually be interpreted as evidence that rent control is succeeding in preventing displacement due to unaffordability. Of course this is all a moot point if as long as landlords can make use of no-fault evictions to achieve the same ends. But this is not a shortcoming of Rent Control as a policy. Rather it is a failure of San Francisco’s rent control ordinance to adequately protect its rent controlled tenants from the kinds of profit-driven evictions that are incentivized under vacancy decontrol. The data in this study show that rent control can in fact achieve one of its most important policy goals, but that in order to do so it *must* be coupled with more stringent eviction protection measures. With local and statewide COVID-19 eviction moratoriums due to be lifted, it is more important than ever that San Francisco reexamine the role that no-fault evictions play in undermining the goals of its rent control policy.

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And in the meantime

Throwing the boat away after springing a leak when you just forgot to

At face value, the consistently positive effect (~1.4%) estimated here seems to validate critics of rent control who have long maintained that the negative, indirect effects of the policy most likely undercut its benefits.

Housing stability in this sense is usually measured via reduced rates of residential mobility, and attributed to the greater affordability of controlled rents. Economists are quick to point out that such reduced mobility is often indicative of a misallocation of housing resources and therefore not necessarily a net positive in market terms [[8]](https://www.zotero.org/google-docs/?ZA0xvo).

* Other potential sources of bias:
  + inaccurate assessor data for evictions occurring before 2007:
    - cannot infer the rent control status of the property at the time of the eviction notice.
    - fit RD with and w/o evictions prior to 2007 and no significant change
  + unit counts could be off when aggregating by address
    - could include commercial units
    - double counting units for multi-building properties with centralized addresses (e.g. park merced)
* Policy implications
  + housing stability

Appendix A. Eviction Frequency by Type and Built-Year Cutoff (1980)

