Moratorium on non-renewals of fire insurance and Home Values: Zip-code level evidence from California *

S.Sayantani †

August 27, 2025

Abstract

Aimed at protecting homeowners from lack of insurance coverage spurred by wildfire events, the California Department of Insurance started enacting zipcode level one-year moratoria on non-renewal of fire insurance in 2019. Across zip codes, these moratorium orders were enacted for different lengths of time, starting and ending non-uniformly in different months between 2019 to 2024. Employing the moratorium information and a widely-used home value index tracking monthly changes in home values, the current paper attempts to estimate the impact of the moratorium on changes in home-valuation at the zip-code level. Across sub-samples based on risk-heterogeneity, a treatment status is assigned to locations that had the moratorium in effect, and the evolution of home values are compared within places subject to similar risk levels. The paper estimates the average treatment effect of the moratoria using a recent variation of the traditional TWFE estimator that is more suited to multiple treatment groups with staggered treatment introductions and withdrawals. The study provides evidence that relative to areas that did not have the protection of moratorium, immediately following the enactment of the moratorium, average home values seem to increase in relatively-high-risk locations. This effect continues to increase as the time-frame expands, possibly implying that the potential damage in relatively-high-risk areas play a less pronounced role in demand for housing over the sense of security that comes with continued insurance renewal. Although more muted, home values are found to reduce in very-high-risk zipcodes.

Keywords: wildfires, insurance, home value, moratorium
JEL Classification: G22; R31; Q58

*Declarations of interest: none.

†Ziman Center for Real Estate, University of California, Los Angeles, sayantani95@g.ucla.edu

1 Introduction

According to the Reuters, 7 of the 10 largest wildfires in California happened after 2017.[1] Unexpectedly, the last few years has also seen withdrawal of multiple insurance companies from their California markets owing to the heavy losses from fire damage incurred by buyers of home-insurance.[2] Consequently, the Insurance Commissioner at the Department of Insurance authored a bill to protect buyers of residential insurance. This important consumer protection law requires a mandatory one-year moratorium on insurance companies canceling or non-renewing residential insurance policies in certain areas within or adjacent to a fire perimeter after a declared state of emergency is issued by the Governor. Following a Governor declaration of a state of emergency, the Department of Insurance partners with CAL-FIRE and the Governor's Office of Emergency Services to identify wildfire perimeters and adjacent ZIP codes within the mandatory moratorium areas. The protection from cancellation or non-renewal lasts for one year from the date of the Governor's emergency declaration. Each year starting 2010, multiple emergency declarations precede one-year moratoria protections that apply to all residential policyholders within the affected areas who suffer less than a total loss, including those who suffer no loss.[3]

While such government mandates may provide immediate relief to homeowners and maintain housing market stability, they can contribute to long-term financial, environmental, and social risks. There are several channels through which the moratorium may impact home values in regions with high wildfire risk in the short and relatively longer term. In the short term, if insurance remains available and relatively affordable due to government mandates, homeowners and buyers may not immediately feel the financial burden of living in a high-risk area. This can sustain or even boost demand for homes in these regions, keeping prices higher than they would be if insurance providers were allowed to leave. The security provided by the moratorium can also create a sense of stability, leading potential buyers to believe that living in the area remains financially viable. This perception can prevent rapid declines in property values that might otherwise occur if insurance became unavailable or prohibitively expensive. In the relatively longer term, if insurers are forced to stay but cannot charge risk-based premiums, they may raise rates across the board (to the

[1] Link - https://www.reuters.com/graphics/CALIFORNIA-WILDFIRES/gdpzyjxmovw/

[2] Link - https://fortune.com/2023/10/31/four-insurers-leave-california-wildfire-risk-allstate-state-farm/

[3] Link for CDI orders on non-renewals - https://www.insurance.ca.gov/01-consumers/140-catastrophes/MandatoryOneYearMoratoriumNonRenewals.cfm

extent that regulations permit), spreading costs to policyholders in lower-risk areas. This could make housing in safer regions relatively less attractive, narrowing the price gap between high-risk and low-risk areas.

In terms of relative differences between areas protected by the moratorium relative to those that are not, the impact of fire risk on home values also depends substantially on how demand interacts with risk perception. Generally, higher fire risk leads to lower valuations. However, in high-risk areas, a moratorium on fire insurance non-renewals may boost demand and prices by providing a sense of security, though the overall effect depends on risk heterogeneity and policy dynamics. In areas where insurers can exit freely, housing prices may also drop more quickly as buyers immediately factor in the increased financial risk of owning an uninsurable property, making the risk more immediately reflected in higher property values in areas protected by the moratorium. As fires continue to occur and sometimes recur in broadly the same locations, there may be significant preemption in how fire-risk and moratorium-security is perceived as well. In very high risk regions that unfortunately observe more fire events and also receive sustained protection from the insurance moratorium, the fear of the fires destroying houses may trump the sense of safety from the moratorium, ultimately reducing home values. In less risky regions however, the channels may be reversed. While there are several challenges in analyzing which forces play larger roles in driving home values, given the possibility of policy endogeneity and the occurrence of the fires themselves coinciding with the adoption of the moratorium, the current paper attempts to causally analyze how the moratorium affects home values over time and across regions with variations in wildfire-risk.

The paper uses a seasonally adjusted home-value index as the primary outcome variable of interest. A dummy variable identifying the presence or absence of the moratorium at the zip-code and month level is the treatment variable of interest. Notably, moratoria orders varied largely both in terms of length and continuity. These orders followed a staggered roll-out design, but did not end uniformly across states. In many cases, based on fire events, the moratoria were instated, revoked and re-instated again through 2020, 2021, 2022, 2023 and 2024. This set-up makes for an interesting and challenging econometric analysis where treatments switch off and on multiple times. The moratorium data are collected from the California Department of Insurance's official website that maintains a history of bulletins on fire insurance moratoria, as well as a list of fires they were

corresponding to.

The baseline specification employed to study this sort of design would usually be a two-way fixed effect model or a generalized difference-in-difference model. The specification would control for zip-code and month fixed effects on an ordinary least square model. However, based on the specific nature of the adoption and withdrawal of the moratorium, the OLS model with TWFE involves some major limitations as described under Methodology. Consequently, the results will focus on those produced by the model(s) more suited to the moratorium at hand, as discussed in the following sections. In order to control for the differences in fire risks across zip-codes, the full sample of zip-codes is divided into three groups based on their fire risk. Fire risk valuations are obtained from the National Risk Indices that Federal Emergency Management Agency (FEMA) publishes for various natural disasters. Within each risk group, treatment status is assigned to zip-codes that had a moratorium in place and control status was assigned to those that did not have a moratorium in place.

The moratorium orders enacted by the Department of Insurance on non-renewals of fire-insurance span a year each from the date of the enactment. However, within the one-year period, if there are one or more fires that endanger any of the regions already under the moratorium, the Department of Insurance often re-enacts a new moratorium order spanning a year from the new date, effectively extending the original moratorium in the affected or endangered zip-codes. Hence, there's significant variation in the lengths of the moratoria, based on whether or not they were reenacted. Given that the re-enactment sometimes occurred only for a subset of the original zip codes protected, often the moratoria orders were adopted in a staggered nature with some zip-codes ordered to follow the moratoria before others. Additionally, after the one-year period, some zip-codes also withdrew the moratoria before others, if deemed safe to do so, with the moratorium being adopted again, in a few cases, after being withdrawn in case of a new fire.

In order to ensure that the effect of the moratorium is identified in isolation from the effects of the fire, it is important to note that the fire coincides with the moratorium on first month of the year-long moratorium adoption alone. It is hence important to study the effect on home values of the moratorium being present not just contemporaneously, but also in quarters ahead when the effect of the fire itself is potentially subdued. The estimator used here, designed by de Chaisemartin & D'Haultfoeuille (2020b) carefully weighs different $DIDs$ every time a switch occurs in the treatment

status and estimates inter-temporal effects of the treatment on the outcome, hence enabling an event-study like analysis even with staggered adoptions of the moratorium. Additionally, out of caution, the CDI mandated the moratorium not just in the zip-codes directly affected by the fires, but also surrounding zip-codes where they assessed a high risk for fires and/or insurance non-renewals in the future. This implies that while there would be some fire-damage based variations in the outcome of locations within a zip-code that suffered the fire, the aggregation of zip-codes allows de Chaisemartin & D'Haultfoeuille (2020b)'s estimator to potentially segregate the effect of the moratorium from the fires. Incidentally, as discussed in the Data section, a larger share of the moratorium covered regions that were classified as low-to-moderate risk by FEMA than those classified as higher risk, enabling the model to study the broader effects of the moratroium across a variety of locations.

Several recent papers have proposed alternative estimators that discuss heterogeneity in treatment timings in settings with staggered treatment timing.[4] Among others, de Chaisemartin and D'Haultfoeuille (2020a) and Imai and Kim (2021) consider settings where units are treated at different times. [5] Seeing as many economists have recently been discussing the challenges in interpreting the estimated coefficients from TWFE models when treatment effects are heterogeneous over time and/or across groups, [6] it is imperative for this kind of study to choose an estimator apt for the specific treatment-timing at hand.

With the treatments in the current paper exhibiting considerable heterogeneity in their starting, progression and ending points, the paper attempts to primarily focus on the set of results that compliment the baseline TWFE regressions by applying an estimator more suited to multiple treatment groups: This estimator is formulated by de Chaisemartin & D'Haultfoeuille (2020b) and is denoted by DID_L.[7] This estimator simultaneously takes into account the heterogeneity in treatment timings, the duration of treatment as well as the lagged dynamic effects of the treatment

[4]Roth et al. (2023) and de Chaisemartin and D'Haultfoeuille (2022b) provide an overview of some of the key developments in the literature, in settings where there are more than two different treatment groups.

[5]Their estimators compare changes in outcomes for units whose treatment status changed to other units whose treatment status remained constant over the same periods. This approach yields a version of causal effect under generalizations of the parallel trends assumption and an additional assumption that requires that the potential outcomes depend only on current treatment status and not on the full treatment history. de Chaisemartin and D'Haultfoeuille (2020a) also propose an estimator that can be applied when treatment turns on and off.

[6]Comparing newly treated units relative to already treated units produces less robust average treatment effects owing to the presence of negative weights in the weighted sum of treatment effects in each treatment group.

[7]In terms of ensuring causality, DID_L relies on a variant of the standard common trends assumption that is tested by a new 'placebo effect' proposed by de Chaisemartin & D'Haultfoeuille (2020a).

to isolate the effect of the moratorium from other confounding factors such as the fires that generated the moratoria themselves. Simply put, the idea is for the estimator is to estimate the effect of having switched treatment (in this case entering or exiting moratorium status) for the first time some periods ago, and for the estimator to be unbiased under dynamic effects.

