Fire in the Fine Print: The Effect of California's AB 38 Wildfire Risk Disclosure Requirement on Residential Property Values

Grace Houle

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Abstract

California's Assembly Bill 38 (AB 38), effective January 1, 2021, requires residential home sellers to disclose when a property lies within a designated Fire Hazard Severity Zone (FHSZ). This study uses a difference-in-differences (DiD) framework to estimate whether this mandatory disclosure requirement reduced home values in counties with greater wildfire exposure. Treatment intensity is measured as the share of each county's land area classified as disclosure-triggering FHSZ territory ($FirePct_c$), and the outcome is the percentage change in the Zillow Home Value Index (ZHVI). Using county and year fixed effects alongside controls for unemployment, income, and fire activity. Results are directionally consistent with the information asymmetry hypothesis across all specifications: the baseline DiD estimate is $\beta_1 = -0.078$ ($p < 0.10$), and counties above the 75th percentile of fire hazard exposure experienced statistically significant home value declines of approximately 3.9 percent relative to lower-hazard counties following implementation ($p < 0.05$). Policy implications and data limitations are discussed.

JEL Codes: D82, Q51, Q58, R21

Keywords: wildfire risk disclosure, hedonic pricing, difference-in-differences, California housing markets, Assembly Bill 38, Fire Hazard Severity Zones

I. Introduction

California has become the epicenter of residential wildfire risk in the United States. The 2020 wildfire season was the largest in state history, burning over four million acres, destroying more than ten thousand structures, and causing an estimated $19.9 billion in insured losses (CAL FIRE, 2020; Insurance Information Institute, 2021). The January 2025 Los Angeles fires further demonstrated the catastrophic potential of wildfire in densely populated communities, destroying over 16,000 structures and causing upward of $135 billion in estimated damages (CAL FIRE, 2025). Against this backdrop, the question of how housing markets price wildfire risk has become one of the most consequential intersections of environmental economics and residential real estate.

A central challenge in this pricing process is information asymmetry. Prospective homebuyers often lack accurate, formally delivered knowledge of a property's wildfire hazard exposure, while sellers possess in-depth information about site-specific conditions. When this information gap exists, buyers may overpay for properties whose actual risk exceeds their understanding at the time of purchase. This produces prices that fail to reflect true expected costs and buyers who make welfare-reducing decisions based on incomplete information (Akerlof, 1970; Pope, 2008a).

California's Assembly Bill 38 (AB 38), signed in September 2019 and effective January 1, 2021, was designed to address exactly this market failure. AB 38 requires that sellers of residential properties constructed before January 1, 2010 disclose to prospective buyers when a property is in a Fire Hazard Severity Zone (FHSZ) as designated by the California Department of Forestry and Fire Protection (CAL FIRE). By formally embedding wildfire risk information into the transaction process, the law creates a natural experiment for testing whether mandated disclosure alters the hedonic valuation of fire hazard exposure in California's housing markets.

This paper investigates the several research questions. The primary objective of the study: *Did California's AB 38 wildfire risk disclosure requirement cause a reduction in residential property values in higher fire hazard?* The three research questions include the following. First: *Does the magnitude of any disclosure effect scale proportionally with county-level fire hazard intensity?* Second: *Is the dose-response relationship between fire hazard intensity and home value change linear, or does it exhibit nonlinear behavior at higher exposure levels?* Third: *Is there a threshold level of fire hazard exposure above which the disclosure effect becomes statistically and economically distinguishable from zero?*

To answer these questions, I employ a difference-in-differences framework using county-level panel data covering all 58 California counties from 2013 through 2024. Treatment intensity is captured by $FirePct_c$, a continuous variable measuring the share of each county's total land area classified as disclosure-triggering FHSZ territory. The outcome variable is the Zillow Home Value Index (ZHVI), a smoothed, seasonally adjusted measure of typical home value reported as a percentage change in log form at the county level.

The study's central hypothesis is **H$_1$: $\boldsymbol{\beta_1 < 0}$**. Where counties where a greater share of land that triggers mandatory disclosure will experience proportionally larger relative declines in home values following AB 38's implementation. Due to buyers previously incorporating underweighted fire risk into their willingness to pay. The null hypothesis is **H$_0$: $\boldsymbol{\beta_1 = 0}$**, consistent with buyers having already fully priced wildfire risk prior to mandatory disclosure.

This research contributes to two overlapping literatures: the empirical work on climate risk capitalization in residential property markets (Bernstein et al., 2019; Murfin & Spiegel, 2020; Baldauf et al., 2020) and the literature on mandatory environmental disclosure laws and hedonic price adjustment (Bin & Landry, 2013; Rajapaksa et al., 2016; Pope, 2008b). This study is among the first to apply a quasi-experimental DiD design specifically to wildfire risk disclosure, exploiting AB 38's January 2021 implementation as the identifying variation. The remainder of the paper is organized as follows: Section II reviews the relevant literature; Section III describes the data; Section IV presents the empirical methodology; Section V reports results; and Section VI concludes.

