Housing Affordability and Domestic Violence: The Case of San Francisco's Rent Control Policies

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Abstract

Policy advocates claim that one benefit of rent control may be decreased intimate partner violence (IPV). However, the theoretical effects of rent control on IPV are ambiguous. Rent control may lessen financial stressors within a relationship and decrease strain that leads to violence. However, it may make leaving the relationship more costly, shifting the bargaining power in the relationship and leading to more violence. We leverage the 1994 expansion of rent control in San Francisco as a natural experiment to study this question. This expansion created variation across ZIP codes in the number of rental units that were newly rent controlled. We exploit this variation in a continuous difference-in-difference design. We estimate an elasticity of -0.08 between the number of newly rent controlled units and assaults on women resulting in hospitalization. This effect translates to a nearly 10% decrease in assaults on women for the average ZIP code. This relationship is not explained by changes in neighborhood composition or overall crime, consistent with the effects being driven by individual level changes in IPV.

JEL: J12, D1, O18
Keywords: Intimate Partner Violence, Rent Control, Housing Costs, Household Bargaining

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1 Introduction

Domestic violence makes up 21% of all violent victimizations in the United States (Truman (2014)). Roughly a third of women report having experienced severe physical violence from an intimate partner, and about 1 in 5 homicide victims are killed by an intimate partner (CDC (2023)). The prevalence and severity of domestic violence in the United States has made it a popular topic of political discourse. Housing policy advocates assert that affordable housing policies, especially rent control, are imperative for decreasing domestic violence, since affordable housing options allow victims to more easily leave their partners. However, rent control policies that include "vacancy decontrol", where the rental price can be increased whenever tenancy changes, can inadvertently lead to housing lock, a situation in which a victim stays with an abusive partner longer to avoid paying the relatively higher rent that they would face if they moved out.

In this paper, we explore the effects of San Francisco's 1994 rent control policy expansion using a continuous exposure event study design. This expansion affected only small, owner occupied buildings built before 1980, which we leverage to create ZIP code–level measures of treatment by the policy change. We find that rent control decreased hospitalizations of women from assault. The decrease cannot be explained by changes in ZIP code demographics or by changes in other violent crime. These effects are consistent with the strain model of domestic violence, where financial strain in a relationship leads to violence.

The existing economic literature typically models intimate partner violence (IPV) as the outcome of bargaining over shared resources in a relationship (Aizer (2010), Munyo and Rossi (2015), Hidrobo et al. (2016), Brassiolo (2016), Haushofer et al. (2019), Baranov et al. (2021), Calvi and Keskar (2023)) or as a result of some kind of strain or loss of control (Card and Dahl (2011), Cesur and Sabia (2016)).[1] Both of these potential channels exist in the context of housing costs and will make different predictions for the effects of specific housing policies. These policies may shift both the overall level of housing costs and how housing costs vary relatively in and out of the relationship.

In the financial strain model, lower housing costs will decrease financial strain, leading to lower levels of violence. In the bargaining model, it is important to distinguish between policies that shift the level of housing costs overall (both inside and outside the relationship) and those that shift the relative costs of housing inside and outside of the relationship. Policies that decrease housing costs overall will change the amount of resources in the relationship

[1] The bargaining model shares many similarities with the commitment model of relationships used in the sociology literature (Manning et al. (2018), Kenney and McLanahan (2006), Stanley and Markman (1992)).

to be bargained over, but will not shift the bargaining power in the relationship. However, policies that decrease housing costs inside the relationship relative to those outside of the relationship will change the attractiveness of the outside option, providing an additional channel through which the level of violence in a relationship may be affected.

This distinction between policies that change the level of housing costs overall and those that change relative housing costs in and out of the relationship is important when we consider the potential effects of rent control and rent stabilization policies on domestic violence. A common feature of such policies, including San Francisco's, is that landlords can "reset" the rent only when the tenant moves. Tenants cannot transfer the rent control provisions to their next apartment. This feature incentivizes tenants to stay in the same unit once they are under rent control, since moving would mean facing a rent increase to the market rate. These policies were structured to keep long-term residents in their neighborhoods. However, victims of relationship violence may continue to reside with their abusive partner because of difficulty finding or affording a new apartment. In the bargaining model of IPV, the fact that the relative cost of leaving a relationship increases with rent control would lead to a decrease in bargaining power for the victim. This decrease in bargaining power would then result in increased levels of violence. Conversely, rent control may reduce financial strain and decrease IPV triggered by financial stress. Thus, the effects of rent control on domestic violence are ambiguous.

We use a continuous treatment difference-in-differences design to determine the impact of the rent control expansion on hospitalized assaults on women. We compare neighborhoods with higher levels of newly rent controlled units to those with lower levels of newly rent controlled units before and after the policy referendum passed in 1994. To construct this measure, we use data from the San Francisco Assessor's Secure Housing Roll to measure rent control exposure at the ZIP code–level. Exposure is measured by the number of apartments with between two and four units that were built in 1979 or earlier. While we do not have data on owner occupancy as of 1994, we do have 1999 owner occupancy data, which we use to construct an alternative measure of treatment: the number of owner occupied buildings with between two and four apartment units in each ZIP code.

To measure instances of domestic violence, we follow Aizer (2010), counting ZIP code–level hospitalizations for assaults on women. We believe this to be a good proxy of domestic violence, since 82% of domestic violence victims are women (Truman (2014)). This measure of domestic violence does not require the victim to have reported her assault to law enforcement, overcoming a common measurement issue in the crime and IPV literature. The measure also captures a very severe form of violence, since the individual will only appear in the hospital data if their injury was severe enough to lead to hospital admission. We log both assaults and policy exposure variables in order to estimate an elasticity. While a more natural specification may be to use per capita measures and estimate a specification in proportions, this is not possible since annual population estimates do not exist during this time frame. To address the fact that there are ZIP codes with zero assaults in some years, we implement these specifications using the inverse hyperbolic sine.

We find that for every one percent increase in exposure to rent control in a ZIP code, hospitalized assaults on women decline by 0.08 percent. In levels, this translates to an almost 10 percent decrease in domestic violence for the average ZIP code. These results are robust to different measures of policy exposure that condition on owner occupancy and to various alternative regression specifications. Additionally, these results are not present when we examine violence against men, suggesting that they are not due to changes in the overall levels of violence in a neighborhood. There is no strong evidence that ZIP code composition shifted due to the policy change, suggesting our results are not driven by changes in neighborhood demographics. We therefore believe the results reflect a change in an individual's propensity to be a victim of domestic violence.

Our effect sizes are in line with other findings in the domestic violence literature. Card and Dahl (2011) find that unexpected sports game losses can increase instances of domestic violence by 10%. Bobonis et al. (2013) find that transfers associated with the Oportunidades program decrease domestic violence by 40%. Brassiolo (2016) finds that Spanish divorce law reform that makes it easier for individuals to divorce decreases domestic violence by 30%. We further benchmark our findings to the overall decline in IPV in the 1990s when IPV fell about 60% (Rennison (2003)). Assuming San Francisco's domestic violence decrease in the 1990s followed the national trend, our result of a roughly 10% decrease would account for roughly 16% of the decline in intimate partner violence in San Francisco.

Our paper contributes to the research on domestic violence by exploring how the change in household resources created by a rent control policy affects domestic violence. Many papers in this literature explore policies and phenomena that lead to changes in IPV, such as the gender wage gap (Aizer (2010)), unilateral divorce law (Stevenson and Wolfers (2006)), and transfer programs (Bobonis et al. (2013) and Angelucci (2007)). Of particular relevance is the literature that studies how household resources affect IPV (Baranov et al. (2021), Carr and Packham (2021), Cesur et al. (2022), Erten et al. (2022), Haushofer et al. (2019)).

This change in household resources created by rent control is of particular interest since it has several features that are not commonly found in other policies that change household resources. First, unlike policies that change the income of one or both partners, rent control, particularly in cities with strong tenant protections, cannot be assigned to a single member of the relationship, but rather is a true shared resource. Second, the financial advantage of rent control is not a resource that can be taken with one of the partners when they leave the relationship; one or both partners must find new housing at the market rate. Even if one partner remains in the rent control unit, they do so without the ability to split rent with a partner, resulting in a large increase in housing costs. Thus, rent control policies will shift the available resources in the relationship relative to those out of the relationship, which leads to different predictions that income increases or cash transfers. In particular, rent control may affect the probability of breakups in a different way than other economic policies. The economics literature has recently begun exploring the dynamics of violence and breakups; Adams-Prassl et al. (2023) explore how breakups may affect the relationship between economic outcomes and domestic violence in the context of Finland.

We also contribute to the literature on the economics of rent control by determining the effects of rent control on the novel outcome of IPV. Previous work on the effects of rent control policies has focused on neighborhood spillovers (Autor et al. (2014)), misallocation (Glaeser and Luttmer (2003); Olsen (1972)), and unemployment (Svarer et al. (2005)). Autor et al. (2019) studies the effects of removing rent control in Cambridge, Massachusetts on crime during a similar time-frame (mid 1990s) to this study. They find a 16% decline in crime following the removal of rent control. This result contrasts with ours, with several possible explanations. First, it is possible that the short run effects of removing rent control and expanding rent control are not symmetric. One possible reason for this is that it takes time for the market rent to diverge from the rent controlled rent, but once these differences have emerged, removing rent control collapses them immediately. A second key difference between this setting and ours is that Cambridge did not allow for vacancy decontrol, where landlords are allowed to re-set the rent to market between tenants. This means that the value of a rent controlled unit does not increase in the length of time that you remain in the unit. Finally, we are limited in our hospital data to looking only at violent crimes that result in severe bodily injury; it is possible that there were effects on property crime or more minor violent crimes that we are unable to detect. It may also be the case that domestic violence does not follow the same patterns as crime overall.

There is also a literature that studies the effects of rent control in San Francisco specifically. Diamond et al. (2019) use the same institutional setting and policy change in 1994;

they track mobility at the individual level and find that rent control decreases displacement from neighborhoods and mobility and reduces the rental stock. Geddes and Holz (2023) also use the same policy change to study evictions and find increases in landlord-initiated eviction proceedings following the expansion of rent control. Asquith (2019) finds increases in owner move-in evictions in rent controlled units in San Francisco when rent prices rise.

The rest of this paper is organized as follows. In Section 2, we discuss conceptually the effects that rent control can be expected to have on domestic violence. We consider both financial strain and bargaining models of violence and focus primarily on cohabitating couples. In Section 3, we describe the institutional details of rent control policy in San Francisco and the policy change that we exploit as a natural experiment. In Section 4, we explain how we construct measures at the ZIP code–level of exposure to the policy change and IPV. In Section 5, we explain our empirical strategy, the required assumptions, and the robustness checks we perform. In Section 6, we present our main result: we find a reduction in female assault hospitalizations in ZIP codes that were heavily treated by the policy change. In Section, 7 we explore alternative mechanisms. Finally, we conclude in Section 8.

2 Conceptual Framework

We consider two primary models of domestic violence: the financial strain model and bargaining model. These models make different predictions of the consequences of a rent control policy. Both models incorporate the fact that rent control effectively increases the household budget of couples: the financial strain model primarily explores that expanded budget set, while the bargaining model focuses on how that budget set is now comparatively larger relative to the budget set outside the relationship, changing the bargaining dynamics in the relationship. We assume that, for cohabitating couples, the decreased rent relative to market rent is a shared household resource by default and is not assigned to one partner.[2] For this reason, we don't consider models of male backlash for cohabitating couples where who the primary tenant is would matter. We primarily focus on cohabitating couples, but also discuss briefly how rent control policy may affect the probability of household formation and the budget sets of non-cohabitating couples.

[2]This assumption is more plausible in settings with strong tenancy rights for occupants regardless of whether they are listed on the lease.

