Does the Salience of Risk Affect Large, Risky Asset Prices?

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Abstract

This paper documents that when a southern California home gets designated to a wildfire risk zone, its price drops by 11% relative to homes just outside the designation boundary. Whereas the risk designation is discontinuous, the underlying risk is continuous — suggesting the price effect is due to greater risk salience rather than greater risk. Moreover, after a nearby fire, transaction prices of homes with a view of the burn scar drop by 5% relative to the prices of otherwise similar homes — an effect significant only for the first year post-fire and too large to be explained by visual disamenities alone.

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1 Introduction

Understanding which factors drive households' risk-taking behavior is a fundamental economic question. Early insights from psychology suggest that the salience of risk could play a critical role (Tversky and Kahneman, 1974).[1] Economists have developed theoretical models proposing multiple mechanisms through which salience may affect choices (Chetty et al., 2009; Bordalo et al., 2013; Gabaix, 2014). For example, Bordalo et al. (2012) formalize a model of choice over lotteries with salient payoffs, where true probabilities are replaced by decision weights. Their model explains observed deviations from the predictions of expected utility theory, including the instability of risk preferences and preference reversals. Furthermore, compelling empirical evidence demonstrates that households are not fully attentive to prices that are not salient in the context of relatively inexpensive consumer goods (Chetty et al., 2009; Finkelstein, 2009; Sexton, 2015).

Three important questions that remain open are whether (1) the salience of risk (as opposed to the salience of prices) affects household behavior, (2) whether the effect of salience on decision-making is present also for very large investment decisions, and (3) whether any individual-level effects are reflected in market prices. This paper speaks to these questions by examining the effect of environmental risk salience on residential real estate prices. Results suggest that the salience of risk locally affects residential real estate prices.

The analysis takes advantage of a unique dataset of real estate sales transactions for over 2 million homes spanning seven southern California counties over 16 years. Using spatial discontinuity designs with home fixed effects, the paper examines whether a change in policy

[1] I use the term salience as defined by: "the phenomenon that when one's attention is differentially directed to one portion of the environment rather than others, the information contained in that portion will receive disproportionate weighting in subsequent judgments" (Taylor and Thompson, 1982).

regulation that makes wildfire risk more salient triggers a behavioral response. The paper then examines a second type of risk salience, namely, the effect of visual cues of burn scars on home prices. This type of risk salience is experiential rather than policy-driven, and is unlikely to be subject to the same confounders.

First, the paper investigates the effect of a marginal expansion of the existing wildfire risk designation implemented in 2007. I compare prices of homes newly assigned to the risk designation relative to neighboring homes not affected by the new designation, controlling for property fixed effects, for a sample of homes without any recent wildfires nearby. The expansion was determined using probabilistic wildfire risk models. Thus, while the new risk designation is spatially discontinuous, the underlying wildfire risk is continuous across the new designation boundary. Under the assumption that changes in potential confounders are not discontinuous across the new risk designation boundary, I interpret these estimates as reflecting an effect of greater risk salience on home prices.

Examining the effect of an array of potential confounders, including changes in insurance premiums and mortgage markets, among others, I fail to find an effect of the new designation on these confounders. Importantly, insurance companies use fine-scale (30m$\times$30m) probabilistic wildfire risk models to adjust premiums across homes based on a set of wildfire risk factors approved by the California Department of Insurance (CDI). The list of authorized risk factors explicitly excludes the new risk designation for setting premiums. Still, as ultimately identifying assumptions cannot be tested, concerns about these potential confounders may remain. This motivates the choice of the second type of risk salience that I investigate since it arises from experiencing a recent natural disaster nearby.

Second, the paper examines the effect of visual cues of burn scars on home prices. To this end, I conduct a viewshed analysis using three-dimensional maps to precisely identify which homes have a burn scar view. I then compare the change in prices (from before to after a fire) of homes with a view of a burn scar with those of homes at the same distance from the burn scar but with no view, while controlling for property fixed effects. By contrast to a new risk designation, this second, experiential type of risk salience is likely to be temporary because the burn scar recovers after a few years. As such, it is more difficult to see how insurance and mortgage markets could react to such a temporary visual disamenity. Insurance and mortgage effects are therefore unlikely to confound this second analysis. However, this type of risk salience is correlated with the experience of natural disaster damages, in particular visual disamenities for homes treated with a burn scar view. The estimates can therefore be interpreted as reflecting a combination of greater risk salience and visual disamenities. I attempt to elicit the relative importance of these two factors by testing for a heterogeneous effect of differentially large burn scars. Doing so may help distinguish the two effects because larger visible burn scars likely generate greater visual disamenities than smaller visible burn scars, whereas risk salience is less likely to scale with the size of the burn scar. On the other hand, it is possible that the burn scar estimates underestimate the salience effect: all homes in the region, including control homes, likely experience some risk salience effect from the recent natural disaster nearby. Taken together, the risk designation and visual cues analyses are subject to a different set of challenges to the interpretation of any price effects to be due to salience. A robust finding of effects in both sets of analyses would make a salience interpretation more likely.

Lastly, I examine the effect of these two treatments on transaction volumes to elicit the likely effect of risk salience on buyer versus seller's risk valuations.

I find that prices of homes newly assigned to the risk designation drop by 10.2% to 10.8% relative to neighboring homes not affected by the new designation. This effect is robust to different selections of the control groups, including whether control homes are restricted to always being located outside the new designation or to always being located on the old risk designation. The effect is persistent over time, consistent with the policy regulation persisting throughout the sample period. Furthermore, a placebo test in which the 'treatment' group consists of homes in the old risk zone both before and after the new 2007 designation shows no effect. Since the treatment occurs just before the 2008-2009 housing crisis, the placebo test reduces the likelihood of a false positive due to macroeconomic factors.

In the case of the second treatment, I find that a burn scar view located within 2km reduces home transaction prices by 4.2% to 5.0%. This effect is strongly significant only for the first year post-fire. By contrast, a burn scar view located between 2km and 4km reduces home values by only 1.9% to 3.2%, relative to homes at the same distance from the burn scar but without a view. A placebo test in which the treatment and control groups consist of homes sold before a burn scar occurs, by contrast, produces inconsistent estimates, and thus does not suggest an effect of future burn scar views (which would suggest the presence of confounding factors). Moreover, larger burn scars are not associated with greater price reactions. Because of the magnitude of the effect, its short-term nature (despite the burn scar remaining visible for multiple years), and the absence of heterogeneity of the size of the visible burn scar, it is unlikely that these estimates are fully attributable to the visual disamenities of the burn scar. Taken together, the results suggest that visual cues of natural disaster damages increase the salience of risk, and thereby affect risk-taking behavior of relevant market participants and the prices of large, risky assets.

Visual cues of wildfire damages trigger higher sales volume, which is easiest to interpret as a stronger decrease in sellers' valuation relative to buyers' valuation in response to salience.

These findings have important aggregate and policy implications, among others because the level of home prices incentivizes new development. As such, making risks more or less salient affects the location of housing developments. This matters in its own right because the location of housing developments determines the economic costs of natural disasters (Rappaport and Sachs, 2003; Kahn, 2005). For example, an estimated 46 million homes and $9.2 trillion in property value are currently located in the wildland-urban interface and are exposed to wildfire risk in the United States (International Association of Wildland Fire, 2013; Radeloff et al., 2018).[2] Policy makers' risk designation choices may therefore effectively disincentivize further real estate development in high-risk areas.

The findings also provide a (positive) answer to the question whether individual behavioral responses affect aggregate prices. Relatedly, the paper informs whether (and confirms that) the findings of existing studies on salience are valid beyond small expenditure consumer goods, and that the salience of attributes other than price itself matters for market outcomes.

My paper contributes to the behavioral finance literature that relates to the salience of risks. For example, Malmendier and Nagel (2011) show that past experience with market downturns affects households' investment decisions, and Dessaint and Matray (2017) find that recent hurricanes lead firms to change their corporate cash holdings. While these two studies examine past experience of risk realizations, I focus on a policy that makes future risk more salient; also, the outcome variable is prices rather than individual household or manager behavior. Frydman and Wang (2019) show that changing the display color of a stock's purchase price strengthens the disposition effect. By contrast, my study focuses on the salience of risk rather than the salience of gains and losses. Heimer et al. (2019) examine

[2]Wildfires in the western United States have increased by about 500% over the last 30-40 years (Resources Radio Podcast, 2018); the 2018 Camp Fire was the single most destructive wildfire in California's history, with over 15,000 structures lost and an estimated value loss of $11 to $13 billion (www.businessinsurance.com).

how the salience of a given deadly risk and associated mortality beliefs varies across age groups, whereas I examine the effect of a variation in risk salience across assets.

The present study further belongs to the quasi-experimental literature that examines how climate and natural disaster risk is capitalized into residential real estate prices, such as future sea-level-rise risk (Baldauf et al., 2019; Bernstein et al., 2019; Bosker et al., 2019; Murfin and Spiegel, 2019), or wildfire risk (McCoy and Walsh, 2018). In particular, Murfin and Spiegel (2019) find no effect of future sea-level rise risk on home prices; Bernstein et al. (2019) find that homes exposed to future sea-level rise risk sell for about 7% less relative to similar homes not exposed to such risk, while Baldauf et al. (2019) find that homes exposed to future sea-level rise risk in climate change "believer" counties sell for a discount relative to similar homes in climate change "denier" counties. In contrast to these studies exploiting variation in risk, my paper focuses on variation in the *salience* of risk, holding risk constant.[3]

More closely related to the topic of the present paper, a couple of studies focus on a change in risk salience that does not arise from a recent realization of risk. Giglio et al. (2018) examine how changes in the salience of future sea-level-rise risk, as proxied by a zipcode-level "climate attention index", relate to home prices. The index measures the frequency of for-sale listings containing climate or flood related text. However, the authors do not claim identification of a salience effect; indeed, the process by which realtors generate the wording of for-sale listings is not known and may be affected by client preferences and local market

[3]In the context of *commercial* real estate markets, which presumably feature a different set of investors than those in the residential real estate markets studied in the aforementioned papers, Eichholtz et al. (2019) find that Hurricane Sandy having unexpectedly landed in New York led investors to update their beliefs about future flood risk in the northern parts of the US East Coast and bid down property prices not only in New York but also in Boston. They use distance from the coast for cross-sectional variation. By contrast to the matching procedure in that paper, the present study uses property-fixed effects to difference out unobserved variation across treated and control properties. In the context of recent natural disasters, McCoy and Walsh (2018) examine the effect of greater risk salience arising from wildfires nearby. In contrast, the present study uses home-fixed effects to control for unobservable variables correlated with the treatment, and exploits the size of the visible burn scar to distinguish the visual disamenity and salience effects.

conditions. In contrast, my study exploits the spatial discontinuity of a policy boundary, which is arguably less likely to be endogenous to market conditions.[4]

Another difference to the aforementioned papers is that I examine a change in risk salience through a change in policy regulation. The present paper is thus more directly policy relevant, because policy makers can – and likely will – implement similar risk zoning in the future. Similar in that respect is Donovan et al. (2007), who find that, after a public disclosure campaign of wildfire risk ratings making risk more salient in Colorado Springs' wildland-urban interface, the prices of higher-risk homes temporarily decreased. An important methodological difference to Donovan et al. (2007) is that the present study exploits panel data. This is important because households in high-risk areas may display systematically different preferences than households sorting to lower-risk homes, including their propensity to react to the salience of risk. The present paper alleviates the concern of potentially endogenous differences in the type of households interested in living in higher- versus lower-risk homes by controlling for property fixed effects. The paper's design thus reduces the first-order concern about sorting.[5]

2 Data

This section describes the data sources and construction of the dataset, including the real estate sales transactions, risk zone designation, insurance and mortgage markets, neighborhood amenities, wildfire characteristics, and burn scar view.

[4] Gibson et al. (2019) examine the effect of Hurricane Sandy and floodplain map updates on home prices in New York City. Since the map updates immediately follow Hurricane Sandy, the effect of the map reassignment cannot be disentangled from the experience of a recent hurricane.

[5] A remaining concern with respect to sorting with the present study's design is that the type of households sorting into particular homes may change before and after the reassignment to a risk designation. To attenuate this concern, I test for the effect of the treatment on changes in household characteristics at the neighborhood level, and find no evidence to that effect.

2.1 Real estate sales transaction data and home characteristics

To capture all the properties likely affected by wildfire risk, I selected zip codes located within a 30km bandwidth of the national forests surrounding the Los Angeles and San Diego basins. Those zipcodes span across seven counties: Santa Barbara, Los Angeles, Orange, Ventura, Riverside, San Bernardino, and San Diego. Transaction records for all properties located within those zip codes sold between January 2000 and December 2015 were purchased from CoreLogic.[6] Starting with a dataset of 2,187,007 unique properties, single family residence (excluding mobile homes) and arms-length transactions of owner-occupied properties (i.e., primary homes) account for 1,215,523 homes. Transactions with missing sale price are omitted, as well as homes sold more than once within the same year are further dropped to eliminate potential house flippers (1,070,639 remaining homes).[7] All prices are deflated using the Consumer Price Index from the U.S. Bureau of Labor Statistics. I further drop homes with sale prices in the bottom and top 1%. Of the remaining 1,011,006 homes, 444,180 are repeat-sales homes, i.e., they sold twice between 2000 and 2015 (I do not observe more than two sales since the CoreLogic dataset only contains information up to the prior sale).[8] To reduce the likelihood that a home experiences significant renovation or unusual damages in-between sales, I drop homes whose price change across sales is in the top and bottom 1% (with 431,000 remaining repeat-sales homes). Summary statistics for

[6]The data contain the date at which the sale is officially recorded. I assume that the sale price is agreed upon 2 months before the sale is officially recorded. Results are qualitatively similar when using a 3-month lag.