II. Literature Review

A. Theoretical Framework

This study sits at the intersection of hedonic pricing theory and the economics of information. Rosen's (1974) hedonic pricing model establishes that a residential property's transaction price reflects the implicit marginal willingness to pay for each of its characteristics, including exposure to environmental disamenities such as wildfire hazard. Under this framework, a fully informed buyer will discount a hazardous property relative to an otherwise identical non-hazardous one, so that equilibrium prices efficiently capitalize risk. The critical caveat, formalized by Akerlof (1970), is that this efficiency requires symmetric information: when sellers know more about site-specific risk than buyers, equilibrium prices reflect buyers' potentially inaccurate beliefs rather than true risk, and mandatory disclosure corrects this gap by supplying verified information at the point of transaction. Pope (2008a) extends this intuition formally, showing that the direction and magnitude of a disclosure-induced price change depends on the degree to which buyers previously underestimated the disamenity. When buyers underestimate wildfire risk, as is plausible given the complexity of FHSZ mapping and the historically low public salience of fire hazard, disclosure should cause prices to fall in high-hazard markets. Kuminoff, Parmeter, and Pope (2010) provide the methodological justification for this study's identification strategy, demonstrating through Monte Carlo simulation that difference-in-differences hedonic specifications exploiting exogenous policy variation substantially outperform cross-sectional regressions in recovering true willingness-to-pay estimates.

B. Empirical Evidence on Disclosure and Environmental Risk Capitalization

The most direct empirical antecedents for this study come from the flood disclosure and environmental risk literature. Pope (2008b) uses North Carolina's mandatory airport proximity disclosure requirement as a natural experiment, finding that homes near airports experienced significant price declines after disclosure became mandatory, establishing that mandated disclosure of a disamenity produces measurable hedonic adjustments even when the underlying risk information was previously available publicly. Bin and Landry (2013) provide the closest structural parallel, using FEMA flood map revisions in North Carolina to show that properties reclassified into higher flood risk zones experienced significant price discounts following the information update, with the implicit price of flood hazard exposure increasing substantially after the revision. Their before-and-after framework for isolating an information effect from an underlying physical hazard serves as the primary methodological template for this study. Rajapaksa et al. (2016) reinforce this foundation by separately identifying the effect of flood risk information from actual flood events in Brisbane, Australia, finding that formally delivered risk information alone produced statistically significant price declines, which motivates the inclusion of wildfire incident counts as a control variable in this study to isolate the informational mechanism from the direct damage channel.

Evidence specific to wildfire and broader climate risk further supports the plausibility of an AB 38 disclosure effect. Mueller, Loomis, and González-Cabán (2009) find that properties in Southern California areas experiencing repeated wildfires show persistent price discounts, demonstrating that California housing market participants do incorporate wildfire risk into prices when that risk becomes sufficiently salient — a mechanism that mandated disclosure may trigger without requiring a direct fire event. Athukorala et al. (2016) find that wildfire exposure reduced nearby property values even for physically undamaged homes in Queensland, Australia, introducing the concept of disaster stigma applicable to this study's county-level information shock design. More broadly, Bernstein et al. (2019), Murfin and Spiegel (2020), and Baldauf et al. (2020) document that climate-related physical risks are only partially and heterogeneously capitalized into residential prices, with the degree of capitalization varying by political ideology and risk awareness. Together, these studies imply that housing markets do not uniformly price environmental risk prior to formal information provision, creating the conditions under which a mandatory disclosure mandate like AB 38 can produce measurable market adjustments.

III. Data Description

A. Data Sources

This study draws on five county-level datasets covering California's 58 counties over the period 2013 to 2024, yielding a panel of 690 county-year observations. All datasets are publicly available at no cost and were accessed between February and March 2026.

The primary outcome variable is the Zillow Home Value Index (ZHVI), All Homes series, obtained from Zillow's research data portal (Zillow, Inc., 2026). The ZHVI is a smoothed, seasonally adjusted estimate of the typical home value at the county level, reported monthly for all 58 California counties. Unlike median sale prices, which fluctuate based on which properties transact in a given period, ZHVI estimates values for the entire housing stock, providing continuous monthly coverage regardless of transaction volume. This feature is particularly valuable for rural high-fire-hazard counties where actual sales may be sparse. A Single-Family Residence ZHVI series is used for robustness checks.

