Rational Eviction: How Landlords Use Evictions in Response to Rent Control*

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Abstract

When designing rent control regulations, policy makers aim to create regulations that ensure affordable and stable housing for current tenants while minimizing exits from the rental market by landlords. Vacancy decontrol provisions that allow rent resets between tenants intend to strike a balance between a lower rent burden for current tenants and future potential profitability for landlords. However, such provisions also increase the incentive for landlords to evict tenants. Such evictions reduce both the anti-displacement and rent reduction effects of rent control. To study the effects of rent control on eviction behavior, we exploit variation across ZIP codes in policy exposure to the passage of the 1994 rent control referendum in San Francisco. We find that a ZIP code with the average level of treatment experiences an additional 34 eviction notices—an 83% increase—and an additional 13 wrongful eviction claims—a 125% increase. These effects were concentrated in low-income ZIP codes and were larger in years when average rent prices rose faster than the allowed rent increases for controlled units.

JEL: R28, R31, R38, H70
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1 Introduction

As housing prices rise, more cities and states are turning to rent control policies with the goal of ensuring long-term affordable housing.[1] In a typical rent control policy in the United States, leases must be renewed at statutorily limited rent increases. Rent control policies reduce the returns from operating in the rental market, creating well-studied incentives to leave it. To moderate these incentives, many rent control policies include "vacancy decontrol" provisions, which allow landlords to reset rents to market rates when tenants move. These policies limit the reductions in returns to operating in the rental market but create incentives to induce tenant turnover, either through tenants moving or evictions. The more often tenants move, the more often a landlord can raise rents to market rates.

In this paper, we examine whether a large-scale rent control expansion in San Francisco led to more eviction notices or increased complaints about wrongful evictions. We document a large increase in eviction notices and complaints, concentrated in buildings directly affected by the policy. We then use a differences-in-differences research design that exploits ZIP code–level variation created by the passage of a 1994 ballot referendum that led to the removal of a rent control exemption for small buildings built before 1980. We find substantial increases in both the number of eviction notices and wrongful eviction claims filed with the San Francisco Rent Board in areas more affected by the policy change.

We begin by documenting a sharp increase in eviction notices reported to the Rent Board overall starting in 1995, when rent control was expanded. Under San Francisco's Rent Ordinance, unless the tenant is in breach of the lease in some way, legal evictions require the landlord or a member of their immediate family to occupy the unit after the tenant leaves, remove all rental units from the rental market under the Ellis Act,[2] or demolish the rental unit. The rise in eviction notices is concentrated in the types of evictions that are most related to rent control–based incentives: owner move-in evictions and Ellis Act evictions. These evictions remove units from the rental stock, at least in the short term. Owner move-in evictions preserve the opportunity for the landlord to return to the rental market. We do not observe large increases in evictions that landlords cannot directly control, such as evictions for non-payment.

We also observe a sharp increase in claims of wrongful eviction. Wrongful evictions include a misrepresentation of the owner's plans to the tenant to convince them to leave

[1] Oregon adopted a state-wide rent control policy in 2019. Saint Paul, Minnesota adopted rent control policies in 2021. New York state expanded rent stabilization policies to more municipalities in 2019.

[2] CA Govt Code § 7060 (2022)

earlier, incorrect notice being given around an eviction, or a self-help eviction in which the landlord either removes the tenant directly or interferes with the habitability of the unit. While there are many ways for a landlord to legally evict, these generally do not allow the landlord to receive the immediate benefit of resetting the rent to the market rate. For example, with an Ellis Act eviction, the owner must remove all units from the rental market. In contrast, wrongful evictions may produce this benefit for the landlord.

Our ZIP code–level differences-in-differences analysis compares ZIP codes with more and fewer units that become newly eligible for rent control based on their age and size. We find an 83% increase in eviction notices filed with the Rent Board and a 125% increase in the number of wrongful eviction claims for ZIP codes with the average level of new exposure to rent control. The increase in evictions occurs gradually over the five years following the policy change, likely due to market rents increasing over time. Our largest effects occur in years in which rent control would be particularly binding, as measured by a large gap between the rise in average rent prices and the allowed rental price increase under rent control.

These effects are large and economically significant. We find an annual effect of an increase of 20.07 eviction notices per 1,000 treated units in a zip code. Over the six years in our post period (1995-2000), this translates roughly into 12% of newly rent controlled units receiving an eviction notice. If even a small fraction of these eviction notices result in tenants moving, these eviction proceedings represent a substantial limitation on the rent control policy's ability to ensure that tenants can stay in their existing housing situation, even if the policy on net resulted in a decrease in mobility (Diamond et al. (2019)).

One important facet of the policy context is that eviction notices and wrongful eviction claims are collected only for rent controlled units. Therefore, some increase in evictions post-expansion would be expected if landlords of newly rent controlled units behave the same as the landlords of previously rent controlled units, who own larger buildings that were rent controlled many years before. We show that evictions increase above and beyond the expected mechanical increase (that which would be expected if the behavior of small-building landlords exactly replicates the behavior of previously controlled landlords), suggesting either that the policy expansion created excess short-run evictions or that small-building "mom-and-pop" landlords optimize differently in the face of rent control than the landlords of larger buildings do. To help distinguish between these two possible explanations, we compare buildings with 3–4 units that are newly rent controlled to buildings with 5–6 units that were previously rent controlled; we observe more evictions at buildings with 3–4 units, which is consistent with our results being driven by reoptimization behavior by landlords in the

period immediately following the implementation of rent control.

We additionally document heterogeneity with respect to median income in a given ZIP code, where our estimated effects for low-income ZIP codes are at least 60% higher than our estimated effects for high-income ZIP codes. This heterogeneity is important in the setting of rent control in which the goal of the policy is to prevent lower-income tenants from being displaced from their homes when rents rise. We do not find similar heterogeneity by rent prices or changes in rent prices suggesting this discrepancy is not being primarily driven by rent-based incentives for landlords. Understanding the distributional consequences is important for policy makers in situations in which concerns about gentrification may be a major driver of the adoption of rent control policies.

Our paper contributes to the literature on rent control (Autor et al., 2014; Early, 2000; Glaeser and Luttmer, 2003; Olsen, 1972; Sims, 2007) by documenting the behavior of landlords who stay in the market, by showing that this response leads to formal complaints filed by tenants, and by highlighting the dynamics of this behavior over time. Much of the literature looking at supply-side responses focuses on how supply to the rental market is affected. The most closely related paper, Diamond et al. (2019), uses the same institutional setting and similar research design and finds that landlords reduced the rental supply by 15% by selling to owner-occupants and redeveloping buildings, while rent control limited renter mobility by 20%. Whereas Diamond et al. (2019) focuses on how landlords respond to rent control by exiting the market, our paper shows how landlords adjust their behavior while staying in the rental market, or at the very least, preserving the option to easily re-enter. These additional evictions can limit the positive anti-displacement effects of rent control.

We make several further contributions. Basu and Emerson (2000)'s model shows how rent control policies such as San Francisco's create an inherent adverse selection problem in which landlords do not know how long a given tenant intends to stay in a unit. More-stationary tenants are less profitable because landlords cannot reset the rent until the tenant leaves. Our finding of an increase in evictions in response to the policy change suggests that landlords newly subject to rent control may be particularly prone to use eviction proceedings to get a new draw from the pool of tenants.

Additionally, we show how the use of vacancy decontrol as a tool in the rental market coincides with periods in which rent control is particularly binding. Whereas one may expect evictions to spike immediately after the policy was passed (through landlords converting to condos or otherwise exiting the market), our results show that evictions do not increase until landlords have a financial reason to attempt to re-let, when market rents across San

Francisco exceed the allowed increase in rental prices.

Finally, our work shows heterogeneity across landlords in the propensity to use these tools; the rise in eviction notices we find exceeds the increase had pre-policy landlord behavior continued. Our results highlight that landlords are able to use the evictions process to circumvent rent control policies and that the use of these evictions proceedings spikes in periods of rapid rent increases.

Other work in this area focuses on the effects of price changes on eviction behavior. Asquith (2019a) and Asquith (2019b) examine how landlords in San Francisco respond to price increases under rent control using bus lines as an instrument for prices. These papers find evidence of an increase in owner move-in evictions, consistent with our findings, but no evidence of increases in Ellis Act evictions. Additionally, there were increases in condo conversions. Pennington (2021) finds that evictions in rent controlled units fall after rents decrease due to an increase in housing supply. Our contributions are complementary to these findings: we document the effects of an expansion of rent control, which may affect the market differently than price changes, specifically study "wrongful" eviction claims, and highlight that different types of landlords may behave differently. In particular, small landlords may have a greater ability to leverage owner move-in evictions.

The rest of this paper is organized as follows. Section 2 discusses the institutional details of rent control in San Francisco. Section 3 describes the theoretical context. Section 4 describes the data, and Section 5 describes our empirical design. Section 6 describes our results: we find an increase in wrongful eviction claims and eviction notices following the removal of a rent control exemption. Finally, Section 7 concludes.

2 Background

In this section, we describe the details of rent control in San Francisco and the policy variation that we study in this paper. In 1979, San Francisco passed its Rent Ordinance (Diamond et al., 2019). Buildings built and occupied before 1979 above a certain size had rent increases capped at 60% of the regional CPI (Asquith, 2019b).[3] Under this legislation, rents would likely fall below market-rate rents over time. Under the Rent Ordinance, small (less than 5 units) owner-occupied buildings were exempted from rent control. This exemption was

[3]The initial limit on rental increases was 7% per year which was lowered to 4% per year in 1984 and to 60% of CPI in 1992

lifted by the passage of a ballot referendum in 1994,[4] which expanded the number of units subject to rent control within San Francisco by about 68% for the average ZIP code. As of 2015, roughly 60% of San Francisco's rental stock was rent controlled (40% of the overall housing stock), largely because a sizable amount of the rental stock was built before 1979 (San Francisco Planning Department 2018).

San Francisco's rent control policy features "vacancy decontrol," which allows landlords to reset the rent to market rate when a tenant leaves the unit (Asquith, 2019a).[5] These regulations also include "just cause" regulations, in which landlords must have grounds for a lease termination or eviction.[6] Landlords can only evict tenants for one of 16 specific legal reasons, and they must have an "honest intent, without ulterior motive."

Landlords can then raise rents to market rate only in a limited number of situations. Specifically, they can raise the rent when a tenant leaves the apartment. Therefore, landlords might be incentivized to offer a cash buyout to existing tenants to induce them to move. Landlords can also perform an owner-move in eviction. Such an eviction could entail the landlord or an immediate family member moving into a unit; it could also entail selling some fraction of the building to a third-party, who could then owner-occupy one unit.[7] On the one hand, this is less financially beneficial, since although they can raise the rent after an owner move-in eviction, they would raise the rent on themselves or an immediate family member. If they re-let the unit before three years has passed, the rent control regulations prohibit them from raising the rent above what the prior tenant would have paid.[8] On the other hand, it is easy for landlords to evade these laws. The Rent Board only audits 10% of units each year, and if a unit's rent is found to be higher than what is allowed based on

[4]The ballot referendum, Proposition I, passed in a close election (51% vs. 49%) on November 8, 1994, and went into effect on December 22, 1994. The rent charged on May 1, 1994, was considered to be the "base rent" for buildings newly subject to rent control. Landlords who had raised the rent in between May 1 and the passage of the referendum were required to provide tenants with refunds. If landlords had not raised the rents for some time prior to May 1, they were permitted to petition the Rent Board for an annual increase above and beyond the allowed increase.

[5]The passage of the Costa-Hawkins Rental Housing Act in 1995 requires all rent control policies in California to allow for vacancy decontrol.

[6]Just cause regulations are generally passed at the same time as rent control policies. In San Francisco, the just cause regulations passed with the 1979 Rent Ordinance.

[7]For an owner move-in eviction to be valid, the owner or relative must move into the unit within three months of an eviction notice and must occupy the unit for at least 36 continuous months. An immediate family member moving into the unit can be grounds for an owner move-in eviction, but only if the owner already resides in the building. Further, if the owner moves out of the unit before their three-year required tenancy is up, the rent charged to the next tenant must not exceed what would have been charged to the tenant who lived in the unit prior to the landlord moving in. After December 18, 1998, an owner move-in eviction requires that the unit be designated as the owner's unit for any subsequent owner move-in evictions.

[8]https://sfrb.org/topic-no-204-evictions-based-owner-or-relative-move

owner tenancy and rent control laws, the landlord will be assessed a fee of up to $1,000 per month when excess rent was charged.[9] There are additional potential benefits associated with leasing to a new tenant: if the previous tenant was likely to reside in the unit for many years, it may be financially advantageous to find a new tenant. Owner move-in evictions thus preserve some option value; the owner can choose when to return to the rental market or keep the unit owner-occupied.

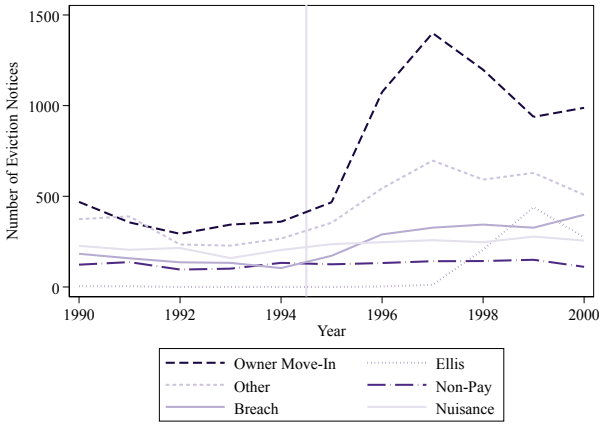
One potential way that landlords can respond to the expansion of rent control is by removing their units from the rental market by converting to condos. Condo conversion was further incentivized by the passage of the Costa–Hawkins Rental Housing Act in 1995. This Act banned cities from applying rent control to single family homes or condominiums. To combat condo conversions, San Francisco has a number of provisions that aim to limit the ability of landlords to convert to condos. In 1993, the city established a 200-unit limit on condo conversion for 6 or fewer unit buildings for four years. In December 1994, the city established lottery pools: the eligibility requirements for these pools effectively blocked landlords of newly rent controlled buildings from participating in the lottery for two years. This creates an upper bound on the number of evictions that can be attributed to the condo conversion lottery. Starting in 1997, applicants could convert to condos if they had 2–6 units, 1 or more units were continuously occupied by one of the owners for three continuous years, and they won the lottery with a cap of 200 units per year. In 2001, the city established a lottery bypass for buildings with 2 units where, conditional on satisfying ownership requirements, an unlimited number of owners could convert to condos. For additional details on this lottery (and the history of rent control in San Francisco), see Asquith and Reed (2021) and Asquith (2020).

Figure 1 shows the total number of eviction notices per year in San Francisco split by type. Prior to the policy change, all eviction types trended comparably. After the policy change, nuisance and non-pay evictions have stayed constant,[10] while there have been increases in owner move-in, breach, and "other" evictions. The evictions that have risen are "no-fault" evictions, which landlords directly control, and which we would expect to rise most in response to a rent control policy. However, these eviction types are also subject to many regulations, and it is possible that many of these eviction notices were wrongfully handled by landlords.

