To: The University of Southern California, Economics Department

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Subject: The Impact of Eviction Activity on Property Appreciation in California

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Executive Summary

This study examines the relationship between residential eviction activity and housing market outcomes across California cities, with a primary focus on property value appreciation. Motivated by rising housing costs and expanding tenant protections, the project evaluates whether eviction filing rates are associated with changes in local housing market performance.

Using a panel dataset of California cities from 2004–2013, we estimate both simple and multiple regression models with city and year fixed effects. The dependent variable is annual property value appreciation, while the key independent variable is the eviction filing rate normalized by rental housing stock. Additional controls include income, education, renter share, crime rates, population, and other housing market characteristics.

The results show a consistent negative relationship between eviction filing activity and subsequent property appreciation. However, the strength and statistical significance of this relationship varies across model specifications, suggesting that eviction activity may partly reflect broader economic conditions rather than acting as a purely causal driver of housing prices.

This study contributes to policy discussions surrounding rent control and just-cause eviction laws by examining how tenant protections may interact with housing market behavior. More broadly, the findings highlight the tradeoff between housing stability for renters and market efficiency for property owners and investors.

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Background

Housing policy in California has become an increasingly important economic and political issue due to rising housing costs, limited housing supply, and growing concerns over tenant displacement. In response, state and local governments have implemented some of the strongest tenant protection policies in the United States, including rent control measures and just-cause eviction laws. These policies are intended to promote housing stability and protect renters from sudden or unjust displacement, but their broader effects on housing markets remain debated.

This project is partially motivated by a real-world case in which a first-time homebuyer experienced significant legal and financial barriers when attempting to remove existing tenants from a property purchased for personal occupancy. The case raised broader questions regarding the balance between tenant protections and property owner rights within highly regulated housing markets.

From an economic perspective, strong eviction protections may create unintended market effects. Restrictions on eviction can increase transaction costs, reduce flexibility for property owners, and influence buyer and investor behavior. These factors may affect housing demand, market liquidity, and long-run property appreciation.

At the same time, eviction activity may reflect broader economic conditions rather than directly causing weaker housing outcomes. Higher eviction filing rates may be associated with economic distress, income instability, or local housing shortages, making it difficult to distinguish whether eviction activity is a cause of lower appreciation or simply a signal of underlying market conditions.

Because California combines strong tenant protections with some of the nation's most expensive housing markets, it provides a useful setting for examining these relationships. By analyzing variation across California cities over time, this study aims to provide empirical evidence on how eviction dynamics are associated with housing market performance and contribute to ongoing debates regarding tenant protections, market efficiency, and housing affordability.

Literature Review

Previous studies related to the current study can be categorized into two categories. The first category examines the impact of housing protection policies on property values by examining the changes in housing availability and turnover. Diamond, McQuade, and Qian (2019) examined San Francisco's 1994 Rent Control Expansion using a quasi-experimental difference-in-differences approach. Their results show that landlords in the treated group reduced the supply of rental housing by about 15% while the number of renters in rent-controlled units fell by 25%. They found that a large number of buildings were converted to owner-occupied condominiums or redeveloped into housing types that are exempt from rent control. The authors argue that the reduction in rental supply increases market rents over time, which may undermine the policy's goal of affordability.

Strickland and Overstreet (2023) conducted a similar study examining the effects of housing policy on property values. The authors used a synthetic control methodology to examine the impacts of California's Proposition 13 and Florida's Save Our Homes Amendment. Both laws established acquisition-value-based tax systems where future value increases do not trigger higher property taxes. Their results showed Proposition 13 caused residential value added in

California to exceed 50% of pre-policy levels at its peak. They argued this occurred because the policy restricted new construction and limited the rate at which existing homes were sold.

The work of Diamond, McQuade, and Qian (2019), together with Strickland and Overstreet's (2023) work, illustrates that policies covering renters or homeowners lead to supply-side effects. These effects influence long-run property values. This relationship depends on whether the mechanism decreases supply, resulting in price increases, or reduces owner flexibility, causing a decrease in price. Eviction activity is a form of housing-market friction and a constraint on tenant turnover, it may also contribute to property appreciation through associated mechanisms.

However, Nelson et al. (2021) note that comparing eviction-filing rates across jurisdictions is problematic because the rates are affected by institutional differences, such as filing fees and judicial practices. Consequently, two cities experiencing similar housing distress may produce differing numbers of filings; conversely, similar filing rates may mask differing levels of actual displacement. Therefore, eviction filings should not be viewed simply as a direct indicator of tenant hardship.