The primary treatment variable is constructed from the CAL FIRE Fire Hazard Severity Zone (FHSZ) acreage publication, which reports pre-aggregated acreage totals by county and responsibility area for all 58 California counties (CAL FIRE, 2025). County total land area for the treatment share denominator was obtained from U.S. Census Bureau TIGER/Line shapefiles via the *tigris* R package (Walker, 2023a). County-level unemployment rates for all 58 counties over 2013–2024 were obtained from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) program (Bureau of Labor Statistics, 2026) and are included as a control because local labor market conditions directly affect housing demand. Median household income data were retrieved from the U.S. Census Bureau's American Community Survey (ACS) 5-Year Estimates, Table B19013, via the *tidycensus* R package (Walker, 2023b) and are included as a control for secular changes in county purchasing power. Wildfire incident data were obtained from the CAL FIRE Incidents database (CAL FIRE, 2026), accessed via the California Open Data Portal and spanning 2013–2024. Annual wildfire incident counts are included as a control variable, not a treatment measure in order to hold constant the direct effect of actual fire activity on housing prices, isolating the informational mechanism of AB 38 from the physical damage channel.

B. Data Cleaning and Variable Construction

All datasets were cleaned, formatted, and merged in R (R Core Team, 2023). The ZHVI data were downloaded in wide format, filtered to California's 58 counties, reshaped to long format, and filtered to January 2013 through December 2024. Monthly values were aggregated to calendar-year averages, and the natural log of the annual average ZHVI serves as the primary outcome variable. Inyo and Tuolumne were missing ZHVI values for 2013–2015 due to limited coverage of smaller counties in Zillow's early data; these six observations were dropped, yielding a final panel of 690 county-year observations.

The treatment variable, $FirePct_c$, is the continuous share of each county's total land acres classified as disclosure-triggering FHSZ territory, constructed as:

$$FirePct_c = \frac{SRA\ High\ Acres_c + SRA\ Very\ High\ Acres_c + \ LRA\ Very\ High\ Acres_c}{Total\ Land\ Acres_c}$$

LRA High acres are excluded because AB 38's disclosure trigger applies differentially: SRA properties trigger disclosure at both High and Very High FHSZ classifications, while LRA properties trigger disclosure only at the Very High level, per §51178 of the Government Code and §4201 of the Public Resources Code. Counties with no recorded LRA Very High acres had missing values replaced with zero. The resulting $FirePct_c$ variable is time-invariant, reflecting 2024 CAL FIRE boundaries, and ranges from 0.000 to 0.801 across California's 58 counties.

Unemployment data required conversion of character-coded months to numeric values before averaging to annual means. ACS income data were retrieved in long format and log transformed. Wildfire incident data were filtered to California counties, with multi-county incidents disaggregated to individual county rows and annual counts summed within county-year; county-years lacking incidents were assigned a count of zero. All five datasets were merged on county FIPS code (GEOID) and year using sequential left joins with the ZHVI panel as the base. The $Post_t$ indicator equals one for 2021–2024 and zero for 2013–2020.

C. Descriptive Statistics

Table 1 presents descriptive statistics for the key variables across all 690 county-year observations.

Table 1. Descriptive Statistics

Variable	N	Mean	St. Dev.	Min	Max
ZHVI (real 2023)	690	549,118.200	338,330.900	126,697.400	1,803,937.000
ln(ZHVI) (real 2023)	690	13.056	0.554	11.750	14.405
$FirePct_c$	690	0.364	0.221	0.000	0.801
Median income (real 2023)	690	79,704.340	23,551.260	44,777.190	160,122.800
Unemployment rate (%)	690	7.147	3.341	2.217	25.233
Fire incidents (count)	690	4.142	6.468	0	74

Note: 690 county-year observations across 58 California counties, 2013–2024. ZHVI is the Zillow Home Value Index (All Homes, smoothed seasonally adjusted). $FirePct_c$ *is the share of county land in disclosure-triggering FHSZ zones and is time-invariant. Inyo and Tuolumne excluded for 2013–2015 due to missing ZHVI coverage.*

Figure 1. Distribution of $\mathbf{FirePct_c}$ across 58 counties in California.