The literature on the impact of wildfires on housing markets highlights the complex relationship between disaster risk perception and property values. McCoy and Walsh (2018) analyze severe wildfires in Colorado's Front Range, finding that while natural disasters significantly increase risk perceptions, these effects are short-lived. Mueller and Loomis (2014) expand on this by examining repeated wildfire exposure in Southern California, demonstrating that the impact varies across different property values, with lower-priced homes experiencing significantly greater price declines. Similarly, Mueller et al. (2018) identify a negative and statistically significant effect of wildfire and post-wildfire flooding on housing prices in Arizona, with treatment effects differing based on proximity to the wildfire perimeter and flood zones. In an Australian context, Koo and Liang (2022) observe that heightened wildfire risk awareness following major events results in declining home values, with the effect persisting for two years. These studies collectively suggest that wildfire risk influences housing markets in both immediate and long-term ways, with variations depending on geography, price distribution, and risk proximity. Studies on the effect of a mandated moratorium on home values are relatively rarer, and hence the current paper contributes to this body of work by complimenting research on how catastrophes impact the housing market by exploring how a policy intended to help with a catastrophe may impact home values.

The fire insurance moratorium is found to impact home values significantly across different risk groups. In relatively high-risk areas, it leads to a steady increase in the Zillow Home Value Index (ZHVI) within months, while very high-risk regions experience a decline. The full sample follows a similar upward trend, likely influenced by lower-risk groups. In relatively high-risk areas, ZHVI increases by about 7% within a month and reaches 15.6% after six months, while very high-risk regions see a decline of around 2.6% after two months. The full sample follows an upward trend, with home values rising about 4% initially and 5.8% by the third month. By 12 months, moderate-risk areas see increases up to 18%, while high-risk regions experience the most substantial surge, reaching 29.5%. Although some further trends are observed across time, those estimates should be interpreted with caution due to potential violations of the estimator's complex version of a parallel

trends assumptions. The conclusion still holds that overall, the moratorium has a pronounced and varied impact over time.

The insurance market is highly regulated in terms of premium increases in general, although the regulations have gone through some changes and are potentially about to undergo further changes following the January 2025 wildfires in the Pacific Palisades and Alta Dena.[8] Hence, given the existing regulations, potential policy prescriptions might involve gradual risk-based pricing, incentives for mitigation, responsible land-use planning, and safety nets for vulnerable homeowners. Further details on some of these alternatives to the moratorium option are discussed in the Conclusion section. However, a careful cost-benefit analysis for each of the options (including the current system of insurance non-renewal moratorium) is necessary to ensure that the solutions to handle the consequences of these catastrophic wildfires are economically efficient, viable and involve minimal unintentional spillovers. This paper contributes towards that effort by analyzing the impact of the moratorium on the housing market.

The literature on natural-disaster homeowners insurance has grown despite limited access to microdata outside of flood insurance. Foundational studies document household decision-making and insurance availability in catastrophe-prone markets (Kunreuther 1996, 2001; Kousky 2011, 2022; Born and Klimaszewski-Blettner 2013; Wagner 2022), while recent work highlights reinsurance pass-through (Keys and Mulder 2024), the pricing of disaster risks (Blonz et al. 2024), and consequences for mortgage performance and under-insurance (Ge et al. 2025; Sastry et al. 2024). Closely related, Boomhower et al. (2023) examine wildfire risk estimation in California using insurer data. This paper shifts the focus from insurer-side decisions to housing market outcomes, exploiting nonrenewal moratoria as a policy-driven shock to insurability and examining capitalization into home values.

Research on insurance regulation provides a complementary foundation. Models of adverse selection under price controls (Einav et al. 2010; Spinnewijn 2017; Cabral and Cullen 2019) and

[8]California's Proposition 103 (Prop 103) requires prior approval from the Insurance Commissioner for insurance rate increases, including insurances related to dwelling fire. The law was passed in 1988 to protect consumers from excessive rate increases. The Proposition includes caveats about insurance increases requiring public hearings for rate increases above certain thresholds, allowing consumers to file objections at these hearings, and requiring insurers to demonstrate that risk factors are statistically correlated with rates.
In January 2025, the CDI issues new regulations that aims to stabilize the state's insurance market. The regulation requires insurance companies to use the same model for both assessing risks and calculating reinsurance costs. The regulation also requires insurers to write or maintain a certain number of policies in high-risk areas.

studies of residual markets (Kousky 2011) illustrate how regulation shapes market efficiency and supply. Prior work finds that California's regulatory interventions have important distributional and efficiency consequences (Born and Viscusi 2006; Born and Klimaszewski-Blettner 2013; Oh et al. 2023; Liao et al. 2022). By analyzing moratoria that simultaneously constrain pricing and risk selection, this paper extends that literature by showing how such regulations influence local property markets rather than insurer participation.

This paper also contributes to the broader climate adaptation literature, which studies how firms and households respond to evolving environmental risks (Barreca et al. 2016; Baylis and Boomhower 2019; Botzen et al. 2019; Kahn 2021). Within this strand, recent research links climate risk, insurance markets, and real estate values (Nyce et al. 2015; Biswas et al. 2023; Issler et al. 2024). You et al. (2024), for example, show that California's moratoria signaled risk to buyers and reduced loan applications. This paper complements these findings by showing that, while credit demand may have slowed, home values in high-risk areas increased following moratorium enactment, with only the most extreme-risk locations experiencing muted declines.

2 Data

2.1 Fire Insurance Moratorium

According to the Reuters, 7 of the 10 largest wildfires in California happened after 2017. Unexpectedly, the last few years has also seen withdrawal of multiple insurance companies from their California markets owing to the heavy losses from fire damage incurred by buyers of home-insurance. Consequently, the Insurance Commissioner at the Department of Insurance authored a bill to protect buyers of residential insurance. This important consumer protection law requires a mandatory one-year moratorium on insurance companies cancelling or non-renewing residential insurance policies in certain areas within or adjacent to a fire perimeter after a declared state of emergency is issued by the Governor. Following a Governor declaration of a state of emergency, the Department of Insurance partners with CAL-FIRE and the Governor's Office of Emergency Services to identify wildfire perimeters and adjacent ZIP codes within the mandatory moratorium areas. The protection from cancellation or non-renewal lasts for one year from the date of the Governor's emergency

declaration. Each year starting 2019, multiple emergency declarations precede one-year moratoria protections that apply to all residential policyholders within the affected areas who suffer less than a total loss, including those who suffer no loss. Figure 4 shows the cumulative months of moratorium that different zip-codes in California were subject to. These moratorium orders were instated for a year each, starting the month of a fire-event, and in case of a reoccurrence of another fire in the same or surrounding regions, they were reinstated in additions of a year each, effectively extending the original coverage. The data in the current paper is a zip-code level panel that runs from January 2017 to June 2024, with the first ever moratorium starting on October 2019, and the latest one continuing through 2024.

2.2 Wildfire Risk Index

In the National Risk Index, risk is defined as the potential for negative impacts as a result of a natural hazard. The risk equation behind the National Risk Index includes three components: a natural hazards risk component, a consequence enhancing component, and a consequence reduction component. EAL is the natural hazards risk component, measuring the expected loss of building value, population, and/or agriculture value each year due to natural hazards. Social Vulnerability is the consequence enhancing component and analyzes demographic characteristics to measure the susceptibility of social groups to the adverse impacts of natural hazards. Community Resilience is the consequence reduction component and uses demographic characteristics to measure a community's ability to prepare for, adapt to, withstand, and recover from the effects of natural hazards. The Social Vulnerability and Community Resilience components are combined into one Community Risk Factor (CRF) which is multiplied by the EAL component to calculate risk using the following equation (see Figure 1).

$$Risk = Expected\ Annual\ Loss\ \times\ Community\ Risk\ Factor$$

$$where\ Community\ Risk\ Factor = f\left(\frac{Social\ Vulnerability}{Community\ Resilience}\right)$$

The function, $f(.)$ is a transformation that maps the ratio between Social Vulnerabilty and Community Resilience to CRF values. This mapping is constructed so that higher social vulnerability and lower community resilience, relative to all other communities at the same level (county or

Census tract), result in higher Risk Index values for a given level of EAL. For more details regarding Social Vulnerability, Community Resilience, Community Risk Factor, and Expected Annual Loss, please refer to the *FEMA National Risk Score Technical Documentation*

The National Risk Index provides three different types of results for Risk and each component used to derive Risk: EAL, Social Vulnerability, and Community Resilience:

- Values - Values for Risk and EAL are in units of dollars, representing the community's average economic loss from natural hazards each year. For Social Vulnerability and Community Resilience, values are the index values for the community provided by the source data sets.

- Scores - Scores represent the national percentile ranking of the community's component value compared to all other communities at the same level (county or Census tract).

- Ratings - Ratings are provided in one of five qualitative categories describing the community's component value in comparison to all other communities at the same level. Rating categories range from "Very Low" to "Very High."

In the risk equation, each component is represented by a score that represents a community's national percentile ranking relative to all other communities at the same level (county or Census tract). The composite Risk Index score is calculated to measure a community's risk to 18 hazard types, including Wildfire Hazard.

All calculations are performed separately at two levels—county and Census tract—so scores are relative only within their level.[9] It is important to note that all scores are constrained to a range of 0 (lowest possible value) to 100 (highest possible value). For every score, there is also a qualitative rating category that describes the nature of a community's component value in comparison to all other communities at the same level, ranging from "Very Low" to "Very High.".[10]The rating is intended to classify a community for a specific component in relation to all other communities at the same level.

[9]Scores are relative, representing a community's relative ranking among all other communities for a given component and level. Scores are national percentiles, not absolute measurements, and should be expected to change over time either by their own changing measurements or changes in other communities.

[10]Rating categories are relative and for risk and EAL, there are no specific numeric values that determine the rating. For example, a community's Risk Index score for one hazard could be 8.9 with a rating of "Relatively Low," while its Risk Index score for another hazard may be 11.3 with a rating of "Very Low."

In FEMA's National Risk Index, a Wildfire Risk Index score and rating represent a community's relative risk for Wildfires when compared to the rest of the United States. A Wildfire Expected Annual Loss score and rating represent a community's relative level of expected building and population loss each year due to Wildfires when compared to the rest of the United States. The U.S. Forest Service Missoula Fire Sciences Laboratory generated a series of raster datasets representing burn probability (BP) and conditional fire intensity level (FIL), also referred to as flame length) for the conterminous U.S., Alaska, and Hawaii through its geospatial Fire Simulation (FSim) system.[11]The cell value in the raster file contains the mean annual BP as a value between 0 and 1 and represents the tendency for the cell area to burn due to a large fire on an annual basis given its landscape, contemporary weather conditions, and probability of containment (see Figure 2). The FIL dataset consists of six raster files, each representing the portion of all simulated fires that burned in the cell area at the specified flame length. These files are also at a 270-meter grid spatial resolution.