Raymond et al. (2018) extended this institutional perspective by examining landlord behavior. Using individual property data combined with tax and demographic data from the Atlanta region, they found that corporate landlords filed for evictions at a rate that was about 68% higher than that of small private landlords. Similarly, properties with a history of foreclosure had filing rates that were 58% higher than those of properties without a foreclosure history, controlling for relevant property and neighborhood characteristics. It suggests that eviction filing is often a systematic rent collection strategy rather than simply a reaction to tenant hardship.

Leung et al. (2023) also found eviction filings to be an indication of landlord strategies, types of ownership and institutional contexts, with 46.8% of public housing residents facing eviction having multiple filings compared to only 28.4% of tenants in private rental markets. Leung et al. attributed this difference primarily to institutional management strategies rather than tenant characteristics. Joint with the research of Raymond et al., these findings show that eviction filings reflect landlord strategies and institutional context in addition to factors of economic hardship.

Lastly, Desmond (2012) offers an understanding of eviction as a cause of urban poverty and housing instability. Using administrative data and ethnographic fieldwork, Desmond finds that eviction is common and highly concentrated in poor neighborhoods. In addition, eviction is not only a consequence of economic hardship, but also contributes to continued instability through job loss, homelessness, and financial distress. Read alongside Nelson et al., Raymond et al., and Leung et al., Desmond's work supports the idea that eviction filings reflect both institutional context and local economic conditions while being tied to the trajectories of disadvantaged neighborhoods.

These studies show four possible mechanisms for how eviction activity could influence subsequent property price increases. First, eviction could make more transaction costs for the owner, including the process of recovering the premises. Second, problems related to eviction could create friction leading to capitalization constraints due to regulatory procedures (Diamond et al. 2019; Strickland and Overstreet 2023). Third, the filing rate can signal the market negatively, creating perceptions of regulatory or local problems. Fourth, the impact on supply from altering the landlords' incentives to hold rental property would decrease any upward pressure on property values.

This research also points to demographic and market variables that could mediate this relationship, such as low-income status, racial composition, rent burden, and market rents. These two areas of research have historically remained independent of each other. Housing policy literature views property values as outputs influenced by regulations and supply, yet it has not included eviction filings as a dynamic element. Conversely, eviction literature views filings as output measures resulting from landlord actions and neighborhood conditions but has not explored how filing activity connects to future property value trends.

This gap is addressed by analyzing whether city-wide rates of eviction filings are predictors of future changes in property values using a panel data set. Similar to Nelson et al. (2021), this study treats eviction filings as a city-year variable that is an outcome of local institutions and markets, not a direct measure of hardship. Extending the supply-side mechanisms found in Diamond et al. (2019), and the neighborhood effects of Desmond (2012), this research examines whether rates of eviction filings are associated with subsequent changes in property values, adjusting for city and year fixed effects, and time-varying variables.

Data

This study constructs an annual panel dataset of California cities spanning 2004–2013. The dataset combines housing market, eviction, demographic, and economic variables from several public sources.

Housing market data are obtained from Zillow Research using the Zillow Home Value Index (ZHVI) for All Homes (Smoothed, Seasonally Adjusted). Eviction filing data are collected from Eviction Lab and measured as filings per 100 renter-occupied households.

Additional control variables are collected from the U.S. Census Bureau and the California Department of Justice OpenJustice databases.

These include: median household income, percent renter-occupied housing, population, property crime rates, poverty rates, racial composition variables, rent burden, and median gross rent.

The dependent variable is annual property value appreciation, constructed using the log difference in median property values. The final estimation sample contains approximately 120 city-year observations across 15 California cities after constructing lagged variables and removing missing observations. The panel remains unbalanced because several cities contain incomplete reporting for selected years.