[9] A punishment of up to six months of imprisonment in the county jail is also possible for landlords who violate rent control laws.

[10] Non-payment evictions do not need to be reported to the Rent Board, which may explain why this is flat. (https://www.sf.gov/information/general-eviction-notice-requirements)

Figure 1: Types of Eviction Notices Over Time



Notes: This figure shows the number of eviction notices filed with the San Francisco Rent Board by type over time. Owner move-in and Ellis Act evictions can be initiated by the landlord without some violation of the lease contract by the tenant. Non-payment, breach, and nuisance evictions all require some kind of action by the tenant. It is not possible to further break down evictions classified as "Other."
Data Source: San Francisco Rent Board Evictions Notices (San Francisco Rent Board, 2021)

Wrongfully handled "no-fault" evictions are one reason why a tenant might file a wrongful eviction claim with the Rent Board. Other reasons could include being forced out because of repair issues or because of landlord harassment. One of the first steps in fighting a wrongful eviction is filing a claim with the San Francisco Rent Board, which handles reports of evictions that violate Rent Ordinances. If a landlord is found to have wrongfully evicted a tenant in violation of the Rent Ordinances, they face financial penalties.

In general, the eviction process begins when a landlord serves an eviction notice to their tenant. The landlord has ten days to file the eviction notice with the Rent Board, but for no-fault evictions, more notice is required. For example, Ellis Act evictions require the eviction notice to be filed with the Rent Board 120 days before the withdrawal date. If the tenant does not move out before the withdrawal date, then the landlord can file and serve an "Unlawful Detainer Summons and Complaint" to the tenant in order to remove them from the unit; the tenant has five days to respond in court. The court will set a trial date and if the landlord wins, a sheriff will carry out the eviction.

A wrongful eviction claim can be made at any time during the eviction process,[11] though

[11]Rather than fighting the eviction so that it never takes place, a tenant may also choose to sue the landlord for damages, though this would likely be a more expensive route and may require waiting until the eviction is complete.

many grounds for wrongful eviction inherently require a notice to be served first.[12] There is no financial cost to filing a wrongful eviction claim; however, the tenant must be aware of the option to do so and pay the hassle cost of filing. Once a wrongful eviction claim is filed, the Rent Board determines whether there is evidence of an unlawful eviction. If so, there will be an investigatory hearing before an Administrative Law Judge, who prepares a report for the Rent Board of Commissioners. The Rent Board of Commissioners will then determine whether to take further action (including making a referral to the District Attorney for criminal prosecution).[13] Wrongful eviction claims may vary with tenant incentives. The longer a tenant stays in a rent controlled unit, the lower their relative rent, and the higher the benefit to filing a wrongful eviction claim if they are evicted.

3 Theoretical Context

In this section, we introduce the choices that landlords must make and discuss how rent control with vacancy decontrol changes the incentives that landlords face. Rent control not only shifts the returns to remaining in the rental market but also changes the value of a new tenant, both through a new opportunity to select a tenant and to set rent. These changes in relative values will affect both the decision to stay in the rental market and landlords' attempt to turn over tenants.

We model landlords as making rational decisions at the end of the lease period for each tenant. We first lay out how landlords will behave before rent control with unrestricted rent setting ability and no guaranteed renewals. Landlords first decide whether to stay in the rental market. They remain in the rental market if the expected value of discounted future profits from the rental market exceeds the value of the property; otherwise, they will sell the unit and exit the rental market.

Once they decide to stay in the rental market, they decide whether to offer a lease renewal to their existing tenant. They offer a new rental price, taking into account the probability that their tenant accepts and given the value of that tenancy to the landlord. There is some cost of turning over the unit to a new tenant. Because tenants vary in quality (how well they take care of the unit and their propensity to pay rent on time), there is some uncertainty

[12]For example, a wrongful eviction claim may allege that the "just cause" written on the eviction notice is incorrect or does not apply. The eviction notice must have been served before this wrongful eviction claim can be submitted since it refers to the eviction notice itself.

[13]The Rent Board does not publish statistics on the proportion of eviction notices or wrongful eviction claims that result in criminal prosecution or other legal action.

about the quality of a new tenant. The value of an existing tenant is already known. Under this system, long-staying tenants have a higher value to the landlord because they minimize uncertainty and turnover costs. If the landlord decides not to renew the lease, they receive a new set of applications from the tenant pool at the new rental price they post. They pick a new tenant from the set of applications to maximize the expected value of the tenant.

After rent control, the choice set of the landlord changes. Again, they must decide whether or not to stay in the rental market. The landlord's options to leave the rental market are limited by regulations.[14] If they stay in the rental market, they face three options. The first option is to continue renting the unit to the existing tenant. If they chose that option, the rent is limited by the rent control regulation. Therefore, continuing to rent to the existing tenant has become a weakly worse option than it was without rent control, depending on whether market rents have risen above the rent increase allowed under the rent stabilization policy.

The second option is to legally remove the tenant. This option would involve renting to family members (owner move-in evictions), selling a portion of the building (to enable another owner to live in the building),[15] or paying the tenant to leave voluntarily. After a certain time period (for an owner move-in eviction), the owner may then re-rent the unit. Under rent control, the incentives for the landlord may change both rental prices and the tenants that they select. Nagy (1997) suggests the prices charged upon re-letting may be even higher than those charged in the uncontrolled sector. Basu and Emerson (2000) show that the value of tenants to landlords decreases with the duration that the tenant stays in the unit, allowing additional future resets of the rental price to occur. Landlords will elect this option if the benefits of the new lease (higher rent, better tenant selection given rent control regime) exceed the costs of the legal eviction (paperwork costs, costs of foregone rent for having a family member or additional owner occupy the unit for a period of time).

The third option is to illegally remove the tenant. If unchallenged, this option potentially brings the benefits of higher rent and better tenant selection immediately. However, there are considerable risks and costs involved in a wrongful eviction. The Rent Board audits 10% of units each year to ensure landlords are charging the correct rent. If, for example, a landlord evicted a tenant due to their immediate family member moving in, but then the family member did not move in and the unit was instead leased at a price higher than

[14] As discussed above, to leave the rental market, landlords must convert to condos, demolish or substantially renovate the building, or perform an Ellis Act eviction.

[15] This option may result in the temporary removal from the rental market, but it does not preclude the unit from being offered on the rental market in the future.

that which the previous tenant paid, the landlord can be fined up to $1,000 per month of overcharged rent and can be sentenced to up to six months in the county jail. Tenants also have the ability to file wrongful eviction claims. Fighting these claims imposes some cost initially on the landlord with additional penalties if landlords are found to have violated rent control policies.

Several factors may shift the decision making of the landlords. First, note that tenant selection incentives will be strongest when tenants were selected when there was no rent control. Without rent control, landlords are incentivized to screen tenants for longevity. They always have the option of non-renewal or large rent increases, so tenants that minimize turnover costs are preferable.[16] However, this tenant is far from optimal under rent control; landlords would prefer to maximize turnover as long as increases in market rent year over year are sufficient to cover turnover costs.[17] Landlords who selected tenants when there was no rent control may be stuck with low-turnover tenants who they would prefer to replace with newly screened tenants. These incentives may mean that there is a transition period for landlords whose properties are newly rent controlled; they may behave differently from landlords whose properties were previously rent controlled in the short run. However, in the long run, earlier re-optimizations may result in similar behavior to landlords whose properties were already subject to rent control. This suggests that there may be additional displacement effects in the short run that policy makers may wish to consider if the goal of the policy is to keep existing residents in their homes (as opposed to minimizing displacement of the first set of tenants who lease after a new rent control policy is put in effect).

Second, landlords' propensity to engage in wrongful eviction behavior will vary depending on the likelihood of being challenged by a tenant or being caught engaging in wrongful behavior. A landlord is less likely to wrongfully evict a tenant who the landlord anticipates will fight the eviction or has the means to win a larger settlement in the ensuing legal fight. Tenants with lower incomes or who live in neighborhoods with lower median incomes may be even more likely to face a wrongful eviction because their landlords may believe that lower income is correlated with a lower likelihood of hiring legal counsel and effectively navigating the legal system.

Finally, the distortionary effects of rent control on the behavior of landlords depends on the degree to which rent control is binding and expectations of landlords about future rent increases. In the extreme, suppose that rent control limits are such that there is no reasonable

[16]For example, without rent control, a tenured faculty member may be ideal.

[17]With rent control, a one-year postdoc or visiting assistant professor may be your preferred tenant.

expectation of rent limitations (for instance, the maximum landlords are able to increase rent is 1,000% year over year). In such a world, we would expect no change in landlord behavior as a result of the policy. In contrast, if market rents are rapidly growing relative to the allowed rent increase, we would expect to see large changes in landlord behavior to induce turnover.

This heterogeneity leads us to make several testable predictions that we explore in the data. First, we predict an increase in eviction proceedings that exceeds that of previously rent controlled landlords in the short term. Second, this increase should be concentrated in time periods where market rents exceed the allowable rent increases. Third, we should see larger increases in wrongful eviction claims in neighborhoods where landlords are less likely to expect a successful challenge to an eviction.

4 Data

In this section, we describe the data we use to measure the expansion of San Francisco's rent control policy at the ZIP code level and the response to this policy by landlords in terms of eviction notices and wrongful eviction claims filed by tenants.

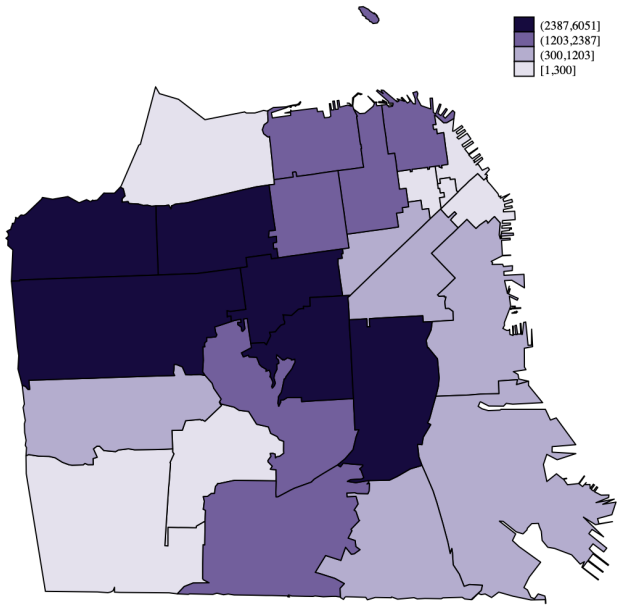
4.1 Measuring Rent Control

To understand the level of rent control treatment in each ZIP code, we use data on each unit's address, the number of units in the building, and the year the building was built for all residential units in the San Francisco Assessor's Secure Housing Roll from 1999. Since the rent control policy only affected small buildings with fewer than 5 units that were owner-occupied and built before 1980, we categorize buildings as newly rent controlled if they have less than 5 units and were built in 1979 or earlier. The policy only affected owner-occupied units, so we further restrict our definition to buildings with at least 2 units.[18] We describe the construction of this ZIP code level measure in Appendix Section A. We do not have an exact owner-occupancy measure in 1994 when the policy was passed. Figure 2 provides a map of our measure by ZIP code.

Some buildings may be counted as treated when they were actually rent controlled both before and after the policy change because they were not owner-occupied. This miscatego-

[18]Single family homes became exempt from rent control in 1995 with the passage of the Costa–Hawkins Rental Housing Act.

Figure 2: ZIP Code Level Exposure to Change in Rent Control Policy



Notes: This map depicts exposure at the ZIP code level for the San Francisco policy change. Exposure is measured as the number of units in a ZIP code in buildings with 2–4 units that were built prior to 1979. Data sources: 1999 San Francisco Assessor's Secure Housing Roll and authors' calculations

rization may attenuate our results.[19] In subsection A.2 of the data appendix, we discuss an alternative measure of exposure that attempts to adjust for this owner-occupancy element, defining the alternative treatment measure as the number of buildings with between 2 and 4 units, which were built before 1980, and which were owner occupied in 1999. Our results are robust to using this different measure.

We construct further measures of treatment that address the discrepancies that may result from our treatment data coming several years after the implementation of the policy. First, we use other publicly available data sets on building demolitions and parcel splits to directly correct this measure. However, because there are discrepancies between these data sets and the assessor data we primarily use (likely due to the hand-filled nature of the assessor data and updating lags across paper records), we additionally construct "worst-case" scenario treatment measures that assume that: 1) all new single family homes built between 1995 and 1999 replaced a duplex (to account for demolitions), 2) that all condos built before

[19]This attenuation results from the fact that all units we classify as untreated were definitively not treated, but some units we classify as being treated may not have been treated in reality because they were not owner occupied. The treatment effect based on the number of treated units may actually be higher on a per-unit basis because the number of treated units is overestimated.

1980 were converted in the post period, 3) all condo modifications were condo conversions that replaced rent controlled units, and 4) all new condos replaced rent controlled units (to account for condo conversions). Finally, we construct a measure that assumes all new construction in the post period replaced rent controlled units. These measures are highly correlated because there were relatively few changes to the housing stock in San Francisco in the 1990s.

4.2 Measuring Evictions

We measure the response of landlords to the rent control expansion using the ZIP code–level number of eviction notices and wrongful eviction claims. Both measures originate with the San Francisco Rent Board.[20] We do not observe eventual outcomes; eviction notices may not result in an eviction and wrongful eviction claims may not result in landlords being found in violation of regulations. Our eviction notices data have address information for our entire sample period and the reason for the eviction beginning in 1997.[21] During our sample period, these data are missing ZIP codes of many units and the reason for eviction for many units prior to 1997; we discuss these limitations and our attempts to rectify the missing information in Appendix subsection A.3. Data on wrongful eviction claims exists at the ZIP code level.[22]

Both wrongful eviction claims and eviction notices can only be submitted by individuals who already live in or are landlords of rental units subjected to rent control. When rent control expands, even if the total number of evictions stays constant, the number of wrongful evictions claims could increase solely due to more tenants living in rent controlled apartments and therefore being allowed to submit a wrongful eviction claim to the Rent Board. We discuss the implications of this and rule out that this mechanical effect drives results in Section 5.

Table 1 displays summary statistics for our analysis dataset. Panel A reports characteristics at the ZIP code level from the 1990 census. We report these statistics for ZIP codes overall, for ZIP codes below the median level of exposure (the "low treatment" group), and for ZIP codes above the median level of exposure (the "high treatment" group). We also report the difference for each characteristic between the control and treatment groups and

[20] One limitation of these data is that only certain types of evictions are required to be reported to the Rent Board; other types of evictions may be optionally reported. (https://www.sf.gov/information/general-eviction-notice-requirements)

[21] We thank Brian Asquith for generously sharing the address-level data on evictions post-1997.

[22] This limitation precludes breaking out wrongful eviction claims per type or looking at the address level.