In the current analysis, the census-tract to zip-code crosswalk file is used to assign the wildfire risk scores to zip-codes within California. The following risk groups are assigned to different risk scores:

- Risk group 0: Risk scores ¡85 with a risk rating of less than "Relatively Moderate"
- Risk group 1: Risk scores 85-95 with a risk rating of "Relatively Moderate"
- Risk group 2: Risk scores 95-99 with a risk rating of "Relatively High"
- Risk group 3: Risk scores ¿99 with a risk rating of "Very High"

For zip-codes subject to the moratorium that have missing risk scores, risk scores are assumed to be the median risk scores available in the data within the range of 85-100. Figure 3 shows a map of variation in Wildfire Risk Groups within California zip-codes present in the final dataset used for the empirical analysis.

[11]FSim is designed to simulate the occurrence and growth of wildfires under tens of thousands of hypothetical contemporary fire seasons in order to estimate the probability oigure a given area (i.e., pixel) burning under current (circa 2014) landscape conditions and fire management practices. The BP raster dataset models the probability of an area being burned by a large fire (i.e., a fire that escapes initial fire suppression and spreads) at a 270-meter grid spatial resolution.

2.3 Zillow Home Value Index

The Zillow Home Value Index, henceforth abbreviated as ZHVI is based on "Zestimate®". The Zestimate is a is Zillow's home valuation model that is used to estimate a home's market value. A Zestimate incorporates public, MLS and user-submitted data into Zillow's proprietary formula, also taking into account home facts, location and market trends. It is not an appraisal and can't be used in place of an appraisal. This estinate is regularly calculated for more than 100 million individual homes nationwide and is generated through an ensemble of machine learning models and incorporates data from a variety of sources including public data, user-generated data and real estate data from direct feeds or multiple listing services, accounting for home attributes and geographic location.

This estimate is then used to evaluate the ZHVI, which is calculated for approximately 90,000 regions in the United States at various levels, going down to the zip-code level (used in the current paper). The methodology to evaluate ZHVIs could be applied to any consistent set of homes, such as those in a securitized mortgage pool, a residential REIT portfolio or a school district's property tax base. Given that the ZHVI attempts to capture appreciation of a market as it existed at a given time, some homes are removed from and added to the property universe in order to account for the evolving and expanding housing stock in an area.

One of the core functions of the ZHVI is to calculate total housing stock appreciation in a market. Total market appreciation is calculated as a weighted average of each home's appreciation in the property universe. The individual home appreciations are calculated using $z_{i,t-1}$ and $z_{i,t}$, the home's Zestimate at time $t-1$ and t, respectively.

Each individual home's appreciation is then multiplied by a weight, $w_{i,t-1}$. This weight is the home's Zestimate divided by the sum of all Zestimates in a region, representing that home's share of the total market value. Homes that are more valuable both contribute more to overall appreciation and represent a larger market share. For example, a home worth $\$100,000$ that appreciates 10% month-over-month adds \$10,000 in value, while a home worth \$1 million that appreciates 10% adds \$100,000 in value. And because the more expensive home adds more dollar value, it represents a larger portion of the total market appreciation. The total market appreciation, A_t, is the sum of the weighted appreciation of each home.

$$a_{1,t} = \frac{z_{-,t} - z_{i,t-1}}{z_{i,t-1}}$$

$$w_{i,t-1} = \frac{z_{i,t-1}}{\Sigma_{j=1}^{n} z_{j,t-1}}$$

$$A_t = \Sigma_{i=1}^{n} \; w_{i,t-1} \, a_{1,t}$$

The ZHVI captures appreciation solely attributable to market changes, and not from properties adding additional space or improvements — for example, when a homeowner adds an additional bedroom or other addition to their property. To account for these kinds of changes to the housing stock, those properties that do undergo significant physical changes have their appreciation set to the median appreciation for that region and property universe.

Notably, the ZHVI measures the value of the typical home in every period. To this end, the present-day index level of the ZHVI is set to the mean Zestimate of the property universe at time t. In order to translate the current home value level to the previous time period, the total appreciation is chained backward as follows:

$$I_{t-1} = \frac{I_t}{1 + A_t}$$

The index is also seasonally adjusted by applying a LOESS-based (Locally Estimated Scatterplot Smoothing) seasonal decomposition to the appreciations before performing the chaining. In addition, the index levels are smoothed using a 3-month moving average. The ZHVI isavailable for three market tiers — a bottom, middle and top, roughly corresponding to one-third of the market apiece. The middle-tier ZHVI encompasses all homes falling between the 35th and 65th percentile of home values. This mid-market range is closest to the standard definition of a "typical" home in an area, and the middle-tier ZHVI is the outcome variable of interest in the current paper.

Table 1 provides a summary of how the ZHVI and moratorium varies across risk groups. The mean of the fire insurance moratorium values across risk groups averaging around 0.1 implies that around a tenth of the zip-codes under consideration had observed the moratorium at some point. Interestingly, regions in risk group 0 (¡Relatively Moderate risk) and in risk group 2 (Relatively

High risk) have a higher incidence of moratorium, followed by risk group 3 and risk group 1 among the full sample of locations considered. This implies that a relatively larger share of zip-codes under low-to-moderate risk regions were protected by the moratorium, on an average, compared to the share of zip-codes under other risk-groups.

3 Methodology

3.1 Baseline Two-way Fixed Effects

For the home value analysis, owing to the limitations of this model (as described in the next section), this section solely serves to provide the structure of a baseline TWFE model to set up the context for a relatively elaborate version of the model that will follow. Consequently, the results from the effect of moratorium on ZHVI corresponding to this model appear in the appendix, but not in the main text.

Exploiting the assumption that any unmeasured determinants of the outcomes are either time invariant or group invariant, the baseline regressions in the paper follow a generalized difference-in-difference (generalized DID) or a two-way fixed effects (TWFE) model. The model then estimates the average treatment effect on treated (ATT) or the expected effect of the treatment for individuals in the treatment group. To estimate the impact of fire-insurance non-renewal moratoria on home values, the TWFE model regresses the outcome on zip-code level moratoria indicators and a full set of zip-code and month fixed effects. The home value index is log-transformed which enables the treatment variables to interpret the specific effect of the policy in terms of percentage changes in home values.

The baseline regressions run are hence specified as:

$$ln(ZHVI)_{zm} = \alpha + \gamma_z + \gamma_m + \delta^{\hat{DD}} D_{zm} + \epsilon_{zm} \tag{1}$$

where

$ZHVI$ is the zip-code level Zillow Home Value Index in zip-code z and month m,

γ_z & γ_m are the zip-code and month fixed effects respectively,

D_{zm} is the treatment taking the value of 1 if the moratorium is in place in zip-code z and month

m, and 0 if there is no moratorium.

Bertrand et al. (2004) show that the standard errors for generalized DID estimates are inconsistent if they do not account for the serial correlation of the outcome of interest. Because the outcomes studied usually vary at the group and time levels, it thus makes sense to correct for serial correlation. The authors show that using cluster–robust standard errors at the group level where treatment occurs provides correct coverage in the presence of serial correlation when the number of groups is not too small. Since the treatments are at county and state levels in our datasets, the groups are definitely not too small. The baseline results presented in the paper use the cluster-robust standard errors discussed above.

It is also important to note that while the baseline TWFE model and the consequent robust version, DID_L estimator takes= into account all housing market variables constant across zip-codes and across months, due to lack of availability of data at levels as granular and frequent as zip-code and month, it is challenging to control for further factors that may change across months and smaller geographies than zip-codes.

The expected sign of the average treatment effect over time may range from being positive to negative based on how the demand for housing interacts with the perception of fire risk. There could be multiple factors driving demand for houses in regions subject to different degrees of fire risk. In general, one could expect residential demand (and hence valuation) to be inversely correlated to the fire risk scores in a region. So everything else held constant, locations that suffer from higher risk of fire would observe lower home values relative to locations that have lower fire risk. However among places that are susceptible to significant fire damage, the presence of moratorium on non-renewals of fire insurance might provide a sense of security to potential homeowners and subsequently, raise the demand (and hence the valuation) for housing in locations where the moratorium exists relative to those where it does not. To answer which effect dominates the other requires controlling for the heterogeneity in risk levels, followed by exploring dynamic effects of the introduction of the moratorium at each risk level.

A significant challenge in interpreting the results is isolating the effect of the fire itself from the effect of the moratorium. Indeed, towards the first few months of observation, the effects of the fire that induced the moratorium in the first place might confound the demand-based effects arising

from the moratorium. However, given that each moratorium order was enacted to be in effect for a full year after the fire, the models employed to estimate the causal effects of the moratorium alone need to carefully account for the changes in treatment status, duration of the treatment and longer-term effects of the moratorium. As such, it is important to choose a suitable model that that can successfully scrape out the effect of the moratorium alone in a dynamic setting. Hence the next couple of sections discuss in detail the choice of estimator and its capabilities. To provide an overview, this estimator is a weighted average, across (say t) time periods, of DIDs comparing the $t-l-1$ to t outcome evolution across the following groups: those that are treated at the start of the panel and whose treatment changed for the first time in $t-l$, (also called the first-time switchers); and those groups with the same treated status, from period 1 to t (called the not-yet switchers. DID_L estimates the effect of having switched treatment for the first time l periods ago. As an overview, the idea is for the estimator to estimate the effect of having switched treatment for the first time some (say l periods ago, and for the estimator to be unbiased under dynamic effects.

Based on discussions by Roth et al. (2023) and de Chaisemartin and D'Haultfoeuille (2022c), even though traditionally two-way fixed effects or difference-in-difference setups work with two groups (control and treatment) and two periods (pre-treatment and post-treatment), a growing literature in econometrics has been discussing procedures that extend to staggered DID setups and setups where treatments are non- binary and reversible.[12] With the intention of using two-way fixed effects in situations that involve relatively more complicated treatment timings, this literature delves deep into the sort of assumptions needed to make use of unconventional treatment and control groups.

Intuitively, given parallel trends assumptions and no anticipation assumptions, in a staggered setting, the average treatment effect can be identified by comparing the expected change in outcome for treatment groups between a certain period to that for a control group not-yet treated in that period.[13] There are a host of econometric models (used by Borusyak et al. (2021), Gardner (2021),

[12]For instance, Callaway and Sant'Anna (2021) extend the regression adjustment, inverse propensity-score weighting (Abadie (2005)) and doubly-robust estimators (Sant'Anna and Zhao (2020)) to estimate their version of average treatment effect on the treated. Wooldridge (2021) proposes an alternative regression adjustment procedure that is suitable for staggered setups, which, in the case of violations in some assumptions on pre-treatment periods, may lead to the estimator being more biased than Callaway and Sant'Anna (2021)'s. de Chaisemartin and D'Haultfoeuille (2020a, 2022a) and Borusyak et al. (2021) consider estimators which include covariates in a linear fashion.