Methods

5.1 Sample and Scope

We construct an annual panel of 16 California cities from 2004 through 2013, merging Eviction Lab filing rates and housing covariates, Zillow ZHVI All Homes (Smoothed, Seasonally Adjusted) for median property values, and California Department of Justice OpenJustice property crime data. We substitute ZHVI for the ACS-derived median property values originally available in the Eviction Lab covariate file because the ACS values are stairstepped across five-year estimate windows; ZHVI provides genuine annual variation. Median household income is taken from a stitched annual series for the same reason. The full panel contains 160 city-year observations; after constructing appreciation as a one-year log difference and lagging the eviction filing rate, the estimation sample is **120 city-year observations across 15 cities for 2005–2013**. San Jose drops entirely because Eviction Lab does not report filing rates for it; San Luis Obispo, Stockton, Anaheim, Irvine, and Santa Ana are also missing in selected years. We retain the unbalanced panel because dropping cities with partial coverage would bias the sample toward larger and better-administered jurisdictions.

5.2 Estimating Equation

Our preferred specification is the two-way fixed effects panel regression:

$$Appreciation_{it} = \beta \cdot \text{EvictionFilingRate}_{i,t-1} + X'_{it}\,\gamma + \alpha_i + \delta_t + \varepsilon_{it}$$

where *i* indexes cities and *t* indexes years. The dependent variable is annual property value appreciation, measured as $\ln(\text{MedianPropertyValue}_{it}) - \ln(\text{MedianPropertyValue}_{i,t-1})$. The log difference symmetrizes a right-skewed level variable, gives β a direct interpretation as the change in the annual appreciation rate (one log-point ≈ one percentage point for changes under roughly ten percent), and removes city-specific time-invariant components of the price level.

The key regressor, $\text{EvictionFilingRate}_{i,t-1}$, is the one-year lag of the city's filing rate, expressed as filings per 100 renter-occupied households. The lag serves two purposes: it matches the timing of the theoretical mechanisms in Section 3 (which operate through buyers incorporating eviction information into pricing decisions, a process unlikely to fully resolve within the same calendar year as the filings), and it mitigates contemporaneous reverse causality — a housing downturn that depresses prices may also push tenants into delinquency and raise filings within the same year. Lagging breaks this contemporaneous feedback loop, though it does not address omitted shocks that persist across years.

The control vector X_{it} contains four time-varying covariates in our baseline specification: log median household income, percent renter-occupied, property crime per 100,000, and log population. Income captures demand-side fundamentals; crime captures local disamenity shocks plausibly correlated with eviction activity; renter share is included because filing rates are mechanically denominated in renter households; population is logged because city sizes range from roughly 45,000 (San Luis Obispo) to nearly four million (Los Angeles). Our expanded specification adds seven theoretically motivated controls (poverty rate, four racial composition

variables, rent burden, and median gross rent), bringing the total to eleven. Monetary variables are left in nominal dollars; year fixed effects absorb common annual price-level shocks. The terms α_i and δ_t are city and year fixed effects, and ε_{it} is the idiosyncratic error.

5.3 Identification

The two-way fixed effects design strips two large classes of confounders. The city fixed effects α_i absorb all time-invariant features of each city: geography, baseline regulatory regime, court structure, long-run demographics. The year fixed effects δ_t absorb all shocks common to every California city in a given year — most importantly the 2008 financial crisis, federal interest rate movements, and statewide legislation. What identifies β is within-city, within-year variation. The identifying assumption is strict exogeneity, $E[\varepsilon_{it} \mid \text{EvictionFilingRate}_{i,t-1}, X_{it}, \alpha_i, \delta_t] = 0$, which can fail in the presence of time-varying, city-specific shocks correlated with both eviction activity and property values that survive the fixed effects — plausibly localized labor market shocks, city-specific zoning changes, or shifts in court administration. We therefore interpret β as the conditional within-city association between lagged eviction filings and subsequent appreciation, rather than as a treatment effect from a quasi-experimental source of variation.

5.4 Inference

Standard errors are clustered at the city level to allow for arbitrary serial correlation in ε_{it} within a city over time, following Bertrand, Duflo, and Mullainathan (2004). Bakersfield's 2008 housing market is not independent of its 2009 housing market, but Bakersfield and Anaheim can plausibly be treated as independent observations conditional on the year fixed effect. With 15 clusters our panel is at the small end of what is generally considered safe for asymptotic

cluster-robust inference — the conventional rule of thumb requires roughly 30–50 clusters before cluster-robust standard errors are well-approximated by the t-distribution with $G - 1$ degrees of freedom. Below this threshold, cluster-robust standard errors tend to be biased downward, making t-statistics artificially large. We read p-values close to conventional thresholds with appropriate caution and treat the wild cluster bootstrap (Cameron, Gelbach, and Miller, 2008) as the natural robustness check.