2.1 Financial Strain Model

In a financial strain model, loosely based on the loss-of-control model in Card and Dahl (2011), violence may occur with some probability at any given time. This probability of violence depends on the level of underlying stress in the relationship. Financial strain will be a major determinant of this level of underlying stress; as financial strain increases, so does the likelihood of intimate partner violence.

Rent control changes the level of financial strain by shifting out the budget set for households that are the beneficiaries of rent control. Rent controlled tenants pay relatively less rent than their non-rent controlled peers over time: as market rent prices increase, non-rent controlled tenants' rent increases, but rents stay the same for rent controlled units. This relative decline in rent prices leads to a relative decline in financial strain, resulting in less violence. In the long run, these effects may attenuate if general equilibrium forces increase rental prices for non-rent controlled units in the area. In the financial strain model, we would expect to see a decline in IPV due to rent control in the short run.

The financial strain model has been shown to drive violent behavior in a number of contexts. Cesur and Sabia (2016) use the strain theory to explain why veterans who were engaged in active duty were more likely to commit intimate partner violence and child abuse. Card and Dahl (2011)'s results also align with the strain model: when an individual's local sport team unexpectedly loses, the strain of that loss leads to an increase in intimate partner violence. This phenomenon is not unique to housing and intimate partner violence: Holz et al. (2023) show that stress due to a peer's injury leads police officers to act more violently in their use of force.

2.2 Bargaining Model

Alternatively, we consider a model where violence is the outcome of bargaining in the relationship. The economics literature has modeled violence both as expressive and instrumental (Baranov et al. (2021)). In a model with expressive violence, the abuser values both consumption and violence, and his partner values consumption and safety. The couple then bargains over the level of violence in the relationship, where the outside option of leaving the relationship involves the victim moving out and finding market-rate housing.[3] This lowers the value of the outside option for the woman, increasing the difficulty of leaving the abu-

[3]Recall that even if you move into a new rent contolled apartment, there is no price restriction for new tenants. Even a move between identical rent controlled apartments may result in large increase in rent prices.

sive partner. There is then a decrease in the victim's bargaining power and an increase in violence.

Similar dynamics may exist in other housing settings where changes in housing markets will change the relative cost of housing inside and outside of the relationship. For example, rising interest rates will change the bargaining power in relationships where couples own their home, as the cost of housing outside the relationship rises relative to the cost of housing in the relationship.

In a model with instrumental violence, men do not have a direct preference for violence, but instead use violence to gain control over resources. This creates an additional channel through which rent control could increase violence in the bargaining model. Rent control increases the available household resources left after paying rent. To gain access to more of these shared resources, men may resort to additional violence.

Many papers in the economics literature have used bargaining models with either expressive or instrumental violence to explain patterns in intimate partner violence. These include Aizer (2010), Munyo and Rossi (2015), Hidrobo et al. (2016), Brassiolo (2016), Haushofer et al. (2019), Baranov et al. (2021), and Calvi and Keskar (2023).

The bargaining model also maps conceptually to one of the main models of domestic violence discussed in the sociology literature, the commitment theory model. This model is discussed in relation to housing and cohabitation. In this model, the level of instability and violence in a relationship is directly related to the commitment level in a relationship. When couples move in together, constraints (such as housing constraints) increase without a corresponding increase in commitment, as would be common in a marriage (Manning et al. (2018), Rhoades et al. (2010), Kenney and McLanahan (2006), Stanley and Markman (1992)). The increase in constraints without commitment can lead to violence. Additionally, whether a woman stays with her abuser has been linked to the level of resources she has access to and the degree of power within the relationship (Gelles (1976)). In a bargaining model, any factor that increases difficulty in leaving a relationship is considered to decrease bargaining power of the partner who wants to leave. In this framework, marriage, or any analogous increase in commitment, could be thought of as changing the bargaining problem as whole.

2.3 Non-Cohabitating Couples

Until now, we have primarily considered couples who are cohabitating. However, housing policy may also shift patterns of household formation. Additionally, the effects of changes in budget sets may differ if the couple does not yet share resources in a non-cohabitating relationship.

The effects of rent control on household formation will depend on the housing status of both members of the couple before the expansion of rent control. If one or both members live in a newly rent controlled apartment, housing lock may prevent cohabitation if doing so would require moving from the rent controlled unit. If neither member in the couple live in a newly rent controlled apartment, upward pressure on rent prices may increase the likelihood of beginning to cohabitate. In the short run, this effect is likely to be small and the effects of housing lock will dominate.

Cohabitation may increase the likelihood of violence for several reasons. First, cohabitation may occur simultaneously with the pooling of resources with more potential for conflict as couples have more shared resources over which to bargain over. Second, living together will increase exposure. Third, the sociology literature documents that cohabitation without commitment (i.e. marriage) can lead to increased violence (Manning et al. (2018), Rhoades et al. (2010), Kenney and McLanahan (2006), Stanley and Markman (1992)). Thus, in the short run, we may expect that rent control will decrease violence among non-cohabitating couples as it creates an additional friction to household formation. In the longer run, the effects will be ambiguous as equilibrium housing prices adjust.

Rent control will also change the budget set of couples who continue to not cohabitate. The effects of this change in the budget set will be ambiguous as they will depend on who lives in a newly rent controlled unit.[4] If the male partner lives in a newly rent controlled apartment, this will decrease his financial strain, decreasing violence. If the female partner lives in a newly rent controlled apartment, she may now have relatively more financial resources, triggering male backlash and increasing domestic violence. The short run effects will be ambiguous since they depend on which partner lives in newly rent controlled housing. In the long run, equilibrium rent price changes will further complicate the analysis since those who live in market-rate housing will also be affected.

3 Rent Control in San Francisco

In 1979, San Francisco passed its Rent Ordinance. This enacted a rent control policy for existing buildings with five or more units and smaller buildings with units that were not

[4] This is a key distinction from when the couple cohabitates. Recall that in the cohabitating case, we consider the reduced rent from rent control to be a shared resource that is not assigned to one member of the couple.

owner-occupied. As part of this rent control policy, landlords in these buildings were not allowed to raise rents by more than a statutorily established amount and must renew leases. However, they were permitted to reset the rent to market rates when tenants move (a common feature of rent control policies known as vacancy decontrol). The policy did not extend to new construction; only buildings built in 1979 or earlier were controlled. Additionally, owner-occupied buildings with four or fewer units were exempted from the policy to protect "mom-and-pop" landlords. In November 1994, the city passed by referendum a new rent control law, Proposition I, which lifted the exemption on small buildings, effectively controlling all buildings built before 1980. Units built after 1980 are never subject to rent control (Diamond et al. (2019)).

Most landlords of newly controlled buildings were not allowed to raise rents by more than a statutorily-limited amount until the current tenants moved out. At that point, the landlords could reset the rent of the apartment to the market rate, with some exceptions for landlords who had not historically raised rent prices.[5] This policy change affected ZIP codes across San Francisco variably, depending on how many small (four units or fewer), old (built in 1979 or before), owner-occupied buildings exist in the ZIP code. For more information on the history and details of rent control in San Francisco, see Asquith and Reed (2021).

San Francisco's rent control policy allows landlords to raise the rent in an unlimited fashion if a tenant leaves of their own volition. This feature contrasts with rent stabilization policies found elsewhere (for instance, Los Angeles, Cambridge, New York for tenants who moved into units after 1971) that restrict the rent of a unit, regardless of whether the tenant moves or stays. These policies may have different impacts on domestic violence and equilibrium rent prices than San Francisco's rent control policy. Recent rent control policies, such as that passed in Oregon, share this feature with San Francisco, where landlords are able to reset rent to market in between tenants, but are limited in how much they can raise rent for a given tenant.

[5]For example, if a landlord had not increased the price between 1991 and 1994, they were entitled to raise the rent by 7.2%, as long as they filed a petition with the city to do so and gave the tenant proper notice of the increase. The longer the period of no increase, the higher the landlord was entitled to raise prices. The largest allowed increase was 15.2% for landlords who had not raised rent between 1989 and 1994. The ordinance also required that rent increases made after May 1st of 1994 be refunded to tenants, possibly leading to large lump-sum payments to tenants in newly controlled units. We do not have any data on whether such payments occurred in practice.

4 Data

We use data from two main sources. We measure exposure to the policy using data on the number of newly rent controlled units that comes from the San Francisco Tax Assessor's Office. We measure IPV using data on the number of hospitalizations resulting from assaults that comes from California's Department of Health Care Access and Information (HCAI, formerly OSHPD) from 1990-2000. We supplement these data with information from the 1990 and 2000 Census on ZIP code–level characteristics. Unfortunately, during our time window, there are no intermediate estimates of the values of these characteristics available at the ZIP code–level.

4.1 Measuring Rent Control

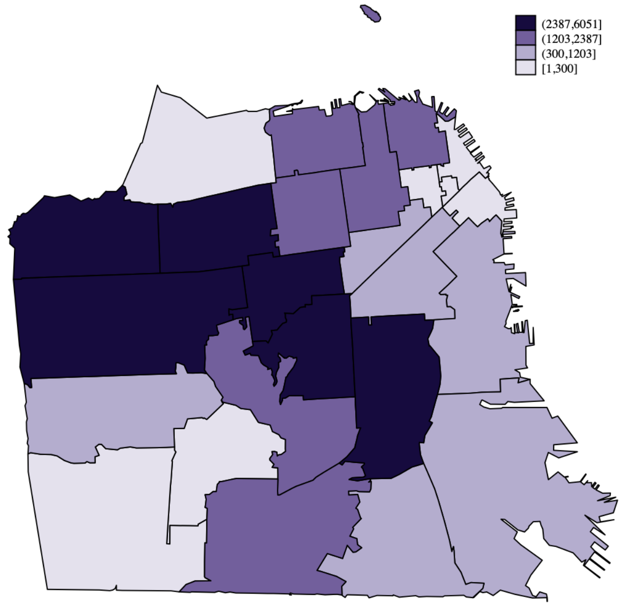
We use data from the San Francisco Assessor's Secure Housing Roll from 1999 to determine the number of units treated by the policy change at the ZIP code–level, following the same steps as in Geddes and Holz (2023). These data include the owner's mailing address, the address of the property, the year the building was built, and the number of units in the building. We restrict to buildings with residential use codes. Appendix A discusses how we handle missing unit numbers, building ages, or ZIP codes. To validate our data cleaning choices, we compare our final measures of the housing stock against measures from the Census in Appendix Table A1. There are only minor discrepancies that could be explained by the addition or demolition of buildings between 1999 and 2000.

We identify units as treated if they are located in buildings built prior to 1980 with two to four units and aggregate to the ZIP code–level.[6] Figure 1 shows this measure of treatment plotted on a map of San Francisco.

We additionally construct several alternative measures of treatment, designed to account for potential mismeasurement due to the fact that the earliest version of the Assessor data we could obtain is from 1999, several years after the policy. First, we develop a version that accounts for owner occupancy. We define owner occupancy as any unit whose address matches the owner's mailing address, where the city and state of the mailing address are San Francisco, CA. Second, we attempt to account for the fact that buildings treated by rent control could have been converted into condos or demolished and replaced in response to the policy. We do so by varying how we classify condos and new construction to create various alternative measures of treatment.

[6] The most granular geographic information on hospitalizations is at the ZIP code–level.

Figure 1: 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 1980. Data source: 1999 San Francisco Assessor's Secure Housing Roll and authors' calculations.

Figure A1 shows these alternative measures of treatment. Table A2 reports the correlation matrix between these various measures. These measures of treatment are all highly correlated, largely because there was not substantial new construction in San Francisco between 1995 and 1999, and there were substantial limitations on owner's ability to convert to condominiums during this time period.

4.2 Measuring Intimate Partner Violence

To measure IPV, we use administrative data on hospital admissions from California's Department of Health Care Access and Information (HCAI) to determine the number of hospitalizations resulting from assault. These data cover the universe of hospital admissions in California's hospitals and contain information on patient ZIP codes,[7] patient demographics, and diagnostic codes. We use the External Cause of Injury Codes (E-codes), which state the

[7] We drop some observations due to missing ZIP codes. 0.6% of the observations are dropped because the ZIP code was labeled as "unknown", 0.1% of the observations are dropped because the patient lives outside of the US, and 2.1% of observations are dropped because the patient is homeless.

underlying cause of the injury that resulted in the admission.