[13]Callaway and Sant'Anna (2021) consider two options of comparison: one that uses only never-treated units and one that uses all not-yet-treated units. While the former is a more conventional DID setting, when there are a relatively small number of periods and treatment cohorts, the latter may be more reasonable.

Liu et al. (2022), and Wooldridge (2021)) that fit a TWFE regression using observations only for units and time periods that are not-yet-treated. They then infer the never-treated potential outcome for each treated unit using the predicted value from this regression. Sun and Abraham (2021) propose an estimator that uses either the never-treated units(if they exist)or the last-to-be-treated units as the comparison. Cengiz et al. (2019) run a stacked regression where each treated unit is matched to not-yet-treated controls and there are separate fixed effects for each set of treated units and its control. Several other recent papers propose similar estimation strategies with some subtle differences in how they construct the control group and the weights they place on different cohorts or time periods.

The many estimators available differ in who they use as the comparison group (not-yet-treated versus never-treated) as well as the pre-treatment time periods used in the comparisons. This leads to some trade-offs between efficiency and the strength of the parallel trends assumption needed for identification. The best estimator to use hence depends on the context, specifically, on which groups could be used for most sensible comparisons, and how confident the researcher is in parallel trends for all periods.[14]

Given all the different estimators developed, in this paper, with the staggered treatment adoption, non-uniform withdrawal and switching on and off of treatments, the body of estimators developed by de Chaisemartin and D'Haultfoeuille (2020b) appears to be the optimal choice. This base version of this estimator uses a weighted measure of DIDs amongst units whose treatment status changes between consecutive periods. The estimator used here for the final results is an intertemporal version of the same baseline estimator. The authors have engaged in a very detailed analysis of how and under what conditions, among other comparisons, their estimator uses not-yet treated units (as controls) to treated units to achieve an efficient DID; and further explore the application of their estimator in an event-study analog of their base model. The following discussion explores some of the challenges usually associated with this body of estimators and how de Chaisemartin and D'Haultfoeuille work around these concerns. Using their estimator, the ability to view not-yet treated units as controls and applying an event-study version of the TWFE model allows successful estimation of DIDs even in the absence of a large (never-treated) control group.

[14] According to Roth et al. (2023), the various heterogeneity-robust DID estimators typically produces similar answers, for practical intents and purposes.

3.2 Negative Weights and Base Model

This section sets up de Chaisemartin and D'Haultfoeuille's base model. While the set-up is necessary to explain the event-study version of this model, in order to simplify the understanding of the estimator, only the essential components are retained in this section.

Assuming that the full set of zip-codes under consideration has been divided into groups that receive treatment or do not receive treatment during a certain time duration.[15]For instance, all zip-codes that adopt the moratorium in September 2020 and withdraw the moratorium in September of 2021 are in one group, while those that adopt moratorium in November of 2020 and withdraw in November of 2021 are in a different group. The de Chaisemartin and D'Haultfoeuille set-up involves a total of say $\bar{g}+1$ groups and $\bar{m}+1$ periods denoted by g and m respectively such that for every $(g,m) \in \{0,...,\bar{g}\} \times \{0,...,\bar{m}\}$, $N_{g,m}$ denotes the number of observations in group g at period m, and $N = \sum_{g,m} N_{g,m}$ is the total number of observations.

Then, for every $(c,g,m) \in \{1,...,N_{g,m}\} \times \{0,...,\bar{g}\} \times \{0,...,\bar{m}\}$, let $D_{c,g,m}$ and $(Y_{c,g,m}(0), Y_{c,g,m}(1))$ respectively denote the treatment status and the potential outcomes without and with treatment of observation c in group g at period m. The outcome of observation c in group g and period m is $Y_{c,g,m} = Y_{c,g,m}(D_{c,g,m})$. Then, for all (g,m), let

$$Y_{g,m}(0) = \frac{1}{N_{g,m}} \sum_{c=1}^{N_{g,m}} Y_{c,g,m}(0)\,, \;\; Y_{g,m}(1) = \frac{1}{N_{g,m}} \sum_{c=1}^{N_{g,m}} Y_{c,g,m}(1)$$

Consequently, for any $(g,m) \in \{0,...,\bar{g}\} \times \{0,...,\bar{m}\}$, the average treatment effect in cell (g,m) is

$$\Delta_{g,m} = \mathbb{E}\left(\frac{1}{N_{g,m}} \sum_{c=1}^{N_{g,m}} \left(Y_{c,g,m}(1) - Y_{c,g,m}(0)\right) \right). \tag{2}$$

Let $\upsilon_{g,m}$ denote the residual of observations in cell (g,m) in the regression of $D_{g,m}$ on group and period fixed effects:

$$D_{g,m} = \beta + \lambda_g + \Lambda_m + \upsilon_{g,m}.$$

Assuming non-colinearity in the regressors of (7), the average value of $\upsilon_{g,m}$ across all treated (g,m)

[15]All of the zip-codes within each group have the same treatment status, at any given point of time.

cells differs from 0. Let weight $w_{g,m}$ be $\upsilon_{g,m}$ divided by that average:

$$w_{g,m} = \frac{\upsilon_{g,m}}{\sum\limits_{(g,m):D_{g,m}=1} \frac{N_{g,m}}{N_1} \upsilon_{g,m}} \tag{3}$$

where $N_1 = \sum\limits_{c,g,m} D_{c,g,m}$ is the number of treated observations.

Using (2) and (3), if $\delta^{\hat{DD}}$ denotes the coefficient of $D_{c,m}$ (following (7)) and δ^{DD} denote its expectation, under the common trends assumption, de Chaisemartin and D'Haultfœuille (2020a) show that δ^{DD} is equal to a weighted sum of the treatment effect in each treated (g,m) cell (in a binary treatment case):

$$\delta^{DD} = \mathbb{E}\left(\sum_{(g,m):D_{g,m}=1} \frac{N_{g,m}}{N_1} w_{g,m} \Delta_{g,m} \right) \tag{4}$$

Notably, the weights $w_{g,m}$ sum to one but may be negative.

δ^{DD} compares the evolution of the outcome between consecutive time periods across pairs of groups that ideally compare treated units to only control units. However, what is considered the control group in some of these comparisons may be treated at both periods. Hence, while comparing newly treated units relative to "never treated" units and newly treated units relative to "not-yet treated" units involves no issues, comparing newly treated units relative to already treated units makes a causal interpretation unclear. The negative weights are especially an issue when the ATEs are heterogeneous across groups or periods.[16]

To overcome the problem with negative weights, de Chaisemartin and D'Haultfœuille (2020a) propose a new estimator, DID_M, that estimates the average treatment effect across all the (g,m) cells whose treatment changes from $m-1$ to m. This new estimator is a weighted average of two difference-in-differences, one for units that switch in to the treatment, and one for units that switch out of treatment.[17] The weighting of the two DIDs makes the resulting estimator valid, even if the treatment effect is heterogeneous over time or across groups.[18]

[16]This problem has been discussed in further detail by Borusyak and Jaravel (2018), Goodman-Bacon (2021), de Chaisemartin and D'Haultfoeuille (2020a), Sun and Abraham (2021), and Callaway and Sant'Anna (2021).

[17]For the units switching in, the first DID compares the outcome evolution of groups going from untreated to treated, and of groups untreated at both dates across two consecutive time periods. Using the units that switch out, the second DID compares outcome evolution of groups treated at both dates, and of groups going from treated to untreated across consecutive time periods.

[18]The causal effect of the DID_M estimator is denoted by:

3.3 Version of Estimator with intertemporal effects

While the preceding section discusses the usual problems associated with designs that involve heterogeneity in treatment timing, this section explores a variation of the static estimator described above. This new estimator DID_L was proposed and studied by de Chaisemartin and D'Haultfoeuille (2020b) as a heterogeneity-robust DID estimator, that can be used in applications where the treatment is either non-binary and/or non-staggered, and where the lagged treatments may affect the outcome. The presence of potential lagged effects of the moratorium lends itself perfectly to the use of DID_L in the current paper. This section will explore a variation of the setting described in the preceding section to briefly describe the DID_L estimator that is used to elicit the results presented in the following section.

Let $Y_{g,t}(d_1, ..., d_m)$ denote the potential outcome of g at m, if its treatments from period 1 to m are equal to $(d_1, ..., d_t)$. Let F_g denote the first period at which group g's treatment changes, and

$$\delta_{g,l} = E(Y_{g,F_g-1+l} - Y_{g,F_g-1+l}(D_{g,1}, ..., D_{g,m}))$$

be the expected difference between g's actual outcome at $F_g - 1 + l$ and the counterfactual "status quo" outcome it would have obtained if its treatment had remained equal to its period one value from period one to $F_g - 1 + l$. To estimate $\delta_{g,l}$, the estimator $DID_{g,l}$ is proposed comparing the $F_g - 1$ to $F_g - 1 + l$ outcome evolution between group g, and groups whose treatment has not changed yet at $F_g - 1 + l$, and with the same treatment as g at period one. The $DID_{g,l}$ estimators are then aggregated across groups. From the aggregation the $DID_{g,l}$s are discarded such that at $F_g - 1 + l$, g has experienced both a strictly lower and a strictly larger treatment than its period-one treatment.[19] In some cases, $\delta_{g,l}$ is the effect of having been exposed to a weakly higher treatment

$$\Delta^M = \sum_{(g,m): D_{g,m} \neq D_{c,m-1}, m \geq 1} \frac{N_{g,m}}{N^M} \Delta_{g,m}$$

where $N^M = \sum_{(g,m): m \geq 1, D_{g,m} \neq D_{c,m-1}} N_{g,m}$

Δ^M is the average ATE of all switching cells. In staggered adoption designs, Δ^M is the average of the instantaneous treatment effect at the time when a group starts receiving the treatment, across all groups that become treated at some point. Further discussion is available in Chaisemartin and D'Haultfœuille (2022a).

[19] For such (g, l)s, $\delta_{g,l}$ can be written as a linear combination, with negative weights, of the effects of increasing different treatment lags. Among the remaining (g, l)s, either

$D_g, F_g > D_{g,1}, D_g, F_{g+1} \geq D_{g,1}, ..., D_{g,F_g-1+l} \geq D_{g,1}$ or $D_g, F_g < D_{g,1}, D_{g,F_g+1} \leq D_{g,1}, ..., D_{g,F_g-1+l} \leq D_{g,1}$.

for l periods, while in others, $\delta_{g,l}$ is the effect of having been exposed to a weakly lower treatment for l periods.[20]

When the $DID_{g,l}$ estimators are finally aggregated, de Chaisemartin and D'Haultfoeuille (2020b) multiply the aggregation by minus one the $DID_{g,l}$ for groups such that $D_g, F_g < D_g, 1$. This finally yields a DID_L estimator of the effect of having been exposed to a weakly higher treatment dose for l periods. This estimator allows estimating trajectory-specific versions of DID_L.