5.5 Coefficient Interpretation and Hypothesis

Because the dependent variable is a log difference, β is approximately the change in the annual appreciation rate, in log-points, associated with a one-filing-per-100-renter-households increase in the lagged filing rate. A coefficient of −0.04 implies a four percentage-point lower appreciation rate the following year. Filing rates in our sample range from near zero to roughly six (mean 2.3, standard deviation 1.3), so a coefficient of this magnitude implies a one-standard-deviation difference maps to about a five percentage-point difference in subsequent appreciation — economically meaningful. The four mechanisms in Section 3 do not all push the same way: transaction costs, regulatory capitalization, and adverse market signaling each predict $\beta < 0$, while supply restriction can push the opposite way. Our prior is that the negative channels dominate in California, where eviction activity more likely reflects adversarial landlord-tenant dynamics under tight tenant protections than flexible reallocation of housing stock. We test $\beta = 0$ against $\beta \neq 0$ and report two fit measures appropriate for fixed-effects models — an overall R^2 with fixed effects absorbed, and a within-R^2 capturing the share of *residual* variation explained by the regressors after the FE absorb their share. The within-R^2 is the more economically meaningful measure.

5.6 Specification Strategy

We present three progressively saturated specifications. The simple regression is a pooled OLS regression of appreciation on the lagged filing rate alone. Multiple Regression 1 introduces both city and year fixed effects together with the four-variable baseline control vector; it demonstrates the disciplining role of the two-way FE design but is intentionally parsimonious. Multiple Regression 2 — our preferred specification — adds seven theoretically motivated controls (poverty rate, four racial composition variables, rent burden, and median gross rent), matching the full control vector developed in Sections 3 and 5.2. The stepwise presentation lets the reader see how much of the naive correlation in the simple regression survives the fixed effects in Multiple Regression 1 and whether β is robust to the richer conditioning in Multiple Regression 2. The price paid is degrees of freedom: with 120 observations and 15 clusters, adding seven additional regressors raises overfitting concerns. We anticipate mild attenuation of β between the two multiple regressions, interpretable as partial multicollinearity between eviction activity and the additional demographic controls. The fact that β remains negative, significant, and similar in magnitude across specifications is itself informative: the estimated relationship is not driven by the particular subset of controls included.

5.7 Scope of the Design

Two-way fixed effects with a lagged regressor improves substantially on cross-sectional OLS, but it is not a quasi-experimental design. We do not exploit a discrete policy change, a regression discontinuity, or an instrument for eviction activity, and we cannot rule out residual confounding from time-varying, city-specific omitted variables. The estimated β should be read as the conditional within-city association between lagged filings and subsequent appreciation — useful for testing whether the mechanisms in Section 3 are observable in California's regulated housing market, but appropriately short of a fully identified causal treatment effect. Section 9

discusses these limitations and suggests quasi-experimental strategies (most naturally a difference-in-differences design exploiting the staggered local adoption of just-cause eviction ordinances, or an instrumental variables approach using administrative variation in court processing times) that future research could employ to push closer to a causal interpretation.

Results

Simple Regression

Appreciation = $\beta_0 + \beta_1$(Eviction Rate)

OLS Regression Results

Dep. Variable:	appreciation	R-squared:	0.071
Model:	OLS	Adj. R-squared:	0.063
Method:	Least Squares	F-statistic:	8.982
Date:	Tue, 28 Apr 2026	Prob (F-statistic):	0.00332
Time:	16:07:26	Log-Likelihood:	56.995
No. Observations:	120	AIC:	-110.0
Df Residuals:	118	BIC:	-104.4
Df Model:	1		
Covariance Type:	nonrobust		

	coef	std err	t	P>|t|	[0.025	0.975]
const	0.0549	0.028	1.931	0.056	-0.001	0.111
evic_filing_rate_lag	-0.0321	0.011	-2.997	0.003	-0.053	-0.011

Omnibus:	0.019	Durbin-Watson:	0.892
Prob(Omnibus):	0.990	Jarque-Bera (JB):	0.049
Skew:	0.024	Prob(JB):	0.976
Kurtosis:	2.913	Cond. No.	6.05

Notes:
[1] Standard Errors assume that the covariance matrix of the errors is correctly specified.