These diagnostic codes have been previously used in the economics literature on domestic violence (Aizer (2010)). Aizer (2010) reports that about 80% of assaults on women are related to IPV. We focus primarily on women since 82% of IPV is committed against women (Truman (2014)). We additionally exclude children from our sample.

We identify a patient as having been assaulted if they are assigned E-codes describing an assault as the reason for the hospital admission. We list the specific E-codes that we use and their definitions in Appendix B. This measure of IPV does not require reporting to the police, so it avoids some of the drawbacks typically associated with measures of domestic violence constructed from criminal records. A health practitioner only needs to suspect an injury is due to assault in order for it to show up as possible IPV in our dataset.

One caveat to this measure is that it measures some of the most severe instances of domestic violence. Roughly half of IPV results in injuries, with 11% of incidents resulting in serious injuries (Truman (2014)). About 34% of women who are injured in IPV need medical care, but only half of women who need medical care seek it in a professional medical setting, as opposed to seeking care from a neighbor or family member (Truman (2014)). Based on a back-of-the-envelope calculation using these statistics, our measure of domestic violence will capture roughly the most severe 8% of the instances of IPV.

Another important characteristic of this measure of IPV is that it may capture a change in the intensity rather than the prevalence of violence. It is unclear if violence significant enough to result in hospitalization scales linearly with other forms of violence, so we must be cautious in the interpretation of our results. A reduction of 10% in hospitalizations may result from a 10% reduction in overall violence; however, it also could be caused by a decrease in the severity of violence, with total instances of violence remaining constant. While we may not be able to distinguish between these two, we note that reducing both severity and quantity of violence is beneficial in a vacuum. It is important to note, however, that the most severe forms of domestic violence occur when women leave abusive relationships (Rakovec-Felser (2014)), and so a decrease in severe violence may result from a decreased propensity to leave an abusive relationship.

We also identify men who have been hospitalized as a result of an assault, which we can use to assess whether our results are driven by underlying trends in violence. While some of these men's assaults may have resulted from IPV, the bulk of the assaults likely were not. Only 10% of assaults on men can be attributed to domestic violence (Truman (2014)).

Our measure of IPV conforms to known patterns in domestic violence. Based on data from the National Crime Victimization Survey, domestic violence most commonly affects women between the ages of 18 and 24 and Black women of all ages (Truman (2014)). We show in Appendix Figures D1 and D2, which plot the distribution of assaulted patient characteristics and the characteristics of the full patient population, that we match these two fact patterns.

4.3 Demographic Characteristics

We measure the demographic characteristics of ZIP codes in the 1990 and 2000 Census and from the demographic characteristics of hospital admissions. Panel A Table 1 reports averages of these characteristics both before and after the policy change. Column (1) reports averages for the entire sample of 25 ZIP codes. Columns (2) and (3) divide the sample into ZIP codes whose level of treatment was below the median level of treatment and ZIP codes whose level of was above the median. Column (4) reports the difference between the two columns and whether the differences are statistically significant.

We find that the low and high treatment ZIP codes are similar on non-population related characteristics both before and after the policy. These ZIP codes have comparable incomes, rents, patient ages, and minority patients. High treatment ZIP codes have more patients in general, more White patients, and more total housing units.

Panel B reports treatment and outcome averages for these three groups of ZIP codes. We find that high treatment ZIP codes have more assaults on women pre-policy, although this difference is not statistically significant. There is a smaller difference, although also not statistically significant, post policy. This pattern is similar for assaults on men. By construction, high treatment ZIP codes have more newly rent controlled units. They also have more previously rent controlled units.

In Appendix Table A4, we assess whether Census characteristics can be used to predict the number of treated units. We do not find a statistically significant relationship between any Census characteristics and the number of treated units, and the R^2 of this regression is quite low.

5 Empirical Strategy

We use a difference-in-differences strategy to determine the effect of rent control on IPV. We compare ZIP codes with high policy exposure to ZIP codes with low exposure before and

Table 1: Summary Statistics

Panel A: Characteristics	(1) Full Sample	(2) Low Treatment	(3) High Treatment	(4) Difference
Median Income 1990	45,698.68	44,922.08	46,415.54	1,493.46
Median Income 2000	58,886.84	56,642.83	60,958.23	4,315.40
Median Rent 1990	869.28	869.00	869.54	0.54
Median Rent 2000	1,006.36	1,028.83	985.62	-43.22
Pre-Policy Black Patients	495.28	511.77	480.06	-31.71
Post-Policy Black Patients	435.45	472.61	401.14	-71.47
Pre-Policy White Patients	1,642.54	968.77	2,264.48	1,295.71***
Post-Policy White Patients	1,640.53	972.31	2,257.36	1,285.05***
Pre-Policy Asian Patients	531.16	352.22	696.34	344.122**
Post-Policy Asian Patients	607.56	419.93	780.76	360.826*
Pre-Policy Hispanic Patients	322.14	130.27	499.246	368.979
Post-Policy Hispanic Patients	298.37	126.24	457.256	331.02
Pre-Policy Median Age	54.00	52.17	55.692	3.526
Post-Policy Median Age	58.87	56.90	60.686	3.79
Pre-Policy Patients	3,043.90	1,997.72	4,009.62	2,011.89***
Post-Policy Patients	2,870.79	1,957.99	3,713.37	1,755.39**
Total Housing Units	12,622.28	6,927.42	17,879.08	10,951.66***
Panel B: Treatments and Outcomes				
Pre-Policy Assaults on Women	7.74	6.98	8.45	1.46
Post-Policy Assaults on Women	4.21	3.71	4.68	0.97
Pre-Policy Assaults on Men	37.81	30.48	44.57	14.09
Post-Policy Assaults on Men	18.69	15.69	21.45	5.75
# Prev. Rent Controlled	4,882.08	2,412.17	7,162.00	4,749.83**
# Treated	1,688.48	358.42	2,916.23	2,557.81***
Observations	25	12	13	25

Notes: This table reports summary statistics split by the level of treatment of the ZIP code. The full sample includes all ZIP codes. Low treatment ZIP codes have fewer than the median number of units that became newly rent controlled in 1994. High treatment ZIP codes have more than the median number of units that became newly rent controlled in 1994. Data sources: 1999 San Francisco Assessor's Secure Housing Roll, HCAI Inpatient Database

after the 1994 policy change. We measure exposure using the number of small apartments built before 1980 in each ZIP code.

We estimate the following specification:

$$\text{Log}(\#\text{ assaults on women}_{it}) = \alpha_i + \tau_t + \beta \cdot \text{Log Exposure}_i \times \text{Post}_t + \epsilon_{it} \tag{1}$$

where α_i are ZIP code–level fixed effects, τ_t are year fixed effects, Exposure$_i$is the number of units newly treated by the rent control expansion in ZIP code i, and $Post_t$ is an indicator for years after 1994.

We estimate corresponding specifications in an event study framework, using the following specification:

$$\text{Log}(\#\text{ assaults on women}_{it}) = \alpha_i + \tau_t + \sum_{\tau=1990,\tau\neq 1994}^{2000} \beta_\tau \cdot \text{Log Exposure}_i \cdot \mathbf{1}\{\text{Year} = \tau\} + \epsilon_{it} \tag{2}$$

where we estimate different coefficients β_τ for the interaction of our exposure variable with each year leading up to and after the policy change. We omit 1994, which is the year the referendum passed, as our reference year. This event study specification allows us to visually assess for the presence of pre-trends and assess whether there is a time pattern in the response to the policy.

We additionally estimate specifications that allow ZIP codes in different terciles of treatments to be on different linear time trends. We do a similar exercise for terciles of assaults against men in the pre-period, which allows ZIP codes with different baseline violence rates to trend differently. We also include controls for the number of previously rent controlled units and for the demographics of women admitted to San Francisco hospitals from give ZIP codes. These address concerns that rent control causes equilibrium changes in the housing market and in ZIP code demographics, respectively.

Interpretation of a causal effect requires four assumptions. First, we assume that there were no anticipatory effects of the policy. The passage of this policy was unexpected; the policy passed in a close election, receiving 51% of votes (Harrison (1994)). It is thus unlikely that there would be changes in household behavior in anticipation of the policy.

Second, we assume that absent treatment, ZIP codes with high exposure to the rent control policy would have trended similarly to those with low levels of exposure. We can examine pre-trends estimated in an event study specification for evidence that this assumption is violated.

Third, we must assume that there are no spillovers across units that were differentially treated. This assumption is difficult to test and may be a strong assumption in our settings;

if the rent control policy change alters displacement across ZIP codes, less treated ZIP codes may be affected by the policy in subtle ways. There are two drivers of spillovers that we are concerned about. The first is that the expansion of rent control may cause changes in the broader market. To address these potential changes, we can estimate alternative specifications that include the number of previously rent controlled units. ZIP codes with more pre-existing rent controlled units may be differentially affected by spillovers. We also use Census data to look at the effects on median rent prices. The second is that new residents to San Francisco may have chosen to locate in highly treated neighborhoods in the pre-period and must locate in less treated neighborhoods in the post-period. We address the potential change in location decisions of new residents by looking at the effects of the policy on neighborhood demographics.

Finally, we assume that there is homogeneity in treatment effects, meaning that potential treatment effects must be unrelated to policy exposure (Callaway et al. (2024)). This assumption is required due to the continuous nature of our specification. If individuals that are at higher risk for IPV (and therefore could have larger treatment effects if treated) all live in neighborhoods that happened to be heavily treated by the policy change, then this assumption would not be met. In Appendix Table A4, we test whether we can predict the number of treated units from Census characteristics. We do not find evidence of strong relationships between observed characteristics of ZIP codes in 1990 and the number of units that become rent controlled.[8] Because we cannot accurately predict the number of treated units using Census demographics and do not find large differences across ZIP codes by treatment levels except through variables related to the size of the ZIP code, we think this assumption is reasonable.

While per-capita estimates would be desirable, the data required to estimate them does not exist. In the 1990s, there are not reliable ZIP code–level estimates of population at the annual level. If instead we were to estimate policy exposure as a proportion of buildings in a ZIP code, there would be no analogous denominator for the outcome of assaults, which should also scale with population. For these reasons, we instead focus on estimating elasticities in order to explore proportional effects.

To account for the fact that some ZIP codes experience no assaults in a given year, we implement these specifications using the inverse hyperbolic sine. We test robustness to this choice by using the more standard log specification and dropping observations with zero

[8] This table is also reassuring that we are not merely picking up the effects of the Earned Income Tax Credit (EITC) that went into effect in 1995. It is not the case that low income ZIP codes or ZIP codes with a greater fraction of households with children under 18 were more treated by the expansion of rent control.

assaults and by using $log(1 + assaults)$ instead. Verifying that our estimates are not driven by choice of model specification is important in this setting because the inverse hyperbolic sine may be a less accurate approximation to logged values when it is taken over small values (Bellemare and Wichman (2020)), and the number of assaults on women is low. The inverse hyperbolic sine is also not scale invariant (Chen and Roth (2024)), so it is important to compare results to scale invariant measures like logs, to ensure the estimates are not artificially inflated or deflated. We additionally estimate a Poisson specification since a count model of assaults may be appropriate in our setting. Finally, we estimate alternative specifications in levels.

To test for alternative explanations, we estimate specifications similar to Equation 1 with male assault hospitalizations as well as the number of hospitalizations for various demographic groups on the left hand side.