In the presence of the following conditions (that hold in the case of the current fire insurance moratorium treatment design):

(i) requires that there exist groups with the same period-one treatment. This is essentially a restriction on the support of the period-one treatment. This condition subsumes parallel trends for the status-quo outcome, conditional on the period-one treatment

(ii) Zero treatment at baseline $\forall g, D_{g,1} = 0$ satisfied by the current binary treatment design (0, 1 for moratorium indicators), where groups can join and then leave treatment.

(iii) No anticipation

The estimator DID_L can also be considered a weighted average of event study effects as follows:

$$DID_L = \frac{1}{N_l} \Sigma_{g:Fg-1+l \leq T_g} S_g DID_{g,l}$$

where

$L = max_g(T_g - F_g + 1)$ denote the largest l such that $\delta_{g,l}$ can be estimated for at least one g. Under assumption (i), $L \geq 1$. For every $l \in \{1, ..., L\}$,

$N_l = \#\{g : F_g - 1 + l \leq T_g\}$

is the number of groups for which $\delta_{g,l}$ can be estimated. For all g such that $F_g \leq T$,

$$S_g = 1\{D_{g,F_g} > D_{g,1}\} - 1\{D_{g,F_g} < D_{g,1}\}$$

is equal to 1 (resp.-1) for groups whose treatment increases (resp. decreases) at F_g.

[20]Thus, $\delta_{g,l}$ does not satisfy the no-sign reversal property. The no-sign reversal property was introduced in a static model where the outcome is only affected by the current treatment (Small and Tan, 2007). When the outcome can be affected by the current and lagged treatments, this property is well-defined only under the assumption that all those treatments affect the outcome in the same direction. The assumption that the current and lagged treatments all affect the outcome in the same direction is very strong. Thus, satisfying the extension of no-sign reversal is an even more minimal requirement than in a static model.

The average of $S_g\delta_{g,l}$ is

$$\delta_l = \frac{1}{N_l}\Sigma_{g:F_g-1+l\leq T_g} S_g\delta_{g,l}$$

For groups with $S_g = 1$, $D_{g,m} \geq D_{g,1}\forall m$, so $\delta_{g,l}$ is the effect of having been exposed to a weakly higher treatment dose for l periods. Conversely, for groups with $S_g = -1$, $D_{g,m} \leq D_{g,1}\forall t$, so $\delta_{g,l}$ is the effect of having been exposed to a weakly lower dose for l periods. Taking the negative of $\delta_{g,l}$ for those groups ensures that δ_l is an average effect of having been exposed to a weakly larger dose for l periods.

Further, to test for parallel trends in an event-study format, de Chaisemartin and D'Haultfoeuille (2020b) propose placebo estimators one can use to test assumptions (i) and (iii) denoted by $DID^{pl}_{g,l}$ that mimicks $DID_{g,l}$.[21]

Like $DID_{g,l}$, it compares the outcome evolution of g to that of groups with the same baseline treatment as g, and that have kept that treatment from period 1 to $F_g - 1 + l$. But unlike $DID_{g,l}$, it compares those groups' outcome evolutions from period $F_g - 1 - l$ to period $F_g - 1$, namely before group g's treatment changes for the first time. Accordingly, $DID^{pl}_{g,l}$ assesses if g and groups not yet treated at $F_g - 1 + l$ experience the same evolution of their status-quo outcome over l periods, the number of periods over which parallel trends has to hold for $DID_{g,l}$ to be unbiased for $\delta_{g,l}$. Whereas $DID_{g,l}$ goes from the past (period $F_g - 1$) to the future (period $F_g - 1 + l$), $DID^{pl}_{g,l}$ goes from the future (period $F_g - 1$) to the past (period $F_g - 1 - l$). This follows the standard practice in event-study regressions, where the reference period is the one before the event. I

DID_L applies to any TWFE regressions, not only to those where adoptions are staggered and treatments turn off at the same time. The estimator, hence, lends itself perfectly to the econometric design of the current paper with a binary treatment that is adopted in a staggered fashion and is withdrawn at different points in time. Following DID_L, this paper presents a secondary set of results for each dataset in order to supplement the baseline results with a set of more rigorous TWFE estimations. DID_L relies on no anticipation and parallel trends assumptions on both

[21] For any $g : 3 \leq F_g \leq T_g$ and $l \in \{1, ..., min(T_g - F_g + 1, F_g - 2)\}$, the placebo estimator is defined as:

$$DID^{pl}_{g,l} = Y_{g,F_g-1-l} - Y_{g,F_g-1} - \frac{1}{N^g_{F_g-1+l}} \sum_{g':D_{g',1}=D_{g,1},F'_g>F_g-1+l} (Y_{g',F_g-1-l} - Y_{g',F_g-1})$$

One can show that under Assumptions (i) and (iii), $E(DID^{pl}_{g,l}|D) = 0$.

potential outcomes, and de Chaisemartin and D'Haultfœuille (2020b) propose a placebo estimator that can be used to test these assumptions. The placebos compare the outcome trends of switchers and non-switchers, before the switchers switch. For the parallel trends assumption to hold, the reported "placebo" estimator should not significantly differ from 0.

4 Results

The results section focus on two of the de Chaisemartin and D'Haultfœuille estimators, one with and one without the inclusion of the intertemporal effects. To provide context, the baseline TWFE results appear in Table A1 of the appendix. With $ln(ZHVI)$ as the outcome variable, the results should be interpreted as how the presence of moratorium affects the Zillow home value indices in terms of percentage changes.

Table 2 shows the contemporaneous effect of the moratorium on home values and while the full sample shows a positive and significant effect of the presence of moratorium on changes in home values, sub-groups based on risk-heterogeneity do not produce any significant results. Based on the full sample, the presence of moratorium is associated with a muted 0.84% increase in the ZHVI at 90% confidence level. However, the placebo effect is also significant at 90% confidence implying that the DID_M estimator's version of the parallel trends assumption is not being met. The results here showing no significant effect of moratorium on home values instantaneously are not unexpected given that home values generally take time to update. The contemporaneous results may also not be completely dependable on account of the results being confounded on account of the first treatment period coinciding with the event of the fire itself.

Table 3 and Figures 5, 6 and 7 show the results from using de Chaisemartin and D'Haultfœuille's DID_L estimator incorporating intertemporal effects. While Table 3 and Figure 5 shows the effects of the moratorium a quarter ahead, figures 6 and 7 show the effects for 6 months and 12 months (a year) in advance. The discussions of the results will focus on the effects that are significantly different from zero with atleast 90% confidence.

The results in table 3 and figure 5 start showing the patterns that emerge in a more pronounced fashion in Figures 6 and 7 as the time frame expands. The only significant results in Table 3 are observed in Risk Group 2 and the full sample. With the dependent variable being a log-transformed

index, each of the coefficients, $Coeff$ presented in the table should be interpreted as a % change from a base of what the ZHVI was 1 period before the moratorium starts. The % change magnitude is calculated as follows:

$$\%\ change\ in\ index = (\ e^{coeff} - 1) * 100$$

Hence, a period after the moratorium starts, for the relatively high risk group (Fig. 5(c)), at 95% confidence, the fire insurance moratorium is found to be associated with an approximately 7.04% increase in ZHVI. Two periods after the treatment, this value increases to 8.7% increase in the Zillow Home Value Index, at the 99% confidence level. One more period ahead, the coefficient stays approximately steady. However, the placebo effect is now significantly different from zero implying that the parallel trends assumption has not been met for this estimate. The same holds true for the coefficients corresponding to the moratorium 1 and 2 periods after the treatment for the Risk Group 3 sub-sample. For the full sample (Fig. 5(a)), probably driven by the results for Risk Groups 2 (and potentially Risk Group 0), a month after the moratorium is enacted, with 95% confidence the ZHVI increases by an average of about 4%. A month further, the moratorium is found to be associated with a home value increase by about 4.7%, which then amplifies to about 5.8% one more month ahead (a quarter from the start). For the very high risk group (Fig. 5(d)), the direction of the moratorium impacting the ZHVI is reversed, with a 2.6% reduction in ZHVI two periods after the adoption of the moratorium (with 90% confidence) The results from the full sample also show that the placebo effect is insignificant all through the quarter implying that the parallel trends assumptions were met for this set of DIDs.

Owing to the large number of lags and leads involved, the results associated with the effect of the moratorium 6 months ahead and a year ahead are presented only in figures 6 and 7.The confidence intervals corresponding to the point estimates being (mostly) above or below 0 implies that the coefficient is significantly different from zero. For the leads or placebos, a point estimate significantly different from zero would imply non-conformation to the parallel trends assumption for that exact period. For the lags, it would imply a significant increase or decrease in ZHVI from its base at event-time -1.

Figure 6 shows that six months after the start of the adoption of the moratorium, while there

are no significant lagged effects of the moratorium early on in the relatively moderate risk group (Fig. 6(b)), there's a significant (with 90% confidence) 7.2% increase in ZHVI after 5 months and a 6.1% increase 6 months ahead. The placebo effect is also significantly different from zero, implying a lack of parallel trends in that period. In the relatively high risk group (Fig. 6(c)), the placebo effect is only significantly different from zero for the 6th lead. The lags are all significant with at least 90% significance. A quarter in, the moratorium increases the ZHVI by about 8.9%, which increases to about 15.6% after 6 months of the adoption. From the very high risk group (Fog 6(d)), the only lag coefficient that is significant with 90% confidence is the 5th lag. The magnitude of the coefficient implies that 5 months after its adoption, the moratorium is associated with a 4.7% reduction in the home values in very high risk regions. Overall, the intertemporal trends in the cumulative sample (Fig. 6(a)) consisting of all risk groups follow similar patterns to the trends in the relatively high risk group. Owing to placebo effects being significantly different from 0 corresponding to the 5th and 6th month following the adoption of the moratorium, the coefficients for the 5th and 6th months may not be dependable. However, in the first 4 months following the adoption, each month the moratorium progressively raises ZHVI by 3.9%, 4.7% and 6.1%.

Figure 7 shows that when considering the effects of the moratorium 12 months after the start of the adoption, there are no significant lagged effects of the moratorium in the very high risk group (Fig. 7(d)). In the relatively moderate risk group (Fig. 7(b)), in the 10th month following the adoption, the ZHVI is higher by 7.2% which increases to about 18% in the 11th month. In the relatively high risk group (Fig. 7(c)), while around the 7th month post adoption, the moratorium is associated with an increase in ZHVI by 16.3%, from the 8th month onward the increase in ZHVI jumps to about 29.5%. The increases post the 10th month should be interpreted with caution owing to significant placebo coefficients corresponding to the 11th, 12th and 13th leads.