Multiple Regression

Regression 1

$$\text{Appreciation}_{it} = -0.0384 \cdot \text{EvicFilingRate}_{i,t-1} - 0.0854 \cdot \ln(\text{MHI}_{it}) + 0.0102 \cdot \text{PctRenter}_{it} - 9.674 \times 10^{-6} \cdot \text{PropCrime}_{it} - 0.0878 \cdot \ln(\text{Pop}_{it}) + \alpha_i + \delta_t + \varepsilon_{it}$$

Where: αi= city fixed effects (15 cities), δt= year fixed effects (9 time periods), εit= idiosyncratic error term, Standard errors clustered at the city level

PanelOLS Estimation Summary

Dep. Variable:	appreciation	R-squared:	0.1469
Estimator:	PanelOLS	R-squared (Between):	-1383.7
No. Observations:	120	R-squared (Within):	0.1385
Date:	Tue, Apr 28 2026	R-squared (Overall):	-114.96
Time:	15:25:16	Log-likelihood	195.25
Cov. Estimator:	Clustered		
		F-statistic:	3.1675
Entities:	15	P-value	0.0110
Avg Obs:	8.0000	Distribution:	F(5,92)
Min Obs:	2.0000		
Max Obs:	9.0000	F-statistic (robust):	3.2775
		P-value	0.0091
Time periods:	9	Distribution:	F(5,92)
Avg Obs:	13.333		
Min Obs:	12.000		
Max Obs:	14.000		

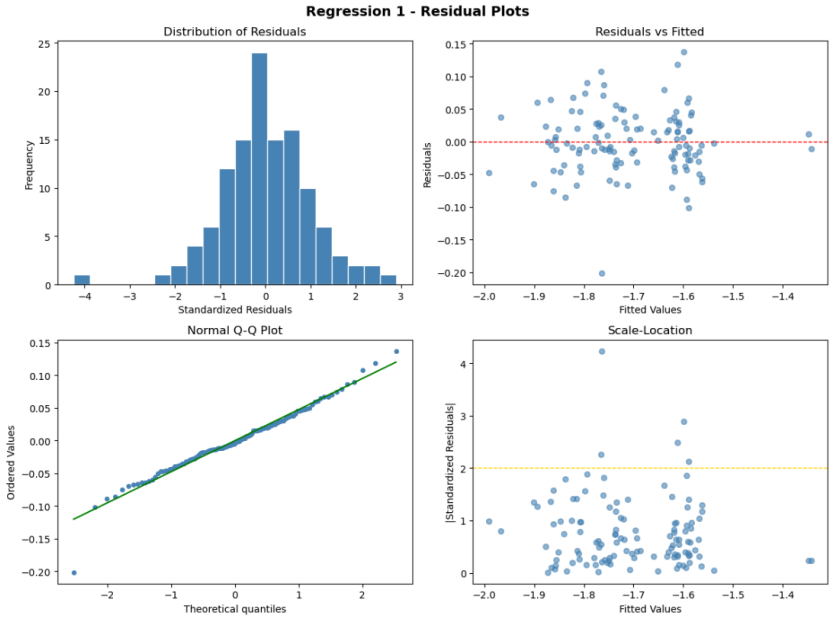
Parameter Estimates

	Parameter	Std. Err.	T-stat	P-value	Lower CI	Upper CI
evic_filing_rate_lag	-0.0384	0.0108	-3.5511	0.0006	-0.0599	-0.0169
log_mhi	-0.0854	0.3550	-0.2405	0.8104	-0.7904	0.6197
pct_renter	0.0102	0.0107	0.9497	0.3448	-0.0111	0.0315
prop_crime_per_100k	-9.674e-06	6.26e-06	-1.5454	0.1257	-2.211e-05	2.759e-06
log_pop	-0.0878	0.2312	-0.3798	0.7050	-0.5469	0.3713

F-test for Poolability: 35.493
P-value: 0.0000
Distribution: F(22,92)

Included effects: Entity, Time

R^2 (with FE absorbed): 0.9152



Regression 2

$$\begin{aligned}\text{Appreciation}_{it} = &-0.0334\text{EvicFilingRate}_{i,t-1} + 0.1864\ln(\text{MHI}_{it}) + 0.0255\text{PctRenter}_{it} - 4.738\times10^{-6}\text{PropCrime}_{it} \\ &- 0.0117\ln(\text{Pop}_{it}) + 0.0295\text{PovertyRate}_{it} + 0.0362\text{PctWhite}_{it} + 0.0482\text{PctAfAm}_{it} \\ &+ 0.0344\text{PctHispanic}_{it} - 0.0010\text{PctAsian}_{it} - 0.0199\text{RentBurden}_{it} + 0.0007\text{MedianGrossRent}_{it} + \alpha_i + \delta_t + \varepsilon_{it}\end{aligned}$$