[7]I discard second homes as preferences for such homes may differ from those for primary homes. I discard homes that are flipped because they are often renovated before going back on the market.

[8]Linking the CoreLogic transactions data (truncated to two sales) with the untruncated Zillow dataset (ZTRAX), I check that the distribution of homes with more than two sales does not significantly differ across treatment and control groups based on observables.

the full- and repeat-sales samples are comparable (Table 1).[9]

2.2 Buyer demographics

To access buyers' mortgage application information, I link the sales transaction data with the Home Mortgage Disclosure Act (HMDA) data, which includes household income, race, ethnicity, and loan information, as in Bayer et al. (2016). (I do not use the gender of the loan applicant as it contains many missing values.) HMDA observations with no mortgage year, no loan amount, no lendername, or indications that the lender was a private lender, are dropped. The sales transaction data contain loan information only for the most recent sale and, thus, for each home the demographics information corresponds to that of the most recent buyer. Matching on mortgage year, lender name, loan amount and type, county, and census tract, and keeping properties with unique matches, I obtain a 50.2% and 53.0% matching success rates in the case of the new risk designation and burn scar view treatments, respectively. These matching success rates compare favorably with those reported in Bayer et al. (2016). Buyer demographics are depicted in Panel B of Tables 2 and A4, and discussed in Sections 2.4 and 2.6.

2.3 Wildfire and neighborhood characteristics

The California Department of Forestry and Fire Protection (CALFIRE; fire.ca.gov) provides spatial data on wildfires, the wildland-urban interface (WUI), and Fire Hazard Severity

[9] All properties are geo-coded to obtain exact latitude and longitude coordinates. Homes located inside national forests are excluded due to concerns of belonging to different housing markets. Homes inside a wildfire perimeter or within a 50m buffer outside a wildfire perimeter are further discarded to ensure that homes potentially damaged by wildfires are excluded. However, note that wildfires in southern California did not usually destroy large numbers of homes during the study time period (California Department of Forestry and Fire Protection, 2018).

Zones (FHSZ). The wildfire data contain spatial information on burn perimeters and start and containment dates. Because very small fires likely do not affect risk perceptions, I discard fires smaller than 50 acres. As a result, the analysis includes 251 fires between 1998 and 2015. Burn perimeters range from 51 to 270,686 acres (with median and mean sizes of 695 and 5,634 acres, respectively; Table A3).[10] National forests spatial layers come from the National Datasets maintained by the US Forest Service (data.fs.usda.gov). State and local parks layers come from the California Protected Areas Data Portal (calands.org/cpad). Spatial data on primary roads come from the US Data Catalog (catalog.data.gov). The 2000 census tract boundaries and census characteristics, including median household income, race, and ethnicity come from the American Community Survey. In ArcGIS, I calculate slope, elevation, and distances to the nearest risk designation boundary, nearest burn scar, nearest national forest, nearest state or local park, and nearest primary road.

2.4 Risk zone designation

CALFIRE produces Fire Hazard Severity Zones (FHSZ), which I will refer to generically as "risk zones" hereinafter. Risk zones are managed by the State of California (state responsibility area) or local governments of California (local responsibility area), depending on which jurisdiction is primarily responsible for fire protection and suppression efforts in that area.[11] By law, sellers have to disclose to potential buyers whether their home is located on a wildfire risk designation prior to the sale.

[10]The study includes some of California's largest wildfires, including the 2003 Cedar Fire (271k acres; third largest), the 2007 Witch Fire (162k acres), and the 2009 Station Fire (161k acres). It is noteworthy that the Cedar and Witch fires overlapped by over 40,000 acres despite being only four years apart. It illustrates the short fire-interval existing in southern California, which contrasts with that of forested areas in the rest of the Istern United States.

[11]One implication of a wildfire risk designation is that new construction must comply with stricter building codes for new construction (California building code 7a) and requiring greater defensive space. This formalizes what insurance companies were already requiring of high-risk homes before the new designation.

Because the implementation of risk designations is only mandatory in the state responsibility area, I restrict the analysis to homes assigned to the new state risk designation.[12] While early maps for the state responsibility area were in place prior to 2000, the new state risk designation was adopted throughout California in November 2007 under Title 14 ordinance. The new risk designation revised and expanded the old risk designation by exploiting new, updated risk data and wildfire risk models. The expansion of the old risk designation was marginal, and in most places did not exceed a few kilometers, as shown in Figure 1.

The risk designations are determined using continuous variables that reflect the probability of an area to burn, including the amount of fuel, slope, aspect, and distance to high-risk wilderness. The risk zones do not account for private risk mitigating actions on a given property, such that households do not have the ability to influence the assignment of their home to the risk designation. In urban and peri-urban areas (the focus of the present study), the state risk zone designations are categorized into three hazard levels based on distance buffers to high fire-hazard wilderness areas. Therefore, a high-hazard wilderness area will typically be surrounded by risk designations of decreasing hazard severity, i.e., ranging from very high to high, and then moderate, such that the hazard severity attenuates sequentially over space.[13] For the present analysis, I pool risk designations across hazard severity levels and focus on the binary assignment of whether a home is located 'inside' or 'outside' the risk

[12] CALFIRE worked with 117 local governments in the seven counties encompassing my study area to develop and recommend very high fire-hazard zones in local responsibility areas. However, adoption decisions across the different local governments have been on a case-by-case basis. Because of the endogeneity of such decisions, I discard sales post-2007 in the new recommended risk zones in local responsibility areas. (Figure A1 includes the new recommended, but not mandatory, risk zone designation in local responsibility areas that are excluded from the analysis.)

[13] Based on conversations with David Sapsis, a CALFIRE fire specialist in charge of designing and implementing the 2007 risk zone designation, the true hazard rate is in essence continuous over space, including across the hazard severity level designations, as it is determined by continuous variables. Furthermore, visual inspection of the new state risk designation boundary reveals that it typically does not follow highways, rivers, or other structures that may affect risk discontinuously (http://egis.fire.ca.gov/FHSZ). Figure A2 shows examples of risk designation maps in the SRA and LRA.

designation.

Summary statistics of the home and neighborhood characteristics for the homes newly assigned to the 2007 state risk designation (treated homes) and not newly designated homes (control homes) are shown in Panel A of Table 2.[14] On average, homes in the new risk designation are at higher elevation, farther from a major road, and less expensive than homes outside the new designation. This is line with the notion that the new designation marginally expands the old risk zone by including high-risk land in remote and high-elevation areas. These differences in observable characteristics may suggest differences in time-invariant unobservables across treated and control homes, which I address with property-fixed effects. Yet, one remaining caveat is that treatment may be correlated with changes in unobservables influencing home prices. The identifying assumption is that *at least part* of the treatment affecting home prices is uncorrelated with changes in unobservables. Panel B of Table 2 indicates that the subset of buyers for whom I have demographic information purchased treated homes that are on average more expensive than the homes in the sales transaction sample (Panel A) – this can be partly explained by the small number of buyers matched from HMDA. Thus, one should be careful when interpreting buyer demographics. For example, it appears that mean household income greatly exceeds the median census tract household income across all treatment and control groups. Loan applicants are on average slightly less likely to identify as white than in the American Community Survey census tract statistics, but are equally likely to identify as hispanic.

[14] To isolate the effect of the new risk designation on risk salience, I focus on homes that do not experience any wildfire before the time of sale. Thus, I select for this analysis homes with no fire within 10km above 1000 acres during the three years prior to the sale.

2.5 Insurance and mortgage markets

A concern with interpreting the effect of the new risk designation on home prices as an effect of greater risk salience is that changes in insurance premiums may vary discontinuously across the new designation boundary. Fire damages are covered as part of regular home insurance policies. Wildfire risk represents a major part of insurance companies' business in California and insurers generally use probabilistic wildfire risk models, typically with a 30-meter resolution, to determine each home's propensity to burn.[15] The wildfire risk factors in those risk models are strictly regulated by the California Department of Insurance (CDI). They include continuous variables such as the amount of fuel, slope, aspect, distance to high-risk wilderness areas, and a measure of access for fire suppression (Cignarale et al., 2017). Importantly, CDI explicitly prohibits the use of wildfire risk zone designations in the list of wildfire risk factors used by insurance policy underwriters.[16] In Section 4.4, I analyze more formally the effect of the new risk zone designation on zipcode-level insurance premium

[15] Risk Management Solutions (https://www.rms.com/software/risk-modeler) and Corelogic (Jeffrey et al., 2019) are examples of commercial probabilistic wildfire risk models used by the insurance industry. In addition to the wildfire risk score generated by such models, the other most common factors relevant to wildfire in underwriting guidelines are roof material and brush and vegetation clearance to the property line (Dixon et al., 2018). Furthermore, note that even though CDI allows insurers to use probabilistic models to determine how to vary rates across homes by wildfire risk, CDI does not allow the use of such probabilistic models to project expected future losses and determine the average rate level for an insurer's overall book of business. Rather to do so, insurers are required to use past loss history to set the so-called catastrophe load for wildfire risk (Dixon et al., 2018; Issler et al., 2019).

[16] This rule was publicized in the media and on state agencies' website through a Memorandum of Understanding signed on October 2007 between CDI, CALFIRE, and the insurance industry, announcing that the new risk designation would not affect insurance premiums. The memorandum was published at the time on the CDI website (http://www.insurance.ca.gov/0400-news/0100-press-releases/0060-2007/...), the CALFIRE website (http://www.fire.ca.gov/...), and in the Insurance Journal (see https://www.insurancejournal.com/news/west/2007/10/16/84317.htm), among others. Consistent with this ruling, Steve Inlow, who works in in an insurance policy underwriting business representing about 20 insurance companies in Orange County, confirmed in an interview published in the Orange County Registar that his business does not use the state hazard maps (see https://www.ocregister.com/2012/03/05/could-fire-maps-hurt-property-values). Finally, according to conversations with CDI representative, Jim Leung, he reports that insurance premiums have not been affected by the new risk designation.

quotes over the years 2004-2011, and do not find any effect of the treatment. A separate concern is that the new risk designation could affect homeowners' access to home insurance. First, homes with a federally backed mortgage are required to have home insurance, i.e., the vast majority of homes in my sample. Second, this concern is alleviated by the existence of CDI's FAIR Plan, which guarantees that all properties in California, regardless of wildfire risk, are eligible for coverage from the FAIR Plan (Cignarale et al., 2017; Dixon et al., 2018).

Another potential source of concern to identification is that the new risk designation triggers changes in the mortgage market. Based on personal communications with bankers and mortgage brokers conducting business both inside and outside the risk designation and underwriting mortgages to a large number of lenders in my study area, the new risk designation does not influence lending practices (Table A2). A formal analysis of the mortgage market (presented in Section 4.4) indicates that the loan to value ratios do not change in response to the treatment close to the designation boundary, thus alleviating this concern.

2.6 Burn scar views

To determine which homes have a view of a burn scar, I conduct a viewshed analysis using ArcGIS's Viewshed tool with a Digital Elevation Model (DEM) of the terrain from the USGS National Elevation Dataset (with a 10m spatial resolution) to predict what a 5-foot tall person can see from the property in a 4km radius. I then intersect each property's 4km-radius viewshed with all the burn scar perimeters. Because the Digital Elevation Model only takes into account the bare earth, considerable measurement error may be associated with the burn scar view variable. To resolve part of this imprecision, I collected Light Detection and Ranging (LiDAR) data to construct a Digital Surface Model (DSM) that accounts for structures on the earth such as buildings and vegetation. One limitation of this approach is that LiDAR data are only available for three counties — San Diego, San Bernardino, and Riverside.[17] Summary statistics of the home and neighborhood characteristics for repeat-sales homes sold within 4km of a burn scar and during the first two years post-fire are shown in Panel A of Table A4, with homes depicted in Figure 2. Homes with a burn scar view (treated) are at slightly higher elevation than homes without a burn scar view (control) — a difference I control for with property fixed effects. Panel B suggests that the buyers for whom I have demographic information purchased homes that are comparable to the homes in the sales transaction sample based on observed characteristics (Panel A). The mean household income of those buyers is slightly above the median census tract household income, and in particular for homes with a burn scar view relative to homes without a burn scar view and within 2km. Loan applicants are on average equally likely to identify as white as reported in the median census tract statistics but are slightly less likely to identify as hispanic.

3 Empirical strategy

I use the hedonic pricing method to value the effect of greater wildfire risk salience on home prices (Rosen, 1974). The average treatment effect on the treated (ATT) is subject to biases if the homes that receive treatment are systematically different from those that do not. For example, homes located near the new risk designation or near burn scars may experience different amenity levels, e.g., school quality or wilderness vistas. Failure to control for an unobservable that is correlated with both the treatment and home prices will lead to biased estimates. The fundamental issue is that one does not observe the counterfactual for treated homes, i.e., the prices of treated homes absent treatment. I take advantage of the repeat-

[17] I am not aware of other valuation studies using fine-resolution, LiDAR data to explore the effect of measurement error in the visual amenity variable.

sales homes to control for home and neighborhood time-invariant unobservables. In the sections below I discuss times fixed effects and specific spatial sample restrictions chosen to improve the comparability of the treatment and control groups and mitigate concerns about changes in unobservables that may be correlated with both treatment and prices. I further review potential confounders, including changes in the insurance and mortgage markets, and changes in neighborhood composition in Section 4.4. Last, the two types of risk salience I investigate are either policy-driven or experiential, and are thus unlikely to face the same confounders.