To summarize, in the relatively high risk group, the moratorium is found to be associated with increases in home values, which themselves increase in magnitudes as the time-frame expands starting from the 1st month of moratorium adoption. Home values in the very high risk group have a more mellowed reaction to the moratorium, with the direction in which the home values change reversed.

5 Conclusion

With the differential ZHVI trends in places that were subject to the moratorium versus those in similarly risky regions that weren't subject to the moratorium, it can be concluded that the moratorium created significantly different outcomes in home valuation than what would have been the case in its absence. The moratorium raised home values in high risk regions while lowering them in even higher risk regions, relative to the status quo. The distortion of home values may further bleed into reduced risk perception in high-risk regions and incentivize over-building in these areas. While the policy was intended to protect homeowners, it led to some unintended consequences in the housing market. With the goal of providing a similar kind of protection that involves potentially lower degrees of distortion to the housing market, some alternatives to the moratorium are listed below.

- Risk-Based Subsidies Instead of Broad Moratoriums - Instead of blanket mandates forcing insurers to stay, governments could provide targeted subsidies or reinsurance programs that scale based on actual fire risk. This approach would help create a more

- Gradual Premium Adjustments with Transparency - Regulations on premium increases could be adjusted to allow gradual, phased-in risk-based pricing rather than sudden, large hikes. Insurers should be required to provide clear, long-term risk assessments so homeowners understand future costs before purchasing property. Currently, the CDI in their Dec 2024 Release has taken steps to create a uniform insurance pricing strategy and reliable rates, although final increases in rates are subject to CDI's approval.[22]

- Incentivizing Risk Reduction Measures - Homeowners and communities could receive insurance discounts or tax incentives for fire mitigation efforts, such as defensible space creation, fire-resistant materials, and community firebreaks. Governments could collaborate with insurers to standardize and promote these incentives.

[22]The regulation goes hand-in-hand with forward-looking wildfire catastrophe models that can better predict future rates. Under the current system of historical data, insurance consumers are paying balloon premiums and rate spikes after major wildfires, without increased availability.
The Dec 2024 Release by CDI also attempts to prevent "Model shopping", which describes when insurance companies choose one model that produces higher rates for consumers, and another that lowers their reinsurance costs. To prevent model shopping, the regulation requires insurance companies utilize the same model for both. This promotes more consistent approaches to assessing risks, and balances the scales for consumers.

- Managed Retreat & Buyout Program - In areas where the risk is unsustainably high, policymakers could establish voluntary buyout programs to help homeowners relocate to safer regions rather than artificially sustaining risky housing markets. These programs could be funded through a mix of federal disaster relief funds and state/local contributions.

- Regional Risk Pooling and Public-Private Partnerships - Instead of forcing private insurers to bear all the risk, states could create regional risk pools or public-private insurance models that spread risk more effectively. While the California FAIR plan does exist as a state-sponsored insurer-of-last-resort, their efficacy has been questioned time and again. This alternative explores a hybrid public-private insurance provison.[23]

While the pros and cons to the listed alternatives should be tested for costs and efficacy, the attempt here is to inform policy-makers to explore other options and ascertain if there's a possibility of reducing the magnitude of negative externalities on the housing market, while still protecting on the homeowners and their homes from future fire events.

[23] A hearingwas initiated by the CDI to address consumers' concerns about the FAIR plan's limited coverage, high cost, issues regarding claims handling and lack of additional expenses in case of temporary relocation due to a fire.

6 Tables and Figures

6.1 Tables

Table 1: Summary Statistics of Home Value and Moratorium

Variables	**Risk group 0**	**Risk group 1**	**Risk Group 2**	**Risk Group 3**	**Full Sample**
Risk Rating	< Relatively Moderate	Relatively Moderate	Relatively High	Very High	
Fire Insurance Moratorium					
Observations	7020	19170	24390	5940	56520
Mean	0.164245	0.0630151	0.1430914	0.1094276	0.1150212
Standard Deviation	0.3705242	0.2429965	0.3501732	0.3122012	0.3190504
Zillow Home Value Index					
Observations	7017	19170	24379	5940	56506
Mean	30094.08	30936.38	25985.12	26730.69	28253.5
Standard Deviation	16909.19	17457.57	15330.24	13955.58	16312.02

Source: Zillow, California Department of Insurance & author calculations

Table 2: Results from de Chaisemartin & D'Haultfoeuille DID_M estimator without intertemporal effects

Dependent Variable: **ln(ZHVI)**	**Risk group 1**	**Risk Group 2**	**Risk Group 3**	**Full Sample**
Risk Rating	Relatively Moderate	Relatively High	Very High	
Fire Insurance Moratorium	0.004	0.009	0.005	0.008^{*}
	(0.008)	(0.007)	(0.008)	(0.004)
Placebo Effect	-0.001	-0.029	-0.007	-0.026^{*}
	(0.003)	(0.024)	(0.012)	(0.015)
Zip-code Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Switchers	157	433	91	848
Post-treatment Observations	1704	2714	584	6218

Notes: Asterisks indicate statistical significance at the following levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Placebo effects being insignificant imply that the DID_M estimator's version of parallel-trends assumption has been met
Switchers imply the number of observations that underwent a change in treatment status

Table 3: Results from de Chaisemartin & D'Haultfoeuille DID_L estimator with intertemporal effects - Effects of the moratorium a quarter ahead

Dependent Variable: **ln(ZHVI)**	**Risk group 1**	**Risk Group 2**	**Risk Group 3**	**Full Sample**
Risk Rating	Relatively Moderate	Relatively High	Very High	
1 period before and after treatment				
Fire Insurance Moratorium	0.007	0.068**	-0.009	0.039**
	(0.020)	(0.029)	(0.009)	(0.019)
Placebo Effect	-0.003	0.026	0.010**	0.028
	(0.009)	(0.044)	(0.05)	(0.023)
2 periods before and after treatment				
Fire Insurance Moratorium	0.006	0.084***	0.023*	0.046**
	(0.021)	(0.033)	(0.014)	(0.020)
Placebo Effect	-0.026	0.011	0.026**	0.006
	(0.017)	(0.046)	(0.012)	(0.025)
3 periods before and after treatment				
Fire Insurance Moratorium	0.013	0.086**	-0.022	0.054**
	(0.024)	(0.040)	(0.015)	(0.026)
Placebo Effect	−0.036**	−0.11**	0.017	-0.008
	(0.018)	(0.05)	(0.024)	(0.026)
Zip-code Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Switchers	83	208	41	410
Observations	1265	1335	347	3383

Notes: Asterisks indicate statistical significance at the following levels: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Placebo effects being insignificant imply that the DID_M estimator's version of parallel-trends assumption has been met
Switchers imply the number of observations that underwent a change in treatment status

6.2 Figures

Figure 1: National Risk Index Qualitative Rating Legend and Illustration of Risk Component Scores

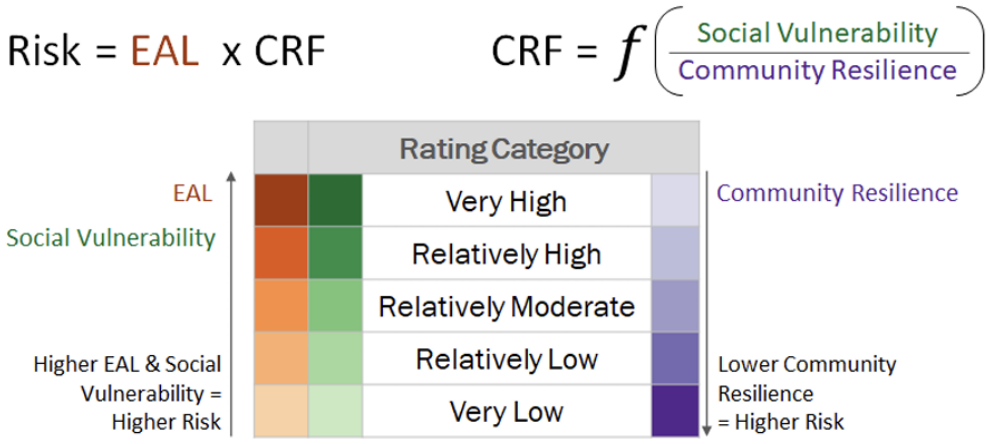
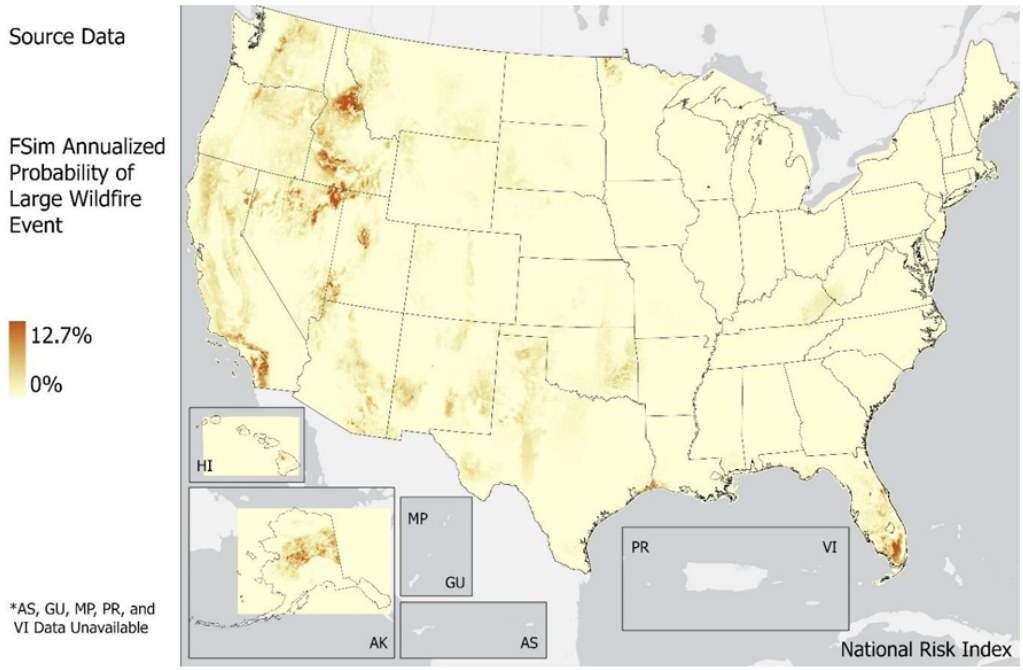


Illustration of Risk Component Scores

County	EAL	Social Vulnerability	Community Resilience	Risk
County 1	99.66	78.84	23.65	99.68
County 2	99.87	37.43	78.36	99.65
County 3	99.51	73.07	70.85	99.55
County 4	97.59	98.82	1.15	98.31
County 5	97.09	53.85	45.23	96.85
County 6	81.40	99.71	1.30	87.75
County 7	77.41	71.96	0.86	84.98
County 8	52.40	12.35	94.30	47.98
County 9	47.38	50.06	33.90	48.49
County 10	9.19	28.13	75.84	7.86