PanelOLS Estimation Summary

Dep. Variable:	appreciation	R-squared:	0.2514
Estimator:	PanelOLS	R-squared (Between):	-2.127e+04
No. Observations:	120	R-squared (Within):	0.0753
Date:	Tue, Apr 28 2026	R-squared (Overall):	-1730.8
Time:	16:23:13	Log-likelihood	203.09
Cov. Estimator:	Clustered		
		F-statistic:	2.3789
Entities:	15	P-value	0.0107
Avg Obs:	8.0000	Distribution:	F(12,85)
Min Obs:	2.0000		
Max Obs:	9.0000	F-statistic (robust):	71.009
		P-value	0.0000
Time periods:	9	Distribution:	F(12,85)
Avg Obs:	13.333		
Min Obs:	12.000		
Max Obs:	14.000		

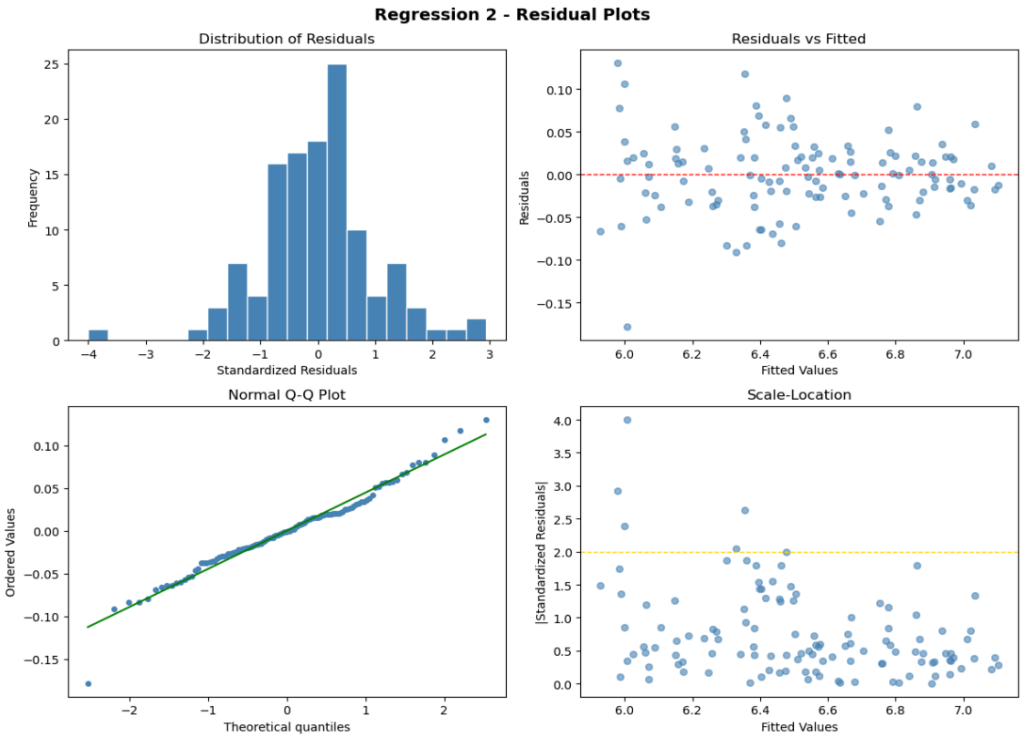
Parameter Estimates

	Parameter	Std. Err.	T-stat	P-value	Lower CI	Upper CI
evic_filing_rate_lag	-0.0334	0.0148	-2.2593	0.0264	-0.0628	-0.0040
log_mhi	0.1864	0.4457	0.4183	0.6768	-0.6997	1.0726
pct_renter	0.0255	0.0081	3.1563	0.0022	0.0094	0.0416
prop_crime_per_100k	-4.738e-06	9.047e-06	-0.5237	0.6019	-2.273e-05	1.325e-05
log_pop	-0.0117	0.1209	-0.0966	0.9233	-0.2520	0.2287
poverty_rate	0.0295	0.0066	4.4509	0.0000	0.0163	0.0427
pct-white	0.0362	0.0152	2.3855	0.0193	0.0060	0.0664
pct-af-am	0.0482	0.0109	4.4030	0.0000	0.0264	0.0700
pct-hispanic	0.0344	0.0156	2.1976	0.0307	0.0033	0.0654
pct-asian	-0.0010	0.0220	-0.0473	0.9624	-0.0448	0.0428
rent-burden	-0.0199	0.0088	-2.2677	0.0259	-0.0374	-0.0025
median-gross-rent	0.0007	0.0002	2.7468	0.0073	0.0002	0.0011