3.1 Effect of the new risk zone designation on home prices

I exploit the spatial discontinuity arising from a new risk designation, implemented throughout the state of California in November 2007 and which marginally expands the pre-existing risk designation (which I refer to as the 'old' designation). I compare the value of homes newly assigned to the risk designation relative to homes nearby that did not experience a change in designation. Specifically, I focus on homes within 500m, or between 500m and 1km, of the new risk designation boundary. While the risk zone designation is spatially discontinuous, the underlying wildfire risk is a function of continuous risk factors and is, in essence, continuous (as detailed in Section 2.4 and illustrated in Figure 3). Conditional on changes in the unobservables correlated with prices being not (too) discontinuous across the new risk zone boundary, the estimates can be interpreted as the effect of greater risk salience on home prices. Identification does not require the treatment to be perfectly uncorrelated with changes in unobservables, but that there is at least an exogenous variation component in the treatment.

My quasi-experimental design consists of a difference-in-differences approach around the spatial discontinuity, while controlling for time and property fixed effects, as shown in equation (1).

$$\ln p_{it} = \beta NewDesignation_i + \gamma Post_t + \delta NewDesignation_i \times Post_t + \lambda_i + \mu_{gt} + \epsilon_{it}. \quad (1)$$

In this equation, the dependent variable is the natural log of home i's sale price at time t. As illustrated in Figure 3, the treatment group, $NewDesignation_i$, is defined as homes that are in the new 2007 risk designation, while controls are nearby homes not affected by the new designation, either because they are always outside the new designation or they were already inside the old risk zone prior to the new designation. $Post_t$ is post November 2007, while λ_i denotes home fixed effects. Figure 5 illustrates the spatial distribution of a sample of treated and control homes across the new risk designation in Ventura and San Diego counties. Visual evidence in support of the common trends assumption for pre-November 2007 prices in the treated and control groups is shown in Figure 4. Trends in the treated group possibly indicate a slight anticipation of the treatment in 2007, possibly due to a public announcement preceding the November implementation in early 2007. Differences in observable attributes, such as remoteness as characterized by greater distance to primary roads and higher elevation, can explain differences in the price levels of treated and control homes, hence stressing the importance of using home fixed effects.[18]

Because my approach relies on time variation, I control for heterogeneity in temporal shocks across the region. For example, macro-level housing shocks could drive price changes

[18]Differential population growth across the new designation is unlikely to affect these estimates. Indeed, I omit new constructions and focus on repeat-sales homes pre- and post-treatment. Second, housing supply is inelastic in the region (Green et al., 2005). For example, Saiz (2010) reports MSA-level elasticities for Los Angeles-Long Beach, Riverside-San Bernardino, and San Diego of 0.63, 0.67, and 0.94, respectively. Last, the common trends assumption would not hold if a development pressure affected home prices differentially across the new designation boundary in the pre-treatment period.

and confound the effect of wildfire risk. Thus, I rely on time-varying fixed effects at the county level, μ_{gt}, to control for unobservables at the local and macro level, including either year-by-quarter fixed effects combined with quadratic county trends or county-by-year-by-quarter fixed effects.

3.2 Effect of burn scar views on home prices

For the second treatment, I measure the effect of visual cues of burn scars on home prices. This treatment is unlikely to be correlated with changes in insurance or mortgage markets as burn scar views are temporary, semi-random, and are not uncorrelated with wildfire risk, all else equal, including the distance to the burn scar. Using the repeat-sales model, I estimate equation (2) where careful selection of the sample of treated and control homes determines β_j, the estimated ATT effect of having a burn scar view ($View_{jit} = 1$) in the first and second years post-fire, with $j = \{1, 2\}$.[19]

$$\ln p_{it} = \sum_{j=1}^{2} (\beta_j View_{jit} + \gamma_j View_{jit} \times Large_{jit}) + \lambda_i + \mu_{gt} + \epsilon_{it}. \tag{2}$$

In equation (2), the dependent variable is the natural log of home i's sale price at time t. λ_i are home fixed effects, μ_{gt} are temporal and spatial fixed effects and/or trends as in equation (1) – i.e., year-by-quarter fixed effects and quadratic county trends or county-by-year-by-quarter fixed effects. Conditional on the error terms being uncorrelated with home prices,

[19]Burn scars take many years to fully recover (Breslin, 2013). However, focusing on sales in the first two years post-fire allows me to capture first-order effects. Furthermore, to isolate the effect of a single wildfire, I discard homes that experience a second fire in the five years prior to the sale. In addition, because the human eye would have trouble distinguishing burned shrubs from unburned shrubs (the predominant vegetation type in the region) from more than a few kilometers away, I focus on homes within 4km of burn scars. McCoy and Walsh (2018) find that a 5km threshold is appropriate in a Colorado setting with forests and burned trees visible from farther away.

the estimates β_j can be interpreted as a combination of the effect of visual disamenities from the burn scar and changes in risk salience on home prices. To investigate the relative importance of these visual disamenities, I allow for heterogeneity in the burn scar view intensity, where γ_j denotes the effect of large visible burn scars on home prices ($Large_{jit} = 1$ above 10 acres).

To estimate equation (2), the analysis focuses on repeat-sales homes for which one of the sales occurred within both 4km of a burn scar and two years post-fire. Figure 6 illustrates the distribution of homes treated with a burn scar view compared to those without a view for two fires in my sample. As expected, homes closer to the burn scar are more likely to have a burn scar view than homes farther away. Also, homes with a view of the burn scar tend to be clustered together, which highlights the need to control for spatial variables correlated with the burn scar via home fixed effects and clustered standard errors, along with placebo tests.

To isolate the effect of the visual cues of burn scars on home prices from the effect of variables that vary with the distance from the burn scar, e.g., possibly changes in risk levels or other burn scar disamenity, I compare treated homes to control homes that are located in the same distance bin from the burn scar, and pin down most of the variables that vary over space. Thus, I run separate models for different distance bins from the burn scar to capture the heterogeneous effect of the burn scar view over space while controlling for potential changes in burn scar proximity effects. The thinner the distance bin, the more heterogeneity is allowed for, but the fewer the number of observations and the potentially less precise the estimates. (I test multiple bin widths and show results for 2km-bin widths in Table 5 and relegate results for the 1km-bin width to Table B2.)

4 Results

This section presents the estimated effects of the new risk zone designation and of visual cues of wildfire burn scars on home prices.

4.1 Effect of the new risk zone designation on home prices

The new risk zone designation reduces the value of homes newly assigned to the risk designation relative to homes that do not change assignment status (Table 3). The preferred model specification restricts the analysis to homes within 500m of the risk designation boundary (Panel A) to mitigate concerns of unobservable trends varying over space across the treated and control homes. Every model features home fixed effects and either quadratic county trends and year by quarter fixed effects or county by year by quarter fixed effects, with robust standard errors clustered at the census tract level.

When the control group consists of homes always outside the new designation or inside the old risk designation (columns (1) and (2)), the effect of being newly assigned to the risk designation reduces home values by 10.2% to 10.8% within 500m (Panel A) and by 10.6% to 12.5% between 500m and 1km of the boundary (Panel B).[20] I investigate three alternative choices of the control group in columns (3) through (8). First, I restrict the control homes to being located in the same zipcodes as the treated homes (columns (3) and (4)). This restriction ensures that only homes that are located in similar neighborhoods as treated homes are used as controls. Based on observables, and in particular elevation and distance to primary road, this new control group is more similar to the treatment group (as depicted

[20] Homes inside the old risk designation may be used as controls if they are located within 500m or 1km of the new 2007 risk zone boundary. This is the case when the marginal expansion of the risk designation is small (less than 500m or 1km), as illustrated in Figure 3 (right panel).

in Table A1). However, this restriction is also conservative as neighboring, control homes just outside of the new designation boundary are excluded if they happen to belong to an adjacent zipcode as that of a treated home. The smaller number of control homes increases internal validity, if there are concerns about changes in unobservables, but at the expense of reduced estimates precision. Overall, the results remain qualitatively similar with the new risk designation leading to a reduction in home values of 6.3% to 7.4% within 500m (Panel A) and by 7.2% to 7.3% between 500m and 1km of the new risk designation boundary (Panel B). Second, I now restrict the control homes to always being located outside the new risk designation. Results are quantitatively similar to those in columns (1) and (2) when dropping these homes from the control group (columns (5) and (6)). Third, I then restrict the control homes to being located inside the old risk designation (columns (7) and (8)). Results are quantitatively similar within 500m (Panel A), but lose significance between 500m and 1km of the new risk designation boundary due to the small sample size (Panel B).

A placebo test using homes inside the old risk zone both pre- and post-new designation as 'treatment' compared to homes outside the new risk designation both pre- and post-new designation (controls) shows no effect of the 'treatment' (Table 4; columns (1) and (2)). The placebo test rules out that the main estimates capture a local effect affecting other, non-treated homes in the area. It is reassuring given that the new designation coincides with the beginning of the housing crisis. I run a second placebo test restricting controls to being located in the same zipcodes as treated homes (columns (3) and (4)). Estimates remain insignificant.

To investigate whether the effect of the policy change persists over time, I regress home prices on the new risk designation by post treatment, where post treatment is now divided into three time periods: immediately after the policy change (2007-2009), medium term

(2010-2012), and long term (2013-2015), pooling all observations within 1km of the new risk designation boundary to maintain a good sample size representation for each time period. Estimates in Table B1 indicate that the effect of the policy change do not attenuate over time. This may not be surprising if the new designation induces local buyers to always weigh the wildfire risk attribute for homes newly assigned to the risk designation relatively more in their decision-making than for homes that do not change risk designation status across sales. In this sense, the effect of greater risk salience triggered by a new policy regulation has the potential to be more persistent in nature than that of experiential types of risk salience, which attenuates as the damages and/or memories of the past natural disaster fade away. This latter fact has been extensively reported in the literature on the effect of recent flood events on CEO behavior, home prices, and flood insurance take-up, among others (e.g., Bin and Landry (2013); Gallagher (2014); Dessaint and Matray (2017); Muller and Hopkins (2019)).

Causal inference requires assuming that, conditional on the fixed effects, the error terms are uncorrelated with the treatment. In practice, one may be concerned that confounding factors violate this assumption. For example, one may be concerned about insurance premiums changing discontinuously across the new risk designation boundary in response to the treatment. I review in details potential confounders in Section 4.4, and do not find evidence that they change discontinuously in response to the new risk designation. Still, identifying assumptions are not testable. This motivates the choice of my second type of risk salience, which exploits cross-sectional variation in the salience of a recent natural disaster nearby. By contrast to the literature on recency of experience effects, in my design every home experiences the nearby natural disaster, but only some of them are being reminded of it through having a view of the burn scar.

4.2 Effect of burn scar views on home prices

Table 5 shows that having a view of a burn scar decreases home prices from 4.2% to 5.0% within 2km of a burn scar in the first year post-fire (Panel A, columns (1) and (2)). The effect is in general attenuated the farther a home is from the burn perimeter, with home values reduced by 1.9% to 3.2% between 2km and 4km (Panel B). Homes selling during the second year post-fire show little or weak burn scar view effects, although the effect is statistically significant for homes in the 2-4km bin, possibly due to the larger sample size. Yet, estimates in columns (1) and (2) may be attenuated by measurement error since the bare earth model (DEM) does not capture physical structures obstructing the view such as buildings and vegetation. To identify the extent of this potential concern, I determine the viewshed using LiDAR data that account for structures on the ground to assign homes to the treatment and control groups with less error (e.g., Figure 6; bottom panels). The tradeoff being that LiDAR data are only available for three counties such that the sample size is reduced. Estimates in columns (3) and (4) are not statistically different (at the 10% level) from the estimates in columns (1) and (2). The results are further robust to an array of specifications and sample definitions, including omitting sales during the first quarter post-fire and restricting visible burn scars to be above a minimum size threshold, e.g., 0.1 or 0.5 acre. In Table B2, the width of the distance bins is refined to 1km to increase the accuracy with which the distance to the burn scar is controlled for. Results are qualitatively similar.