Source: FEMA National Risk Index Technical Documentation

Figure 2: National Burn Probability Raster



Source: FEMA National Risk Index Technical Documentation

Figure 3: Wildfire Risk Groups across California

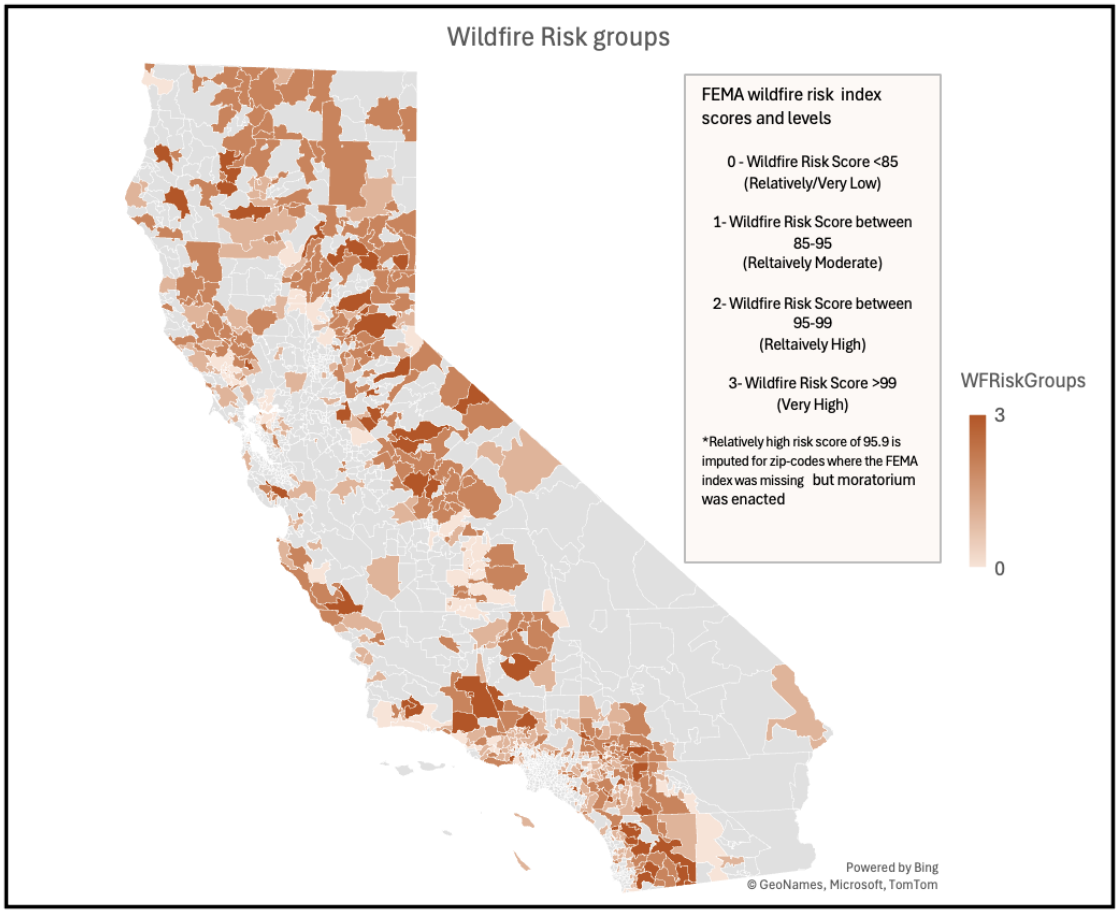


Figure 4: Fire Insurance Moratorium lengths across California

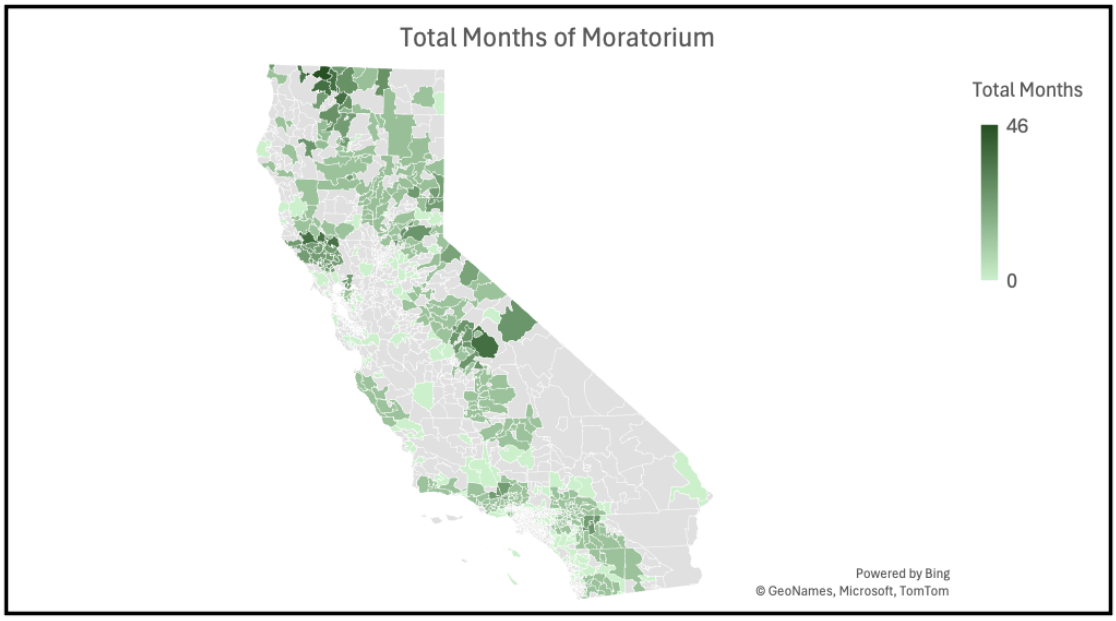
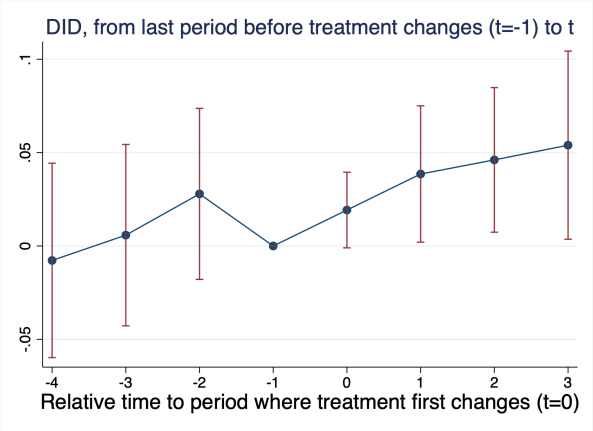
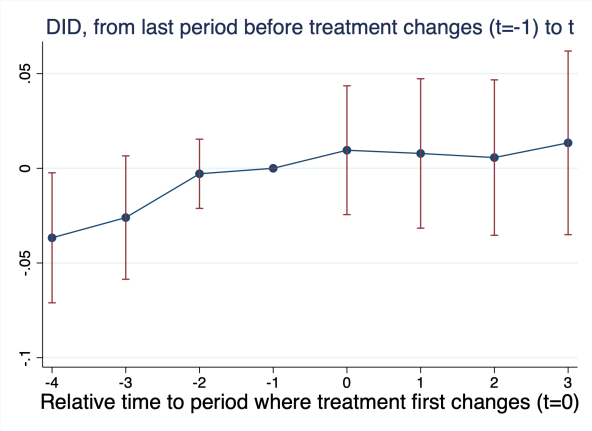


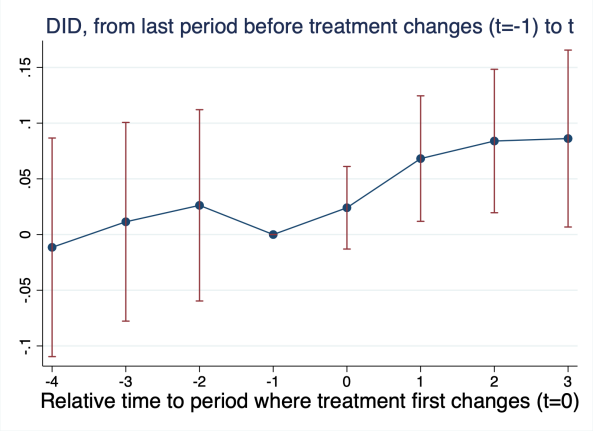
Figure 5: Effect of moratorium on % change in home values - 1 quarter ahead



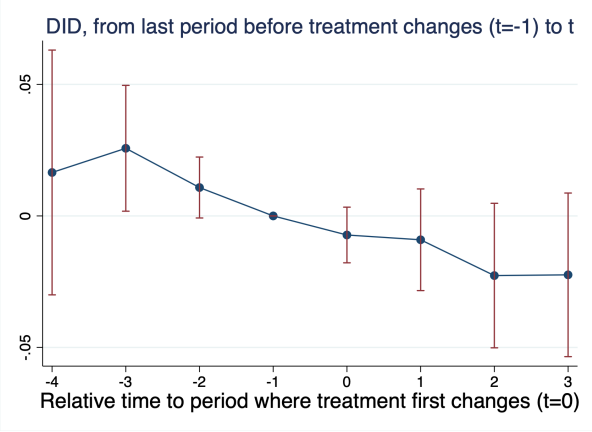
(a) Results from **all** risk groups



(b) Results from **relatively moderate** risk group

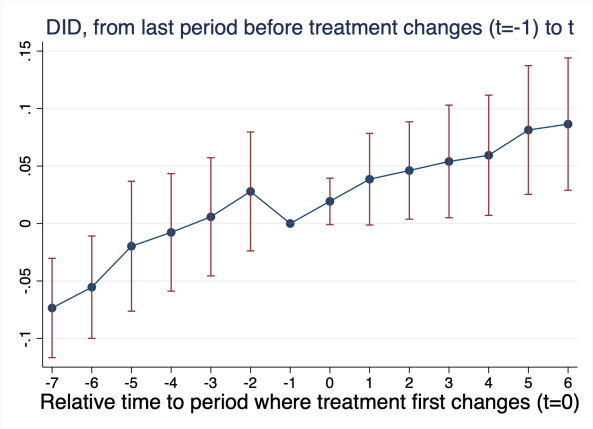


(c) Results from **relatively high** risk group

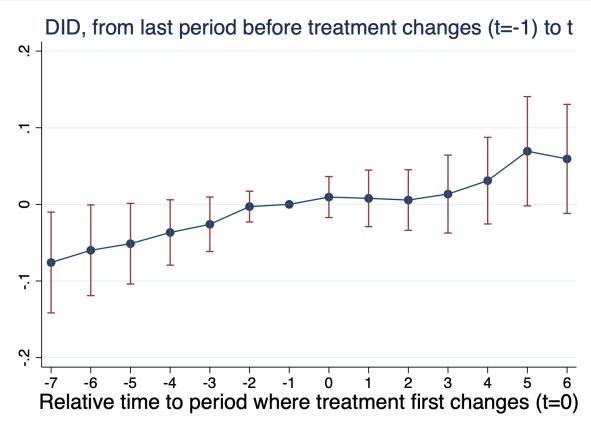


(d) Results from **very high** risk group

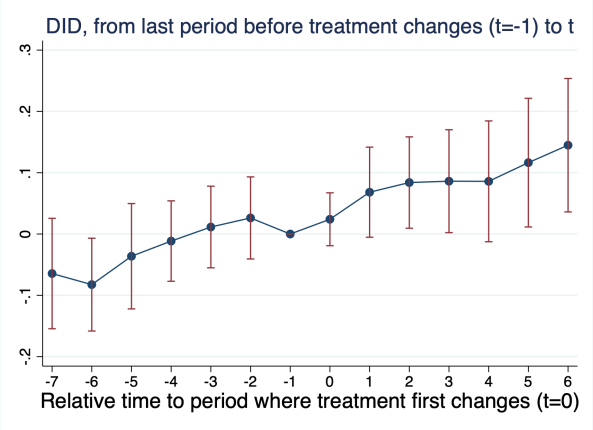
Figure 6: Effect of moratorium on % change in home values - 6 months ahead