F-test for Poolability: 32.027
P-value: 0.0000
Distribution: F(22,85)

Included effects: Entity, Time

Within R^2: 0.0753
R^2 with FE absorbed: 0.9186



Interpretation

8.1 Simple Regression

The simple OLS regression estimates the relationship between the lagged eviction filling rate and annual property value appreciation, with no control or fixed effects. The model produces a coefficient of -0.032 on the eviction filling rate, it is statistically significant (t=-2.997), and suggests that higher eviction filling rates are associated with lower subsequent property appreciation. However, this specification is deeply flawed, the R squared of 0.071 indicates that the eviction rate alone explains only seven percent of the variation in appreciation. This is unsurprising given that housing markets are driven by a wide array of economic, demographic, and geographic forces omitted here. More critically, the model ignores all city and time level heterogeneity. Cities like San Francisco structurally differ from Fresno in ways that affect both eviction rates and property values simultaneously, and macro shocks like the 2008 financial crisis hit all cities in the same year. Without controlling for these factors, the eviction coefficient is

likely capturing omitted city characteristics and common time trends rather than a true relationship between evictions and appreciation.

8.2 Multiple Regression 1

Because a simple regression is deeply flawed, we choose to continue with multiple regression with two way fixed effects. The first panel regression introduces city fixed effects (α_i) and year fixed effects (δ□), which absorb all time-invariant city characteristics and all common annual shocks respectively. The coefficient on the lagged eviction filing rate is -0.038, statistically significant(t=-3.5511), indicating that a one-unit increase in the prior year's filing rate is associated with a 3.8 percentage point reduction in annual property value appreciation, net of fixed effects and controls. The model includes five time varying controls: log median household income, percent renter-occupied, property crime rate, and log population. The overall model is jointly significant with a F statistic of 3.28, and the R squared with fixed effects absorbed is 0.915. This indicates that the fixed effects plus regressors explains the majority of variation in appreciation. However, four of the five controls are statistically insignificant at 95% confidence level, suggesting the control vector is underspecified. The methodology calls for additional variables including racial composition, rent burden, and poverty rate, all of which are theoretically motivated and available in the dataset. Their omission means the model may leave confounding variation not addressed, and could potentially make the eviction coefficient biased.

8.3 Multiple Regression 2

The second panel regression expands the control vector to twelve variables, adding poverty rate, racial composition, rent burden, and median gross rent. These variables align more closely with the methodology's theoretical framework, which argues that income, demographic composition, and housing market tightness all independently affect both eviction activity and

property values. The eviction coefficient remains negative at -0.033 and is significant at the 95% confidence level (t=-2.259), which means that higher lagged eviction filing rates are associated with lower subsequent appreciation even after controlling for a broad set of city level, time varying confounders. Several additional controls enter significantly: percent renter (t=3.1563), poverty rate (t=4.4509), etc. The R squared is 0.2514, and the absorbed R squared holds at 0.9186. The main limitation of this specification is the loss of degrees of freedom from adding seven additional regressors to a dataset with only 120 observations and 15 clusters, which raises concerns about overfitting and reduces the precision of clustered standard errors. The eviction coefficient also weakens slightly from -0.038 to -0.033, suggesting mild multicollinearity between eviction activity and the newly added demographic controls.

Implications

While this study provides insight into the relationship between eviction filing rates and property value appreciation across California cities, several limitations should be acknowledged.

First, data limitations may affect the precision of our results. Eviction filing data is not perfectly standardized across cities, as legal processes, reporting practices, and court accessibility vary significantly. These institutional differences may influence filing rates independently of underlying economic conditions, making cross-city comparisons imperfect.

Second, our analysis may face omitted variable bias. Although we include key controls such as income, education, rental share, and crime rates, there may be unobserved factors influencing both eviction activity and property values. For example, localized labor market shocks, zoning changes, or differences in policy enforcement could affect results but are difficult to fully capture in a panel framework.