6 Effect of Rent Control on Female Assault Hospitalizations

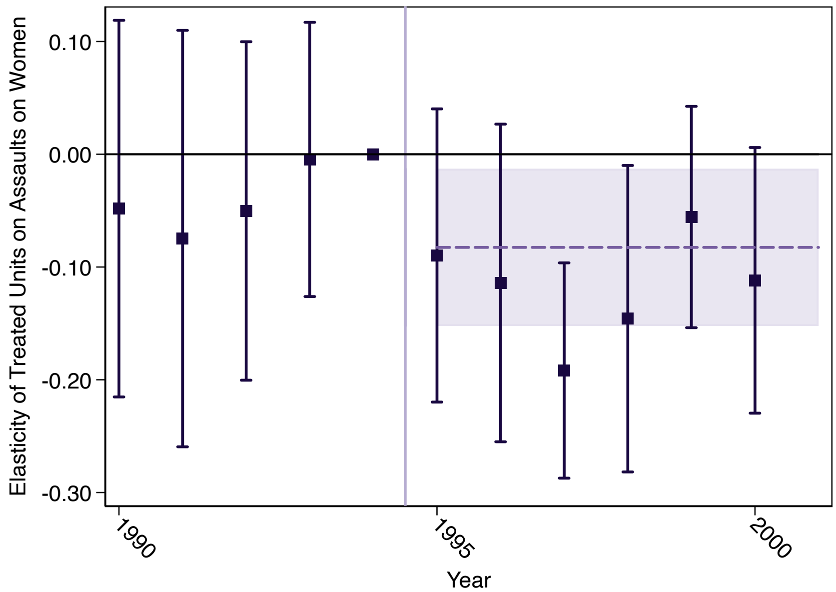
We begin by estimating Equation 2 for female assault hospitalizations. As shown in Figure 2, there is a decrease in hospitalized assaults on women following the implementation of the policy. Here, we plot the coefficients of β_τ, the coefficient on the number of treated units interacted with dummy variables for each year. The purple dashed line shows the difference-in-difference estimate of β in Equation 1.

This plot presents visual evidence in support of our assumption of parallel trends. There is no evidence that ZIP codes with more treated units were trending differently in the period before the policy change.

In Table 2, we report the difference-in-difference estimate shown in the purple dashed line, along with estimates from alternative specifications. Column 2 allows ZIP codes who are in different terciles of treatment to be on different time paths by including tercile-specific linear time trends. Column 3 includes male assault tercile-specific linear time trends. Column 4 controls for the inverse hyperbolic sine of the number of previously rent controlled units.

We find that a 1% increase in the number of newly rent controlled units results in a 0.08% decrease in the number of assaults resulting from domestic violence. The average number of newly rent controlled units is 1,688 and the average number of assault pre-policy is 7.7, so this is equivalent to 16 additionally rent controlled units leading to a reduction of .006 assaults. These results translate to just under a 10% decrease in IPV for the average

Figure 2: Effects of Rent Control Exposure on Female Assault Hospitalizations



Notes: This figure shows our estimates of the event study specification in equation 2. Each point corresponds to the coefficient β_τ. This regression uses the inverse hyperbolic sine of the number of units as the treatment variable and the inverse hyperbolic sine number of hospitalizations per ZIP code as the outcome. Error bar show 95% confidence intervals with standard errors clustered at the ZIP code–level. The dashed line shows the estimated difference-in-differences coefficient. Data sources: 1999 San Francisco Assessor's Secure Housing Roll, HCAI Inpatient Database

ZIP code. This reflects a meaningful change in the number of assaults, given that we are capturing a very severe form of IPV.

Our estimated effects are robust across the alternative controls we include. Including the number of units that were previously rent controlled attenuates the result somewhat, but largely serves to make our estimates noisier. Appendix Figure C1 shows the estimates from the equivalent event study specifications.

In Table 3, we report estimates from alternative ways of specifying our regression model. Recall that we have two issues we want to address in our choice of specification: it is not possible to calculate per capita assaults and there are several ZIP codes with zero assaults. For comparison, Column 1 again reports our preferred estimates using an inverse hyperbolic sine. Column 2 is a log-log specification. This specification drops observations for ZIP codes with zero assault hospitalizations. Column 3 addresses this issue in an alternative way by

Table 2: Effects of Rent Control Exposure on Female Assault Hospitalizations

	(1) Assaults	(2) Assaults	(3) Assaults	(4) Assaults
Post=1 × IHS NumTreated	-0.0826**	-0.105***	-0.0837**	-0.0684
	(0.0338)	(0.0316)	(0.0391)	(0.0795)
Treatment Tercile Specific Trends		X		
Male Assault Tercile Specific Trends			X	
Previously Rent Controlled				X
R-Squared	0.812	0.816	0.813	0.812
Dep Var Mean	5.82	5.82	5.82	5.82
Observations	275	275	275	275

Standard errors in parentheses

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

Notes: This table reports the results of regressions of the IHS of assaults on women on a post-1994 indicator interacted with the IHS of the number of treated units. Column 1 reports the results of equation 1. Column 2 includes treatment tercile specific linear time trends. Column 3 includes male assault tercile linear time trends. Column 4 includes a control for the IHS of the number of units that were previously rent controlled in a ZIP code. Data sources: 1999 San Francisco Assessor's Secure Housing Roll, HCAI Inpatient Database

adding 1 to the number of assaults for each ZIP code. Column 4 is a Poisson model. Column 5 reports our estimates in levels.

Our results are consistent across different ways of implementing a log specification. The effect sizes are attenuated in the Poisson specification. Our estimates are noisier in the level specification, but the effect sizes are consistent with what we find in our preferred specification. Appendix Figure C2 shows the estimates from the event study specifications for these alternative specifications.

We additionally estimate specifications that address the noise that is present in our measurement of treatment. We account for owner occupancy and address that the policy may have led to condo conversions or new construction in several ways. We show estimates from specifications that include these alternative measures of treatment in Appendix Table A3 and Figure A2. Perhaps not surprisingly, given the high correlation between our preferred measure of treatment and these alternative measures, our estimates of the effects of rent control are largely similar.

The resulting decrease in domestic violence falls well within the range of treatment effects

Table 3: Alternative Specifications

	(1) IHS Assaults	(2) Log Assaults	(3) Log(1+Assaults)	(4) Assaults	(5) Assaults
Post=1 × IHS NumTreated	-0.0826**				
	(0.0338)				
Post=1 × Log NumTreated		-0.100*		-0.0471	
		(0.0493)		(0.0567)	
Post=1 × Log(1+NumTreated)			-0.0729**		
			(0.0285)		
Post=1 × NumTreated					-0.390
					(0.464)
Specification	IHS	Log	Log(1+X)	Poisson-Log	Levels
R-Squared	0.812	0.799	0.821		0.780
Dep Var Mean	5.82	5.82	5.82	5.82	5.82
Observations	275	233	275	275	275

Standard errors in parentheses

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

Notes: This table reports results of alternative regression specifications. Column 1 is our preferred specification. Column 2 is a log-log specification. Column 3 is log(1 + x) on both sides. Column 4 is a Poisson specification. Column 5 reports our effects in levels. Data sources: 1999 San Francisco Assessor's Secure Housing Roll, HCAI Inpatient Database

found by other papers in the literature. Card and Dahl (2011) find that unexpected sports game losses can increase instances of domestic violence by 10%, Bobonis et al. (2013) find that transfers associated with the Oportunidades program decrease domestic violence by 40%, and Brassiolo (2016) finds that Spanish divorce law reform that makes it easier for individuals to divorce decreases domestic violence by 30%.

IPV fell nationally by 60% over the course of the 1990s (Rennison (2003)). If San Francisco followed this trend, then our results suggest that rent control accounted for about 16% of the decline in IPV in the 1990s.

7 Mechanisms

Thus far, we have shown that the expansion of rent control in San Francisco led to decreases in the number of hospitalizations due to assault of women in areas that were heavily affected by the policy. This decline may have occurred for three reasons. First, there may have been a decline in IPV. Second, there may have been a decline in violence overall. Third, there may have been a decline in the propensity to seek care at a hospital. We present evidence that this decline is not consistent with either of the latter two explanations, suggesting that our results are primarily driven by a decline in IPV.

We then discuss several reasons why the expansion of rent control may have lead to a decline in IPV. We first discuss how rent control may have affected the housing market, changing the budget set of households through changing rent prices. We then explore whether there were indirect economic effects of the policy: we test whether the policy affected labor market outcomes or household formation. Finally, we test whether our results are being primarily driven by changes in neighborhood composition.

7.1 Alternative Explanations for Assault Hospitalization Declines

Non Intimate Partner Violence

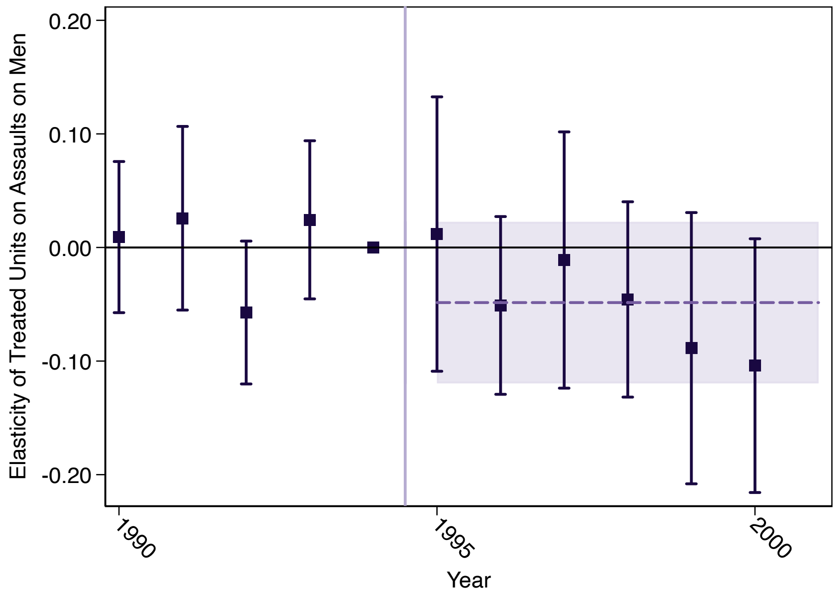
One possible cause of our results that does not involve changes in intimate relationships is a change in the level of violent crime. To test whether the the decrease in assaults on women we estimate is due to a decrease in violent crime overall, we estimate the effects on assaults on men, who are less likely to be assaulted due to IPV and more likely to be assaulted due to general violent crime. Truman (2014) document using the National Crime Victimization Survey that 55% of serious violent crime against males is committed by strangers.

In Figure 3, we find a null effect of exposure on assaults resulting in hospitalization for men, suggesting the decreases in assaults on women are not due to changes in violent crime overall. In later years, there are negative estimates of the effects of the policy; these effects are not statistically significant and are not consistent with the years in which we see the largest effects for women, which are the years immediately following the policy change.