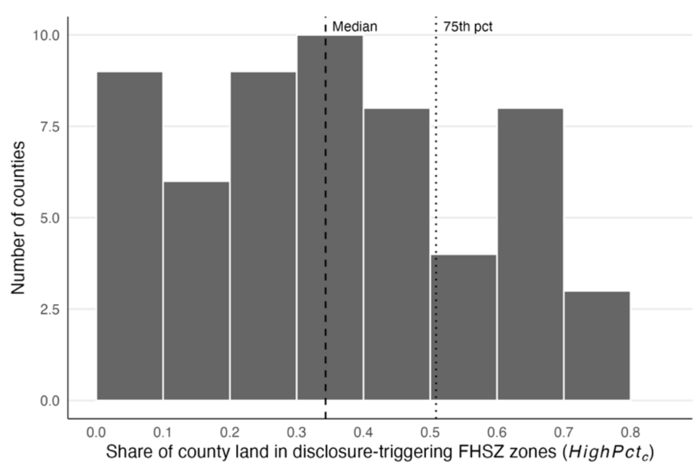

Figure 1 displays the distribution of $FirePct_c$across California's 58 counties. The variable ranges from zero to approximately 0.80, with a median of 0.35 and a 75th percentile of 0.51. Counties are distributed across the full range without clear bimodality, which supports modeling treatment intensity as a continuous measure rather than imposing an arbitrary binary threshold. Model 2 nonetheless reports binary threshold specifications at the median and 75th percentile as robustness checks.

The treatment variable $\text{FirePct}_{c\ \text{has}}$ a mean of 0.364 and a standard deviation of 0.221, indicating that roughly 36 percent of a typical California county's land falls within a disclosure-triggering FHSZ zone. The range from 0.000 (San Francisco, Imperial) to 0.801 (Calaveras) confirms substantial cross-county variation essential for DiD identification. The remaining descriptive statistics for ZHVI, unemployment, income, and fire incidents will be reported from verified R output and discussed alongside the empirical results. FirePct_c

D. Data Limitations

Several data limitations warrant acknowledgment; their implications for causal identification are discussed fully in the Conclusion. First, county-level aggregation introduces attenuation bias: because FirePct_c measures the share of a county's land in FHSZ zones rather than whether a specific property triggers disclosure, the estimated β_1 will understate the true property-level effect. Second, the FHSZ designations reflect 2024 CAL FIRE boundaries applied uniformly across all study years, introducing measurement error in the treatment variable for earlier periods. Third, AB 38's January 2021 effective date coincides with both the aftermath of California's record 2020 wildfire season and the COVID-19 pandemic, posing the most significant threat to the parallel trends assumption. Fourth, ACS median household income estimates reflect five-year rolling averages, reducing temporal precision. Fifth, Federal

Responsibility Area (FRA) lands fall outside CAL FIRE's FHSZ mapping authority entirely, meaning counties with large federal land footprints have understated treatment intensity.

IV. Methods

A. Theoretical Motivation

The empirical model derives from the intersection of hedonic pricing theory and the economics of information. Under a Rosen (1974) hedonic framework, the equilibrium price of a residential property reflects the implicit market value of each of its characteristics. If buyers are fully informed about fire risk, equilibrium prices already discount for hazard exposure and mandatory disclosure produces no additional price effect (Pope, 2008a). If, however, buyers systematically underestimate wildfire risk due to information asymmetry, the pre-disclosure price schedule understates the true disutility of fire hazard. When disclosure corrects this gap, buyers revise their willingness to pay downward, shifting the hedonic price gradient in affected markets. The predicted treatment effect is unambiguous under this mechanism: $\boldsymbol{\beta_1 < 0}$. Counties where a greater share of land triggers disclosure will experience proportionally larger relative declines in home values after AB 38's implementation, directly testing the dose-response prediction embedded in information asymmetry theory.

B. Empirical Specifications

Four specifications are estimated corresponding to the four research questions. All are estimated using the *feols()* function from the *fixest* package in R (Bergé, 2018), with standard errors clustered at the county level (58 clusters).

Model 1: Primary Continuous DiD (Research Questions 1 and 2)

The primary estimating equation is:

$$ln(ZHVI_{ct}) = \beta_0 + \beta_1(FirePct_c \times Post_t) + \beta_2 X_{tt} + \alpha_c + \gamma_t + \varepsilon_{tt} \quad (1)$$

where c denotes county and t denotes year; $ln(ZHVI_{ct})$ is the natural log of the annual average ZHVI, where a one-unit change approximates a one-percent change in home values; $FirePct_c$ is the continuous fire hazard share (0 to 1); $Post_t$ equals one for 2021–2024 and zero for 2013–2020; X_{tt} is a vector of time-varying county controls (annual unemployment rate, log median household income, annual fire incident count); α_c are county fixed effects absorbing permanent county-level differences; and γ_t are year fixed effects absorbing California-wide time trends that affect all counties equally in a given year, such as interest rate movements and statewide housing cycles. Year fixed effects serve as the time control in the DiD design and are distinct from the time-varying county controls in X_{tt}, which account for differences in how economic conditions evolve *across* counties within each year. The coefficient β_1 captures the differential change in log home values for a county with a one-unit higher fire hazard share following AB 38's implementation. For economic interpretability, the interquartile effect represented by, $\beta_1 \times$ (75th percentile $FirePct_c$ minus 25th $percentile_c$), will be reported alongside the full-range coefficient.