Table 1: Comparison Between Low and High Treatment ZIP Codes

Panel A: Demographics

Variable	(1) Full Sample	(2) Low Treatment	(3) High Treatment	(4) Difference
Median Income	45698.68	44922.08	46415.54	1493.46
	(14783.99)	(19287.25)	(9745.48)	(6182.05)
1990 Median Rent	869.28	869.00	869.54	0.54
	(218.61)	(296.19)	(123.12)	(91.95)
2000 Median Rent	1006.36	1028.83	985.62	-43.22
	(346.60)	(476.96)	(175.38)	(145.82)
Share Black	0.12	0.17	0.08	-0.09
	(0.14)	(0.17)	(0.08)	(0.05)
Share White	0.55	0.51	0.59	0.08
	(0.18)	(0.20)	(0.16)	(0.07)
Share Owner Occupied	0.32	0.33	0.31	-0.02
	(0.24)	(0.30)	(0.17)	(0.10)
Share Welfare	0.11	0.12	0.09	-0.03
	(0.08)	(0.09)	(0.06)	(0.03)

Panel B: Treatments and Outcomes

Total Housing Units	12622.28	6927.42	17879.08	10951.66***
	(7874.73)	(5213.87)	(6061.00)	(2256.64)
# Prev. Rent Controlled	4882.08	2412.17	7162.00	4749.83**
	(5024.15)	(2729.11)	(5650.23)	(1755.68)
# Treated	1688.48	358.42	2916.23	2557.81***
	(1715.41)	(374.74)	(1534.44)	(439.75)
Pre-Policy Eviction Notices	41.04	21.28	59.28	37.99***
	(30.85)	(21.82)	(26.77)	(9.74)
Post-Policy Eviction Notices	84.17	37.46	127.29	89.84***
	(66.35)	(31.18)	(60.96)	(19.17)
Pre-Policy Wrongful Claims	10.32	3.77	16.37	12.60***
	(9.69)	(4.75)	(9.19)	(2.90)
Post-Policy Wrongful Claims	28.93	11.88	44.67	32.79***
	(27.00)	(11.56)	(27.87)	(8.43)
Observations	25	12	13	25

Standard errors in parentheses

$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$

Notes: This table reports the averages of key variables. Column (1) reports averages for the full sample, while columns (2) and (3) report averages for ZIP codes with below and above the median level of treatment, measured by the number of newly rent controlled units. The difference between the ZIP codes with higher exposure to the policy change and lower exposure to the policy change is reported in column (4). Stars represent p-values of a t-test comparing the difference in column (4) to zero.

Data Sources: 1999 San Francisco Assessor's Secure Housing Roll (Office of the Assessor-Recorder, 1999), San Francisco Rent Board Evictions Notices and Wrongful Evictions Claims (Board, 2005; U.S. Census Bureau and Social Explorer, 2022)

test whether this difference is statistically significant. We find no differences between our less treated and more treated ZIP codes. In Appendix Table C2, we include these variables continuously in a regression framework and find no statistically significant relationship between any of these characteristics and the number of treated units.

Panel B examines housing characteristics. We see that there are differences across our highly treated and less treated ZIP codes in both pre- and post-period housing characteristics. Our highly treated ZIP codes both have more newly rent controlled units and more units that were previously rent controlled. There are also differences in levels of the number of eviction notices and wrongful eviction claims before the policy.

4.3 Patterns

In this section, we explore patterns in the data related to our three primary hypotheses. Recall we would expect to see increases in evictions at newly rent controlled buildings where landlords have not previously been able to set rent or pick tenants optimally given rent control regulations. These increases should be larger in periods when there is market pressure to do so in the form of rapidly rising rents. Finally, we should expect more eviction activity in neighborhoods where landlords are less likely to anticipate challenges from tenants.

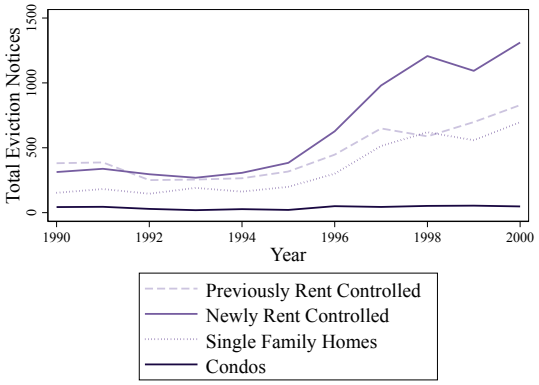
We start by plotting the number of evictions at different building types over time in Figure 3, subfigure (a). We classify buildings as being previously rent controlled (5 or more units, built before 1980), newly rent controlled (2–4 units, built before 1980), single family buildings (1 unit, not condo classified), and condominiums (1 unit, classified as condos). Before the policy change, all building types trend similarly in terms of eviction notices. After the policy change, there is a sharp increase in evictions at buildings directly affected by the policy change: newly rent controlled buildings.

Then, in subfigures (b), (c), and (d), we show the average number of eviction notices, wrongful evictions claims, and owner move-in eviction notices by ZIP code for ZIP codes in the lowest, middle, and highest tercile of the number of units newly treated by rent control as determined by the age and unit size of the building. Before the referendum's passage in 1994, all three terciles had roughly constant average reports of eviction notices and wrongful eviction claims. Starting in 1996, we see sharp increases in the number of wrongful eviction claims made in ZIP codes with medium or high levels of units newly exposed to rent control policies. However, in ZIP codes where relatively few units are newly exposed to rent control, we see that wrongful eviction claims remain roughly constant.

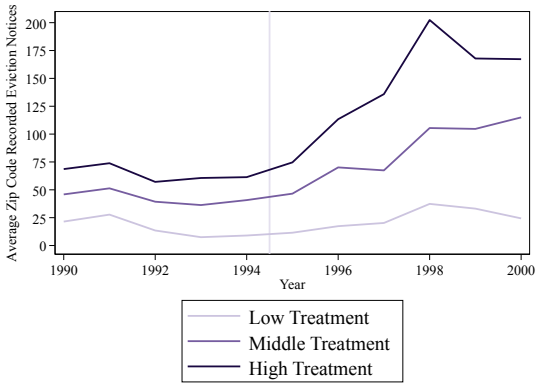
Because reliable ZIP code–level data on eviction notice types is not available before 1994,

Figure 3: Eviction Notices and Wrongful Eviction Claims Over Time

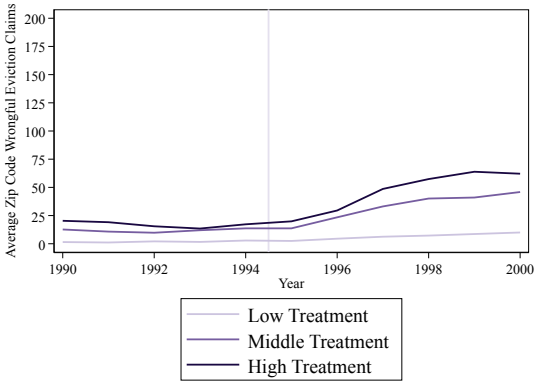
(a) Eviction Notices By Building Type



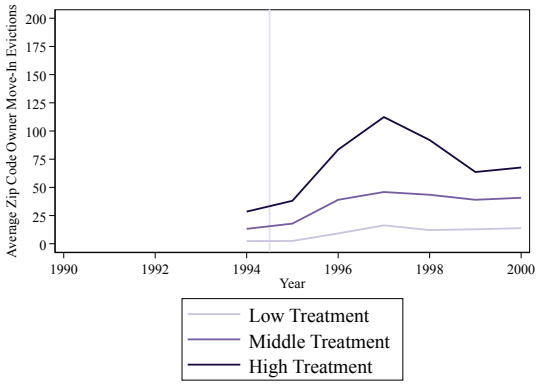
(b) Eviction Notices By Treatment Terciles



(c) Wrongful Eviction Claims By Treatment Terciles



(d) Owner Move-In Eviction Notices By Treatment Terciles



Notes: Subfigure (a) plots total evictions in San Francisco by different classifications of buildings. Newly rent controlled buildings are buildings with 2–4 units built before 1980. Previously rent controlled buildings are buildings with 5+ units built before 1980. Condos are buildings listed as condos built before 1980. Subfigures (b) and (c) show the average number of eviction notices and the average number of wrongful eviction reports made to the San Francisco Rent Board, respectively, by year for three treatment tercile groups. Subfigure (d) splits owner move-in eviction notices by treatment terciles for ZIP codes. These data are not available with any reliability at the ZIP code level before 1994. Low treatment ZIP codes are those in the lowest tercile of units newly exposed to rent control after the policy change. Middle treatment ZIP codes are those in the middle tercile. High treatment ZIP codes are those in the highest tercile. Our treatment is measured by the number of units that are newly rent controlled; they are in buildings that were built before 1980 and have 2–4 units. The timing of the policy change is marked by a vertical line.
Data Sources: 1999 San Francisco Assessor's Secure Housing Roll (Office of the Assessor-Recorder, 1999), San Francisco Rent Board Evictions Notices and Wrongful Evictions Claims (San Francisco Rent Board, 2021)

we plot owner move-in evictions by treatment terciles starting in 1994 when the data became available. Owner move-in evictions are within control of the landlord, whereas other eviction types either require the landlord to convert to a condo or sell the unit, or require the tenant to have been at fault for the eviction. Thus, we would expect the increase in evictions to be concentrated in owner move-in evictions. From this, we see that the increase in owner move-in evictions in Panel A is driven largely by ZIP codes in the highest treatment tercile.

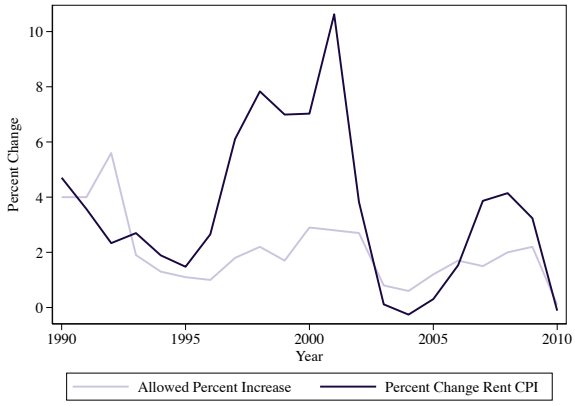
Next, we explore whether evictions increase more in times where market rents exceed the rate of increase allowed. Figure 4 displays the allowed rent increase relative to the percent change in the rent CPI along with the number of evictions in different treatment ZIP codes over time. In the ten years between 1995 and 2005, the percent change in rent CPI was far higher than the approximately 2% increases that landlords were allowed, leading to a large incentive to evict and reset rents to the higher market rates. During this same timeframe, evictions were highest, especially for the ZIP codes in the highest tercile of treatment. Subfigure (b) displays this pattern, plotting eviction notices by treatment tercile over the longer time horizon.

Finally, we explore whether differences in ZIP code median income are correlated with evictions. We break ZIP codes into six groups, based on treatment tercile and whether the ZIP code has above or below median income. Figure 5 displays both eviction notices (subfigure (a)) and wrongful eviction claims (subfigure (b)) for these groups. The solid lines display the effects for each treatment tercile for low-income ZIP codes, whereas the dashed lines display effects for high-income ZIP codes. There are higher levels of eviction notices and wrongful eviction claims in low-income ZIP codes than in high-income ZIP codes, suggesting that low-income ZIP codes experienced the brunt of the increase in evictions after the rent control policy was expanded. Appendix Figure B5, subfigure (b) shows wrongful evictions per eviction notice by treatment tercile and income; there does not appear to be a larger or smaller increase in the proportion of notices also being submitted as wrongful eviction claims in low- versus high-income ZIP codes.

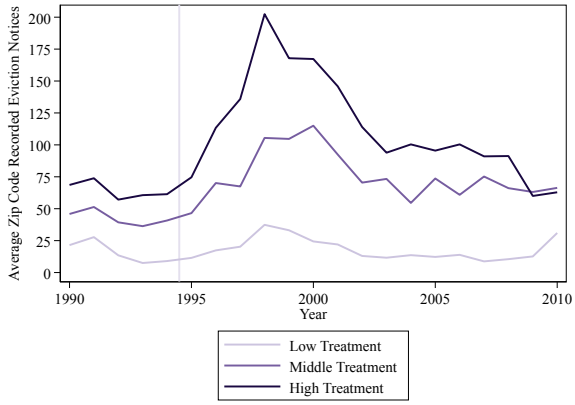
Together, the patterns in this section show that evictions increased most in the most heavily treated areas, that evictions increased more during times when the incentive to evict was higher due to higher market rents, and that these increases were more dramatic in lower-income areas.

Figure 4: Long-Run Rent Price and Eviction Notice Dynamics

(a) Allowed Rent Increases and Rent CPI



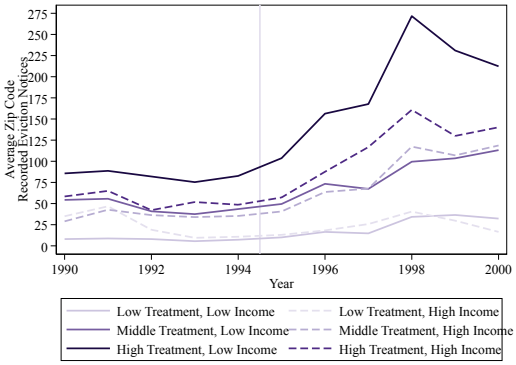
(b) Eviction Notices



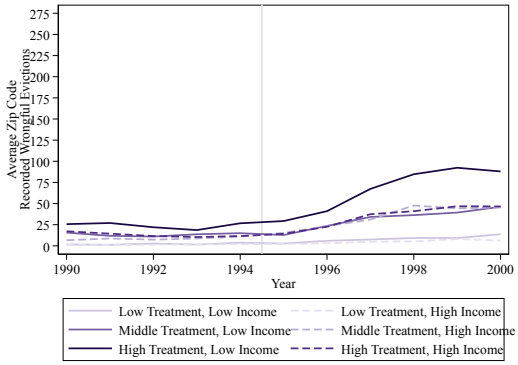
Notes: Subfigure (a) shows allowed rent increases and the San Francisco rent price index, both in percent changes year over year. The allowable increase is set to be 60% of the percent increase in the consumer price index in San Francisco, calculated annually for the one-year period ending each October 31. Subfigure (b) shows the average number of evictions by treatment tercile from 1990 until 2010.
Data Sources: Rent Arbitration Board (Rent Arbitration, 2022), U.S. Bureau of Labor Statistics, 1999 San Francisco Assessor's Secure Housing Roll (Office of the Assessor-Recorder, 1999), San Francisco Rent Board Evictions Notices and Wrongful Evictions Claims (Board, 2005)

Figure 5: Evictions by Treatment Tercile and Income

(a) Eviction Notices



(b) Wrongful Eviction Claims



Notes: Subfigure (a) shows eviction notices over time, split into treatment terciles separately for ZIP codes with low (below median) income versus high (above median) income. Panel B shows wrongful eviction claims for the same groups. Appendix Figure B5, subfigure (b) displays the same for the ratio of wrongful eviction claims to eviction notices.
Data Sources: Rent Arbitration Board (Rent Arbitration, 2022), San Francisco Assessor's Secure Housing Roll (Office of the Assessor-Recorder, 1999), San Francisco Rent Board Evictions Notices and Wrongful Evictions Claims (Board, 2005)

5 Empirical Design

Thus far, we have presented descriptive evidence documenting a rise in evictions following the passage of the 1994 referendum that lifted the exemption for small owner-occupied buildings. This rise in evictions was concentrated largely in buildings that would have been newly exposed to rent control, in ZIP codes with a high concentration of newly rent controlled buildings, and in eviction types that are in the direct control of landlords, as opposed to those that would require some fault of the tenant. This evidence is consistent with the citywide increase in evictions being driven by the policy change.