Last, a placebo test is conducted in columns (5) and (6) in which repeat-sales homes are sold one or two years *prior* to a fire. Homes that would have had a view of that future burn perimeter are assigned to the treatment group, while homes that would not are assigned to the control group. As previously, I further allow for a differential effect of the fire one or two year(s) after the sale and of large future burn scars. Most estimates are insignificant and, overall, future fires have an inconsistent effect on current prices.[21]

To put the estimates in columns (1) and (2) in perspective, assume a home can be rented out annually for 5% of its purchase value. For a drop in value to be justified by lower amenities alone, a home with a burn scar view within 2km would have to be rented out rent-free for the first year post-fire relative to its neighbor in the same distance bin from the burn scar but without a burn scar view. In other words, there would be no amenity value at all from living in the home one year after the fire. The magnitude of the effect suggests that the estimate is unlikely to be fully attributable to the burn scar visual disamenities alone. In particular, the effect of a view of a burn scar is not greater for larger visible burn scars, which likely generate greater visual disamenity. Burn scars can be barren of any shrubs or trees for several years after a wildfire and can take up to ten years to fully recover (Breslin, 2013). Thus, while the view of the burn scar is most extreme in the first couple of years post-fire, the visual disamenities attenuate gradually over multiple years following the wildfire. Furthermore, even though the presence of the burn scar may be noticeable from far, it is less likely that distant burned shrubs are associated with significant visual disamenities beyond a kilometer. However, it is possible that being reminded of a recent fire through visual cues of the burn scar draws attention to wildfire risk. Taken together, these considerations suggest the visual disamenity alone are unlikely to entirely drive the results. Therefore, the estimates suggest that greater risk salience through visual cues of recent burn scars affects home prices. Note however that such salience effect is temporary (and almost not detectable

[21] There are a few instances with either positive or negative coefficients. In particular, within 2km, large visible burn scars one year pre-fire have a positive effect (4%) on home prices in the specification with quadratic county trends, which goes away in the more flexible specification with county-by-year-by-quarter fixed effects. There is also a negative effect of burn scar views on home prices (-3%) in the 2-4km bin two years pre-fire. Such contradictory estimates are difficult to interpret and are most likely attributable to false positives.

past the first year post-fire). Note also that while comparing homes with a burn scar view to similar homes without a view is helpful for inference, it likely underestimates the total risk salience effect of a nearby wildfire, because home prices of both treatment and control homes in the vicinity of the burn scar are likely to be depressed (at least temporarily).

4.3 Changes in households' beliefs in response to risk salience

To elicit whether sellers or buyers are more affected by changes in risk salience, I explore the effect of the two treatments on the sales transaction volume as measured by the number of sales in each quarter, using the full sample of homes (rather than only the repeat-sales homes). Columns (1) and (2) of Tables C2 and C3 depict the results of a difference-in-differences approach with census tract fixed effects and time fixed effects. Whereas Table C2 shows no evidence that the new risk designation influences sales volume, Table C3 suggests that burn scar views trigger an increase in sales compared to control homes with no view. Turnover increases by 8% for homes within 2km of a burn scar, and 5% for homes in the 2-4km bin (p-values<0.01 in both cases). Given that prices of homes with burn scar views decrease (Table 5), I know that demand for burn-scar-view housing decreases on average. I also know the supply of housing is fixed — as housing supply is inelastic and wildfires in the region and study time period typically do not affect housing supply. The increase in sale volume thus must come from differential changes in demand for housing. Housing demand comes from current owners ("sellers") of homes or from potential owners ("buyers"). If buyers' valuations predominantly decreased whereas sellers' valuations remained constant, fewer pairs of buyers and sellers would trade. This prediction of lower sales volume is inconsistent with estimates in Table C3. By contrast, if it is predominantly the sellers' valuation that decreases vis-à-vis the buyers', the prediction is an increase in sales volume. Thus, the evidence is consistent with a stronger decrease in sellers' valuations compared to the decrease in buyers' valuations.

4.4 Examination of potential confounders

I now turn to the analysis of potential confounders that may challenge my results' interpretation, namely, changes in insurance premiums, neighborhood composition, and external financing.

4.4.1 Changes in insurance premiums

Since I do not control for the cost of individual households' home insurance policy, one may be concerned that differential changes in insurance premium across the new risk zone designation boundary may drive the estimates. To put the magnitude of insurance premiums in perspective, recall that fire damages are covered under regular home insurance. In May 2007 (prior to the new policy implementation), mean (median) quotes in zipcodes predominantly located on the future new risk zone designation amounted to \$1363 (\$1471) per year, with standard deviation of \$448 (based on over 150 quotes). For a discontinuous change in insurance premium to fully explain the estimates in Table 3, the differential change in premium across the designation boundary would have to be well over 100% its baseline value (using a 3% discount rate over a 30-year horizon). Such increases are not authorized by the California Department of Insurance (CDI) since they are not justified by a change in one of CDI's authorized wildfire risk factors (Cignarale et al., 2017; Dixon et al., 2018).[22]

[22]According to Dixon et al. (2018), average premium per policy in 2014 was \$1160 in the 24 high-risk zipcodes of the Western part of San Bernardino County (and \$780 and \$930 in the 16 low-risk and 13 medium-risk zipcodes, respectively) based on insurance contracts from five admitted insurers. The rate per \$1,000 of coverage rose 15% between 2007 and 2014 in the high-risk zipcodes. Note that in low- (high-)risk zipcodes 98% (91%) of policies are purchased in the admitted insurance market, while other homeowners may purchase policies through the State FAIR program. Furthermore, given current insurance regulations, the behavior of insurers and policyholders, and taking climate change into account, Dixon et al. (2018) predict

More formally, I examine the effect of new the risk zone designation on insurance premium quote using difference-in-differences regressions (Table C1). I collected annual insurance premium quotes from CDI for the median home in my sample based on price, size, and age characteristics. The quotes are at the zipcode level from the years 2004 to 2011 (with the exception of year 2008, which was not available) and come from admitted insurers in Los Angeles, San Bernardino, Riverside, Orange, and San Diego counties. The quote for a given zipcode is assigned as treated if the zipcode is predominantly located on the new risk zone designation, and as control otherwise. The variable $Post$ indicates post 2007. In the specification with county fixed effects, quotes in zipcodes predominantly on the new risk zone designation ($Treat$) face an additional premium of $78 per year. However, specifications both with or without county fixed effects indicate no significant effect (at the 10% level) of the new risk designation on insurance premium quotes. I do not examine this confounder in the context of the burn scar view treatment since it would be difficult (and in addition not authorized) for insurers to discriminate premiums across burn scar view status, all else equal.

4.4.2 Changes in neighborhood composition

My quasi-experimental design exploits home fixed effects to control for households sorting into neighborhoods with different risk levels having potentially different preferences, including their propensity to react to the salience of risk. However, one may still be concerned that the type of households sorting into treated or control homes may change before and after treatment. For example, changes in the salience of risk may be confounded with changes in

that the market share of the admitted insurers and the rate per $1,000 of coverage in the admitted market are expected to show improvement or little change on average by 2055 in the zipcodes that currently face the highest fire risk in the region.

risk belief priors if a greater share of households move in from regions with different wildfire risk experience. To investigate such shifts in neighborhood composition I examine whether the demographics of the buyers change in response to the treatment. To this end, I link the transactions data, using the full sample of homes (rather than only the repeat-sales homes), to the two datasets containing buyers' income, race, and ethnicity, and address of origin. For each of these variables, I use a difference-in-differences analysis with census tract fixed effects and time fixed effects to examine correlations between the effect of the treatment on buyer characteristics at the neighborhood level.

Linking the full sample of sales to the HMDA dataset, I do not find evidence that neighborhood composition, as measured by income, race, and ethnicity, correlates with changes in risk zone designation or burn scar view (Tables C4 and C5). Moreover, to the extent that risk preferences are influenced by these demographic characteristics, these results suggest that risk preferences do not substantially change in response to the two treatments.

To test whether the households moving in treated homes in response to the treatments may have different wildfire risk priors, I link the sales transaction dataset with the Zillow Transaction and Assessment Dataset (ZTRAX), which contains the address of origin of the buyers. One caveat is that for the majority of buyers, the address of origin in the Zillow data is registered to that of the new home. Omitting these observations, buyer's address of origin is identified for 11% of homes. Of those, 95% come from California. Using a difference-in-differences framework, I do not find evidence that the number of California buyers changes in response to the two treatments (columns (3) and (4) in Tables C2 and C3). These results provide suggestive evidence that households' wildfire risk belief priors, as proxied by state of origin, do not change dramatically.

4.4.3 Changes in loan to value ratio

Another concern may be that the estimates are confounded by changes in access to external financing. For example, if lending agencies perceive treated homes as riskier relative to control homes (all else equal), and reduce loan amounts, one would expect the loan to value ratio to decrease. To explore this mechanism, I construct a loan amount to purchase price ratio using the HMDA data and methodology described above. I use difference-in-differences analyses to examine the effect of the risk designation and burn scar view treatments on the loan to value ratio (columns (5) and (6) in Tables C2 and C3). Overall, I do not find evidence that the burn scar view treatment is correlated with the loan to value ratio or that the new risk designation is correlated with the loan to value ratio around the policy discontinuity, i.e., within 500m. Further away from the new risk designation boundary (between 500m and 1km), the loan to value ratio estimates decrease by 14% to 17% (significant at the 10% and 5% level, respectively). This change may be explained by a larger change in risk across treated and control homes since these homes are located between 1km and 2km apart. This might be the case, for example, if the risk gradient tilts upward over time, with a greater risk increase near the wilderness relative to that for control homes farther inside the urban area (see Figure 3). This result strengthens the importance of my spatial discontinuity design by focusing on homes just around the designation boundary as the risk (or other confounders) is more likely to change differentially over time the farther away homes are located from each other.

5 Conclusions

Using the southern Californian residential real estate market as a laboratory, the present paper offers evidence suggesting that the salience of risk affects residential real estate prices. Homes assigned to a new risk designation face prices that are 10.2% to 10.8% lower relative to those of nearby homes that do not change risk designation status, and homes with a view of a burn scar sell for 4.2% to 5.0% less than neighboring homes for which the burn scar is not visible one year post-fire. Salience of risk – or its absence – may therefore contribute to explaining why households underestimate risk, including climate risk, and locate in disaster-prone areas, as documented in Giglio et al. (2018) and Bakkensen and Barrage (2019). In addition, the effect of the assignment of risk zones is of direct relevance to policy-makers since risk zoning is a common management tool to inform local residents of underlying natural disaster risks. My findings suggest that risk zone designations can be an effective way to influence prices and, thus, future resource allocation. Taking the results and their interpretation at face value, one may further conjecture that salience of risks may affect households' risk-taking behavior in other domains as well, including retirement savings and health insurance.

However, this study is subject to several caveats. First, to be sure of the suggested interpretation of the evidence as salience effects, ideally one would measure the effect of greater wildfire risk salience on household risk perceptions rather than the effect on asset prices. Interpreting the estimates as the effect of greater risk salience on home prices requires that potential confounding factors are uncorrelated with the treatments. I find no evidence that changes in insurance premiums, neighborhood composition, or access to lending are correlated with the treatments. In addition, the two types of risk salience that I investigate, namely, policy-driven and experiential, are unlikely to be subject to the same confounders, alleviating concerns that a single factor might confound both sets of the results. As such, the evidence presented appears most consistent with risk salience effects. In particular, an alternative interpretation of the effects of the new risk designation treatment is that of information disclosure. I do not find direct supporting evidence for this interpretation – the vast majority of buyers come from the region rather than from a different state and are therefore most likely already familiar with wildfire risk. However, this analysis does not rule out that the new risk designation policy conveys relevant information. The burn scar effects, however, are more difficult to interpret as an information effect. Taken together, therefore, I interpret the results as pointing to a salience effect.

Second, the spatial discontinuity with home fixed effects design that the present study exploits is aimed towards producing estimates with high internal validity. This stringent design requires effectively discarding the majority of the sales transactions in the original dataset. Whereas the full and repeat-sales samples are comparable along observable characteristics, they may differ in unobservable ways. This concern may limit the external validity of the estimates.

Finally, multiple theories offer mechanisms that can explain how salience can lead to observed deviations from standard models' predictions, including, among others, bounded-rationality (Chetty et al., 2009) and decision weights with salient payoffs (Bordalo et al., 2012). Further, risk salience effects on asset prices may arise even if current market participants' risk perceptions are unaffected by risk salience, but instead beliefs about future market participants' beliefs are affected. The quasi-experimental design I exploit does not allow me to identify which of these mechanisms (or which combination thereof) generate the results – a caveat my study shares with other empirical research on risk salience (e.g., Giglio et al. (2018) and Frydman and Wang (2019)). I leave this important question for future research.

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List of tables

Table 1 Summary characteristics for the full and repeat sales samples (with pooled sales).

	Full sales sample		Repeat-sales sample	
	Mean (sd)		Mean (sd)	
Sale price (k$2015)	514.92	(568.75)	503.34	(304.67)
Age	38.65	(23.98)	36.81	(24.85)
Living area (k sqft)	1.84	(0.72)	1.88	(0.74)
# bedrooms	3.32	(0.82)	3.35	(0.82)
# bathrooms	2.34	(0.82)	2.39	(0.82)
Swimming pool (0/1)	0.20	(0.40)	0.20	(0.40)
Dist. green space (km)	0.56	(0.49)	0.57	(0.51)
Elevation (m)	241.10	(194.64)	261.11	(197.84)
Slope	2.92	(4.23)	2.94	(4.22)
FHSZ (0/1)	0.07	(0.26)	0.08	(0.27)
WUI (0/1)	0.47	(0.50)	0.50	(0.50)
Dist. main road (km)	1.42	(1.17)	1.45	(1.20)
Median hh. income (k$)	75.89	(28.02)	76.05	(27.81)
% white	65.47	(18.10)	65.84	(17.43)
% hispanic	38.29	(24.44)	38.68	(23.80)
Years between sales	3.27	(3.56)	5.31	(3.12)
# of sales	1,455,186		862,000	
# of census tracts	4,084		4,017	

Table 2 Summary characteristics of the repeat-sales homes for different distances from the new risk designation boundary. Treated homes are newly assigned to the 2007 risk designation. Controls do not change risk designation assignment, either they are always located outside the (new or old) risk designation or are always inside the old risk designation. Panel A: Home and neighborhood characteristics. Panel B: Buyer demographics linked to home and neighborhood characteristics. Panel B comes from matching the sales transaction data from Panel A with the HMDA data to obtain demographic information (income, race, and ethnicity) of the buyer for the most recent sale. Note that Panel B depicts a subsample of the sales in Panel A (matching rate of 50.2%).