Propensity to Seek Care

Given that rent control alters the budget constraint of households, one concern is that individuals who are the beneficiaries of rent control may be more likely to seek medical care. If this were the case, we would expect to see an increase rather than decrease in domestic violence. We additionally can test whether this is the case by examining hospitalizations resulting from accidents, by using E-codes in a similar manner as we do with assaults. Appendix B lists the specific codes that we use to identify accidents. Figure 4 shows the

Figure 3: Effects of Rent Control Exposure on Male Assault Hospitalizations

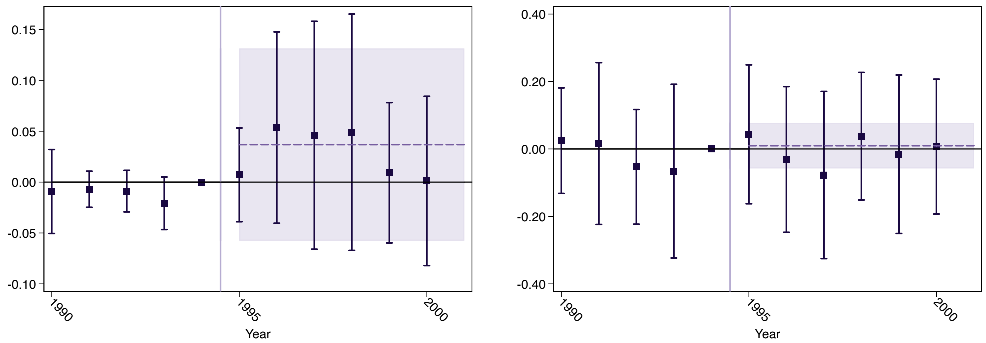


Notes: This figure shows our estimates of the event study specification in equation 2 with male assault hospitalizations as the outcome. Each point corresponds to the coefficient β_τ. This regression uses the inverse hyperbolic sine of the number of units as the treatment variable and the inverse hyperbolic sine number of hospitalizations per ZIP code as the outcome. Error bar show 95% confidence intervals with standard errors clustered at the ZIP code–level. The dashed line shows the estimated difference-in-differences coefficient. Data sources: 1999 San Francisco Assessor's Secure Housing Roll, HCAI Inpatient Database

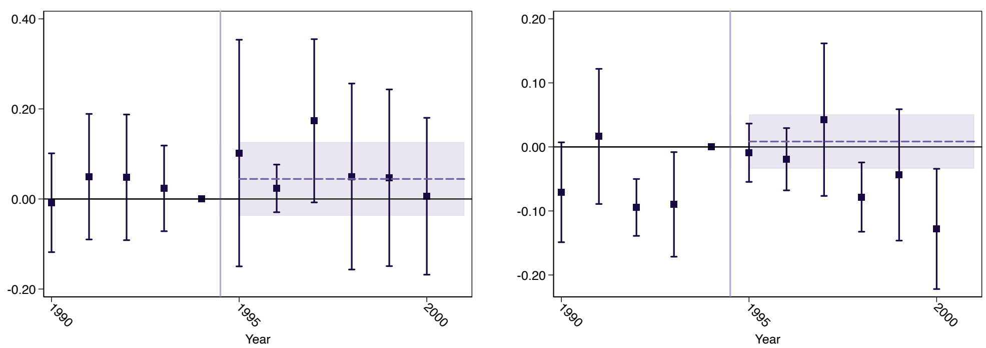
effects of rent control on accidents for both men and women in subfigures (a) and (b). We find no effects for men and positive effects for women. These effects would bias our estimates upwards if they reflect an increased tendency to seek medical care following the policy change.

We also look at the effects on drug and alcohol abuse, which could contribute to a loss of control and more violence. We show our event study estimates for these hospitalizations in subfigures (c) and (d). We find no evidence that rent control changed the number of substance abuse hospitalizations.

Figure 4: Effects of Rent Control Exposure on Other Hospitalizations



(a) Female Accident Hospitalizations (b) Male Accident Hospitalizations



(c) Female Drug and Alcohol Hospitalizations (d) Male Drug and Alcohol Hospitalizations

Notes: This figure shows our estimates of the event study specification in equation 2 with the alternative types of hospitalizations as the outcome. Each point corresponds to the coefficient β_τ. This regression uses the inverse hyperbolic sine of the number of units as the treatment variable and the inverse hyperbolic sine number of hospitalizations per ZIP code as the outcome. Error bar show 95% confidence intervals with standard errors clustered at the ZIP code–level. The dashed line shows the estimated difference-in-differences coefficient. Data sources: 1999 San Francisco Assessor's Secure Housing Roll, HCAI Inpatient Database

7.2 Effects of Rent Control

Housing Market Changes

To assess the size of the financial impact of rent control, we compare the rent increase for someone who paid the average ZIP code median rent in 1990 if their rent grew at the same rate as the rental CPI for San Francisco to the rent increase if they instead paid the maximum increase allowed by the rent control policy. The average ZIP code median rent in 1990 was $867.65. If this grew following the rental CPI, in 2000, that person would be paying $1313.37. If instead that unit was rent controlled, the maximum amount that could be charged for the unit in 2000 would be $1093.53, an over $200 monthly difference.

However, this is a very indirect way to measure the equilibrium effects of the policy on rent. While rent control mechanically lowers rent for those who receive the benefits of rent control, it may have the effect of raising rents for the rest of the market as it limits the supply of market rent housing (and thus, the rental CPI we use above may be inflated). Additionally, the rental CPI may not accurately capture differences within San Francisco in the evolution of rent prices over time. Unfortunately, records of ZIP code–level rents from the 1990s are difficult to obtain. To assess the effects of rent control on rent, we use data on median gross rents from the US Census. These rents are self-reported by individuals who respond to the long form version of the Census. We only have three years of data available to use: 1980, 1990, and 2000, which makes it difficult to evaluate how rents were evolving prior to the passage of the 1994 referendum or the time pattern of rent prices after its passage.

We estimate a similar event study specification to our main specification with these three years of rent data and do not find strong evidence of rent declines following the policy. The event study plot is available in Appendix Figure H1. This analysis likely masks heterogeneity in the effects on rent prices. Rent control will have the biggest effects on units where market rents are rising, so while rents may stay low for some units in a ZIP code, they may simultaneously rise for other units in an area, creating a wedge between rent controlled rents and market rents.

There are likely other changes in the housing market as well. Diamond et al. (2019) find that rent control limits renters' mobility but decreases the rental supply as landlords redevelop buildings or convert to condos. Geddes and Holz (2023) find that the expansion of rent control increases the number of wrongful eviction claims, likely related to improperly done owner-move in or Ellis Act evictions. These changes may lessen the benefits of rent control in the medium to long term, as renters whose units are removed from the rental stock no longer have the benefits of rent control, consistent with the fact that we see a gradual fade-out of some of our effects over time.

Household Formation

As we discussed in Section 2, one consequence of rent control policies may be changes in the formation or dissolution of households. We explore this by testing whether there are changes

Table 4: Effects of Rent Control on Marriage and Employment Outcomes

	(1) IHS % Married	(2) IHS % Divorced	(3) IHS % Separated	(4) IHS %Unemployed
Post=1 ×	0.0193	-0.000770	0.00120	-0.00659
IHS NumTreated	(0.0164)	(0.00240)	(0.000993)	(0.00784)
R-Squared	0.875	0.936	0.958	0.719
Dep Var Mean	.4	.1	.02	.06
Observations	50	50	50	50

Standard errors in parentheses

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

Notes: This table reports estimates of a difference-in-difference regression on Census characteristics at the ZIP code–level. Data Sources: 1999 San Francisco Assessor's Secure Housing Roll, US Census Bureau

in the ZIP code level marriage, divorce, or separation rates from the US Census.[9] We present these results in Table 4 in Columns (1)-(3). We find no effects on marriage, divorce, or separation rates, suggesting that our main results are not being driven by changes in household formation.

Labor Market Effects

Beyond the direct effects of rent control on rent prices, it is possible that rent control may change labor market decision making of individuals whose budget sets have changed. This may affect the exposure that partners have to each other or change the relative bargaining power in the relationship if one partner is now contributing more of the household income. We test whether this is a major factor driving our results by testing whether there were large changes in the unemployment rate in ZIP codes more heavily treated by the policy change. In Column (4) of Table 4, we find no evidence of changes in the unemployment rate in response to the expansion of rent control.

Demographic Changes

Because we do not have data at the individual address level, it is difficult to distinguish between changes in IPV that come from changes in individual behavior versus changes in who

[9] Unfortunately, we are unable to measure romantic partners who are cohabitating who are not married in the Census data.

Table 5: Effects of Rent Control on Census Characteristics

	(1) IHS Population	(2) IHS Median Income	(3) IHS Median Rent	(4) IHS % Poverty	(5) IHS Female 18-24
Post=1 × IHS NumTreated	0.0666**	-0.00460	-0.0307	-0.0139***	0.0393
	(0.0284)	(0.0198)	(0.0224)	(0.00252)	(0.0395)
R-Squared	0.993	0.977	0.951	0.979	0.986
Dep Var Mean	29894	52293	938	.14	1398
Observations	50	50	50	50	50

Standard errors in parentheses

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

Notes: This table reports estimates of a difference-in-difference regression on Census characteristics at the ZIP code–level. Data Sources: 1999 San Francisco Assessor's Secure Housing Roll, US Census Bureau

lives in a given ZIP code who may have different propensities for violence. Both explanations may be relevant for policy makers, but have different interpretations; the first suggests a true drop in violence, the second suggests a displacement of that violence. This is a particularly important concern in the context of rent control policy, where one of the main intentions of such a policy is preventing displacement. Diamond et al. (2019) find substantial reductions in renter mobility following the expansion of rent control to small, owner-occupied buildings in San Francisco in 1994.

To explore whether this reduction in renter mobility could explain the decreases in IPV that we find, we use data from the Census and patient demographics from the HCAI discharge data. We would ideally estimate our difference-in-difference specification with various measures of ZIP code–level demographics, such as age, and race, that we show in Appendix Figures D1 and D2 are correlated with domestic violence, on the left hand side (Pei et al., 2019). Unfortunately, ZIP code–level characteristics are not available at an annual level during our sample period. Instead, we use Census characteristics at the ZIP code–level from 1990 and 2000 and patient characteristics at the ZIP code-year level and estimate whether we observe changes in these characteristics post policy.

Table 5 reports our estimates looking at the effects on Census ZIP code–level characteristics from 1990 and 2000. We examine the effects on population, median income, median rent, the percent living under the federal poverty line, and the population that is female and

18-24.[10] We find small positive effects on the size of the population. Given our effects are not expressed per capita, this change would bias us towards finding increases in the number of assaults, rather than the declines we find. We find no effects on the median income, median rents, or the number of women 18-24. We look at the effects on the number of women 18-24 because this group is the group that is most likely to be the victims of IPV. Finding no effects here suggests that our effects are being driven by changes in behavior rather than changes in the composition of the ZIP code.

We do find small declines in the share of the population living under the poverty line, even though median income does not change. Given poverty is one predictor of IPV, this change could contribute to our results. It is relatively small compared to our main effects, so we think it is unlikely to be the main driver of our results, but could contribute to the negative effects we find.

Because these characteristics are only available in 1990 and 2000, it is impossible to determine if ZIP codes were trending similarly in these characteristics. Rather than the effect of the policy, these estimates may reflect that the highly treated areas were trending differently before the policy in these characteristics.

For this reason, we next examine whether there are effects of the policy on patient characteristics in the inpatient data. Table 6 reports these estimates. We find no effects of the policy on the number of Black, White, or Asian patients or on the median age of patients. We find increases in Hispanic patients. However, contemporaneously with our policy change, the classification of race and ethnicity in our hospital data changed, which may have affected this result.

To aggregate these measures, we test in aggregate whether any of demographic changes should have lead to a change in predicted IPV, based on characteristics alone. To predict IPV, we first run a regression of IPV on the number of patients of each race who were admitted to the hospital, and median age of hospitalized patients. The results of this predictive equation are shown in Appendix Table D1.

Figure 5 shows our event study estimates for this predicted measure of IPV. We do not find changes in predicted assaults on women based on demographic changes at the ZIP code–level. Because of this, we believe that, despite the reduction in mobility created by rent control, it is unlikely that changes in demographic characteristics of ZIP codes are the primary drivers of our results.

[10] Race and ethnic categories changed between the 1990 and 2000 Census so we do not include them here.