To further support this hypothesis, we now present a differences-in-differences design that allows us to more rigorously test whether ZIP codes with different levels of exposure to the policy were trending similarly prior to the passage of the policy. We can also quantify the size of the effects over time and test whether the effects were different in ZIP codes with different characteristics.

5.1 ZIP Code–Level Analysis

To explore whether ZIP codes with more newly rent controlled units experience increased eviction rates, we use a continuous treatment differences-in-differences design. This design exploits variation across ZIP codes in the number of units who become exposed to rent control policies after the passage of the voter referendum in late 1994.

We estimate the following linear regression model:

$$Y_{it} = \alpha_i + \tau_t + \beta \cdot \text{Post}_t \times \text{Number Treated}_i + \epsilon_{it}, \tag{1}$$

where α_i are ZIP code–level fixed effects;[23] τ_t are year fixed effects; Post_t is an indicator for whether the year is 1995 or after; and Number Treated_i is the number of newly treated units at the ZIP code level, calculated as discussed in Section 4. The two outcomes we study are the number of eviction claims at the ZIP code level and the number of wrongful eviction claims at the ZIP code level.[24]

We estimate this model both in levels (as written in Equation 1), but also as fractions of the total number of units in ZIP codes to account for the fact that denser ZIP codes may have more treated units because they have more units overall.

The parameter we aim to identify is the average causal response (ACR), which indicates the additional increase in evictions due to an additional treated unit (Angrist and Imbens, 1995). To identify the ACR, we must make three assumptions: the Stable Unit Treatment Value Assumption (SUTVA), parallel trends, and the homogeneity of the treatment effect across differently treated groups (Callaway et al., 2021).

SUTVA requires that there are no spillovers between our treatment groups. This assumption could be violated in our setting if there are either anticipatory effects or spillovers across different ZIP codes. We find no evidence of anticipatory effects in our raw data plots in Figure 3. Additionally, given that the ballot referendum passed by a small margin, it is unlikely that tenants or landlords would have known whether the referendum would pass ahead of time (Harrison, 1994).

A bigger concern is spillovers across units. To interpret our results as causal effects,

[23] We do not include time-varying ZIP code controls due to a lack of availability of data during the time period we study.

[24] Ideally, we would be able to run this analysis separately by different types of eviction notices (owner move-in, Ellis Act, non-payment, etc.). However, data on the type of eviction notice is available only for a (likely selected) subset of eviction notices prior to 1997.

we must assume tenants in units in less treated areas are not increasingly (or decreasingly) evicted due to price equilibrium effects caused by the policy change. This is more likely to be true in areas that were previously heavily rent controlled because the high stock of rent controlled units would lead to stronger equilibrium effects. The fact that we find that the post-1994 increase in evictions was largely concentrated in units that we would predict were directly affected by the policy bolsters this assumption.

While we cannot measure overall rent effects due to lack of available data on rents at the ZIP code level in the 1990s, we can control for the number of previously treated units. We also can sign the size of the bias under the assumption that removing more units from market rents would create upward pricing pressure on market rents. This pricing pressure would create incentives for landlords in all rent controlled units to evict tenants, so if neighborhoods that are less treated by the policy still experience an increase in evictions due to the policy, our estimated effects will be a lower bound.

We also make a parallel trends assumption that requires that neighborhoods would have experienced similar trends in eviction levels had they been assigned a different number of treated units. Our raw data plots support this assumption. To further address concerns that ZIP codes with higher levels of treatment may have trended differently from those with lower levels of treatment, we estimate several alternative specifications. First, we include treatment tercile–specific linear time trends. If it was the case that ZIP codes with more housing units (and thus more treated units) were trending differently in the 1990s, this specification should allow us to capture these different trends. Second, we include the interaction between the number of previously treated units and the post period. This both allows us to capture spillovers and to account for differences in the housing stock across more and less treated ZIP codes. Finally, we estimate regressions at the census-tract and building type–ZIP level (described in greater detail in Section 5.2) that allow us to include ZIP–year fixed effects to account for the fact that ZIP codes with more treated units may have trending different in the absence of the policy.

Finally, we must assume the homogeneity of treatment effects across differently treated groups. Regardless of the number of rent controlled units in ZIP codes, the treatment effect of adding an additional rent controlled unit must be the same. This assumption would be violated if ZIP codes (or the individuals who live there) could select into different levels of treatment, since the covariates of individuals in neighborhoods who selected into different levels of treatment may affect their potential treatment effect. If this was the case, we would expect to see differences in characteristics potentially related to effect sizes based on the

number of treated units. We find no evidence of differences in characteristics across groups in Panel A of Table 1, although we note that we are not powered to detect small differences. In Appendix Table C2, we examine these relationships in a linear regression framework and find no evidence of demographic characteristics predicting treatment.

5.1.1 Dynamics

To explore the dynamics of the effects over time, we break down the pre and post periods into individual years in an event study specification. We interact each year with the number treated units in a given ZIP code. There is a single treatment time in our setting, so event time and calendar time are equivalent. Our event study specification is:

$$Y_{it} = \alpha_i + \tau_t + \sum_{t \neq 1994} \beta_t \cdot \text{Year}_t \times \text{Number Treated}_i + \epsilon_{it}. \tag{2}$$

We normalize to 1994, the year that the policy passed.

5.1.2 Heterogeneity by Median Income

Finally, we explore heterogeneity by median income in the 1990 census. We group ZIP codes into whether their median income in the 1990 census is above or below the median across ZIP codes. We estimate the following specification:

$$\begin{aligned} Y_{it} = \alpha_i + \tau_t + \gamma_t \times \text{Low Income}_i + \beta_1 \cdot \text{Post}_t \times \text{Number Treated}_i + \\ \beta_2 \text{Post}_t \times \text{Number Treated}_i \times \text{Low Income}_i + \epsilon_{it}, \end{aligned} \tag{3}$$

where Low Income_i is an indicator for whether a ZIP code's median income was below the median for San Francisco in the 1990 census, and γ_t are year low income fixed effects.

5.2 Address-Level Analysis

We supplement our main analysis with analysis that uses the individual-level evictions data we have available. We merge address-level eviction notices data with data from the Assessor's office.[25] We then can identify the size of the building at which the eviction occurred. We

[25] Data on wrongful eviction claims are only available at the ZIP code–level so we are limited to studying eviction notices.

construct a ZIP code–level measure of the number of evictions at buildings with 2-4 units to estimate the direct effect of the policy. Recall that our main specification may pick up both evictions at newly rent controlled buildings as well as evictions at previously rent controlled buildings.

We additionally use these address-level data to identify the number of evictions that occur at the census tract level as well as the number of newly treated units at the census tract level. These data allow us to estimate two additional specifications that include ZIP code by year fixed effects.

First, we leverage information on the classification of the building to isolate the effects on newly treated buildings as follows:

$$\text{Evictions}_{ijt} = \alpha_j + \alpha_{it} + \beta \cdot \text{Post}_t \times \text{Number Treated}_i \times I(\text{Newly Treated}_j) + \epsilon_{ijt},$$

where Evictions_{ijt} measures the number of eviction in ZIP code i for building type j in year t, α_j are building type fixed effects, α_{it} are ZIP code by year fixed effects, and $\text{Post}_t \times \text{Number Treated}_i \times I(\text{Newly Treated}_j)$ captures the intensity of treatment at the ZIP code level for only the newly affected buildings (this term is zero for all other building types). We classify buildings as newly rent controlled (2-4 unit buildings built before 1980), previously rent controlled (5+ unit buildings built before 1980), condo buildings built before 1980, and single family homes.

Second, we estimate regressions at the census tract level as follows:

$$\text{Evictions}_{cit} = \alpha_c + \alpha_{it} + \beta \cdot \text{Post}_t \times \text{Number Treated}_c + \epsilon_{cit},$$

where Evictions_{cit} measures the number of eviction notices in census tract c in ZIP code i in year t, α_c are census tract fixed effects, α_{it} are ZIP code by year fixed effects, and $\text{Post}_t \times \text{Number Treated}_c$ is the interaction of being in the post period with the number of treated buildings in the census tract.

6 Results

6.1 Effect of Rent Control on Eviction Notices

Table 2 reports our coefficient estimates from Equation 1. We examine the effects of rent control expansion on eviction notices in Panel A and on wrongful eviction claims in Panel B. Column (1) reports the main differences-in-differences effect in thousands of treated units. For every additional 1,000 newly rent controlled apartments, there are 20.07 additional eviction notices filed in that ZIP code and an additional 7.632 wrongful eviction claims. Since there were about 1,688 newly rent controlled units in each ZIP code on average, these effects constitute an 83% and 125% increase over the pre-treatment average level of evictions for the averagely treated ZIP code.[26]

In column (2), we add treatment tercile–specific linear time trends. In column (3), we control for the effect of previously rent controlled units by including the interaction term between the number of previously rent controlled units and the post period. Our results are robust to the inclusion of these additional controls.

We cluster standard errors at the ZIP code level, which is our level of treatment variation. Since there are only 25 ZIP codes, we report p-values from a wild cluster bootstrap, which more accurately estimates clustered standard errors when the number of clusters is small (Cameron, Colin A. et al., 2008; Canay et al., 2021). p-values for each relevant regression estimate are calculated using the routine provided by Roodman et al. (2019).

Table 3 presents regression estimates from alternative specifications that leverage the data at the eviction notice level merged to the assessor data. Column (1) presents our baseline specification for comparison. Column (2) presents results from a specification in which our treatment variable is instead the fraction of total units in a ZIP code who are newly rent controlled, and our outcome variable is the fraction of units in a ZIP code that experience an eviction. Column (3) uses an alternative outcome measure: how many evictions we see at 2–4 unit buildings built before 1980. Column (4) includes as additional control groups other building types and includes ZIP code–year fixed effects. Column (5) estimates our main specification at the census tract level, which additionally allows for the inclusion of ZIP code–year fixed effects. Our main result is robust to these alternative ways of specifying the regression model.

[26] In Appendix Figure B5, we show that wrongful evictions increase as a constant fraction of eviction notices.

Table 2: Differences-in-Differences Estimates of the Effects of Rent Control

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Eviction Notices						
Num Treated (1000s) × Post	20.07***	15.39***	19.50***	15.24***	6.986	15.26***
	(2.926)	(3.776)	(3.044)	(2.374)	(4.214)	(2.323)
Num Treated (1000s) × Post				9.662***	11.99***	9.669***
× Low Income				(2.651)	(3.637)	(2.677)
Treatment Tercile		X			X	
Prev. Treated Control			X			X
N	275	275	275	275	275	275
R^2	0.882	0.890	0.883	0.892	0.901	0.892
P-value Num Treated × Post	0	0.00300	0	0.00300	0.187	0.00400
P-value # Treated× Post× Low Income	.	.	.	0.0140	0.0290	0.0100
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Wrongful Eviction Claims						
Num Treated (1000s)× Post	7.632***	7.090**	7.197***	4.191***	2.017	4.030***
	(2.344)	(2.761)	(2.506)	(1.184)	(2.364)	(1.152)
Num Treated (1000s) × Post				6.718**	7.403**	6.653**
× Low Income				(2.705)	(3.300)	(2.834)
Treatment Tercile		X			X	
Prev. Treated Control			X			X
N	275	275	275	275	275	275
R^2	0.830	0.840	0.831	0.847	0.858	0.847
P-value Num Treated× Post	0	0.0850	0	0.0160	0.429	0.0120
P-value # Treated× Post× Low Income	.	.	.	0.165	0.148	0.219

Notes: This table shows estimates from differences-in-differences regressions. In Panel A, the outcome variable is wrongful eviction claims, and in Panel B, the outcome is eviction notices. For both panels, column (1) shows the results from our preferred specification, a differences-in-differences regression using the number of treated units at the ZIP code level as a continuous treatment measure. Columns (1)–(3) show average effects, whereas columns (4)–(6) add heterogeneity by median income. Columns (2) and (5) add treatment tercile–specific linear time trends to the regression, and columns (3) and (6) control for the number of previously rent controlled units. Treatment is measured in 1,000s of newly rent controlled units. If eviction notices increased solely due to the mechanical effect (following the same eviction pattern as the pre-period units), then we would have expected an increase of 12.587 eviction notices and 3.665 wrongful eviction claims. Wild cluster bootstrap p-values are reported in the last two rows.
Data Sources: 1999 San Francisco Assessor's Secure Housing Roll (Office of the Assessor-Recorder, 1999), San Francisco Rent Board Evictions Notices and Wrongful Evictions Claims (Board, 2005; U.S. Census Bureau and Social Explorer, 2022)

Table 3: Differences-in-Differences Estimates: Alternative Specifications

	(1) Baseline	(2) Fractional	(3) Treated Unit Evictions	(4) ZIP Code–Year FEs	(5) Census Tract
Num Treated × Post	20.07***		16.88***	17.48***	19.02***
	(2.926)		(1.951)	(2.271)	(0.812)
Frac Treated × Post		0.0144***			
		(0.00343)			
N	275	275	275	1100	6182
R-Squared	0.882	0.650	0.875	0.662	0.621
P-Value	0	0.00100	0	0	0

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows estimates from differences-in-differences regressions where eviction notices are our outcome of interest. Column (1) reports our baseline estimates. Column (2) reports estimates where both the outcome and treatment variable are expressed relative to the number of units in a zip code. Column (3) replaces the outcome variable with the number of eviction notices in the ZIP code for only treated units. Column (4) is run at the building type-ZIP code–level and includes ZIP code–year fixed effects. Column (5) is run at the census tract level and includes ZIP code–year fixed effects. Wild cluster bootstrap p-values are reported in the last row.
Data Sources: 1999 San Francisco Assessor's Secure Housing Roll (Office of the Assessor-Recorder, 1999), San Francisco Rent Board Evictions Notices and Wrongful Evictions Claims (Board, 2005; U.S. Census Bureau and Social Explorer, 2022)