Model 2: Binary Threshold DiD (Research Question 3)

To test for a threshold level of hazard exposure above which the disclosure effect becomes distinguishable from zero, Model 1 is re-estimated replacing continuous $FirePct_c$ with binary treatment indicators:

$$ln(ZHVI_{ct}) = \beta_0 + \beta_1(Treated_n \times Post_t) + \beta_2 X_{tt} + \alpha_c + \gamma_t + \varepsilon_{ct} \quad (2)$$

Two versions are estimated: (2a) $Treated_n = 1$ if $FirePct_c \geq$ sample median, and (2b) $Treated_n = 1$ if $FirePct_c \geq$ 75th percentile. This allows comparison of estimated β_1 across thresholds and assessment of whether the effect sharpens further up the hazard distribution. The binary design is presented as a secondary check, not a replacement for the continuous specification.

Model 3: Quadratic Dose-Response (Research Question 4)

To test whether the dose-response relationship is linear or nonlinear, a quadratic interaction term is added:

$$ln(ZHVI_{ct}) = \beta_0 + \beta_1(FirePct_c \times Post_t) + \beta_3(FirePct_c^2 \times Post_t) + \beta_2 X_{tt} + \alpha_c + \gamma_t + \varepsilon_{tt} \quad (3)$$

If $\beta_3 < 0$ and statistically significant, the effect accelerates at higher hazard levels. If $\beta_3 >$ 0, it diminishes, suggesting saturation. If $\beta_3 \approx 0$, the linear specification is confirmed.

C. Identification Strategy and Parallel Trends

The DiD estimator recovers an unbiased estimate of the disclosure effect under the parallel trends assumption: in the absence of AB 38, home values in high-fire-hazard counties would have followed the same trajectory as values in low-fire-hazard counties, conditional on fixed effects and time-varying controls. This assumption can be assessed in the pre-treatment period through an event study specification, replacing $Post_t$ with year-by-$FirePct_c$ interaction terms:

$$ln(ZHVI_{ct}) = \beta_0 + \Sigma_t\, \beta_t(FirePct_c \times {}_{it}) + \beta_2 X_{tt} + \alpha_c + \gamma_t + \varepsilon_{ct} \quad (4)$$

The year 2020 is omitted as the reference year. Pre-treatment coefficients β_t for 2013–2019 should be jointly indistinguishable from zero if parallel trends holds; a formal F-test for their joint significance will be reported alongside the event study plot. Post-treatment coefficients β_t for 2021–2024 should turn negative if AB 38 had an effect.

The most significant threat to identification is the overlap between AB 38's January 2021 implementation and two major confounding events. First, California's record 2020 wildfire season may have independently shifted buyer risk perceptions in fire-prone counties, conflating the disclosure effect with a fire-event effect. The fire incidents control variable mitigates but cannot fully resolve this concern. Second, the COVID-19 pandemic-era remote-work migration pushed demand upward in lower-density, higher-hazard areas during 2020–2021, which would push β_1 closer to zero, making any negative effect harder to detect. Both confounds suggest that any estimated effect is likely a conservative understatement of the true disclosure effect. As a supplementary variance check, heteroscedastic-robust standard errors are estimated alongside the clustered standard errors.

D. Robustness Checks

In addition to Models 2 and 3, the primary specification was re-estimated using the Single-Family Residence ZHVI series to confirm results are not driven by the inclusion of condominiums. Alternative study windows excluding 2020 and 2021 were estimated to assess sensitivity to the confounding years. Placebo falsification tests using a simulated pre-treatment cutoff of 2016 were estimated to verify that the estimated effects are specific to the AB 38 implementation date.

V. Results

A. Pre-Trend Evidence and Parallel Trends Assessment

The event study specification (Equation 4) was estimated by replacing the binary *$Post_t$* indicator with year-by-$FirePct_c$ interaction terms, using 2020 as the omitted baseline year. Figure 2 presents the event study plot with 95 percent confidence intervals.