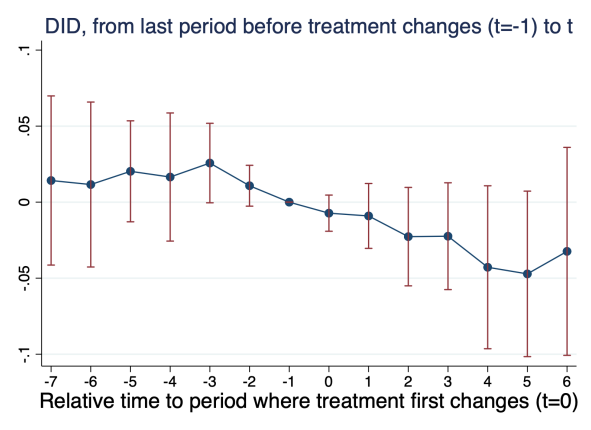
(a) Results from **all** risk groups



(b) Results from **relatively moderate** risk group

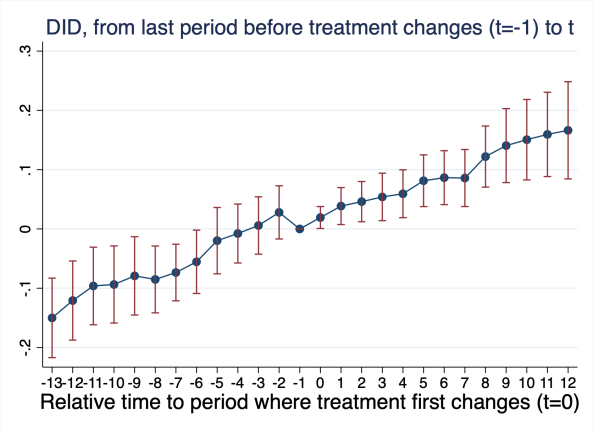


(c) Results from **relatively high** risk group

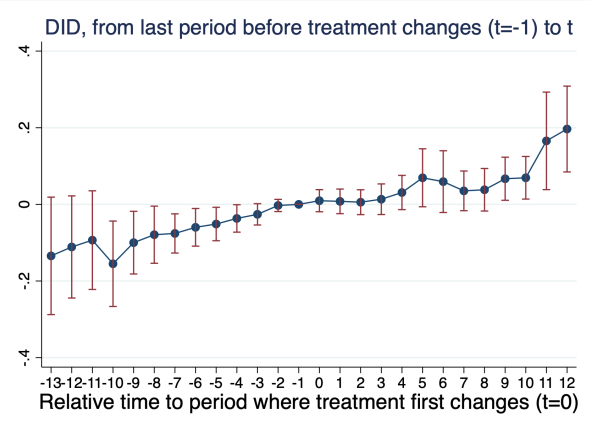


(d) Results from **very high** risk group

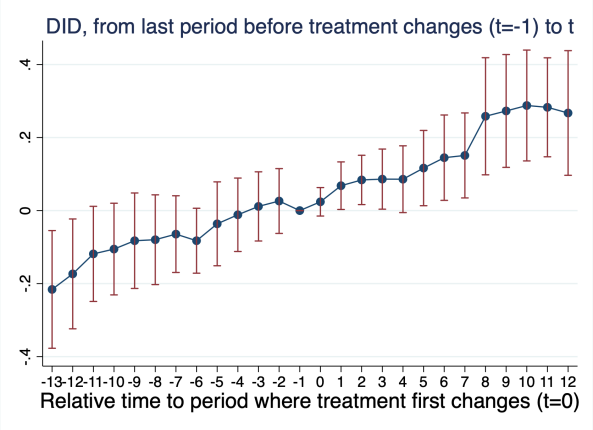
Figure 7: Effect of moratorium on % change in home values - one year ahead



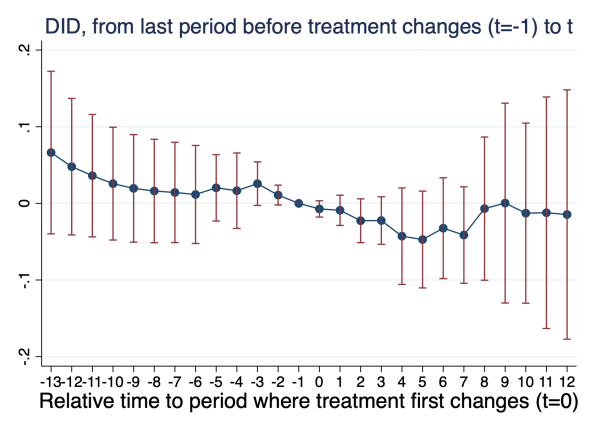
(a) Results from **all** risk groups



(b) Results from **relatively moderate** risk group



(c) Results from **relatively high** risk group



(d) Results from **very high** risk group

References

Abadie, A. 2005 Semi parametric Difference-in-Differences Estimators *Rev. Econom. Stud.* 72(1), 1–19.

Bertrand, M., Duflo, E., Mullainathan, S. 2004. How Much Should We Trust Differences-In-Differences Estimates? *The Quarterly Journal of Economics* vol. 119 (1), 249–275, https://doi.org/10.1162/003355304772839588

Borusyak, K., Jaravel, X. 2018 Revisiting Event Study Designs, with an Application to the Estimation of the Marginal Propensity to Consume

Borusyak, K., Jaravel, X., Spiess, J. 2021 Revisiting Event Study Designs: Robust and Efficient Estimation. arXiv:2108.12419[Econ].

de Chaisemartin, C., D'Haultfœuille, X. 2020. Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review* 110 (9): 2964-96.

de Chaisemartin, C., and d'Haultfoeuille, X. 2020 Difference-in-Differences Estimators of Intertemporal Treatment Effects *The Review of Economics and Statistics*. Available at SSRN: https://ssrn.com/abstract=3731856 or http://dx.doi.org/10.2139/ssrn.3731856

de Chaisemartin, C., D'Haultfœuille, X. 2022b. Two-Way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey. *National Bureau of Economic Research* Working Paper No. 29691.

Gardner, J. 2021 Two-stage differences in differences. Working Paper.

Goodman-Bacon, A. 2018 Difference-in-Differences with Variation in Treatment Timing *National Bureau of Economic Research* Working Paper No. 25018.

Goodman-Bacon, A., Marcus, J. 2020. Using Difference-in-Differences to Identify Causal Effects of COVID-19 Policies. *Survey Research Methods* 14(2), 153-158. https://doi.org/10.18148/srm/2020.v14i2.7723

Imai, K., Kim, I. S. 2021. On the use of two-way fixed effects regression models for causal inference with panel data. *Political Anal* 29(3), 405–415.

Liu, L., Wang, Y., Xu, Y. 2024 A practical guide to counterfactual estimators for causal inference with time-series cross-sectional data. *Am. J. Polit. Sci.* Volume 68, Issue 1, 160-176.

McCoy, S. J., Walsh, R. P. 2018. Wildfire Risk, Salience & Housing Demand. *Journal of Environmental Economics and Management*, vol. 91, 203–228, `https://ideas.repec.org/a/eee/jeeman/v91y2018icp203-228.html`

Mueller, J. M., Loomis, J. B. 2014. Does the Estimated Impact of Wildfires Vary with the Housing Price Distribution? A Quantile Regression Approach. *Land Use Policy*, vol. 41(4):121–127, `https://www.researchgate.net/publication/263127105_Does_the_estimated_impact_of_wildfires_vary_with_the_housing_price_distribution_A_quantile_regression_approach`

Mueller, J. M., Loomis, J. B., González-Cabán, A. 2009. Do Repeated Wildfires Change Homebuyers' Demand for Homes in High-Risk Areas? A Hedonic Analysis of the Short and Long-Term Effects of Repeated Wildfires on House Prices in Southern California. *The Journal of Real Estate Finance and Economics*, vol. 38(2), 155–172, `https://ideas.repec.org/a/kap/jrefec/v38y2009i2p155-172.html`

Koo, C., Liang, J. November 2022. A view to die for? Housing value, wildfire risk, and environmental amenities *Journal of Environmental Management*, vol. 321, 115940.

Roth J., Sant'Anna, P., Bilinski A., Poe J. 2023 What's trending in difference-in-differences? A synthesis of the recent econometrics literature. *Journal of Econometrics* 235, 2218–2244

Sant'Anna, P., Callaway, B. 2021. Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics* vol. 225 (2), p. 200-230, 2021.

Sant'Anna, P., Zhao, J. 2020. Doubly robust difference-in-differences estimators. *Journal of Econometrics* vol. 209 (1), p. 101-122.

Sun, L., Abraham, S. 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects *Journal of Econometrics* vol. 225 (2), p. 175-199.

Woolridge, J. M. 2021 Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators. Working Paper, pp. 1–89.

Appendix

Appendix Figures

Appendix Tables

Table 1: TWFE results

ln(ZHVI)	**Risk group 1**	**Risk Group 2**	**Risk Group 3**	**Full Sample**
Fire Insurance Moratorium	0.002	0.104***	0.042	−0.096***
	(0.061)	(0.031)	(0.528)	(0.024)
Zip-code Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Treatment	83	208	41	218
Control	130	63	25	410
Observations	213	271	66	628
Adjusted R^2	0.310	0.30	0.310	0.310

Notes: Standard errors are robust to heteroskedasticity. Asterisks indicate statistical significance at the following levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.