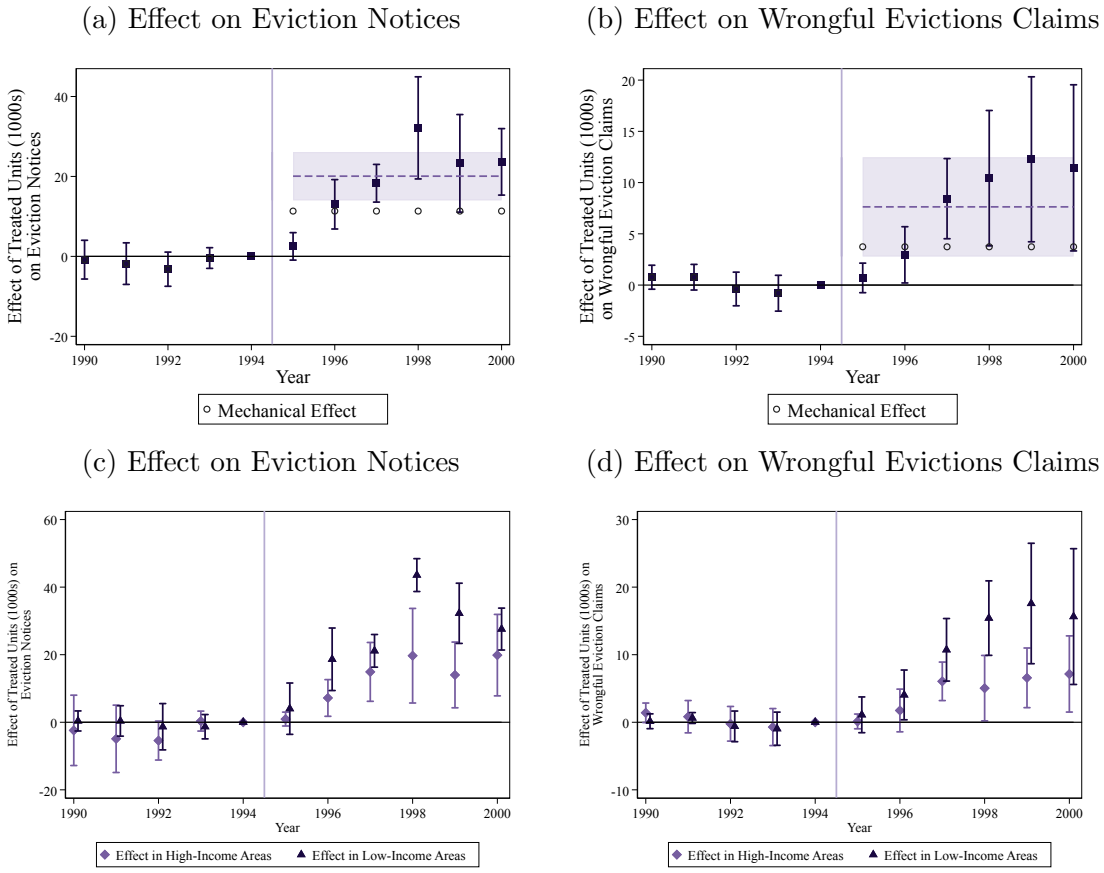
6.2 Dynamics

Figure 6 reports our event study coefficient estimates of Equation 2. While there is very little difference between the high- and low-exposure ZIP codes before the policy changed, supporting the parallel trends assumption, after the policy changed, we see that both eviction notices and wrongful eviction claims rise only in ZIP codes with higher levels of exposure. The effect increases over time, consistent with the increasing incentives to evict. We report our differences-in-differences estimates in the dashed line.

We additionally report what the mechanical increases in eviction behavior resulting from the increase in the number of units covered by the Rent Board would be, assuming that behavior remained constant from the pre-period. This is the increase in eviction notice reporting that would be expected if newly rent controlled landlords behaved similarly to previously rent controlled landlords. We show that our estimated effects for eviction notices greatly exceed the mechanical effect of the policy, consistent with changing eviction behavior in the post period. Our average estimate of the effect on wrongful eviction claims is not statistically different than the mechanical increase; however, the effect for individual years is statistically significantly higher than the predicted mechanical effect. This suggests that

either the type of "mom-and-pop" smaller-scale landlords behave differently than larger-scale landlords in the face of a rent control policy, or that the short-term effects of a rent control policy led to larger increases in evictions than the equilibrium/long-term effect.

Figure 6: Effect of Removing Rent Control Exemptions On Eviction Notices and Wrongful Eviction Claims



Notes: In subfigures (a) and (b), this figure shows our event study coefficient estimates on the interaction between year dummies and the number of treated units in a ZIP code. We normalize 1994 to be zero. Error bars shown are for the 95% confidence interval. The differences-in-differencess estimate for the interaction of the post-period with the number of treated units is shown as a gray dashed line with the 95% confidence interval shown as a shaded region on the graph. Standard errors are clustered at the ZIP code level. Effects are scaled to be the effect per 1,000 treated units. The average number of treated units in a ZIP code is 1,688. The dark purple circles show what the mechanical effect of the policy change would have been if eviction behavior post policy was consistent with eviction behavior pre policy.
Subfigures (c) and (d) show our event study coefficient estimates on the interaction between year dummies and the number of treated units in a ZIP code (light purple) and this added to the interaction of these estimates with the triple interaction with low income (dark purple).
Data Sources: 1999 San Francisco Assessor's Secure Housing Roll (Office of the Assessor-Recorder, 1999), San Francisco Rent Board Evictions Notices and Wrongful Evictions Claims (San Francisco Rent Board, 2021)

We focus primarily on the short-run effects, as the parallel trends assumptions for our identification strategy become harder to justify in the longer run. However, in Figure 7, we present suggestive evidence on the longer-term effects on eviction notices and compare these

effects to the difference between aggregate rent prices and statutorily allowed rent increases. The effects converge to the expected mechanical increase of 57% by the early 2000s and are higher in periods where rent prices are rising faster than the statutorily allowed rent increases. In particular, we see large effects in the run-up to the dot com bubble, where there is considerable growth in housing prices.

6.3 Heterogeneous Effects By Rent and Income

Recall that one prediction of our theoretical model is that landlords should engage more in eviction behavior (particularly the wrongful kind) in situations where they expect the costs of doing so to be lower. These costs may be lower for tenants with fewer resources to fight eviction proceedings. One potential proxy for these resources is ZIP code income levels.

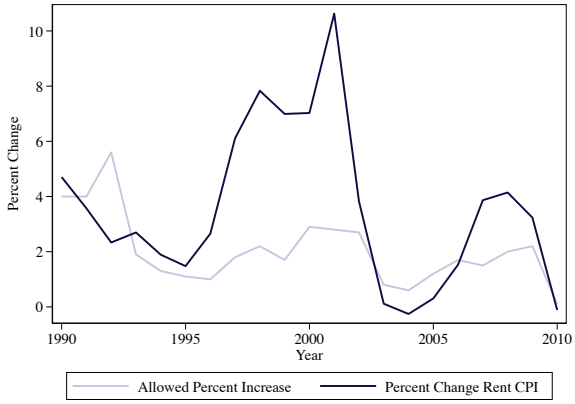
Columns (4)–(6) of Table 2 report the results of Equation 3, evaluating whether low-income ZIP codes are more likely to experience higher evictions. We find that low-income ZIP codes are significantly more affected by the increases in rent control, both in terms of eviction notices and wrongful eviction claims. In our preferred specification, low-income neighborhoods experience nine more eviction notices per 1,000 treated units than high income ZIP codes. This represents effect sizes that are 63% larger in low-income ZIP codes. Similarly, we find evidence of larger effect sizes for wrongful eviction claims for low-income ZIP codes relative to high-income ZIP codes; our estimated effects are 160% larger in low-income ZIP codes. We plot equivalent event study estimates in subfigures (c) and (d) of Figure 6 where we compare the effect over time for low and high income ZIP codes.

One may worry that rent prices and incomes are correlated, and that the large increases in evictions are not due to lower incomes of residents in those ZIP codes but are because rents in lower-income areas start off low and have more room to increase, leading to larger incentives to evict in those areas. In Appendix Figure B8, we show the relationship between 1990 and 2000 rent and income across ZIP codes in San Francisco. Although income and rent are correlated (especially in 1990), we do not find significantly different effects of rent control on evictions in low- versus high-rent areas as we do when splitting by income (see Appendix Figure B7). This suggests that the heterogeneity by income is not explained by differences in rents.

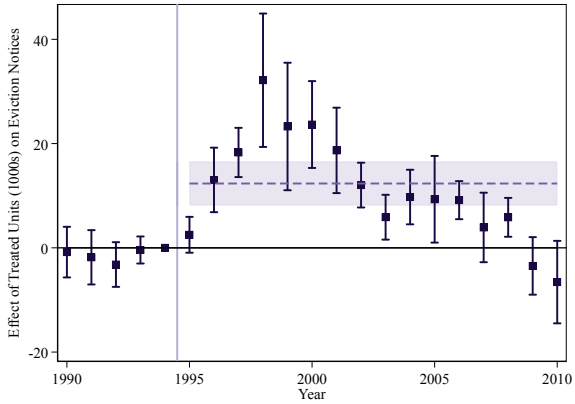
Unfortunately, limited data are available on other characteristics of tenants or landlords in these lower income ZIP codes, so we cannot rule out that lower income ZIP codes are different in other ways. For instance, it may be the case that tenants in lower income ZIP codes are particularly likely to stay in their rental units for a long time, increasing the

Figure 7: Long-Run Rent Price and Effect-Size Dynamics

(a) Allowed Rent Increases and Rent CPI



(b) Effect on Eviction Notices



Notes: Subfigure (a) shows allowed rent increases and San Francisco rent price index, both in percent changes year over year. The allowable increase is set to be 60% of the percent increase in the consumer price index in San Francisco, calculated annually for the one-year period ending each October 31. Subfigure (b) shows our event study coefficient estimates on the interaction between year dummies and the number of treated units in a ZIP code over the longer time horizon from 1990 until 2010. We normalize 1994 to be zero. Error bars shown are for the 95% confidence interval. The differences-in-differencess estimate for the interaction of the post period with the number of treated units is shown as a gray dashed line with the 95% confidence interval shown as a shaded region on the graph. Standard errors are clustered at the ZIP code level. Effects are scaled to be the effect per 1,000 treated units. The average number of treated units in a ZIP code is 1,688.

Data Sources: Rent Arbitration Board (Rent Arbitration, 2022), U.S. Bureau of Labor Statistics, 1999 San Francisco Assessor's Secure Housing Roll (Office of the Assessor-Recorder, 1999), San Francisco Rent Board Evictions Notices and Wrongful Evictions Claims (Board, 2005)

burden of rent control policies for landlords. However, we view these results as suggestive that low income tenants may be particular vulnerable to landlords using eviction proceedings in response to new rent control policies.

6.4 Potential Equilibrium Effects

Evaluating the direct effects of the policy on rent is difficult as rent data at the ZIP code–level from the 1990s are difficult to find. In Appendix Figure B6, we look at the effects of the policy on rent using ZIP code–level rents from the 1980, 1990, and 2000 census and find weak evidence that rents in ZIP codes that were heavily exposed to this rent control expansion experienced small declines relative to other ZIP codes, possibly due to the direct effects of the policy change.

In Appendix A.4, we further explore how the policy may have affected both units directly affected by the policy and those indirectly affected by the policy by leveraging address-level data, which allows us to estimate the effects separately for buildings likely to have been directly affected (those newly rent controlled) and those affected only indirectly (those that were rent controlled before the policy change). In Appendix Table C6, we estimate the effect of the policy separately for eviction notices at buildings with different classifications. In Panel A, we find a statistically significant effect of the policy only for evictions at buildings that were likely newly rent controlled. We also look at the effects of the number of previously rent controlled units to explore the spillover effects of the policy. In Panel B, we find effects of the number of previously rent controlled units are largest for evictions at previously rent controlled buildings. In Panel C, we include both the number of newly treated units and previously rent controlled units and find that the effects of newly rent controlled units are concentrated on newly rent controlled buildings, where there are possible spillovers to previously rent controlled buildings.

6.5 Additional Robustness Checks

Beyond the alternative specifications already discussed, we perform additional robustness checks in the appendix. First, we use a binary measure of treatment, which discretizes exposure, to address concerns that rent control exposure should not enter the specification linearly. We compare neighborhoods with above-median numbers of newly rent controlled units to those with below-median exposure. We find large effects comparable to our main specifications.

Second, we use an alternative measure of the number of newly rent controlled units to address concerns that we have mismeasured treatment by not accounting for the fact that buildings needed to be owner-occupied to be eligible for the exemption to rent control prior to 1994. In Appendix A, we detail the steps we take to account for this regulatory feature. We find that use of this alternative measure of policy exposure does not substantively change our results; while our estimates change in size, this change is proportional to the change in our treatment variable.

We report alternative specifications that evaluate the sensitivity of our results to how we measure treatment in Appendix Tables C4 and C5. Columns (1) and (2) are the same as the main specifications reported in the paper. Columns (3) and (4) use a measure of treatment that accounts for owner occupancy. Columns (5) and (6) instead use a binary measure of treatment based on whether ZIP codes are above or below the median level of treatment.

Finally, we further investigate whether our effects can be attributed to the mechanical increase in the number of units whose evictions would be reported to the Rent Board beyond comparing our effect sizes to the effect that would be expected if newly rent controlled units experienced evictions at the same rate as previously rent controlled units. Only units covered by the Rent Ordinance will generate either eviction notices or wrongful eviction claims in our data. Non-rent controlled units may still receive eviction notices for non-payment of rent, but these are not required to be reported to the Rent Board. Additionally, these units are not covered by "just cause" provisions, and so tenancies can be ended without eviction proceedings at all.

Because only those in rent controlled units were eligible to submit eviction notices and wrongful eviction claims to the Rent Board, we worry that the effect of rent control on evictions is not due to a behavioral change, but instead mechanical. Suppose 1% of tenants are evicted regardless of rent control status. If an additional 100 buildings are subjected to rent control, then an increase of one eviction would be expected, mechanically. To explore whether the effects we find are mechanical or due to behavioral change, we examine the proportion of rent controlled tenants that face eviction notices before and after the policy change. If the rate of eviction stays constant over time, then the effects are purely due to changes in data reporting, whereas if the rate increases at the time of the policy change, then effects are likely due to behavioral changes by landlords.

To evaluate whether our effects are driven by the change in reporting, we compare the average number of eviction notices and wrongful eviction claims to the number of units covered by the Rent Ordinance before and after the expansion of rent control. In 1994, there

were 930 eviction notices recorded with the Rent Board across San Francisco and 122,052 total rent controlled units. This represents a 0.7% eviction rate for rent controlled units. By 1998, there were 2,867 evictions with 164,264 rent controlled units, a 1.7% eviction rate, more than doubling the eviction rate in 1994.

In Appendix Figure B4, we show a time series of the number of eviction notices and wrongful eviction claims, which demonstrates a large jump up in the ratio of evictions to rent controlled units following the policy change. This figure suggests that just after rent control expanded, landlords adjust by evicting tenants, many times wrongfully. The gradual increase after the policy change makes sense; as time passes from the policy change, the difference between market rent, which can freely increase, and controlled rents, which can increase by at most about 2% each year, increases. With it increases the incentive to evict a tenant in order to reset the rent to market rate or convert the building to condos. Indeed, Diamond et al. (2019) find an 8 percentage point increase in condo conversions following this same rent control expansion.