	0-500m				500m-1km			
	Treated		Control		Treated		Control	
	Mean	(sd)	Mean	(sd)	Mean	(sd)	Mean	(sd)
	Home and neighborhood characteristics							
Sale price (k$2015)	519.26	(228.47)	773.92	(456.06)	509.92	(195.09)	691.21	(333.79)
Age	24.66	(14.45)	22.72	(16.17)	17.04	(8.26)	21.93	(17.53)
Living area (k sqft)	2.10	(0.52)	2.39	(0.85)	2.37	(0.50)	2.27	(0.78)
# bedrooms	3.39	(0.66)	3.51	(0.77)	3.62	(0.56)	3.53	(0.74)
# bathrooms	2.44	(0.68)	2.90	(0.89)	2.70	(0.64)	2.79	(0.81)
Swim. pool (0/1)	0.21	(0.41)	0.26	(0.44)	0.18	(0.39)	0.24	(0.42)
Dist. greenness (km)	0.78	(0.41)	0.57	(0.48)	0.68	(0.51)	0.46	(0.43)
Elevation (m)	444.38	(65.76)	246.90	(127.64)	490.14	(53.34)	218.50	(110.49)
Slope	5.19	(3.33)	5.61	(4.41)	5.47	(2.64)	4.67	(4.12)
WUI (0/1)	1.00	(0.00)	0.99	(0.11)	1.00	(0.00)	0.92	(0.26)
Dist. road (km)	3.74	(1.51)	2.01	(1.52)	4.10	(2.10)	1.56	(1.30)
Med. income (k$)	96.53	(13.42)	102.38	(29.07)	93.35	(11.94)	94.23	(26.18)
% white	93.85	(3.33)	81.35	(14.77)	94.80	(2.67)	77.70	(14.18)
% hispanic	11.01	(5.05)	17.41	(13.10)	12.37	(5.71)	22.02	(15.67)
Yrs b/w sales	6.04	(1.88)	3.78	(2.07)	7.28	(1.79)	3.36	(1.86)
# of sales	308		4076		100		3170	
	Panel B: Home and neighborhood characteristics + buyer demographics							
Sale price (k$2015)	778.48	(455.64)	648.68	(330.35)	917.06	(419.10)	567.20	(330.53)
Age	35.80	(27.11)	40.74	(22.92)	38.17	(29.70)	45.54	(23.48)
Living area (k sqft)	2.19	(0.94)	1.91	(0.83)	2.19	(0.64)	1.71	(0.79)
# bedrooms	3.34	(0.88)	3.33	(0.85)	3.33	(0.84)	3.16	(0.89)
# bathrooms	2.76	(1.03)	2.48	(0.93)	2.67	(0.69)	2.22	(0.91)
Swim. pool (0/1)	0.17	(0.38)	0.20	(0.40)	0.33	(0.49)	0.15	(0.36)
Dist. greenness (km)	0.34	(0.41)	0.41	(0.36)	0.15	(0.14)	0.42	(0.32)
Elevation (m)	118.06	(78.49)	137.19	(100.53)	164.15	(110.87)	120.43	(102.95)
Slope	5.35	(5.63)	3.74	(4.05)	9.13	(7.11)	3.43	(4.36)
WUI (0/1)	0.96	(0.20)	0.81	(0.39)	1.00	(0.00)	0.51	(0.50)
Dist. road (km)	1.41	(1.02)	1.32	(1.03)	1.59	(0.91)	1.04	(0.99)
Med. income (k$)	98.90	(41.08)	82.93	(30.88)	125.51	(29.82)	74.75	(29.60)
% white	72.18	(22.85)	70.25	(20.07)	74.02	(18.06)	66.55	(19.65)
% hispanics	22.68	(22.85)	30.01	(25.00)	12.28	(18.97)	32.85	(23.92)
Yrs b/w sales	6.08	(2.40)	4.90	(2.54)	7.23	(1.98)	4.78	(2.63)
HMDA inc. (k$)	182.02	(175.57)	150.39	(117.48)	223.83	(155.54)	126.81	(82.43)
HMDA white	0.74	(0.44)	0.68	(0.47)	0.78	(0.43)	0.70	(0.46)
HMDA hispanics	0.14	(0.35)	0.22	(0.41)	0.00	(0.00)	0.18	(0.38)
# recent buyers	138		380		18		282	

Table 3 Effect of the new risk zone designation on home prices. (1) and (2): Controls do not change risk designation assignment, either they are always located outside the (new or old) risk designation or are always inside the old risk designation. (3) and (4): Control homes are, in addition, restricted to being located in the same zipcodes as treatment homes. (5) and (6): Control homes are restricted to always being located outside the risk designation (new or old). (7) and (8): Control homes are restricted to always being located inside the risk designation (new or old). All specifications include home fixed effects and, either quadratic county trends and year by quarter fixed effects, or county by year by quarter fixed effects.

	Selection of the control group							
	Outside new zone or inside old zone				Outside new zone		Inside old zone	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: 0-500m							
NewDesignation×Post	-0.108***	-0.102***	-0.074***	-0.063***	-0.104***	-0.112***	-0.110***	-0.114***
	(0.025)	(0.030)	(0.020)	(0.020)	(0.030)	(0.034)	(0.040)	(0.061)
N	4384	4384	2100	2100	2968	2968	1724	1724
R^2_{adj}	0.76	0.78	0.79	0.81	0.82	0.84	0.74	0.76
	Panel B: 500m-1km							
NewDesignation×Post	-0.106**	-0.125**	-0.072*	-0.073*	-0.108**	-0.119**	-0.022	-0.025
	(0.044)	(0.050)	(0.036)	(0.040)	(0.054)	(0.059)	(0.061)	(0.093)
N	3268	3268	1234	1234	2978	2978	390	390
R^2_{adj}	0.86	0.87	0.86	0.87	0.86	0.87	0.84	0.88
Home FE	✓	✓	✓	✓	✓	✓	✓	✓
Trends	✓		✓		✓		✓	
Yr×Qtr	✓		✓		✓		✓	
Cty×Yr×Qtr		✓		✓		✓		✓
Same zipcode			✓	✓				

Note: Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table 4 Placebo tests - Effect of the new risk zone designation when homes always in the old risk zone are assigned to treatment. (1) and (2): Control homes are restricted to always being located outside the risk designation (new or old). (3) and (4): Control homes are, in addition, restricted to being located in the same zipcodes as treatment homes. All specifications include home fixed effects and, either quadratic county trends and year by quarter fixed effects, or county by year by quarter fixed effects.

	Control group outside new risk zone			
	(1)	(2)	(3)	(4)
	Panel A: 0-500m			
NewDesignation×Post	0.034	0.030	-0.035	-0.043
	(0.035)	(0.042)	(0.048)	(0.051)
N	4076	4076	1792	1792
R^2_{adj}	0.74	0.77	0.75	0.77
	Panel B: 500m-1km			
NewDesignation×Post	-0.070	-0.031	-0.050	0.005
	(0.052)	(0.067)	(0.056)	(0.053)
N	3168	3168	1134	1134
R^2_{adj}	0.86	0.87	0.86	0.87
Home FE	✓	✓	✓	✓
Trends	✓		✓	
Yr×Qtr	✓		✓	
County×Yr×Qtr		✓		✓
Same zipcode			✓	✓

Note: Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table 5 Burn scar view estimates for the 0-2km and 2-4km bins. (1) and (2): the viewshed is determined using a three-dimensional elevation model (DEM). (3) and (4): the viewshed is determined using a three-dimensional surface model (DSM) with LiDAR data that take into account the vegetation and structures on the ground. (5) and (6): Placebo tests are run for burn scars that will happen one or two years AFTER the sale. The subscripts 1 and 2 refer to the year post-fire for which a coefficient is reported, except in the placebo in which case it refers the year pre-fire. All specifications include home fixed effects and, either quadratic county trends and year by quarter fixed effects, or county by year by quarter fixed effects.

	DEM		LiDAR		Placebo	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: 0-2km			
View_1	-0.042***	-0.050***	-0.026*	-0.033**	-0.017	0.0002
	(0.015)	(0.013)	(0.015)	(0.014)	(0.013)	(0.012)
View_2	-0.020	-0.022	-0.004	0.010	-0.030	-0.010
	(0.015)	(0.013)	(0.017)	(0.015)	(0.019)	(0.015)
$\text{View}_1 \times \text{Large}_1$	0.007	0.007	-0.006	0.002	0.039**	0.016
	(0.018)	(0.017)	(0.021)	(0.016)	(0.016)	(0.012)
$\text{View}_2 \times \text{Large}_2$	0.002	-0.009	-0.004	-0.012	0.007	0.021*
	(0.018)	(0.016)	(0.017)	(0.015)	(0.017)	(0.012)
N	10573	10573	5658	5658	8030	8030
R^2_{adj}	0.84	0.86	0.88	0.90	0.85	0.88
			Panel B: 2-4km			
View_1	-0.019**	-0.032***	-0.027**	-0.027**	-0.010	-0.013*
	(0.009)	(0.008)	(0.012)	(0.012)	(0.008)	(0.008)
View_2	-0.017**	-0.026***	-0.022**	-0.018*	-0.032***	-0.025***
	(0.008)	(0.007)	(0.011)	(0.011)	(0.009)	(0.008)
$\text{View}_1 \times \text{Large}_1$	-0.008	-0.008	-0.011	-0.010	-0.003	-0.013
	(0.014)	(0.014)	(0.020)	(0.019)	(0.011)	(0.011)
$\text{View}_2 \times \text{Large}_2$	0.010	0.004	-0.010	-0.006	0.017	0.019
	(0.014)	(0.012)	(0.019)	(0.018)	(0.013)	(0.013)
N	24770	24770	9248	9248	18976	18976
R^2_{adj}	0.87	0.88	0.87	0.88	0.87	0.89
Home FE	✓	✓	✓	✓	✓	✓
Trends	✓		✓		✓	
Yr×Qtr	✓		✓		✓	
County×Yr×Qtr		✓		✓		✓

Note: Robust standard errors clustered at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

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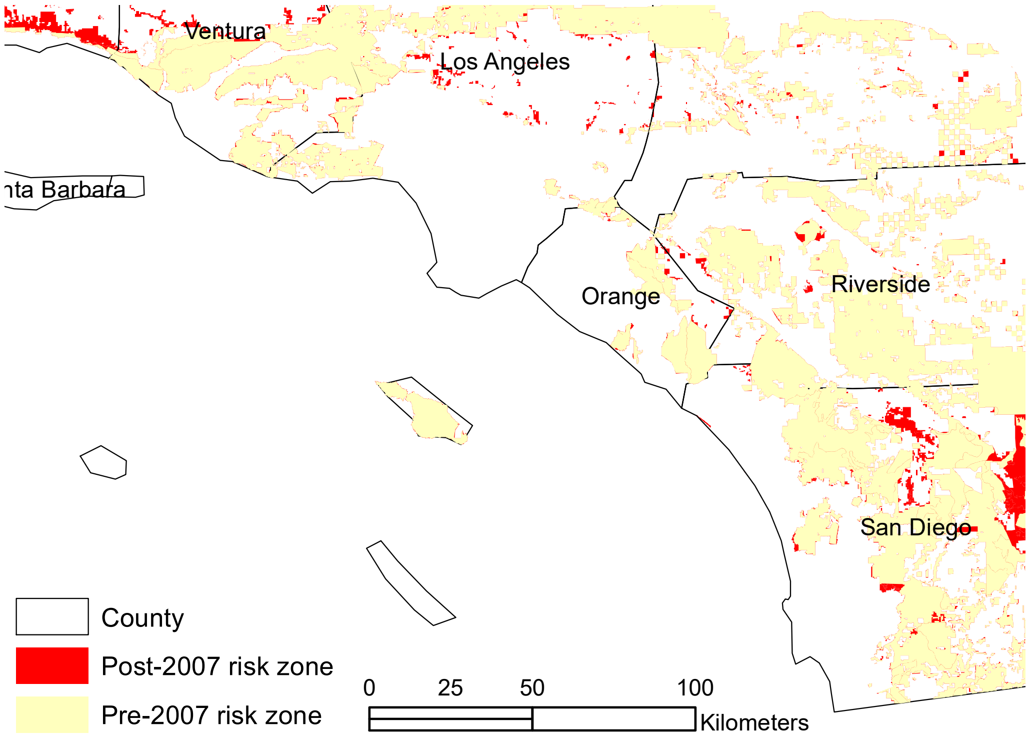


Figure 1 California wildfire risk zone designation for the state responsibility areas, including the new marginal risk zone designation expansion adopted in November 2007.