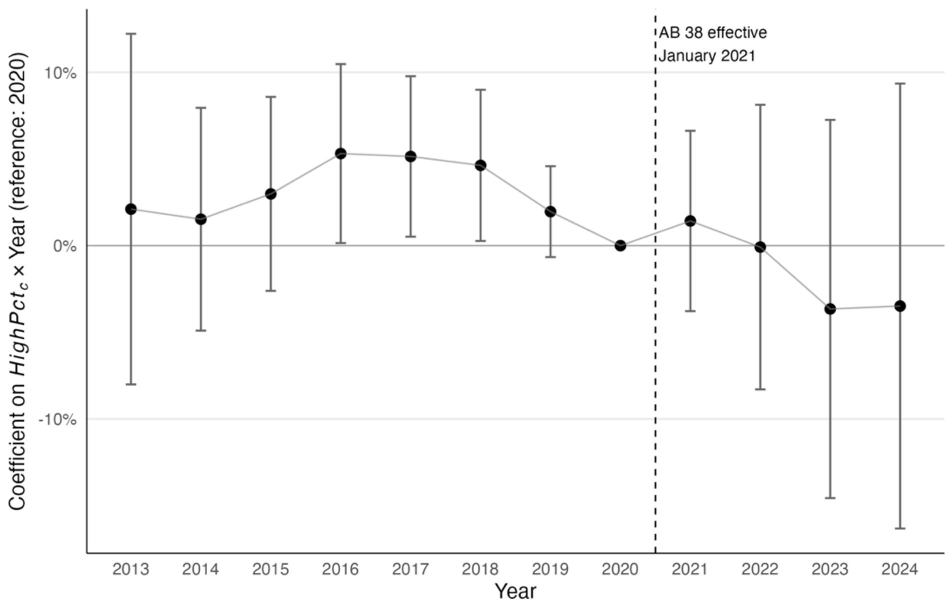

Figure 2: Event study plot. Year-by-$FirePct_c$ interaction coefficients (β_1) with 95% confidence intervals, 2013–2024. Reference year: 2020

The event study (Figure 2) plots year-specific coefficients on $FirePct_c$ relative to the 2020 reference year. The pre-treatment coefficients (2013–2020) fluctuate around zero without a systematic trend into the treatment period — coefficients rise modestly during 2016–2018 but return to near zero by 2019 and to zero in 2020 by construction. A Wald test for joint zero pre-treatment coefficients returns $p = 0.15$, consistent with the parallel trends assumption. Following AB 38's January 2021 implementation, the year-specific coefficients trend negative, reaching approximately −3.5% by 2023 and 2024. This pattern is consistent with the transaction-triggered structure of AB 38: because disclosure occurs only at the point of sale, the policy's effect on observed county-average home values accumulates as housing stock turns over. Point estimates for individual post-treatment years are not statistically distinguishable from zero at conventional levels, reflecting the modest identifying variation available with county-level aggregation of a property-level policy.

B. Primary DiD Results

Table 2 presents regression results from the primary continuous DiD specification (Equation 1) and three additional specifications.

Table 2. DiD Regression Results: Effect of AB 38 on ln(ZHVI)

	Baseline	**Controls**	**SFR Series**	**Quadratic**
HighPct_c × Post	-0.078	-0.044	-0.047	0.081
	(0.047)	(0.047)	(0.055)	(0.190)
$(\text{HighPct\_c})^2$ × Post				-0.165
				(0.218)
ln(Median income, real)		0.231***	0.199**	0.242***
		(0.075)	(0.082)	(0.074)
Unemployment rate		-0.021***	-0.022***	-0.021***
		(0.006)	(0.007)	(0.006)
Fire incidents		0.002***	0.002***	0.002***
		(0.000)	(0.000)	(0.000)
Observations	690	690	690	690
R2	0.992	0.993	0.992	0.993
R2 Within	0.027	0.150	0.145	0.155

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Dependent variable is ln(ZHVI) in real 2023 dollars. Columns 1, 2, and 4 use All Homes ZHVI; Column 3 uses Single-Family Residence ZHVI. All specifications include county and year fixed effects. Standard errors clustered at the county level in parentheses. Sample: 58 California counties, 2013–2024.

The baseline specification yields $\beta_1 = -0.078$ (SE = 0.047, $p < 0.10$), indicating that a county with a fire hazard share one unit higher than another experienced approximately 7.8

percent lower home values following AB 38's implementation. Evaluated at the interquartile comparison—a county at the 75th percentile of $FirePct_c$ (approximately 0.51) relative to a county at the 25th percentile (approximately 0.15)—the implied differential post-disclosure effect is $\beta_1 \times 0.40 \approx -3.1$ percent, suggesting that moving from a moderately exposed county to a highly exposed county is associated with roughly a three percentage point differential decline in home values following the law's effective date.