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | **built before 1980** | | **built after 1980** | |
| **eviction type** | *count* | *pct* | *count* | *pct* |
| unknown | 5431 | 0.359122 | 47 | 0.228155 |
| Breach of Lease Agreement | 3303 | 0.218409 | 78 | 0.378641 |
| Nuisance | 1767 | 0.116842 | 29 | 0.140777 |
| OMI | 1251 | 0.082722 | 3 | 0.014563 |
| Capital Improvement | 550 | 0.036368 | 0 | -- |
| Non-payment of Rent | 532 | 0.035178 | 11 | 0.053398 |
| Habitual Late Payment of Rent | 431 | 0.0285 | 4 | 0.019417 |
| ELLIS | 350 | 0.023144 | 0 | -- |
| Development Agreement | 233 | 0.015407 | 0 | -- |
| Illegal Use of Unit | 229 | 0.015142 | 2 | 0.009709 |
| Breach of Lease Agreement Nuisance | 209 | 0.01382 | 3 | 0.014563 |
| Roommate Living in Same Unit | 136 | 0.008993 | 3 | 0.014563 |
| Other | 125 | 0.008266 | 18 | 0.087379 |
| Unapproved Subtenant | 119 | 0.007869 | 1 | 0.004854 |
| Demolition | 104 | 0.006877 | 0 | -- |
| Denial of Access to Unit | 58 | 0.003835 | 2 | 0.009709 |
| Nuisance Illegal Use of Unit | 51 | 0.003372 | 0 | -- |
| Breach of Lease Agreement Illegal Use of Unit | 35 | 0.002314 | 0 | -- |
| Failure to Sign Lease Renewal | 25 | 0.001653 | 0 | -- |
| Breach of Lease Agreement Nuisance Illegal Use of Unit | 25 | 0.001653 | 0 | -- |
| Condo Conversion | 17 | 0.001124 | 0 | -- |
| Non-payment of Rent Breach of Lease Agreement | 13 | 0.00086 | 0 | -- |
| Non-payment of Rent Habitual Late Payment of Rent | 13 | 0.00086 | 1 | 0.004854 |
| Denial of Access to Unit Breach of Lease Agreement | 12 | 0.000793 | 0 | -- |
| Habitual Late Payment of Rent Breach of Lease Agreement | 12 | 0.000793 | 0 | -- |
| Unapproved Subtenant Breach of Lease Agreement | 12 | 0.000793 | 0 | -- |
| Habitual Late Payment of Rent Breach of Lease Agreement Nuisance | 7 | 0.000463 | 0 | -- |
| Substantial Rehabilitation | 7 | 0.000463 | 0 | -- |
| Roommate Living in Same Unit Nuisance | 6 | 0.000397 | 0 | -- |
| Lead Remediation | 6 | 0.000397 | 0 | -- |
| Breach of Lease Agreement Other | 5 | 0.000331 | 1 | 0.004854 |
| Denial of Access to Unit Breach of Lease Agreement Nuisance | 4 | 0.000264 | 0 | -- |
| Breach of Lease Agreement Failure to Sign Lease Renewal | 4 | 0.000264 | 0 | -- |
| Habitual Late Payment of Rent Nuisance | 4 | 0.000264 | 0 | -- |
| Good Samaritan Tenancy Ends | 4 | 0.000264 | 0 | -- |
| Unapproved Subtenant Breach of Lease Agreement Nuisance | 3 | 0.000198 | 0 | -- |
| Non-payment of Rent Nuisance | 3 | 0.000198 | 0 | -- |
| Denial of Access to Unit Breach of Lease Agreement Nuisance Illegal Use of Unit | 3 | 0.000198 | 0 | -- |
| Unapproved Subtenant Nuisance Illegal Use of Unit | 2 | 0.000132 | 0 | -- |
| Nuisance Other | 2 | 0.000132 | 1 | 0.004854 |
| Denial of Access to Unit Unapproved Subtenant Breach of Lease Agreement | 2 | 0.000132 | 0 | -- |
| Denial of Access to Unit Other | 2 | 0.000132 | 0 | -- |
| Denial of Access to Unit Nuisance | 2 | 0.000132 | 0 | -- |
| Nuisance Capital Improvement | 1 | 0.000066 | 0 | -- |
| Non-payment of Rent Unapproved Subtenant | 1 | 0.000066 | 0 | -- |
| Non-payment of Rent Roommate Living in Same Unit | 1 | 0.000066 | 0 | -- |
| Breach of Lease Agreement Roommate Living in Same Unit | 1 | 0.000066 | 0 | -- |
| Non-payment of Rent Habitual Late Payment of Rent Nuisance | 1 | 0.000066 | 0 | -- |
| Non-payment of Rent Habitual Late Payment of Rent Breach of Lease Agreement Nuisance | 1 | 0.000066 | 0 | -- |
| Non-payment of Rent Denial of Access to Unit | 1 | 0.000066 | 0 | -- |
| Non-payment of Rent Breach of Lease Agreement Nuisance | 1 | 0.000066 | 0 | -- |
| Denial of Access to Unit Breach of Lease Agreement Other | 1 | 0.000066 | 0 | -- |
| Habitual Late Payment of Rent Breach of Lease Agreement Failure to Sign Lease Renewal | 1 | 0.000066 | 0 | -- |
| Unapproved Subtenant Breach of Lease Agreement Nuisance Illegal Use of Unit | 1 | 0.000066 | 0 | -- |
| Unapproved Subtenant Illegal Use of Unit | 1 | 0.000066 | 0 | -- |
| Unapproved Subtenant Nuisance | 1 | 0.000066 | 0 | -- |
| Non-payment of Rent Other | 1 | 0.000066 | 0 | -- |
| Habitual Late Payment of Rent Other | NaN | NaN | 1 | 0.004854 |
| Habitual Late Payment of Rent Roommate Living in Same Unit Nuisance | NaN | NaN | 1 | 0.004854 |

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2. [[6], [9]](https://www.zotero.org/google-docs/?D1XB39), and [[20]](https://www.zotero.org/google-docs/?qA6NRN) could be considered exceptions but these rely on census data rather than disaggregate tenant observations. [↑](#footnote-ref-2)
3. in a secondary model with more moderately sized shocks Asquith found no significant change in eviction rates. [↑](#footnote-ref-3)
4. In fact, the author explicitly ignores evictions of this type because they are heavily regulated and rarely observed. [↑](#footnote-ref-4)
5. In this case, the records were graciously provided to the author, unaltered, courtesy of the Anti-Eviction Mapping Project, a grassroots tenant advocacy group who originally submitted the records request. [↑](#footnote-ref-5)
6. https://data.sfgov.org/Housing-and-Buildings/Eviction-Notices/5cei-gny5 [↑](#footnote-ref-6)
7. Properties with a tenancy-in-common (TIC) class code are excluded from the analysis. [↑](#footnote-ref-7)
8. 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” evictions. See Appendix A For a full enumeration of eviction types, categories, and observations. [↑](#footnote-ref-8)
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