Table 6: Effects of Rent Control on Patient Characteristics

	(1) IHS Black Patients	(2) IHS White Patients	(3) IHS Hispanic Patients	(4) IHS Asian Patients	(5) IHS Median Age
Post=1 $\times$	0.0511	0.0310	0.0605*	0.0356	-0.0135
IHS NumTreated	(0.0817)	(0.0328)	(0.0316)	(0.0308)	(0.0128)
R-Squared	0.975	0.986	0.977	0.989	0.869
Dep Var Mean	462.64	1641.44	309.18	572.84	56.66
Observations	275	275	275	275	275

Standard errors in parentheses

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

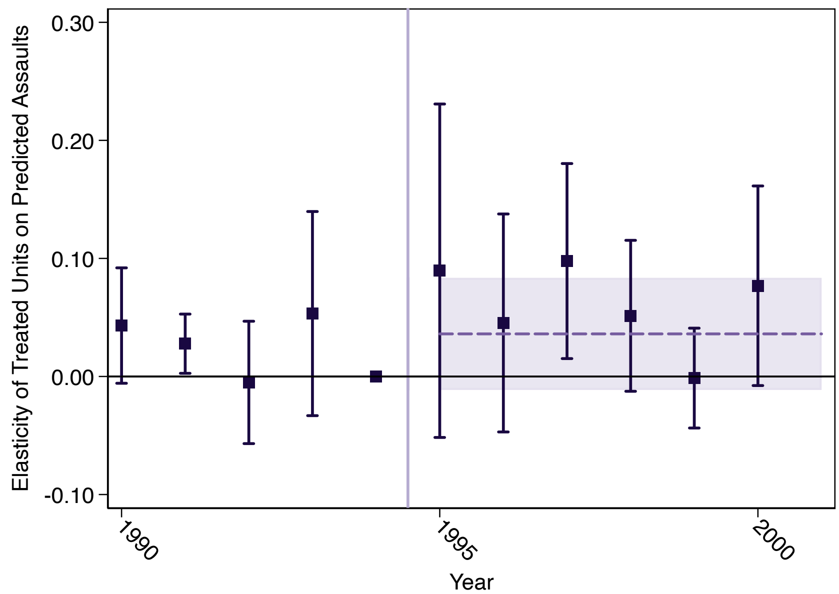
Notes: This table reports estimates of a difference-in-difference regression on patient characteristics at the ZIP code–level. Data Sources: HCAI Inpatient Database, US Census Bureau

8 Conclusion

In this paper, we study the relationship between housing prices and IPV. There are two major models of IPV that are applicable in the setting of housing prices: the financial strain model and bargaining model. These models make different theoretical predictions in the case of rent control, where there exists a stark difference between the rent prices made in a relationship and outside of the relationship. Determining which channel dominates is important when thinking about housing affordability policy, given that helping victims of domestic violence is one reason (among many) that advocates push for new policies to improve affordability. Understanding whether violence related to housing is the result of stress in the relationship or about a victim's ability to leave and their outside option will guide policymakers to very different solutions.

We use a difference-in-differences strategy in the context of San Francisco's rent control referendum in 1994 to find that increased exposure to expanded rent control decreased incidents of domestic violence severe enough to merit hospitalization. We examine trends in assaults on men and hospitalization patterns to rule out that these effects are due to changes in overall crime or hospital-going patterns. Furthermore, our analysis of demographic data from the U.S. Census suggests that moving between neighborhoods does not drive these results. Our estimated effects reflect changes within pre-existing relationships and changes in couple formation. They should be interpreted as aggregate effects of rent control, rather than changes in the propensity to be affected for an individual woman. These results are

Figure 5: Effect of Rent Control Exposure on Predicted Female Assault Hospitalizations



Notes: This figure shows our estimates of the event study specification in equation 2 with our predicted measure of female assault hospitalizations as the outcome. Each point corresponds to the coefficient β_τ. This regression uses the inverse hyperbolic sine of the number of units as the treatment variable and the inverse hyperbolic sin number of hospitalizations per ZIP code as the outcome. Error bar show 95% confidence intervals with standard errors clustered at the ZIP code–level. The dashed line shows the estimated difference-in-differences coefficient. Data sources: 1999 San Francisco Assessor's Secure Housing Roll, HCAI Inpatient Database

consistent with the financial strain model of domestic violence and align with the view of some policy makers that rent control policies will benefit the victims of domestic violence.

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A Treatment Determination Checks

In this section, we present checks to our measure of the number of units in a ZIP code who have been treated. We want to ensure first that our data cleaning procedures are recovering accurate counts of the overall housing stock and then that we are addressing measurement issues in the number of rent controlled units in a reasonable way.

We first validate our data against the 2000 census in Table A1. We find only minimal differences, which could be due to buildings being demolished or built between 1990 and 2000.

Table A1: Comparison of Housing Stock Against US 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. Data Sources: 1999 San Fransisco Assessor's Secure Housing Roll and 2000 U.S. Census.

We then construct alternative measures of treatment that attempt to account for various sources of mis-measurement in our primary measure of treatment that largely arise from the fact that the Assessor data is from several years after the policy change. The first concern is that the exemption was only for owner-occupied buildings. We are hesitant to use the owner address data to identify owner-occupied buildings since our Assessor data is from 1999 several years after the policy, and owner-occupancy may have responded to the policy.[11] For this reason, our preferred measure does not account for this. We construct an alternative measure that does use this information from 1999.

A second concern is that newly rent controlled buildings may have been demolished and replaced with alternative buildings or converted into condos. We construct several

[11]It is possible that landlords occupied units in their small buildings to be eligible for the rent control exemption prior to 1994; once this exemption was removed, the incentive to live in the building is lower.

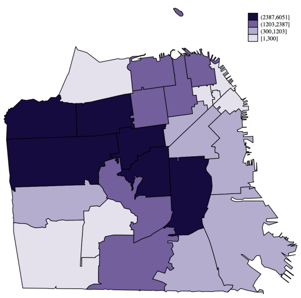
measures that would account for this. We alternatively assume that all single family homes build post 1995 replaced rent controlled buildings, that all condos built before 1979 were converted from otherwise rent controlled apartments, that all condos modified in 1995-1999 were condo conversions, that all new condo construction replaced rent controlled buildings, and that all new buildings replaced rent controlled buildings.

Table A2 reports the correlation between these measures and a falsification measure that is the number of buildings with 5-9 units whose rent control status didn't change. Each of these measures of treatment are very highly correlated, largely because there was not substantial new construction in San Francisco in the 1990s. The least correlated measures with our preferred treatment measure are those that involve older or modified condos. To further convey how similar these various measures are, Figure A1 shows maps of the various alternative measures of treatment we use.

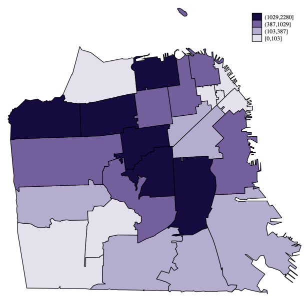
Table A2: Cross-correlation table

Variables	Primary	Owner Occ.	SF	Old Condos	Mod. Condos	New Condos	New Builds	Falsification
Primary	1.000							
Owner Occ.	0.990	1.000						
SF	0.997	0.985	1.000					
Old Condos	0.958	0.968	0.950	1.000				
Mod. Condos	0.949	0.957	0.949	0.988	1.000			
New Condos	0.997	0.987	0.999	0.955	0.954	1.000		
New Builds	0.999	0.989	0.998	0.957	0.950	0.998	1.000	
Falsification	0.878	0.883	0.870	0.929	0.929	0.872	0.877	1.000

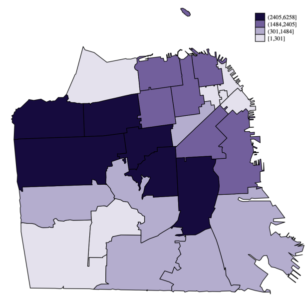
Figure A1: Alternative Treatment Measures



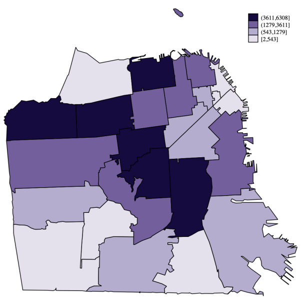
(a) Primary



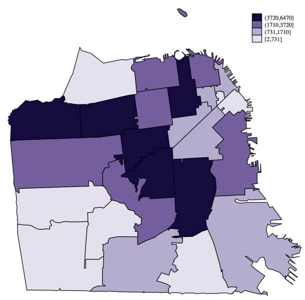
(b) Owner Occupied



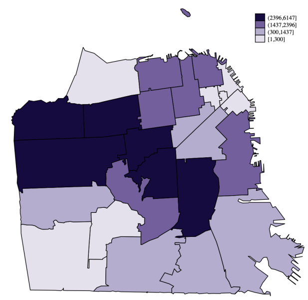
(c) Single Family Adjusted



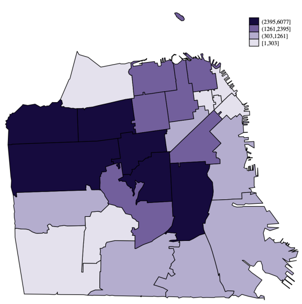
(d) Add Old Condos



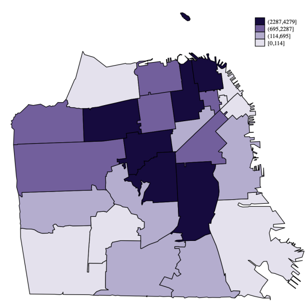
(e) Add Modified Condos



(f) Add New Condos



(g) Add New Builds



(h) Falsification Test: 5-9 units, built before 1980

Notes: Panel (a) shows a map of our preferred exposure measure that has the number of units in buildings with 2-4 units built before 1980. Panel (b) adjusts this measure for owner occupancy. Panel (c) assumes that all new single family homes built between 1995 and 1999 replaced a duplex. Panel (d) assumes all condos built before 1980 were converted to condos after 1994. Panel (e) assumes all condos modified in the post period were treated. Panel (f) assumes all new condos replaced treated units. Panel (g) assumes all new builds replaced treated units. If the new building has more than four units, we assume it replaced a building with 4 units. Panel (h) is our placebo measure of buildings with 5-9 units built before 1980. Data Sources: 1999 San Fransisco Assessor's Secure Housing Roll and 2000 U.S. Census.

While the correlations between these measures are sufficiently high to suggest different treatment measures will not affect our results, we can explicitly test for this. Table A3 reports coefficient estimates where we include these various measures of treatment as alternatives to our preferred specification. Figure A2 shows corresponding event study estimates.

Table A3: Various Measures of Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
Post=1 ×	-0.0721*					
IHS NumTreated Owner Occupied	(0.0357)					
Post=1 ×		-0.0971***				
IHS NumTreated Single Family Homes		(0.0308)				
Post=1 ×			-0.0734*			
IHS NumTreated Recent Old Condos			(0.0363)			
Post=1 ×				-0.0943***		
IHS NumTreated Recent Mod. Condos				(0.0320)		
Post=1 ×					-0.0968***	
IHS NumTreated Recent New Condos					(0.0308)	
Post=1 ×						-0.0843**
IHS NumTreated New Construction						(0.0332)
Observations	275	275	275	275	275	275
R-Squared	0.811	0.813	0.810	0.811	0.813	0.812

Standard errors in parentheses

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

Notes: This table assesses the robustness of our main results to alternative ways of measuring the number of treated units in a ZIP code. Column 1 counts only units which were small, built before 1980, and owner occupied in 1999 based on the address of the owner and address of the unit in the Assessor's Secure Housing Roll. Column 2 includes single family homes as potentially treated units to account for duplexes which were converted to single family homes between the policy change and 1999. Column 3 includes condos which were built before 1980 to allow for inclusion of post-policy condo conversions, Column 4 includes condos whose last modification date falls within 1994 and 2000, and column 5 includes condos which were built between 1994 and 2000. Finally, column 6 uses a measure of treatment which assumes any building built after 1994 replaced a building that was rent controlled in 1994. These alternative treatment measures allow us to include to the best of our ability units that were treated in 1994 but were subsequently converted to condos or replaced. Data Sources: 1999 San Francisco Assessor's Secure Housing Roll, HCAI Inpatient Database

We additionally check whether we can predict the number of treated units from Census characteristics in Table A4. We do not find evidence of strong relationships between observed characteristics of ZIP codes in 1990 and the number of units that become rent controlled.