6.6 Discussion

Several behavioral changes could drive our results: landlords could increase lawful evictions, landlords could increase wrongful evictions, and tenants could change how they challenge evictions. To see our results, landlord behavior must be changing given that we see large increases in eviction notices. These eviction notices are particularly concentrated in owner move-in evictions, which are in the direct control of the landlord and less likely to be influenced by tenant behavior.[27]

This change in evictions could be driven either by an increase in wrongful or legal evictions. We do not find evidence of large changes in the proportion of wrongful eviction claims per eviction notice at the ZIP code level related to the number of newly rent controlled units. However, there is an overall increase in the ratio of wrongful eviction claims per eviction notice: there were 0.25 claims per notice pre policy and 0.34 claims per notice post policy.

Newly rent controlled tenants may behave differently in response to an eviction notice than previously rent controlled tenants. This change could go in either direction: If the policy change coincided with greater awareness of tenant rights, there could be an increase

[27]Lack of data availability on eviction types at a more granular level than city wide prior to 1997 limits us from running our analysis on particular types of evictions at the ZIP code or building level. In Appendix Figure B9, we plot the types of eviction notices for units in newly treated buildings and in previously treated buildings from 1997 to 2000.

in the number of wrongful eviction claims for rent controlled tenants. However, newly rent controlled tenants may either have less incentive to file a claim (their rent is not as far removed from market rent as that of a long-term rent controlled tenant) or may be less aware of the regulations around just cause evictions, making them less likely to file a wrongful eviction claim.

Landlords of newly rent controlled buildings may also behave differently than landlords whose buildings were previously rent controlled landlords. We find large increases in the number of eviction notices filed relative to the number of rent controlled units (see Appendix Figure B4). This may be because of the "mom-and-pop" nature of the newly controlled landlords, but it also could be because of the sudden change of incentives that newly controlled landlords face. Recall that all newly controlled landlords decided to operate in the rental market at a time when they could charge market rates, select tenants, and set rent prices with the understanding they could increase rents. To provide some evidence on the relative importance of these two mechanisms, we plot a comparison between buildings with 2 units, buildings with 3–4 units, and buildings with 5–6 units in Figure 8. It is likely that owners of buildings with 3–4 units would be similar to owners of buildings with 5–6 units in the absence of differential rent control legislation. We show that buildings with 5–6 units do not follow the same dynamics in the late 1990s, suggesting that the adjustment to rent control plays at least some role in driving our results.

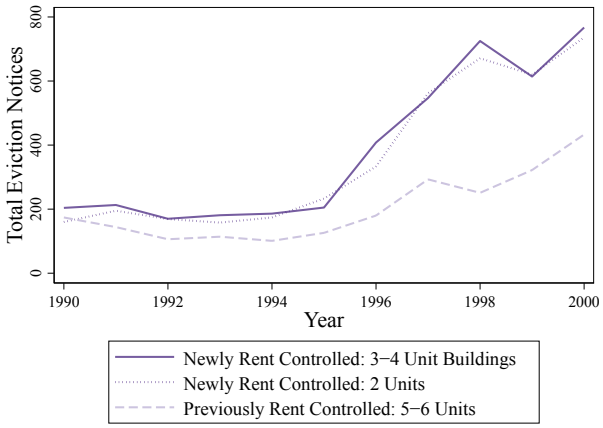
When landlords are newly subjected to rent control regulations, they may respond by either leaving the rental market altogether or by adjusting their behavior while remaining in the rental market. Owner move-in evictions provide one tool that could help landlords achieve either goal. In particular, owner move-in evictions, if executed properly, temporarily remove a unit from the rent controlled market. After the required occupancy period, landlords then have the ability to return to the rent controlled market, set rents, and select tenants in an optimal way given the regulations. They also can chose to continue to have that unit owner-occupied.[28] This option value of owner move-in evictions may make this an attractive option for landlords.

To assess whether our results are fully driven by landlords using owner-move in evictions to fully exit the rental market[29] as an alternative to condo conversions, demolitions, or

[28]One potential concern is that owners of 2 unit buildings anticipated changes to the condo conversion system in 2001 that allowed 2 unit buildings to bypass the lottery system. In Figure, 8, we find no evidence that our results are being driven solely by this behavior; the rise in evictions in buildings with 2 units is comparable to the rise in evictions at buildings with 3–4 units.

[29]Diamond et al. (2019) document frequent selling to owner-occupants following the expansion of rent control in San Francisco.

Figure 8: Evictions By Number of Units



Notes: The figure plots total eviction notices for buildings with 2 units, 3–4 units, and 5–6 units.
Data Sources: San Francisco Rent Board Eviction Notices; 1999 San Francisco Assessor's Secure Housing Roll

rehabilitations, we leverage our building-level evictions data. We measure what percent of evictions in 1995 were followed by another eviction at the same address between 1998–2000. We find that 9.9% of addresses that had an eviction in 1995 also had another eviction between 1998 and 2000, suggesting that while some of our evictions likely represent a true exit from the rental market, others reflect a change in behavior for landlords who stay in the rental market (or at least wish to retain the option to).

7 Conclusion

San Francisco's expansion of rent control in 1994 led to a dramatic increase in rent control. Landlords of newly rent controlled units faced new incentives to turn over the units in order to raise the rents to market levels. One mechanism through which they could do this was eviction, either lawful or unlawful. We study the effects of the policy change on both eviction notices and wrongful eviction claims and find substantial increases in both in ZIP codes that were heavily affected by the policy change.

San Francisco's rent control policy change led to an increase of roughly 20 eviction notices and seven wrongful eviction claims per 1,000 units treated with rent control. Since San Francisco has roughly 1,688 treated units per ZIP code and 25 ZIP codes, we estimate that the rent control policy led to 847 more eviction notices and 322 more wrongful eviction claims than would have happened otherwise. These effects are fairly persistent; it takes about seven

years for our effect sizes to fall to the levels that would be predicted by the increase in rent controlled units that are subject to reporting requirements to the San Francisco Rent Board.

These dynamics suggest that policy makers should expect that the initial few years after the adoption or expansion of rent control policies will result in changes in the market that will adjust to a new equilibrium in the long run. They highlight that while rent control policies may limit displacement in aggregate, there still may be tenants who are displaced as landlords respond to the policy. Given that not all informal evictions will result in wrongful eviction claims, our effects may be lower bounds on the number of tenants who experience some attempt by their landlord to displace them, either through legal or illegal channels.

We further document two sources of heterogeneity in our effect sizes. First, we find that our effects are concentrated in low-income areas. These areas are not necessarily those that saw the largest increases in aggregate rents during the 1990s, suggesting that landlords may be more willing to engage in eviction activity in places where there are fewer resources to fight that behavior. Second, we find that while there is not geographic variation by rent prices, our effects are higher in years when we would expect that the rent control provisions are more binding.

We are cautious about interpreting our results too broadly, as the policy change we study only affects a particular kind of rental unit: small buildings that are owner occupied. "Mom-and-pop" landlords may be more willing to get out of the rental business altogether in response to changes in rent control regulation. Alternatively, they may be positioned to take advantage of owner move-in evictions in a way larger landlords are not. Additionally, small landlords may be more likely to skirt the legal restrictions, either because they are not aware of the correct legal proceedings or because they are more willing to take legal risk than a large-scale landlords.

Even so, our estimates imply that San Francisco's rent control expansion is responsible for a large portion of the increase in evictions in the 1990s. The 847 additional evictions comprise 59% of the increase in evictions overall in San Francisco from 1994 to 2000.

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A Data Appendix

A.1 Assessor Data Cleaning Details

In our analysis, we use data from the San Francisco Assessor's Secure Housing Roll from 1999.[30] The apartment address data from the Assessor's office does not include unit ZIP codes, so we merge the assessor data to the San Francisco's Addresses with Units–Enterprise Addressing System dataset (SFA). The SFA includes complete addresses with ZIP codes, using the parcel number or unit address. While we can match most units with this procedure, some are either missing from the SFA data or do not have unit address information in the assessor data. For these, we either use the Google Map's API to determine ZIP code based on SITUS or use the San Francisco Planning tool to determine the unit's address based on its parcel number.

There are two primary challenges in our use of this data. First, because the assessor dataset is derived from hand-filled forms, there are missing fields for a small number of observations. We address this by imputing information on the missing fields. If any building is listed as having zero units (0.9% of buildings are listed with zero units), we replace the number of units using the median number of units based on that building's class code. This means that, since multi-family residences with class codes of "Flats and Duplexes" have a median of two units, any multi-family residences with that class code will be reassigned to have two units if it has zero units listed in the dataset. Next, for any building that is missing information on the year built (0.6% of buildings are missing the year built), we assume that it was built before 1980 since the median year built of all buildings in San Francisco is 1925.

We additionally explore using future years of assessor data to fill in this missing information as the data will be updated with future transactions; this procedure results in finding information for 0.4% of observations with missing unit information (or where the value for units is 0) using data from 2000 and another 9% of remaining missing observations using 2001 data for units. For year built, using data from 2000 provides information on 16% of missing or nonsensical values, and data from 2001 provides information on 28% of the remaining missing or nonsensical values. However, given our default imputation methods, this results in updates to relatively few buildings and changes the treatment status of even fewer buildings.

The second challenge is that we are unable to observe what the housing stock looked like in 1994, at the time of the policy change. In response to the expansion of rent control,

[30] Records from prior to 1999 have not been digitized.

landlords may have demolished newly rent controlled buildings or converted to condos. In the extreme, we can bound how much this changes our results by assuming all newly built buildings replaced rent controlled buildings or that all condos were converted in the window between 1994 and 1999. We additionally attempt to correct for these changes by relying on other datasets from San Francisco. In particular, we measure how many buildings were demolished between 1995 and 1998 and track how many parcels were split to measure new condos. We do so in the following way.

First, we identify new parcels added between 1994 and 1999. We identify condos as multiple parcels located at the same geographic location with residential zoning. We keep all parcels where there is more than one and less than five parcels at the same location. We then merge these parcels to the assessor data and keep all buildings built before 1980. This procedure results in 869 units, roughly in line with the 200 units per year allowed to convert to condos. We update our ZIP code measure of treatment to add these units back to our number of newly treated units.[31]

Then, we adjust for the number of buildings that were demolished. We find 62 buildings that would have been newly rent controlled were demolished in between 1994 and 1999. We further update our ZIP code measure of treatment.

All resulting measures of treatment are highly correlated. Table C3 reports the correlations between various treatment measures as well as the average number of treated units at the ZIP code level across each adjustment. In this table, we additionally report "worst-case" scenarios that assume that all condos were converted, that new construction replaced rent controlled units, or that building modifications replaced rent controlled units. These measures will be upper bounds on the number of truly rent controlled units in 1994. It is unsurprising that our measures are highly correlated. Condo conversions were highly limited in the 1990s; only 200 units per year could be converted to condos. Additionally, there was a freeze on converting newly rent controlled units for the first couple of years after the expansion of rent control at the end of 1994. We default to using the measure constructed using only the 1999 assessor data due to inconsistencies across publicly available datasets; however, our results are robust to using the alternative measures of treatment we have discussed.

In Appendix Table C1, we compare the number of rental units in our dataset from 1999 to those in the 2000 census to ensure we have correctly categorized and in some cases, imputed, the building size correctly. The differences between measures from our dataset and the census

[31]Note that this is a conservative adjustment; we find inconsistencies with the parcel data and the assessor data that suggest that parcels may remain in the assessor data after they have been removed.

are quite small and could be due to measurement error in either the census or the assessor data (recall that the assessor data relies on hand-filled forms that were later digitized).

In Appendix Table C2, we report results of a regression of various ZIP code characteristics on the number of treated units in our sample to show that the number of treated units in each neighborhood was not confounded with neighborhood characteristics. We find no significant differences in any observable characteristics of neighborhoods. While this is not necessary to interpret our differences-in-differences results as causal effects, it helps establish credibility for the required (and untestable) identification assumptions: that neighborhoods with different levels of treatment would have trended similarly if the policy had not passed, and that treatment effects are homogeneous across neighborhoods with different levels of treatment intensity.

A.2 Alternative Treatment Measure

Although we do not know whether owners occupied their units in 1994 when the policy changed, we can approximate this using a 1999 measure of owner occupancy. To create this, we determine whether the owner's mailing address listed in the Assessor's dataset matches the physical address of the unit. We use this measure of owner occupancy to create an alternative measure of exposure, defined as the number of units in a ZIP code that had between 2 and 4 units, were built before 1980, and were owner-occupied in 1999 (the earliest year of Assessor data available). This alternative measure of exposure to the rent control policy is displayed in Appendix Figure B1. While some areas become more or less exposed based on this alternative measure, the distribution of exposure to rent control looks largely similar to that in Appendix Figure 2.

In Appendix Figure B1 we display a map of San Francisco, where ZIP codes with higher numbers of treated buildings are shown in darker blue. This figure uses the treatment definition that includes 1999 owner occupancy. In comparing to our preferred measure of treatment, which does not account for owner occupancy, it is clear that the two definitions of treatment affect the same neighborhoods to roughly the same extent.

Finally, in Appendix Figure B2, we show the event study results of running our main event study regression using the measure of treatment that includes a 1999 measure of owner occupancy. The pattern of results is the same as those in Figure 6, which does not use any measure of owner occupancy to measure treatment status. While the magnitudes of effects are much larger, since the average number of owner-occupied treated units is lower than the measure that does not take owner-occupancy into account, the proportional effects are

similar to those in our main specification.

A.3 Validating and Cleaning Eviction Notice Data

To measure evictions, we use data from the San Francisco Rent Board, which publishes two measures. First, the Rent Board publishes counts of eviction notices from 1990 and onward for San Francisco as a whole. These counts include eviction notices that were verified by the Rent Board. This dataset is not available at the individual rental unit level before 1997 due to missing fields in the pre-1997 data. However, the total number of annual evictions is published each year starting in 1990. The Rent Board also publishes a dataset at the ZIP code-year level of wrongful eviction claims from 1990 and onward. This dataset includes all allegations of wrongful evictions made by tenants, regardless of whether their eviction was legal or not, and regardless of whether the eviction went through.

We have individual-level data directly from the Rent Board before 1997 and from Brian Asquith from 1997 onwawrd. The Rent Board has provided us with their pre-1997 individual-level eviction notices data with the caveat that the data habr missing fields and have not been audited or checked. First, we check that the pre-1997 data are not missing observations. To do this, we compare the annual number of evictions in the pre-1997 + post-1997 dataset to the published annual number of evictions, finding that the numbers match between the pre-1997 data and the published statistics. If substantial numbers of observations were missing in the data, we would expect to see fewer evictions in the pre-1997 data. We show this comparison in Appendix Figure B3. Since we do not see this, we proceed assuming all evictions are accounted for in the data.