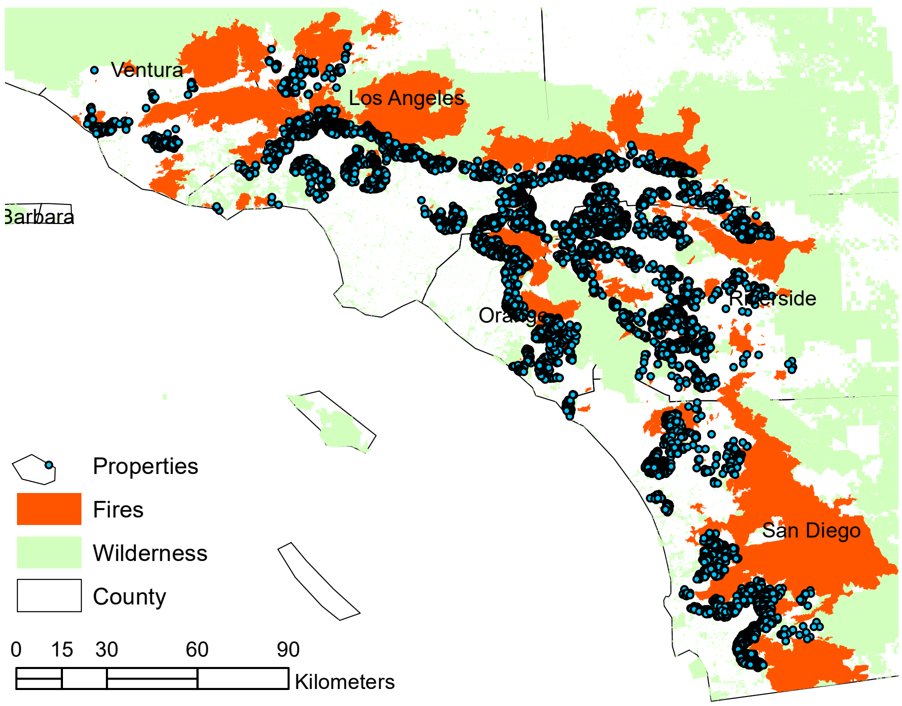


Figure 2 Wildfire perimeters between 1998 and 2015 and repeat-sales homes sold within 4km of burn scar and during the first two years post-fire.

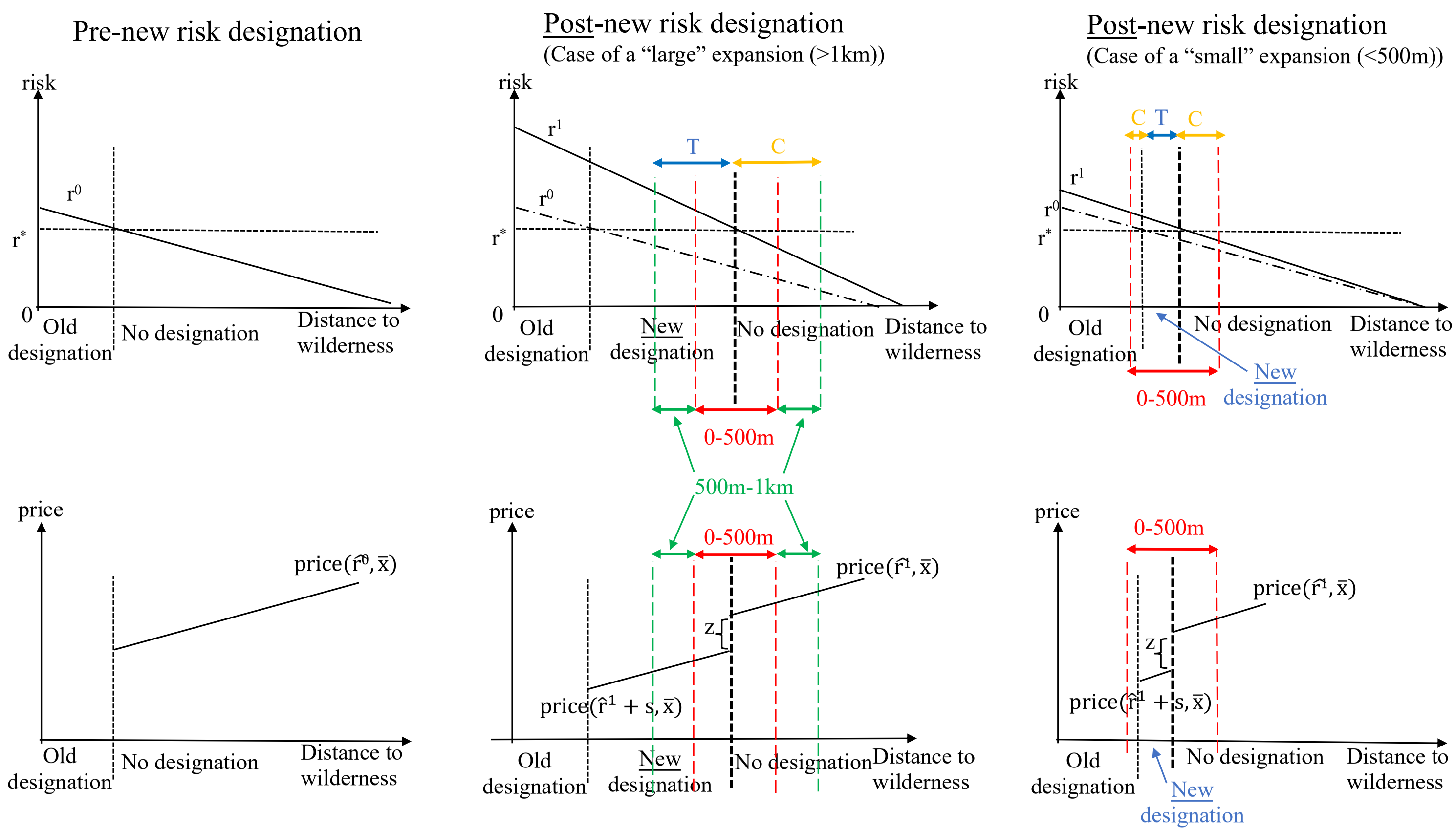
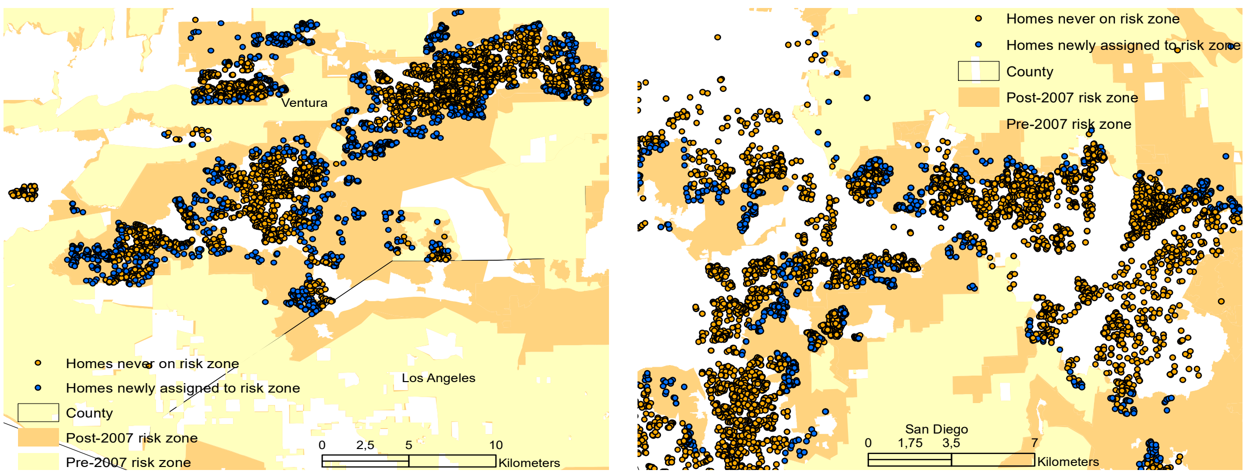


Figure 3 Illustration of the new risk zone designation quasi-experimental design and its potential effect on home prices. Note: top panels depict the regulator's estimated wildfire risk (r) gradient for homes based on their distance from the wilderness. Pre-2007, the regulator had assigned the region for which the risk r^0 exceeded a threshold r^* to the old risk designation (left panels). As the risk in the wilderness increases from r^0 to r^1, the old designation is expanded with a new risk designation in 2007 (middle and right panels). (Risk may increase because of climate change and/or better data and science. Alternatively, instead of the risk increasing, the regulator may decide to reduce the threshold r^*. The results interpretation remains valid under this alternative.) The magnitude of the new risk designation expansion varies across locations depending on the change in risk. For 'large' expansions (middle panels), treated homes ("T") within 1km are compared to control homes always outside the new risk designation ("C"). Alternatively, for expansions smaller than 1km (or 500m as used in the right panels), the treated homes ("T") will cover the entire bandwidth between the old and new risk designation boundary. As a result, treated homes can be compared to control homes always outside the new risk designation ("C") and homes both inside the old and new designations ("C"). Bottom panels depict home prices given households' perceived wildfire risk $\hat{r}$ (all else equal; $\bar{x}$). Pre-new risk designation, households' perceived risk is informed by continuous variables driving risk ($\hat{r}^0$), such as distance from the wilderness. In contrast, post-new risk designation households located inside the new risk designation additionally receive risk designation information (s) when forming their risk perceptions ($\hat{r}^1 + s$). (Home prices in the old risk designation are not depicted as a price discontinuity may exist but I do not have data pre-old risk designation to identify the effect.)

(a) (b)

Figure 4 Visual evidence supporting the common trends assumption for average yearly home prices for the repeat-sales homes newly assigned to the risk designation (treated group) and those not affected by the new designation (control group). Panel (a) includes homes within 500m from the risk designation boundary, while panel (b) includes homes 1km from the boundary.



(a) (b)

Figure 5 Example of homes newly assigned to the 2007 risk designation and homes always outside the risk designation in (a) Ventura County and (b) San Diego County.

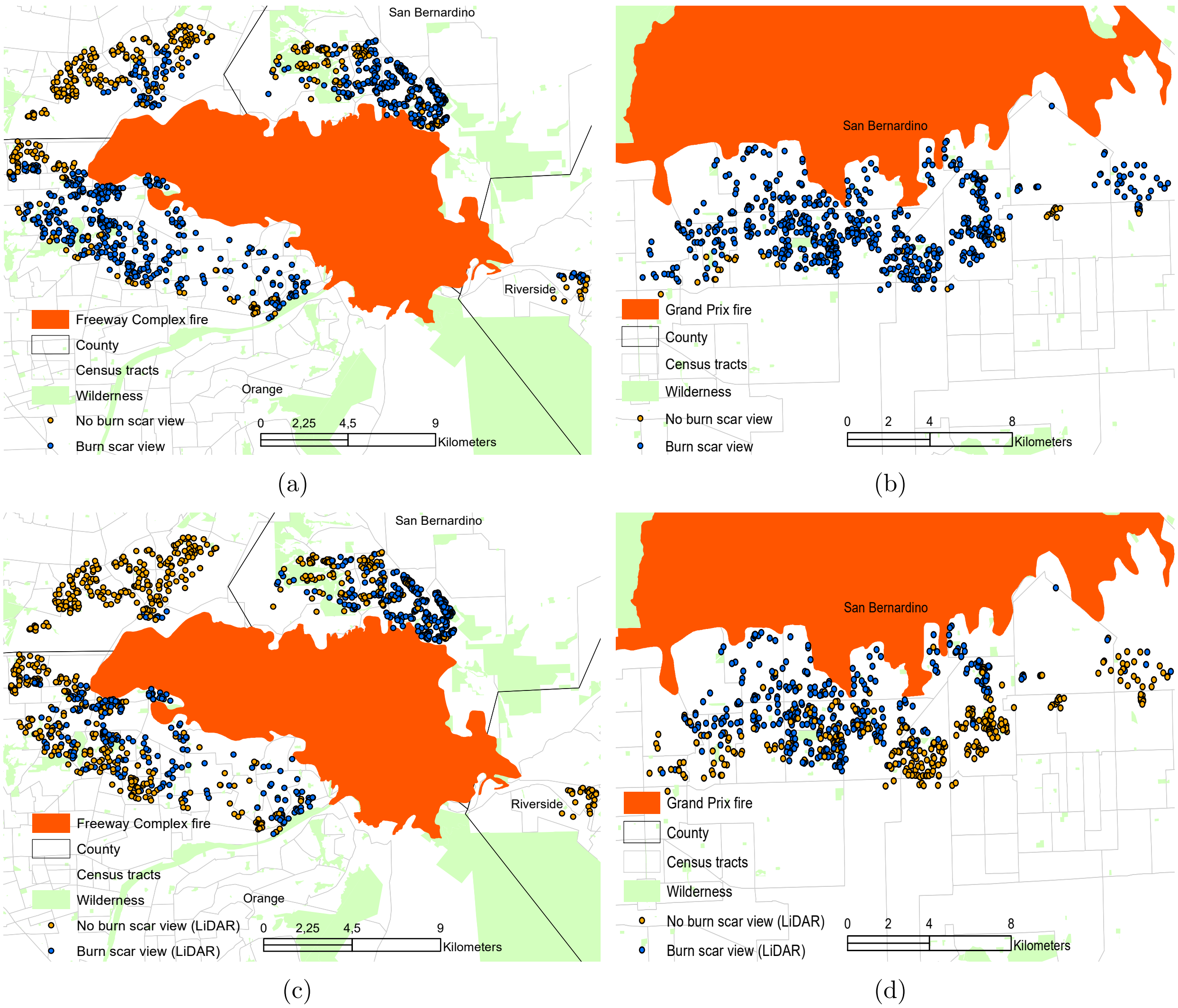


Figure 6 Homes with or without burn scar view sold within 4km and two years post-fire. Left panels show the 2008 Freeway Complex Fire in Orange County, while right panels show the 2003 Grand Prix Fire in Los Angeles County. Top panels use a bare earth model (DEM) to construct the viewshed, while the bottom panels use LiDAR data.