Table A4: Relationship Between Zip Code Census Covariates and Number of Treated Units

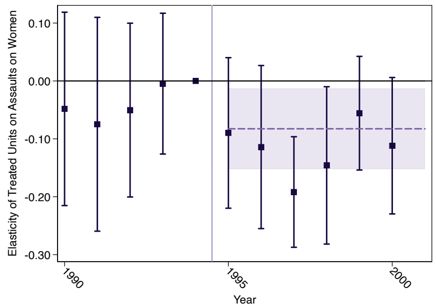
	(1) Number of Treated Units
Median Rent	-0.00380
	(0.00411)
Median HH Income	-0.00000490
	(0.0000958)
% Population Black	-2.568
	(3.798)
% Population White	2.876
	(3.577)
% Owner Occupied	1.619
	(2.871)
% Welfare	-5.294
	(11.44)
% Kids	0.296
	(3.917)
Constant	3.931
	(4.430)
N	25
R^2	0.129

Standard errors in parentheses

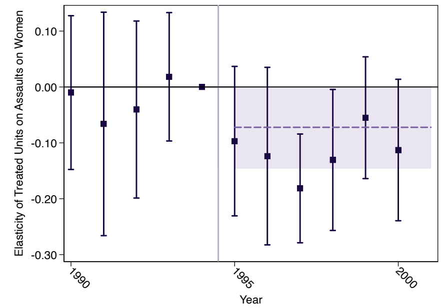
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 was built prior to 1980. Data Sources: 1990 U.S. Census and 1999 San Francisco Assessor's Secure Housing Roll.

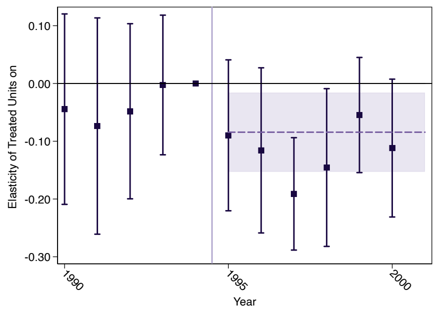
Figure A2: Event Studies: Alternative Measures of Treatment



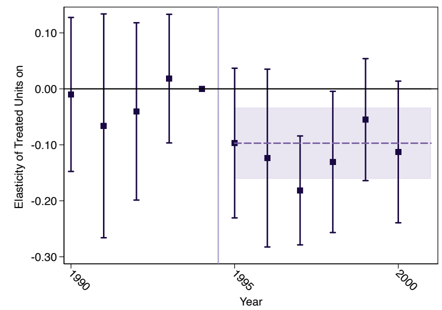
(a) Main Specification



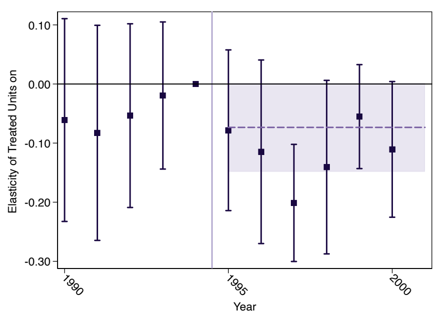
(b) Owner Occupancy Measure



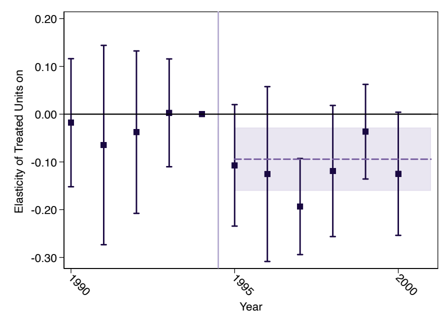
(c) New



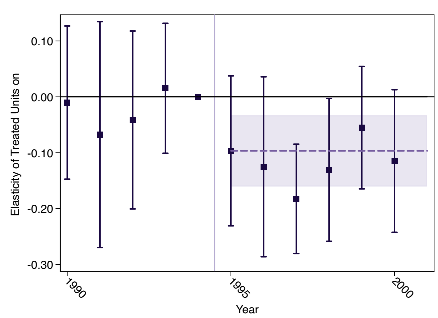
(d) SF



(e) Old Condos



(f) Mod. Condos



(g) New Condos

Notes: Panels A-G display event study versions of the estimates displayed in Table A3

B External Cause of Injury Codes

To define IPV in the hospitalization data, we used external cause of injury diagnostic codes. The codes that we use and their definitions are listed below.

E-codes in the range E960-E969 refer to "Homicide and Injury Purposely Inflicted by Other Persons" and include:

- "Fight, Brawl, Rape"
- "Assault by corrosive or caustic substance, except poisoning"
- "Assault by poisoning"
- "Assault by hanging and strangulation"
- "Assault by submersion [drowning]"
- "Assault by firearms and explosives"
- "Assault by cutting and piercing instrument"
- "Perpetrator of child and adult abuse"
- "Assault by other and unspecified means"
- "Late effects of injury purposely inflicted by other person".

E-codes between 980 and 989 refer to "Injury Undetermined Whether Accidentally or Purposely Inflicted". E904 refers to "Accident due to hunger, thirst, exposure and neglect".

We use the following ICD-9 codes to identify hospitalizations related to substance use disorders or substance abuse:

- 291: "Alcoholic psychoses"
- 292: "Drug psychoses"
- 303: "Alcohol dependency"
- 304: "Drug dependence"
- 305: "Nondependent Abuse of Drugs"

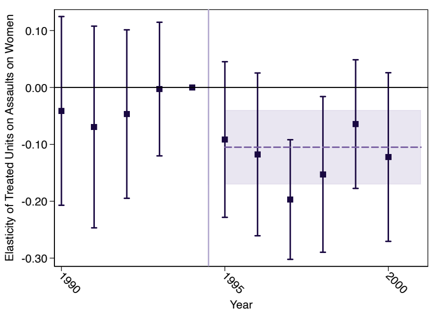
We use the following external cause of injury codes to identify accidents:

- 920: "Unintentional Cut/Pierce"
- 830, 832, 910: "Unintentional Drowning/Submersion"
- 880-889: "Unintentional Fall"
- 890-899: "Unintentional Fire"
- 924: "Unintentional Hot Object"
- 922: "Unintentional Fire Arm"
- 850-869: "Unintentional Poisoning"
- 916-917: "Unintentional Striking"
- 911-913: "Unintentional Suffocation"

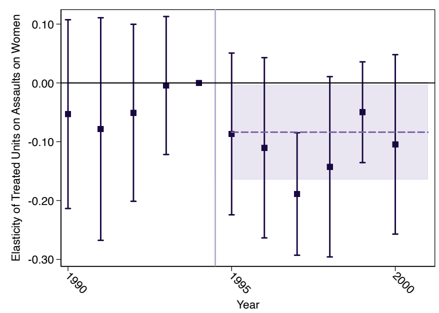
C Additional Specifications

In this section, we present results from alternative specifications to show the robustness of our results. Figure C1 shows event study coefficient estimates corresponding to the regression coefficients shown in Table 2, in Columns (2)-(5). Our results are largely robust to the inclusion of these additional controls; including the number previously rent controlled makes our estimates noisier but does not substantively change our point estimates in the event study specification.

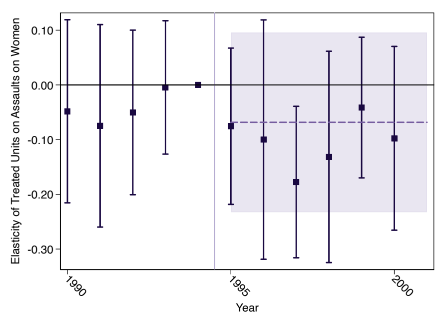
Figure C1: Event Studies: Alternative Controls



(a) Includes tercile time trends



(b) Male assault tercile time trends



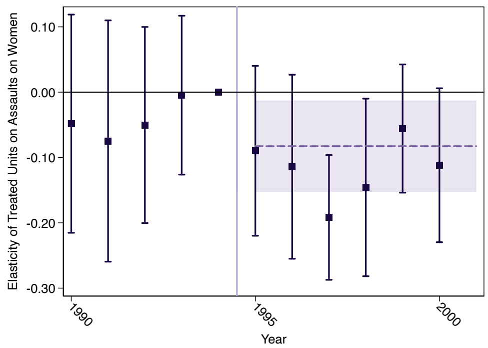
(c) Control for # previously rent controlled

Notes: Panel (a) shows a specification which controls for treatment tercile-specific linear trends. Panel (b) instead controls for male assault tercile-specific linear trends. Panel (c) includes an interaction term for the number of previously rent controlled units. Data Sources: HCAI Inpatient Database and 1999 San Francisco Assessor's Secure Housing Roll.

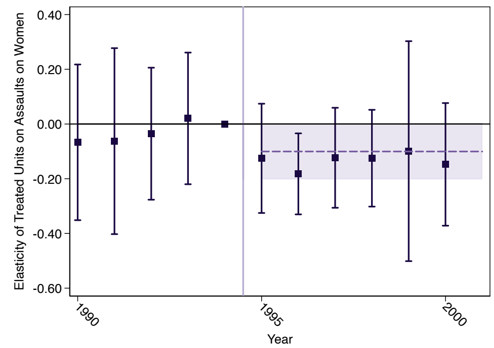
We additional present event study estimates for alternative ways of specifying our model in Figure C2.

We have now presented robustness to alternative controls, measures of treatment, and model specification. Of course, there are numerous combinations of these checks that we

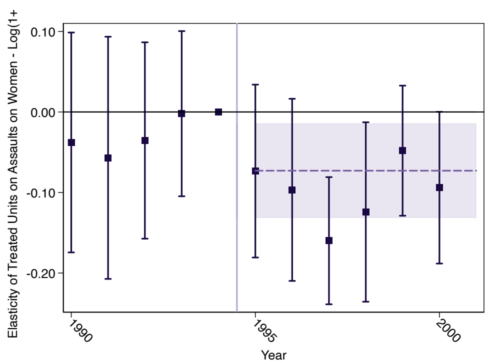
Figure C2: Event Studies: Alternative Specifications



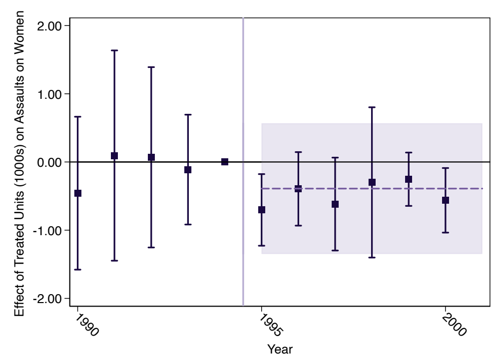
(a) IHS



(b) Log



(c) Bottom Coded Log



(d) Levels

Notes: Panel (a) shows our specification in logs. Panel (b) shows an alternative specification instead using the inverse hyperbolic sine. Panel (c) replaces all zeros with one to avoid dropping these observations. Panel (d) is a linear specification. Data Sources: HCAI Inpatient Database and 1999 San Francisco Assessor's Secure Housing Roll.

could do. We present a selection of them in Tables C1 and C2.

Table C1: Log DID

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post=1 × Log NumTreated	-0.100*					-0.173***	-0.0819
	(0.0493)					(0.0417)	(0.0711)
Post=1 × Log Owner Occupied		-0.0874*					
		(0.0453)					
Post=1 × Log Single Family			-0.117**				
			(0.0561)				
Post=1 × Log New Builds				-0.103*			
				(0.0498)			
Post=1 × Log Condo					-0.0916		
					(0.0549)		
Post=1 × Log Prev. RC							-0.0441
							(0.0775)
Specification	Main	Owner Occupied	SF	New	Condo	Tercile Trends	Previous RC
Observations	233	231	233	233	233	233	231
R-Squared	0.799	0.795	0.799	0.799	0.797	0.815	0.796

Standard errors in parentheses

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

Notes: This table reports robustness of results to various controls in the specification, using logs instead of inverse hyperbolic sine. Data Sources: HCAI Inpatient Database and 1999 San Francisco Assessor's Secure Housing Roll.