Next, we evaluate missing fields in the pre-1997 dataset. We find that 32% of fields are missing ZIP codes, and 30% of fields are missing the reason for the eviction. Only 0.06% of fields are missing an address. For those observations that are missing ZIP codes, we use Google Maps Places API, searching for the unit's address and filling in the ZIP code that Google Maps associates with the address. We find ZIP codes for all but 124 of the 2,649 rows with missing ZIP codes, so that overall we have ZIP codes for 98% of the data. To ensure the Google Maps API accurately assigns ZIP codes, we compare Google Maps ZIP codes to the ZIP codes included in the data. We find that 88% of ZIP codes are correctly identified by the Google Maps API. We proceed by using the ZIP code given by the eviction notices data unless it is missing, in which case we use the ZIP code given by the Google Maps API, acknowledging the existence of some measurement error in the ZIP code variable. Since there is no way to fill in a missing cause of eviction, we proceed with the understanding that

eviction cause data may be unreliable.

A.4 Address-Level Data

In this section, we discuss how we link address-level eviction notice data from the San Francisco Rent Board. We observe 7,921 eviction notices from 1990–1996 and 11,805 eviction notices from 1997–2000. We merge these addresses to address-level data from the Assessor to obtain information on the treatment status of each unit. We are able to successfully match 81% of evictions before 1997 to addresses in the Assessor data. We are able to successfully match 85% of evictions after 1997 to addresses in the Assessor data. We think it is likely that the remaining unmatched represent minor data errors in the eviction notice data.[32]

We then classify evictions as occurring at buildings that were newly treated or previously treated. Newly treated units are units in buildings with 2–4 units built prior to 1980. Previously treated units are in buildings with more than 4 units built prior to 1980. As discussed in Section 4, our measures of treatment are imperfect because we do not observe owner occupancy at the time of the policy change. Many small buildings may have been subject to rent control even before the policy change because they were not owner-occupied.

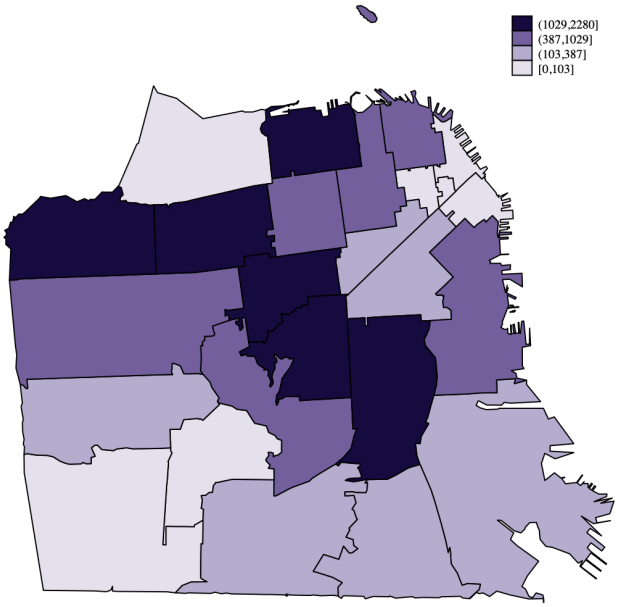
These two categories account for 75% of the evictions we see. We group the remaining units into two further categories. The first is condos built before 1980 where their status as a condo may have changed. We group all remaining units into the final category. The vast majority of these units are single-family dwellings.

Table C6 uses a regression framework to explore where our effects are concentrated. Rather than calculating the total number of eviction notices for each ZIP code, we instead construct the number of eviction notices for buildings in different categories. Panel A looks at our main treatment effects. We find large effects on the number of eviction notices for possibly newly rent controlled buildings, but no effects on evictions at other buildings. We further explore whether there are potential spillovers to other buildings by including as an alternate treatment measure the number of previously rent controlled buildings (in Panel B) and by including both measures jointly (in Panel C). Unsurprisingly, in Panel B, we see increases in evictions at previously rent controlled buildings in ZIP codes with many previously rent controlled units.

[32]We fix errors that are unambiguous. For instance, we correct street names if the wrong street suffix was used (Avenue vs. Street) and there is only one street with that name in San Francisco. Many of the remaining unmatched observations are from streets with multiple suffixes (such as 24th Street and 24th Avenue) where it is unclear whether the building number or the street suffix was incorrect).

B Appendix Figures

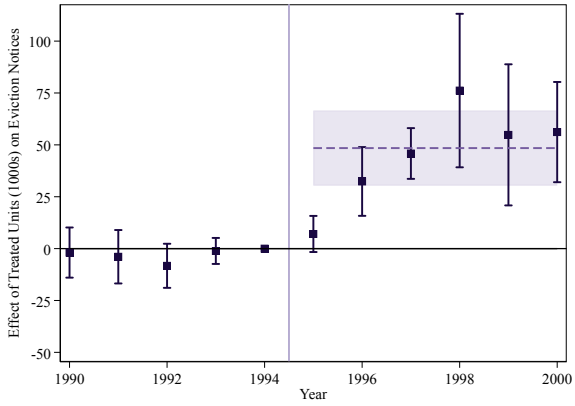
Figure B1: ZIP Code–Level Exposure to Change in Rent Control Policy: Owner Occupancy



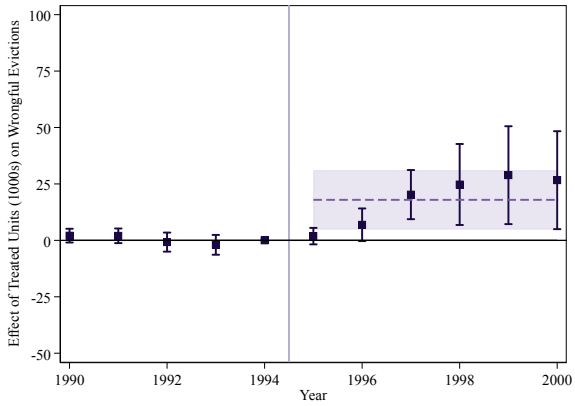
Notes: This map depicts exposure based on owner-occupancy at the ZIP code level for the San Francisco policy change. Exposure is measured as the number of units in a ZIP code in buildings with 2-4 units that were built prior to 1979 where the owner's address matches that of the building.
Data sources: 1999 San Francisco Assessor's Secure Housing Roll and authors' calculations

Figure B2: Effect of Removing Rent Control Exemptions on Eviction Notices and Wrongful Eviction Claims: Owner-Occupancy Robustness

(a) Effect on Eviction Notices



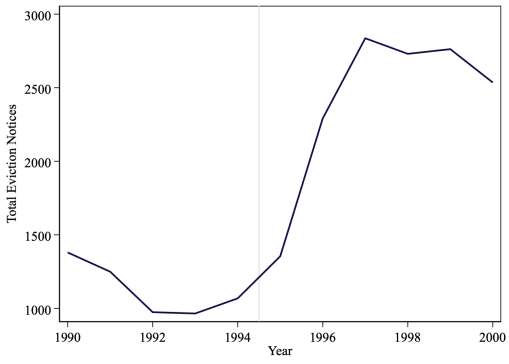
(b) Effect on Wrongful Evictions



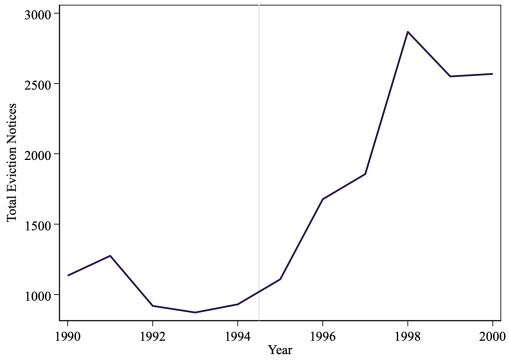
Notes: This figure shows the robustness of event study coefficient estimates to an alternative measure of treatment in which we attempt to condition treatment on the building being owner-occupied. We show our estimated coefficients on the interaction between year dummies and the number of treated units in a ZIP code. We normalize 1994 to be zero. Error bars shown are for the 95% confidence interval. The differences-in-differences estimate for the interaction of the post period with the number of treated units is shown as a gray dashed line with the 95% confidence interval shown as a shaded region on the graph. Standard errors are clustered at the ZIP code level. Subfigure (a) reports effects on eviction notices, while subfigure (b) reports effects on wrongful eviction claims. Effects are scaled to be the effect per 1,000 treated units. The average number of treated units in a ZIP code is 1,688.
Data Sources: 1999 San Francisco Assessor's Secure Housing Roll and San Francisco Rent Board Eviction Notices and Wrongful Eviction Claims

Figure B3: Comparison Between Published and Unpublished Data

(a) Published Data



(b) Unpublished Individual-Level Data

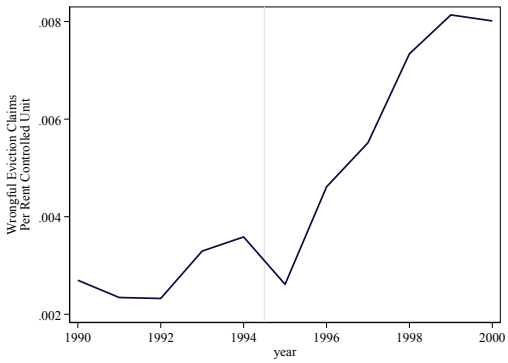


Notes: This figure shows the total number of evictions notices by year. Subfigure (a) displays the overall number of eviction notices over time published by the Rent Board, while subfigure (b) reports the same measure based on the unpublished individual-level data.
Data Sources: San Francisco Rent Board Eviction Notices, published and unpublished datasets

Figure B4: Number of Evictions per Number of Rent Controlled Units

(a) Eviction Notices

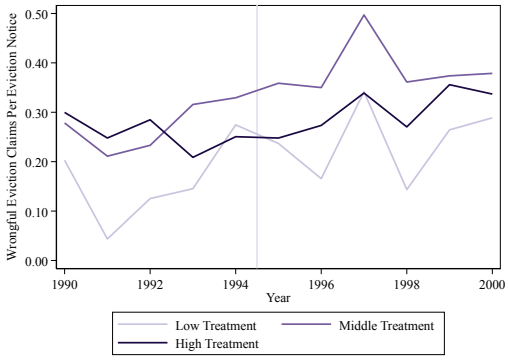
(b) Wrongful Eviction Claims



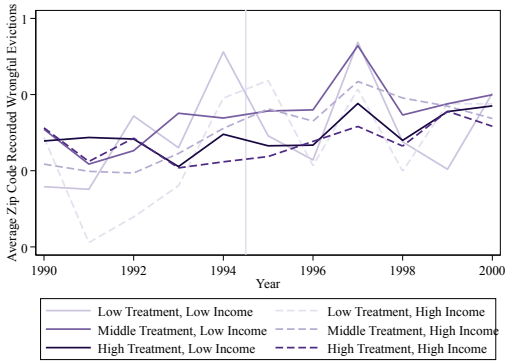
Notes: These figures plot the average fraction of eviction notices (subfigure (a)) and wrongful eviction claims (subfigure (b)) as a fraction of the number of rent controlled units in a given ZIP code over time.
Data Sources: 1999 San Francisco Assessor's Secure Housing Roll and San Francisco Rent Board Eviction Notices and Wrongful Eviction Claims

Figure B5: Wrongful Eviction Claims per Eviction Notice

(a) Wrongful Eviction Claims per Eviction Notices over Time

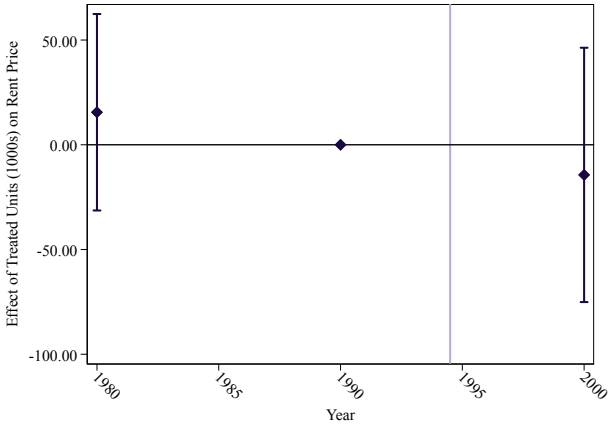


(b) Claims per Notices by Treatment Tercile and Income



Notes: This figure shows the ratio of wrongful eviction claims to eviction notices. Subfigure (a) shows the average fraction of wrongful evictions per eviction notice in ZIP codes, broken into terciles of policy exposure. We assign a value of zero for ZIP codes with no eviction notices. Low-treatment terciles are those in the lowest tercile of units newly exposed to rent control after the policy change. Middle treatment ZIP codes are those in the middle tercile. High-treatment are those in the highest tercile. The timing of the policy change is marked by a vertical line. Subfigure (b) shows wrongful eviction claims per eviction notice over time, split into treatment terciles separately for ZIP codes with low (below median) income versus high (above median) income.
Data Sources: 1999 San Francisco Assessor's Secure Housing Roll and San Francisco Rent Board Eviction Notices and Wrongful Eviction Claims

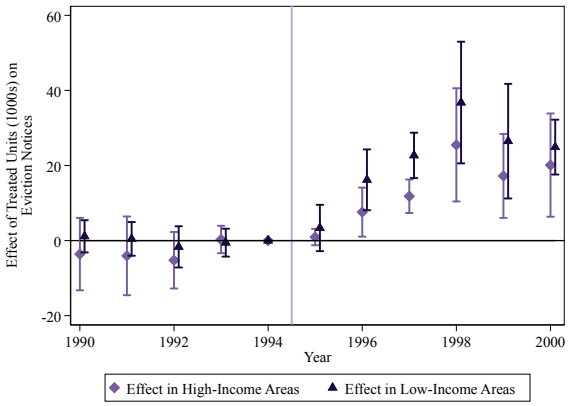
Figure B6: Effect on Rent Prices



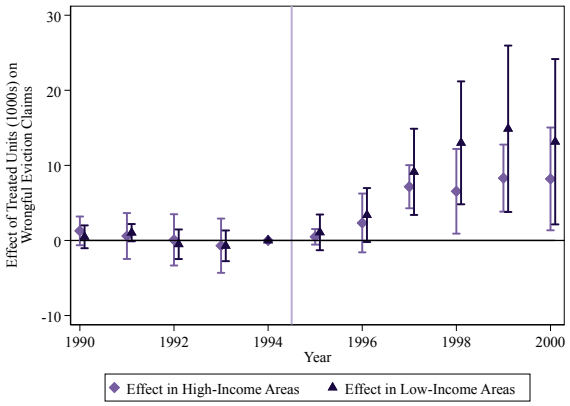
Notes: This figure shows our event study coefficient estimates on the interaction between year dummies and the number of treated units in a ZIP code on self-reported rents from the 1980, 1990, and 2000 decennial census. We normalize 1990 to be zero. Error bars shown are for the 95% confidence interval. Effects are scaled to be the effect per 1,000 treated units. The average number of treated units in a ZIP code is 1,688. Data Sources: 1999 San Francisco Assessor's Secure Housing Roll and 1980–2000 U.S. Census