Internet Appendix

A Additional data description

A.1 New risk zone designation treatment

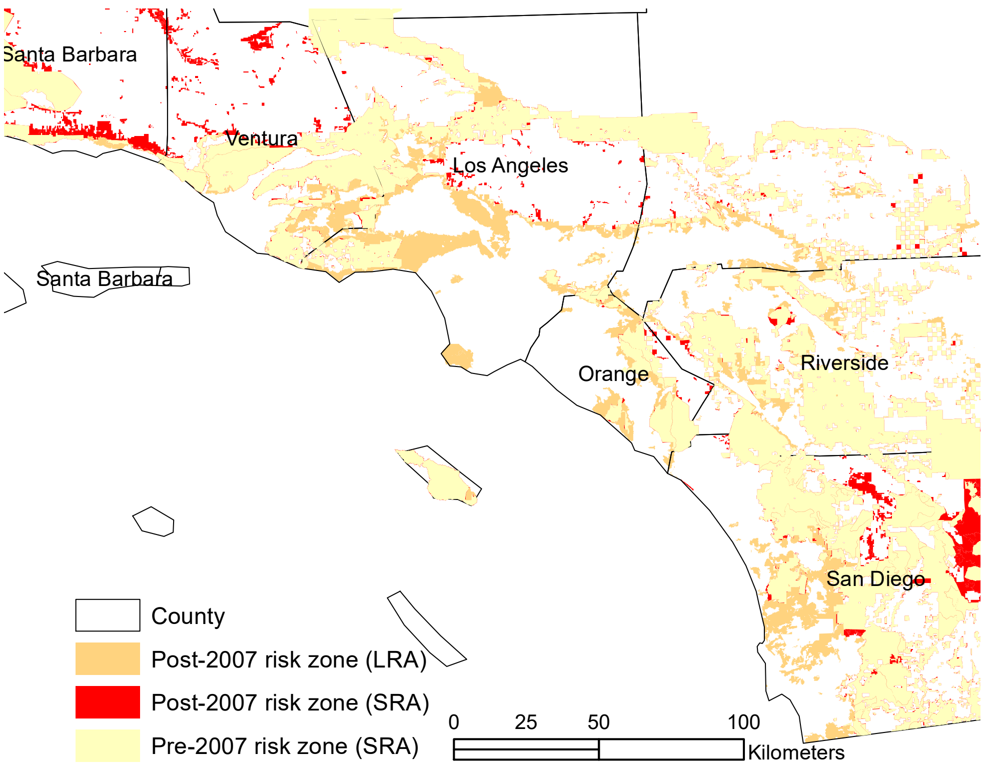
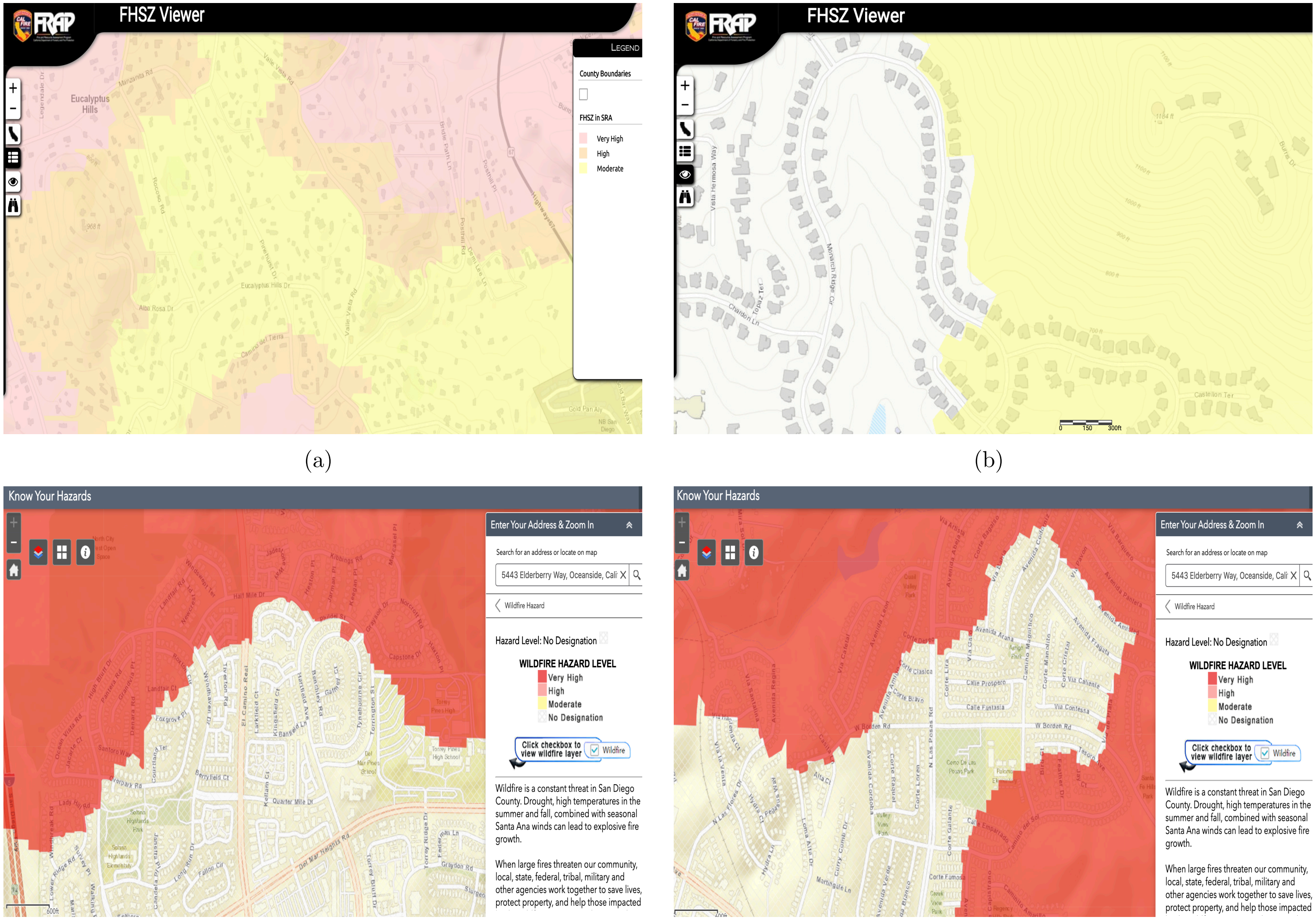


Figure A1 California wildfire risk zone designation, including the new risk designation adopted in the state responsibility areas (SRA; the focus of the present study) in 2007; as well as the recommended risk designation for the local responsibility areas (LRA) adopted on a voluntary basis – each LRA was able to decide whether to adopt and/or modify the recommended designation. (I omit sales in the LRA post-2007 in the analysis.)



(a) (b)

(c) (d)

Figure A2 Examples of wildfire risk designations in the state (top panels) and local (bottom panels) responsibility areas, respectively, in San Diego County. (Source: http://egis.fire.ca.gov/FHSZ.)

Table A1 Summary characteristics of the repeat-sales homes for different distance bins from the new risk designation boundary. Treated homes are newly assigned to the 2007 risk designation. Controls do not change risk designation assignment, either they are always located outside the (new or old) risk designation or are always inside the old risk designation. Control homes are, in addition, restricted to being located in the same zipcodes as treatment homes.

	0-500m				500m-1km			
	Treated		Control		Treated		Control	
	Mean (sd)		Mean (sd)		Mean (sd)		Mean (sd)	
Sale price (k$2015)	519.26	(228.47)	632.92	(373.25)	509.92	(195.09)	619.28	(270.28)
Age	24.66	(14.45)	25.34	(15.04)	17.04	(8.26)	22.38	(14.95)
Living area (k sqft)	2.10	(0.52)	2.25	(0.75)	2.37	(0.50)	2.18	(0.69)
# bedrooms	3.39	(0.66)	3.39	(0.76)	3.62	(0.56)	3.40	(0.72)
# bathrooms	2.44	(0.68)	2.68	(0.80)	2.70	(0.64)	2.64	(0.66)
Swimming pool (0/1)	0.21	(0.41)	0.26	(0.44)	0.18	(0.39)	0.26	(0.44)
Dist. green space (km)	0.78	(0.41)	0.67	(0.50)	0.68	(0.51)	0.56	(0.48)
Elevation (m)	444.38	(65.76)	315.62	(119.95)	490.14	(53.34)	289.26	(111.98)
Slope	5.19	(3.33)	5.63	(3.95)	5.47	(2.64)	4.33	(3.24)
FHSZ (0/1)	0.50	(0.50)	0.47	(0.50)	0.50	(0.50)	0.15	(0.36)
WUI (0/1)	1.00	(0.00)	0.99	(0.07)	1.00	(0.00)	0.97	(0.16)
Dist. main road (km)	3.74	(1.51)	2.10	(1.77)	4.10	(2.10)	1.55	(1.40)
Median hh. income (k$)	96.53	(13.42)	91.12	(23.73)	93.35	(11.94)	91.14	(22.70)
% white	93.85	(3.33)	87.91	(6.69)	94.80	(2.67)	85.21	(6.24)
% hispanics	11.01	(5.05)	16.14	(10.13)	12.37	(5.71)	16.50	(8.96)
Years between sales	6.04	(1.88)	3.97	(2.11)	7.28	(1.79)	3.57	(2.00)
# of sales	308		1792		100		1134	

Table A2 Personal communication with mortgage lenders about the effect of the new risk designation on lending practices. List of the mortgage lenders contacted.

Name	Dan O'Brien	Jamie Cavanaugh	Bryan Hitchock	Maureen Martin	Jamie Mckeon
Company	Landmark Financial	Hillhurst Mortgage	Chase Bank	Maureen Martin	Community Mortgage
Contact info.	805-650-4999	800-570-5626	909-438-8823	619-857-7191	619-857-7192
Position	Mortgage Broker	Mortgage Broker	Senior Loan Officer	Mortgage Broker	Mortgage Broker
Market served	Ventura County	LA County	LA County	San Diego County	San Diego County
Experience	20+ years	-	20+ years	20+ years	6 years
Fire risk zones affect rates or loan amounts?	No	No	No	No	No

A.2 Burn scar view treatment

Table A3 Summary statistics of the wildfires occurring in my study area

Year	Number of fires	Mean fire size (acres)	Min fire size (acres)	Max fire size (acres)	Total area burned (acres)
1998	15	3,727	95	28,136	55,908
1999	11	2,016	107	7,846	22,174
2000	10	1,468	52	11,734	14,679
2001	10	2,325	182	10,438	23,246
2002	19	5,212	65	38,119	99,022
2003	22	33,146	51	270,686	729,204
2004	15	3,305	53	16,447	49,577
2005	11	3,493	65	23,396	38,428
2006	14	6,142	64	40,177	85,990
2007	31	15,192	87	162,070	470,952
2008	13	5,699	65	30,305	74,084
2009	19	10,550	55	160,833	200,459
2010	13	1,264	64	12,582	16,432
2011	8	152	51	411	1,214
2012	9	674	54	2,637	6,063
2013	13	3,904	59	24,060	50,758
2014	11	2,678	78	15,186	29,456
2015	7	459	56	1,287	3,211
1998-2015	251	5,634	51	270,686	1,970,857

Table A4 Summary statistics of the repeat-sales properties that sold during the first two years post-fire for different distances from the burn scar. Panel A: Home and neighborhood characteristics. Panel B: Buyer demographics linked to home and neighborhood characteristics. Panel B comes from matching the sales transaction data from Panel A with the HMDA data to obtain demographic information (income, race, and ethnicity) of the buyer for the most recent sale. Note that Panel B depicts a subsample of the sales in Panel A (matching rate of 53.0%).