Table C2: LOG 1+X DID

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post=1 ×	-0.0729**					-0.0949***	-0.0589
Log(1+NumTreated)	(0.0285)					(0.0266)	(0.0648)
Post=1 ×		-0.0657**					
Log(1+ Owner Occupied)		(0.0303)					
Post=1 ×			-0.0854***				
Log(1 + Single Family)			(0.0264)				
Post=1 ×				-0.0744**			
Log(1 + New Build)				(0.0280)			
Post=1 ×					-0.0646**		
Log(1 + Condo)					(0.0299)		
Post=1 ×							-0.0162
Log(1 + Prev RC)							(0.0562)
Specification	Main	Owner Occupied	SF	New	Condo	Tercile Trends	Previous RC
Observations	275	275	275	275	275	275	275
R-Squared	0.821	0.820	0.822	0.821	0.819	0.826	0.821

Standard errors in parentheses

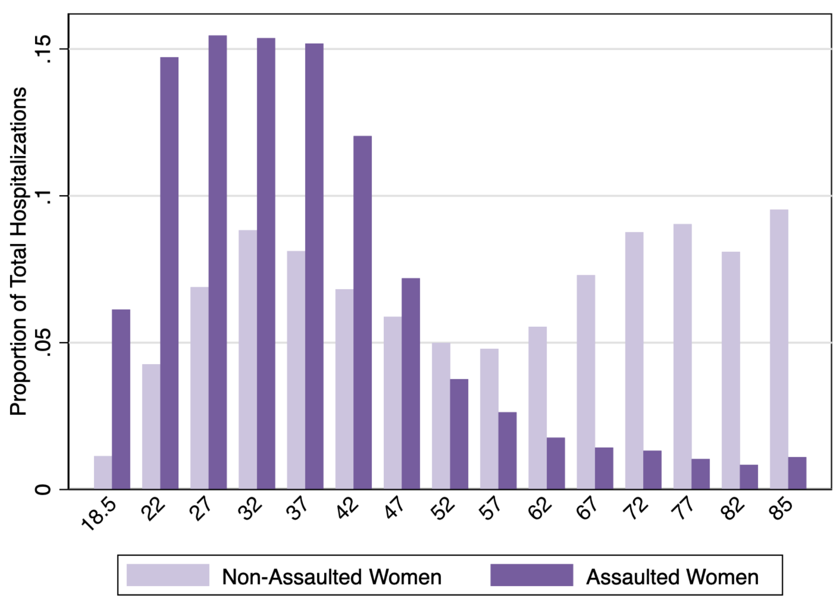
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports robustness of results to various controls in the specification, but adjusting variables using log(1+X) so that variables with with a value of zero can be included in the analysis. instead of inverse hyperbolic sine. Data Sources: HCAI Inpatient Database and 1999 San Francisco Assessor's Secure Housing Roll.

D Demographic Patterns

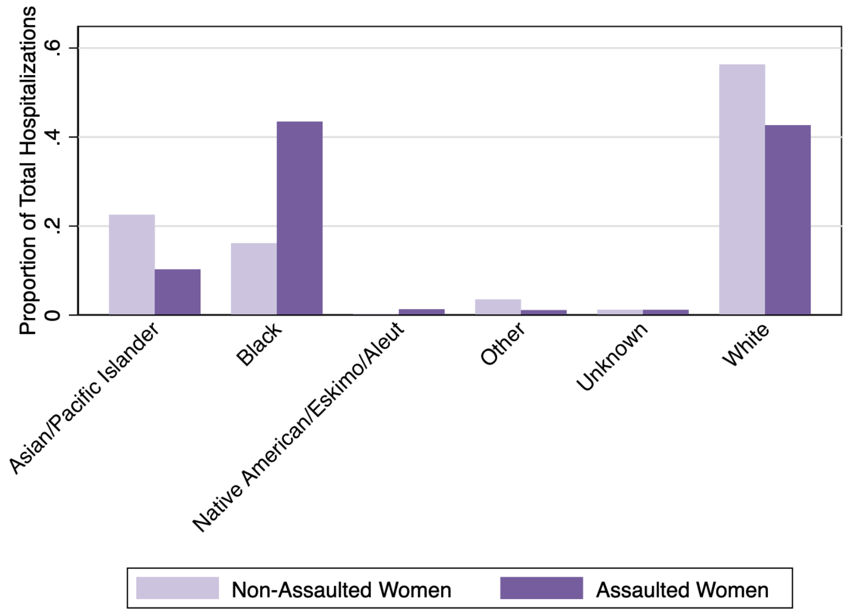
One benefit of using hospitalization data is that it provides rich information on the characteristics of hospitalizations, both for assaulted and non-assaulted patients. This information allows us to confirm that our measure of IPV conforms with the expected patterns from the literature and to assess whether there are other changes in hospitalization patterns in response to the policy.

Figure D1: Age Distribution of Hospitalized Women



Notes: We display a histogram depicting the age structure of assaulted women. Frequencies for all female patients are displayed in dark gray, while frequencies for assaulted female patients are shown in light gray. Assaulted patients appear more likely to be young. Data Source: HCAI Hospitalization Data.

Figure D2: Racial Composition of Hospitalized Women



Notes: We display a histogram depicting the race structure of assaulted women. Frequencies for all female patients are shown in dark gray, while frequencies for assaulted female patients are shown in light gray. Assaulted women appear more likely to be black. Data Source: HCAI Hospitalization Data.

To predict IPV, we first run a regression of IPV on the number of women who were admitted to the hospital from different race and ethnic categories and the median age of hospitalized women. The results of this predictive equation are shown in Appendix Table D1.

In this section, we present additional event study plots looking at the time evolution of the effects of the variation induced by the policy change on demographic characteristics of who is hospitalized. We presented these results in a difference-in-differences table in the main text.

Table D1: Predicting Assaults on Women Based on Zip Code Demographic Characteristics

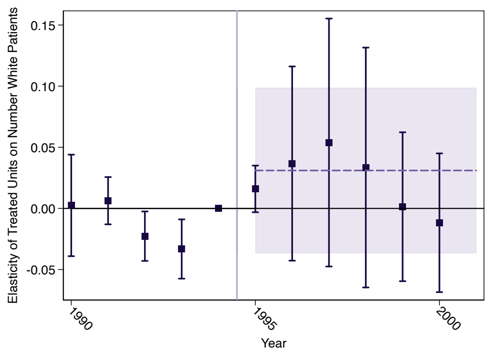
	All Covariates
Number Black Patients	0.00639***
	(0.000534)
Number White Patients	0.00127***
	(0.000382)
Number Asian Patients	0.000652
	(0.000630)
Number Hispanic Patients	0.00234***
	(0.000842)
Median Age	-0.118***
	(0.0280)
Constant	6.370***
	(1.427)
R Squared	0.627
Observations	275

Standard errors in parentheses

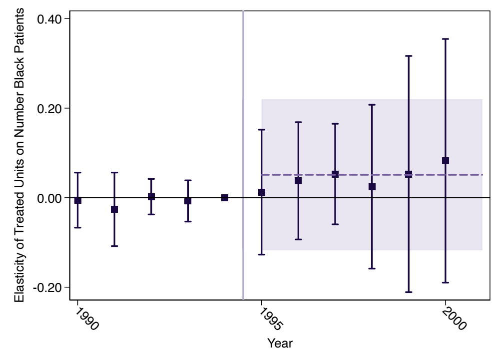
* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: This table shows the results of a regression using demographic characteristics to predict assaults on women using demographic information from hospitalized patients. Data source: HCAI

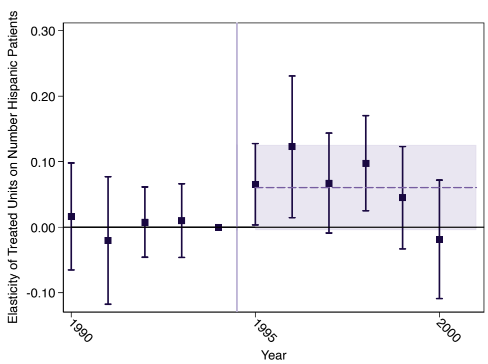
Figure D3: Effects of Rent Control Exposure on Characteristics of Patients



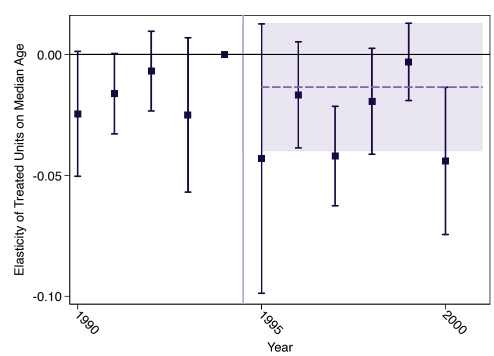
(a) White Patients



(b) Black Patients



(c) Hispanic Patients



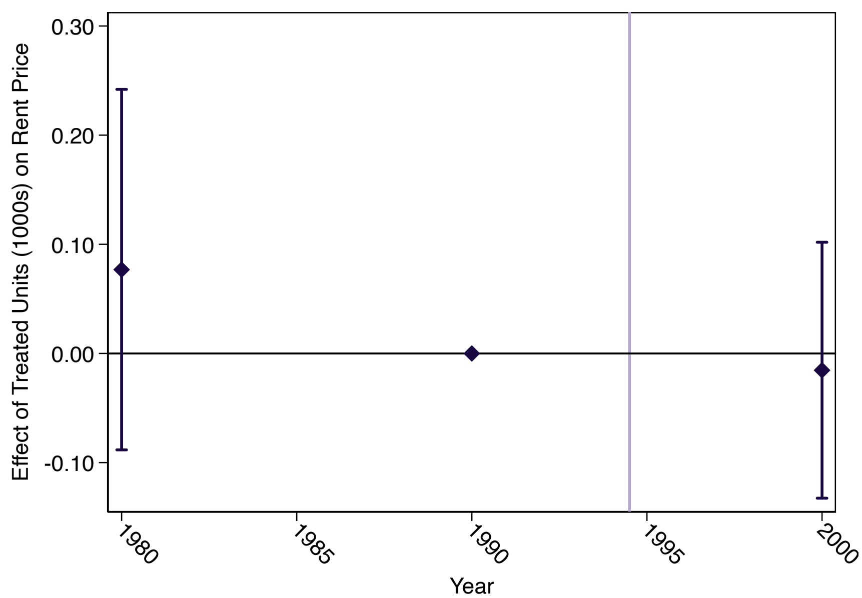
(d) Median Age

Notes: Panel (a) shows the estimated effects for the number of White patients. Panel (b) shows the effects for the number of Black patients. Panel (c) shows the effects for the number of Hispanic patients. Panel (d) shows the effects on the median age of hospitalized women.

E Changes in San Francisco's Housing Market

In this appendix section, we present evidence of rent changes that occur in the San Francisco housing market in response to the policy change. Data on ZIP code–level rent prices dating back to the 1990s is scarce, so we use information on median gross monthly rent from the decennial Census in 1980, 1990, and 2000. These data are self-reported by respondents to the long form Census and so may be variable in whether they include utilities depending on how the respondent interpreted the question.

Figure H1: San Francisco Rent Prices and Rent Control



Notes: Event study estimates of the effects of rent control on rent prices. Standard errors are clustered at the ZIP code–level. Error bars show 95% confidence intervals. Data source: 1980, 1990, 2000 US Census.

We estimate an event study specification similar to equation 2 where 1990 is our omitted category as the year closest to and previous to the policy change. We find no evidence of pre-trends leading up to 1990 (although we only have two data points so we interpret this finding very cautiously), and then a small decline in rent prices in highly treated ZIP codes in 2000, though this effect is not statistically significant.