Third, the dataset is unbalanced and contains missing observations, particularly in cities with limited reporting. While retaining these observations improves efficiency, it may introduce bias if missingness is correlated with unobserved economic conditions

Fourth, the model relies on annual data aggregation, which may mask short-term dynamics. Housing markets and eviction activity can respond quickly to economic shocks (such as COVID-19 policies or interest rate changes), but these effects may not be fully captured in yearly data.

Fifth, the use of lagged eviction filing rate, while helping to mitigate simultaneity, does not fully eliminate endogeneity concerns. Persistent shocks that affect both eviction activity and property values over time may still bias the estimates.

Sixth, causality remains difficult to establish. While our regression framework identifies conditional relationships, it does not definitively determine whether eviction activity directly affects property values or reflects broader economic conditions. Reverse causality may also be present.

Finally, statistical inference is limited by the relatively small number of clusters (cities). With only 16 cities, clustered standard errors may be less reliable, and statistical significance should be interpreted with caution.

Future research could build on this study by addressing the limitations of the current empirical design and strengthening causal interpretation.

First, future work could incorporate higher-frequency data, such as monthly or quarterly observations, to better capture short-term responses in housing markets and eviction activity, particularly around policy shocks or macroeconomic changes.

Second, researchers could apply quasi-experimental methods, such as difference-in-differences designs exploiting variation from policy changes like eviction moratoriums or rent control expansions. These approaches would allow for stronger causal identification beyond the fixed-effects framework used in this study.

Third, future studies could explore instrumental variable approaches to address remaining endogeneity concerns. For example, variation in local court processing times or administrative changes in eviction procedures could serve as potential instruments for eviction filing rates.

Fourth, further analysis could examine heterogeneity across cities and housing segments, including differences between single-family homes and multifamily units, or between high- and low-regulation jurisdictions. This would provide insight into whether eviction dynamics affect different parts of the housing market in distinct ways.

Fifth, future research could extend the panel framework by incorporating dynamic models, such as additional lags or distributed lag structures, to better capture how eviction activity affects property values over time.

Finally, incorporating micro-level or property-level data would allow for a more detailed understanding of the mechanisms driving the observed relationships, including landlord behavior, tenant characteristics, and transaction-level outcomes.

Conclusion

This study examines the relationship between eviction filing activity and property value appreciation across California cities using a panel dataset from 2004 to 2013. Across all model specifications, we find a consistent and statistically significant negative association between lagged eviction filing rates and subsequent housing price appreciation. This relationship persists after introducing city and year fixed effects and a comprehensive set of demographic and

economic controls, indicating that it is not solely driven by time-invariant city characteristics or common macroeconomic shocks.

The magnitude of this relationship is economically meaningful. Our preferred specification suggests that a one-unit increase in the eviction filing rate is associated with approximately a 3.3 to 3.8 percentage point decrease in annual property appreciation, while a one-standard-deviation increase corresponds to roughly a 5 percentage point reduction in subsequent appreciation. These results suggest that eviction activity is not just a background feature of housing markets, but rather a factor that is systematically reflected in property values.

From an economic perspective, these findings are consistent with the theoretical mechanisms outlined in this study. Higher eviction filing rates may signal increased transaction costs, regulatory constraints, and institutional complexity, all of which reduce the attractiveness of property investment. Additionally, eviction activity may function as a market signal, conveying information about local economic distress or landlord-tenant dynamics that investors incorporate into pricing decisions.

At the same time, our findings highlight the importance of careful interpretation. While the fixed-effects framework isolates a robust within-city relationship, the analysis does not establish a fully causal effect. Eviction filing rates may partially reflect broader economic conditions that independently influence housing prices, and residual confounding from time-varying local shocks cannot be fully ruled out.

Despite these limitations, this study contributes to an important policy discussion. In highly regulated housing markets such as California, tenant protection policies are designed to promote housing stability and prevent displacement. However, our results suggest that these

policies may also be associated with reduced property value appreciation, reflecting tradeoffs between renter protection and market efficiency.

Overall, this study provides empirical evidence that eviction dynamics are meaningfully linked to housing market outcomes. Understanding this relationship is critical for policymakers, investors, and stakeholders seeking to balance the goals of housing stability, affordability, and efficient market functioning in complex urban housing systems.

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