Figure B7: Heterogeneous Effects By Rent

(a) Effect on Eviction Notices



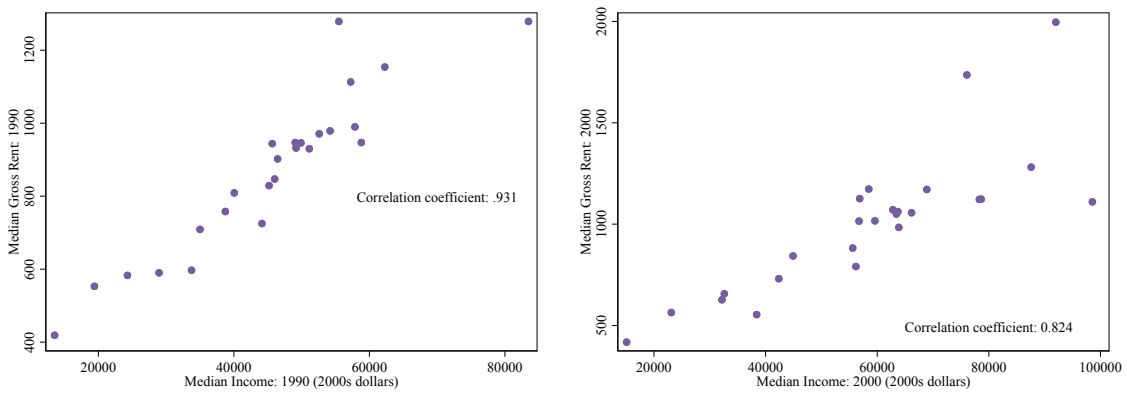
(b) Effect on Wrongful Evictions Claims



Notes: This figure shows our event study coefficient estimates on the interaction between year dummies and the number of treated units in a ZIP code (light purple) and this added to the interaction of these estimates with the triple interaction with low income (dark purple). We normalize 1994 to be zero. Error bars shown are for the 95% confidence interval. Standard errors are clustered at the ZIP code level. Effects are scaled to be the effect per 1,000 treated units. The average number of treated units in a ZIP code is 1,688.
Data Sources: 1999 San Francisco Assessor's Secure Housing Roll (Office of the Assessor-Recorder, 1999), San Francisco Rent Board Evictions Notices and Wrongful Evictions Claims (San Francisco Rent Board, 2021)

Figure B8: Census Rent and Median Income

(a) 1990 (b) 2000

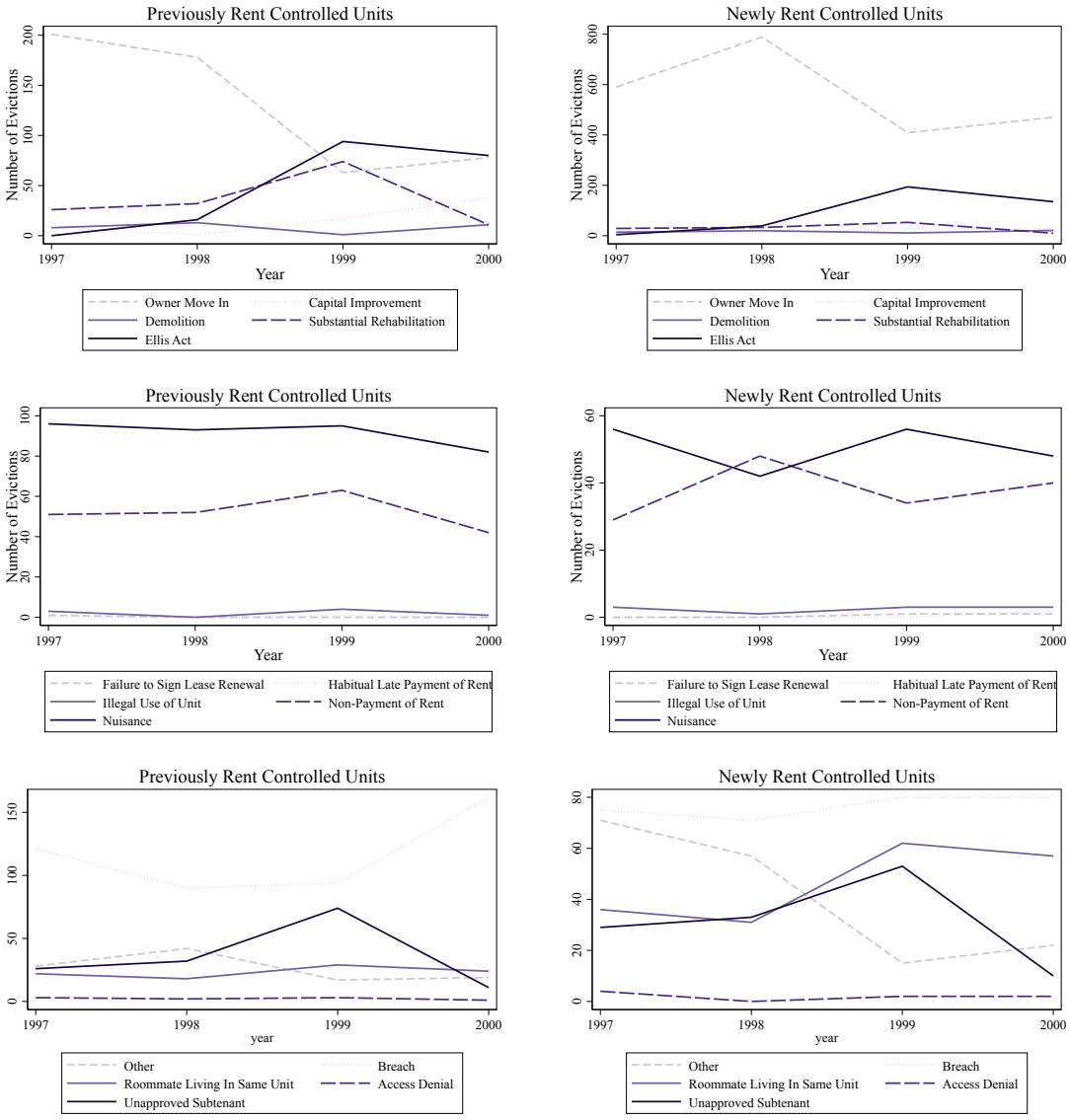


Notes: These figures show the median household income and average rents at the ZIP code level in 1990 and 2000.
Data Sources: U.S. Census

Figure B9: Eviction Notice Type By Building Type: 1997–2000

(a) Eviction Notices at Previously Rent Controlled Units

(b) Eviction Notices at Newly Rent Controlled Units



Notes: This figure shows the number of eviction notices by reason for previously rent controlled units and newly rent controlled units from 1997 to 2000. Recall that reliable data on eviction reasons is not available at the unit level prior to 1997. For eviction notices that list multiple reasons for the eviction, we take the first reason listed.

Data Source: San Francisco Rent Board Eviction Notices

C Appendix Tables

Table C1: Comparison of Housing Stock Against U.S. Census

Building Size	Assessor Data - 1999	Census 2000
Single Family Home	118,078	111,125
Two to Four Units	72,646	80,168
Five to Nine Units	34,671	38,940
Ten to Ninteen Units	32,900	34,996
Twenty or More Units	65,838	79,469
Total Units	324,133	344,698

Notes: We construct aggregate measures for all of San Francisco of the number of units that fall in each category of building and compare them to the same measures from the 2000 Decennial Census.
Data Sources: 1999 San Francisco Assessor's Secure Housing Roll and 2000 U.S. Census

Table C2: Relationship Between ZIP Code Census Covariates and Number of Treated Units

	(1) Number of Treated Units
Median Rent	-0.00380
	(0.00403)
Median HH Income	-0.00000598
	(0.0000932)
% Population Black	-2.434
	(3.031)
% Population White	2.775
	(3.294)
% Owner Occupied	1.675
	(2.703)
% Welfare	-5.734
	(9.302)
Constant	4.111
	(3.632)
N	25
R^2	0.129

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: We regress the number of treated units on census demographic variables to determine whether highly treated ZIP codes were different demographically than less-treated ZIP codes. Treatment is measured by the number of apartments in the ZIP code with between two and four units and that were built prior to 1980. Specifically, we estimate the regression equation: Num Treated$_j = \beta_0 + \beta_1$1990 Rent$_j + \beta_2$Income$_j + \beta_3$Prop Black$_j + \beta_4$Prop White$_j + \beta_5$Prop Owners$_j + \beta_6$Prop on Welfare$_j + \epsilon_j$ for characteristics of ZIP code j.

Data Sources: 1990 U.S. Census and 1999 San Francisco Assessor's Secure Housing Roll

Table C3: Alternative Measures of Treatment

	Avg Treated Units	Correlation with Baseline
Baseline	1688.48	1
2000/2001 Assessor	1666.84	.9999592
Demolition Permits	1693.84	.9999925
Parcel Splits	1724.72	.9997742
SF Replaced RC	1799.12	.9966149
New Builds Replaced RC	1713.44	.9994829
All Condos Converted	2143.8	.9582276
Condo Modifications	2319.68	.948565
Condos Replaced RC	1769.68	.9968405
Owner Occupancy	652.72	.9903856

Notes: This table shows the average number of treated units per ZIP code under a variety of data cleaning methods. The baseline measure uses only the 1999 Assessor data. The 2000/2001 measure fills in information on year built and number of units from the 2000 and 2001 Assessor records. The demolition permit measure adds in all units that were demolished between 1995 and 1999. The parcel split measure adds in all units from new parcels between 1995 and 1999 with 2-4 units. It assumes all new parcels replaced newly rent control units. The next five measures assume various changes from the Assessor data all affected newly rent controlled units. We assume all single family homes replaced rent controlled duplexes, that all new builds replaced rent controlled duplexes, that all condos were converted from rent controlled units in the post period, that all condo modifications represent condo conversions from rent controlled units, and that all newly built condos replaced rent controlled units. The final measure we include in this table is our measure that captures owner occupancy.

Table C4: Differences-in-Differences Estimates of the Effects of Rent Control on Eviction Notices

	(1)	(2)	(3)	(4)	(5)	(6)
Num Treated × Post	20.07***	15.24***				
	(2.926)	(2.374)				
Num Treated × Post × Low Income		9.662***				
		(2.651)				
Owner Occupied × Post			48.45***	34.24***		
			(8.741)	(6.691)		
Owner Occupied × Post × Low Income				30.09***		
				(7.780)		
Treated × Post					51.84***	30.46***
					(11.55)	(10.38)
Treated × Post × Low Income						47.97***
						(9.499)
N	275	275	275	275	275	275
R^2	0.882	0.892	0.877	0.889	0.853	0.883
Wild Bootstrap P-Value	0	0.00200	0	0.00900	0	0.0110
Wild Bootstrap P-Value	.	0.0210	.	0.0140	.	0.0820

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows estimates from differences-in-differences regressions on eviction notices. Column 1 shows the results from our preferred specification, a differences-in-differences regression using the number of treated units at the ZIP code level as a continuous treatment measure. Column 2 adds heterogeneity by median income. Columns 3 and 4 replace the treatment measure with the number of units eligible for the rent control policy that were also owner occupied in 1999. Columns 5 and 6 replace the treatment variable with an indicator for the number of treated units in the ZIP code exceeding the median number of treated units across ZIP codes.
Data Sources: 1990 U.S. Census, 1999 San Francisco Assessor's Secure Housing Roll, San Francisco Rent Board Eviction Notices.

Table C5: Differences-in-Differences Estimates of the Effects of Rent Control on Wrongful Eviction Claims

	(1)	(2)	(3)	(4)	(5)	(6)
Num Treated × Post	7.632***	4.191***				
	(2.344)	(1.184)				
Num Treated × Post × Low Income		6.718**				
		(2.705)				
Owner Occupied × Post			17.95***	8.736**		
			(6.343)	(3.376)		
Owner Occupied × Post × Low Income				19.12**		
				(7.642)		
Treated × Post					20.19***	10.04*
					(5.941)	(5.030)
Treated × Post × Low Income						22.46**
						(8.851)
N	275	275	275	275	275	275
R^2	0.830	0.847	0.822	0.843	0.805	0.843
Wild Bootstrap P-Value	0.00100	0.0200	0	0.0390	0	0.0600
Wild Bootstrap P-Value	.	0.155	.	0.141	.	0.177

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows estimates from differences-in-differences regressions on wrongful eviction claims. Column (1) shows the results from our preferred specification—a differences-in-differences regression using the number of treated units at the ZIP code level as a continuous treatment measure. Column (2) adds heterogeneity by median income. Columns (3) and (4) replace the treatment measure with the number of units eligible for the rent control policy that were also owner-occupied in 1999. Columns (5) and (6) replace the treatment variable with an indicator for the number of treated units in the ZIP code exceeding the median number of treated units across ZIP codes.
Data Sources: 1990 U.S. Census, 1999 San Francisco Assessor's Secure Housing Roll, San Francisco Rent Board Wrongful Eviction Claims

Table C6: Building Type Regression Estimates

	(1) Newly Controlled	(2) Previously Controlled	(3) Condos	(4) Single Family
Panel A: Direct Effects				
Num Treated × Post	16.88***	3.999***	-0.0342	2.036*
	(1.951)	(0.926)	(0.112)	(1.165)
Observations	275	275	275	275
	(1)	(2)	(3)	(4)
Panel B: Spillovers				
Previous Treated × Post	1.936	2.047***	0.119***	-0.698
	(1.513)	(0.363)	(0.0332)	(0.421)
Observations	275	275	275	275
	(1)	(2)	(3)	(4)
Panel C: Combined				
Num Treated × Post	17.00***	2.146**	-0.180	3.156**
	(2.062)	(0.818)	(0.113)	(1.170)
Previous Treated × Post	-0.119	1.787***	0.141***	-1.080**
	(0.181)	(0.223)	(0.0248)	(0.517)
Observations	275	275	275	275

Notes: This table shows estimates from differences-in-differences regressions on eviction notices for different types of buildings for 1990–1996. Panel A includes as the treatment variable the number of newly rent controlled units in a ZIP code. Panel B includes as the treatment variable the number of previously rent controlled units in a ZIP code. Panel C includes separately both the number of newly and previously treated rent controlled units in a ZIP code. The outcome variable in column 1 is the number of eviction notices at buildings that are possibly newly rent controlled (built before 1980 with 2-4 units) in a ZIP code. The outcome variable in column 2 is the number of eviction notices at buildings that were previously rent controlled (built before 1980 with more than 5 units). The outcome variable in column 3 is the number of eviction notices at buildings built before 1980 with 1 unit in the parcel that are condominiums. The outcome variable in column 4 is the number of eviction notices at all other buildings (largely single family homes). Data Sources: 1999 San Francisco Assessor's Secure Housing Roll, San Francisco Rent Board Eviction Notices.