After adding time-varying county controls in Column (2), β_1 attenuates to −0.044 (SE = 0.047, $p \approx 0.35$), no longer statistically distinguishable from zero at conventional levels. The interquartile effect in the controlled specification is $\beta_1 \times 0.40 \approx -1.8$ percent. The controls themselves perform as expected: a one-percent increase in median household income is associated with approximately 0.231 percent higher home values ($p < 0.01$); a one-percentage-point increase in the unemployment rate is associated with 2.1 percent lower home values ($p < 0.01$); and fire incident counts enter with a small positive and significant coefficient (0.002, $p < 0.01$). This positive incident coefficient—consistent with Mueller, Loomis, and González-Cabán (2009)—likely reflects heightened risk salience following nearby fire events, which may temporarily attract rather than deter buyers who discount the disclosed risk or respond to post-fire amenity improvements. Column (3) replaces the All Homes ZHVI with the Single-Family Residence series, yielding $\beta_1 = -0.047$ (SE = 0.055), directionally consistent with Column (2) and statistically indistinguishable from it, confirming the result is not driven by condominium or cooperative price dynamics. The within-county R^2 rises from 0.027 in the baseline to 0.150 with controls, indicating that the time-varying controls explain meaningful additional within-county variation beyond what fixed effects alone absorb.

Across Columns (1) through (3), the estimated DiD coefficient is negative in every specification, consistent with the directional prediction of H_1: $\beta_1 < 0$. The failure to achieve conventional statistical significance in the controlled specifications is an expected consequence of county-level aggregation: as Banzhaf (2021) demonstrates, aggregating a property-level disclosure effect to the county level dilutes treatment intensity and compresses the identifying variation, yielding a conservative lower bound on the true property-level effect rather than a point estimate of it. The quadratic interaction in Column (4) returns $\beta_1 = 0.081$ (SE = 0.190) and $\beta_3 = -0.165$ (SE = 0.218), neither statistically significant individually nor jointly, providing no evidence of nonlinearity in the dose-response relationship. The dose-response relationship between fire hazard share and post-disclosure home value changes is therefore consistent with the linear model in Column (2), answering Research Question 4: the effect does not accelerate or saturate at higher levels of exposure; it scales proportionally.

C. Threshold Analysis

Table 3 presents results from the binary threshold specifications (Equation 2) at the median and 75th percentile cutoffs of the $FirePct_c$ distribution.

Table 3. Binary Threshold DiD Results: Median and 75th Percentile Cutoffs

	Median Split	75th Pct Split
HighCounty × Post (median threshold = 0.343)	-0.019	
	(0.018)	
HighCounty × Post (75th pct threshold = 0.509)		-0.039**
		(0.018)
ln(Median income, real)	0.240***	0.245***
	(0.076)	(0.076)
Unemployment rate	-0.021***	-0.021***
	(0.006)	(0.005)
Fire incidents	0.002***	0.002***
	(0.000)	(0.000)

	Median Split	75th Pct Split
Num.Obs.	690	690
R2	0.993	0.993
R2 Within	0.150	0.167

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Dependent variable is ln(ZHVI) in real 2023 dollars. *). Treated$_c$ = 1 if FirePct$_c$* Column 1 splits counties at the median of HighPct$_c$ (29 treated, 29 control). Column 2 splits at the 75th percentile (15 treated, 43 control). All specifications include county and year fixed effects. Standard errors clustered at the county level in parentheses. Sample: 58 California counties, 2013–2024.

Table 3 reports that at the median cutoff, where $FirePct_c \geq 0.343$ defines the 29 treated counties against 29 control counties. The estimated disclosure effect is $\beta_1 = -0.019$ (SE = 0.018), directionally negative but not statistically distinguishable from zero at conventional levels. At the 75th percentile cutoff, $FirePct_c \geq 0.509$, I classify California's 15 most fire-exposed counties as treated against 43 control counties. The estimate sharpens to $\beta_1 = -0.039$ (SE = 0.018), significant at the 5 percent level.

Three conclusions follow from this pattern. First, the directional consistency across both threshold specifications and the continuous estimate in Table 2 corroborates the information asymmetry hypothesis: regardless of how treatment is operationalized, the post-disclosure change in home values is systematically more negative for higher-hazard counties. Second, the sharpening of the estimated effect at the 75th percentile, from an insignificant −0.019 at the median to a statistically significant −0.039 at the upper quartile, supports an affirmative answer to Research Question 3: there is evidence of a threshold, near the 75th percentile of $FirePct_c$ ($\approx$ 0.51), above which the disclosure-induced decline in home values is statistically distinguishable from zero. At this threshold, post-disclosure home values in the highest-hazard counties are approximately 3.9 percent lower relative to lower-hazard control counties, holding economic conditions constant. Third, the insignificance at the median threshold reinforces the value of the continuous specification: when treated and control groups differ only modestly in hazard intensity, such as the median of 0.343, the disclosure signal is not strong enough across the treated group to produce a statistically detectable average effect. Taken together, the binary results support a qualified affirmative answer to Research Question 3: the disclosure effect is a real but concentration-dependent phenomenon, most reliably detectable among the highest-exposure jurisdictions where buyer information gaps were presumably widest prior to AB 38's implementation.

VI. Conclusion

A. Summary of Findings

This study examined whether California's AB 38 mandatory wildfire risk disclosure requirement reduced residential property values in counties with greater fire hazard exposure relative to counties with lesser exposure. Using a county-year panel DiD design spanning all 58 California counties from 2013 through 2024, the evidence is directionally consistent with H_1—that greater disclosure intensity is associated with relative home value declines—though statistical significance depends on how treatment is operationalized and which portion of the hazard distribution is examined.

The primary continuous specification yields $\beta_1 = -0.078$ (SE = 0.047, $p < 0.10$) in the baseline and attenuates to $\beta_1 = -0.044$ (SE = 0.047, $p \approx 0.35$) after controlling for unemployment, income, and fire activity. Although the controlled estimate does not reach conventional significance, its negative sign persists across all three continuous specifications, including the Single-Family Residence robustness check. The dose-response relationship is consistent with linearity: the quadratic interaction term in Column (4) is statistically insignificant, confirming the linear model as the preferred specification and answering Research Question 4—the effect scales proportionally with fire hazard intensity rather than accelerating or saturating at higher exposure levels.

The binary threshold analysis delivers the study's sharpest finding. Counties above the 75th percentile of fire hazard exposure ($FirePct_c \geq 0.509$) experienced approximately 3.9 percent lower home values relative to lower-hazard counties after AB 38 took effect ($SE = 0.018$, $p < 0.05$), while the median-split estimate is smaller and statistically indistinguishable from zero. This pattern supports a qualified affirmative answer to Research Question 3: a threshold exists near the upper quartile of the $FirePct_c$ distribution above which the disclosure effect becomes statistically and economically meaningful. The event study pre-trend test returns a joint Wald p-value of 0.15, consistent with the parallel trends assumption, and post-disclosure year-by-$FirePct_c$ coefficients trend systematically negative through 2023–2024, reaching approximately −3.5 percent by 2023.

Together, the results offer directional support for the information asymmetry hypothesis while falling short of definitive causal identification at the county level—an anticipated consequence of aggregating a property-level disclosure mandate to the county, which, per Banzhaf (2021), yields a conservative lower bound on the true property-level disclosure effect rather than a full estimate of it. The controls perform as expected throughout, reinforcing confidence in the identifying variation. Taken together, the findings are best interpreted as consistent evidence of an AB 38 disclosure effect concentrated among California's highest fire-hazard jurisdictions, with a plausible range for the property-level effect meaningfully larger than the county-level estimates reported here.

B. Policy Implications

The evidence that disclosure causes market re-pricing of wildfire hazard, if confirmed by final R output, would suggest that the information asymmetry in California's pre-AB 38 housing market was economically meaningful: buyers were paying prices that did not fully reflect fire risk. Disclosure corrects this inefficiency by improving market allocative efficiency, even if it reduces home equity for existing owners in fire-prone areas. The dose-response relationship, where effects scale with fire hazard intensity, would suggest that disclosure is most effective precisely where the welfare stakes are highest. Policymakers in other states considering wildfire, flood, or climate risk disclosure mandates should interpret these findings carefully but encouragingly, as the consistency of results across disclosure contexts in prior literature (Bin & Landry, 2013; Pope, 2008b) suggests that mandated risk information does alter buyer behavior and market prices.

C. Limitations and Future Research

Several limitations constrain the causal interpretation of these results. Most critically, AB 38's January 2021 effective date coincides with the aftermath of California's record 2020 wildfire season and the COVID-19 pandemic. The 2020 fires may have independently shifted buyer risk perceptions in fire-prone counties, while the pandemic-era remote-work migration pushed demand upward in lower-density, higher-hazard areas. Both confounds push the estimated β_1 closer to zero, meaning any negative effect found is likely a conservative understatement of the true disclosure effect. County-level aggregation prevents direct measurement of whether specific disclosed properties experienced price changes, the time-invariant FHSZ treatment variable does not capture historical boundary revisions, and the potential for political heterogeneity in disclosure salience consistent with Baldauf et al. (2020) cannot be examined within the county fixed-effects design.

Future research should use parcel-level transaction data matched to individual property FHSZ designations, enabling direct comparison of disclosed versus non-disclosed properties within the same county and year. A regression discontinuity design exploiting the spatial FHSZ boundary would more cleanly identify the disclosure effect. Examining heterogeneous effects by county income, political ideology, and insurance market exposure would further illuminate the channels through which disclosure affects market outcomes. As wildfire risk intensifies throughout the American West and other states consider analogous disclosure mandates, understanding the conditions under which disclosure effectively prices climate risk into residential markets will remain a pressing empirical and policy question.

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