	0-2km				2-4km			
	No view		Burn scar view		No view		Burn scar view	
	Mean (sd)		Mean (sd)		Mean (sd)		Mean (sd)	
Panel A: Home and neighborhood characteristics								
Sale price (k$2015)	504.88	(278.67)	515.54	(278.96)	457.71	(263.23)	433.70	(228.00)
Age	26.20	(20.61)	27.79	(21.81)	25.08	(20.28)	29.32	(23.19)
Living area (k sqft)	2.17	(0.86)	2.01	(0.77)	2.15	(0.80)	1.95	(0.72)
# bedrooms	3.55	(0.84)	3.45	(0.79)	3.55	(0.81)	3.42	(0.80)
# bathrooms	2.70	(0.86)	2.59	(0.81)	2.67	(0.78)	2.47	(0.77)
Swim. pool (0/1)	0.25	(0.43)	0.19	(0.39)	0.21	(0.41)	0.18	(0.38)
Dist. greenness (km)	0.54	(0.50)	0.47	(0.44)	0.60	(0.60)	0.56	(0.51)
Elevation (m)	258.79	(167.40)	274.60	(174.72)	288.60	(160.83)	307.59	(186.96)
Slope	5.88	(5.79)	3.51	(3.90)	4.05	(4.59)	2.36	(3.11)
FHSZ (0/1)	0.23	(0.42)	0.17	(0.37)	0.16	(0.37)	0.05	(0.21)
WUI (0/1)	0.81	(0.39)	0.80	(0.40)	0.72	(0.45)	0.51	(0.50)
Dist. road (km)	1.76	(1.17)	1.38	(1.19)	1.50	(1.28)	1.27	(1.06)
Dist. burn scar (km)	1.36	(0.46)	1.12	(0.56)	3.28	(0.54)	2.97	(0.55)
Days since fire	421.31	(199.00)	424.96	(205.77)	444.52	(203.55)	436.93	(208.58)
Med. income (k$)	85.59	(28.84)	84.36	(25.30)	83.43	(25.69)	76.30	(24.20)
% white	72.66	(14.39)	68.45	(13.56)	69.09	(15.40)	68.14	(13.69)
% hispanic	31.30	(18.47)	32.68	(22.27)	31.65	(17.78)	36.81	(21.12)
Yrs b/w sales	4.86	(2.16)	4.86	(2.13)	4.82	(2.21)	4.79	(2.17)
# of sales	2174		8398		12234		12522	
Panel B: Home and neighborhood characteristics + buyer demographics								
Sale price (k$2015)	508.72	(295.69)	505.68	(273.27)	459.30	(257.97)	428.53	(222.73)
Age	27.22	(20.50)	28.44	(21.54)	26.97	(20.68)	31.06	(23.53)
Living area (k sqft)	2.11	(0.86)	1.96	(0.77)	2.06	(0.76)	1.90	(0.70)
# bedrooms	3.52	(0.83)	3.44	(0.77)	3.51	(0.80)	3.39	(0.78)
# bathrooms	2.67	(0.90)	2.57	(0.80)	2.61	(0.76)	2.43	(0.76)
Swim. pool (0/1)	0.23	(0.42)	0.19	(0.39)	0.21	(0.41)	0.18	(0.39)
Dist. greenness (km)	0.53	(0.46)	0.46	(0.42)	0.59	(0.59)	0.55	(0.49)
Elevation (m)	251.07	(161.76)	274.26	(174.99)	278.22	(158.59)	310.05	(191.11)
Slope	5.82	(5.89)	3.46	(3.75)	4.03	(4.57)	2.31	(2.97)
FHSZ (0/1)	0.31	(0.46)	0.25	(0.43)	0.21	(0.41)	0.08	(0.26)
WUI (0/1)	0.79	(0.41)	0.80	(0.40)	0.72	(0.45)	0.50	(0.50)
Dist. road (km)	1.80	(1.14)	1.38	(1.21)	1.52	(1.33)	1.28	(1.08)
Dist. burn scar (km)	1.35	(0.46)	1.12	(0.56)	3.26	(0.55)	2.97	(0.54)
Days since fire	427.67	(201.34)	430.44	(205.05)	445.98	(204.06)	443.72	(208.83)
Med. income (k$)	84.29	(29.29)	83.13	(24.75)	82.31	(26.52)	75.24	(23.93)
% white	72.75	(14.41)	68.27	(13.64)	69.36	(15.04)	67.95	(14.05)
% hispanics	32.06	(19.10)	34.19	(22.69)	32.41	(18.37)	37.80	(21.33)
Yrs b/w sales	4.96	(2.18)	4.84	(2.13)	4.82	(2.23)	4.83	(2.19)
HMDA inc. (k$)	128.87	(120.98)	114.54	(92.21)	109.51	(81.92)	102.11	(79.28)
HMDA white	0.70	(0.46)	0.68	(0.47)	0.69	(0.46)	0.73	(0.44)
HMDA hispanics	0.22	(0.42)	0.26	(0.44)	0.25	(0.43)	0.31	(0.46)
# recent buyers	534		2073		2715		3014	

B Additional robustness checks

B.1 New risk designation treatment

Table B1 Effect of the new risk zone designation on home prices over time. The post-new designation period is decomposed into three time periods: $Post_1$: 2007-2009, $Post_2$: 2010-2012, $Post_3$: 2013-2015. The sample is restricted to be located within 1km around the new designation boundary

	0-1km	
	(1)	(2)
NewDesignation×Post_1	-0.082***	-0.078***
	(0.024)	(0.027)
NewDesignation×Post_2	-0.115***	-0.121***
	(0.033)	(0.037)
NewDesignation×Post_3	-0.102***	-0.103***
	(0.036)	(0.038)
Home fixed effects	✓	✓
Quadratic county trends	✓	
Year×Quarter	✓	
County×Year×Quarter		✓
N	7652	7652
R^2_{adj}	0.80	0.81

Note: Robust clustered standard errors at the census-tract level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

B.2 Burn scar view treatment

Table B2 Burn scar view estimates for each 1km bin. The subscripts 1 and 2 refer to the year post-fire for which a coefficient is reported. All specifications include home fixed effects and, either quadratic county trends and year by quarter fixed effects, or county by year and quarter fixed effects.

	0-1km		1-2km		2-3km		3-4km	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
View_1	-0.031	-0.0501**	-0.052***	-0.063***	-0.028**	-0.048***	-0.020**	-0.032***
	(0.025)	(0.022)	(0.016)	(0.016)	(0.013)	(0.014)	(0.010)	(0.010)
View_2	-0.009	-0.015	-0.029*	-0.029*	-0.033***	-0.045***	-0.011	-0.027***
	(0.023)	(0.020)	(0.017)	(0.017)	(0.011)	(0.011)	(0.010)	(0.009)
$\times\text{Large}_1$	0.003	0.016	0.013	0.012	0.002	0.006	-0.044**	-0.035*
	(0.026)	(0.024)	(0.021)	(0.022)	(0.018)	(0.019)	(0.020)	(0.021)
$\times\text{Large}_2$	0.005	-0.011	0.004	-0.006	0.014	0.010	0.005	0.009
	(0.027)	(0.021)	(0.019)	(0.020)	(0.017)	(0.016)	(0.018)	(0.018)
N	4048	4048	6525	6525	9928	9928	14842	14842
R^2_{adj}	0.86	0.87	0.84	0.84	0.86	0.86	0.88	0.87
Home FE	✓	✓	✓	✓	✓	✓	✓	✓
Trends	✓		✓		✓		✓	
Yr×Qtr	✓		✓		✓		✓	
Cty×Yr&Qtr		✓		✓		✓		✓

Note: Robust standard errors clustered at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Effect of proximity to burn scars on home prices

To further test whether burn scars affect home prices through other channels than the visual cues of the damages, I investigate the effect of the distance to the burn scar on home prices. To this end, I focus the analysis on repeat-sales homes for which one of the sales is affected by a wildfire and define the treatment group as homes located within K-km of the burn scar, while the control group consists of homes located between the K-km threshold and 4km. The empirical model shown in the equation below allows for heterogeneity of the proximity effect K_{jit} across the first and second years post-fire $j = \{1, 2\}$, while controlling for properties with a burn scar view ($View_{jit}$).

$$\ln p_{it} = \sum_j (\beta_j K_{jit} + \gamma_j View_{jit} + \delta_j K_{jit} \times View_{jit}) + \lambda_i + \mu_{gt} + \epsilon_{it}.$$

The parameters β_j now reflect the ATT effects of burn scar proximity in year t. I control for property and neighborhood time-invariant unobservables λ_i, and local and macro shocks μ_{gt} through year-by-quarter fixed effects and quadratic county trends, or county-by-year-by-quarter fixed effects. Table B3 shows results for K ranging from 1km to 3km. Treated homes are located within K-km from the burn scar relative to control homes that are further away. The proximity measure is further interacted with the binary indicator for burn scar view. Results show no effect of proximity with estimates that are both statistically and economically insignificant. However, the results indicate a robust price decrease of 2.4% to 3.8% for homes with a burn scar view and within 3km that sold during the first year post-fire. These effects also attenuate in the second year post-fire with price decreases of 1.2% to 3.0%. Overall, these results qualitatively support the burn scar view results in Table 5. (Running separate regressions for homes that have a burn scar view and those that do not, similarly show no effect of the proximity to the burn scar.)

Table B3 Proximity effect estimates within threshold K-km of the burn scar. The subscripts 1 and 2 refer to the year post-fire for which a coefficient is reported. All specifications include home fixed effects and, either quadratic county trends and year by quarter fixed effects, or county by year and quarter fixed effects.

	$K=1$		$K=2$		$K=3$	
	(1)	(2)	(3)	(4)	(5)	(6)
K_1	-0.002	-0.012	-0.003	-0.004	0.011	0.011
	(0.019)	(0.019)	(0.013)	(0.011)	(0.011)	(0.009)
K_2	0.009	0.017	0.014	0.014	0.010	0.011
	(0.025)	(0.025)	(0.013)	(0.012)	(0.010)	(0.009)
View_1	-0.024***	-0.036***	-0.024***	-0.036***	-0.030***	-0.038***
	(0.007)	(0.007)	(0.008)	(0.007)	(0.009)	(0.009)
View_2	-0.013*	-0.026***	-0.017**	-0.030***	-0.016*	-0.030***
	(0.007)	(0.006)	(0.007)	(0.007)	(0.009)	(0.009)
$K_1 \times \text{View}_1$	0.007	0.009	0.006	0.004	0.003	-0.004
	(0.024)	(0.024)	(0.017)	(0.016)	(0.015)	(0.014)
$K_2 \times \text{View}_2$	-0.0003	-0.008	0.003	0.003	0.001	0.002
	(0.026)	(0.025)	(0.016)	(0.014)	(0.013)	(0.012)
N	35343	35343	35343	35343	35343	35343
R^2_{adj}	0.86	0.86	0.86	0.86	0.86	0.86
Home FE	✓	✓	✓	✓	✓	✓
Trends	✓		✓		✓	
Yr×Qtr	✓		✓		✓	
Cty×Yr&Qtr		✓		✓		✓

Note: Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

C Examination of potential confounders

Table C1 Effect of the new risk designation on zipcode-level insurance premium quotes. The quotes come from CDI for the median home in my sample based on price, size, and age characteristics. Quotes come from admitted insurers in Los Angeles, San Bernardino, Riverside,Orange, and San Diego counties and are from the years 2004 to 2011 (with the exception of year 2008). A quote for a given zipcode is assigned as treated if the zipcode is predominantly located on the new risk zone designation, and as control otherwise. *Post* is defined as post 2007.

	(1)	(2)
Treat	-91.84***	78.49***
	(19.44)	(21.04)
Post	-147.0***	-130.2***
	(10.54)	(10.21)
Treat×Post	45.20	28.34
	(29.61)	(28.63)
Constant	1599.5***	
	(7.439)	
N	8763	8763
R^2_{adj}	0.025	0.089
County fixed effect		✓

Note: Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table C2 Difference-in-differences regressions of the new risk designation on outcome variables within five years of the policy change. (1) and (2): Effect of new risk designation on the number of quarterly sales (in log). (3) and (4): Effect of new risk designation on the state of origin of buyers (Outside California is coded as 1; California as 0). Data source: ZTRAX. (5) and (6): Effect of new risk designation on the loan to value ratio (LTV). Data source: HMDA. All specifications include census tract fixed effects and, either quadratic county trends and year by quarter fixed effects, or county by year by quarter fixed effects.

	Sales volume		Buyer origin		LTV	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: 0-500m					
NewDesignation×Post	0.027	0.024	0.046	0.028	0.028	0.038
	(0.035)	(0.033)	(0.039)	(0.041)	(0.022)	(0.030)
N	13458	13458	10823	10823	4247	4247
R^2_{adj}	0.996	0.996	0.004	0.002	0.010	0.033
	Panel B: 500m-1km					
NewDesignation×Post	-0.056	-0.058	-0.034	-0.024	-0.141*	-0.165**
	(0.063)	(0.062)	(0.073)	(0.064)	(0.075)	(0.069)
N	15619	15619	12890	12890	2601	2601
R^2_{adj}	0.996	0.996	0.005	0.013	0.020	0.028
Census tract FE	✓	✓	✓	✓	✓	✓
Trends	✓		✓		✓	
Yr×Qtr	✓		✓		✓	
County×Yr×Qtr		✓		✓		✓

Note: Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table C3 Difference-in-differences regressions of the burn scar view on outcome variables within two years of a wildfire. (1) and (2): Effect of burn scar view on the number of quarterly sales (in log). (3) and (4): Effect of burn scar view on the state of origin of buyers (Outside California is coded as 1; California as 0). Data source: ZTRAX. (5) and (6): Effect of burn scar view on the loan to value ratio (LTV). Data source: HMDA. All specifications include census tract fixed effects and, either quadratic county trends and year by quarter fixed effects, or county by year by quarter fixed effects.

	Sales volume		Buyer origin		LTV	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: 0-2km					
View×PostFire	0.084***	0.081***	-0.008	-0.004	-0.003	0.004
	(0.020)	(0.020)	(0.017)	(0.016)	(0.006)	(0.006)
N	34110	34110	15529	15529	19097	19097
R^2_{adj}	0.94	0.94	0.002	0.005	0.031	0.040
	Panel B: 2-4km					
View×PostFire	0.051***	0.046***	-0.010	-0.018*	-0.005	-0.002
	(0.011)	(0.012)	(0.010)	(0.011)	(0.004)	(0.003)
N	71808	71808	24635	24635	38602	38602
R^2_{adj}	0.91	0.91	0.001	0.003	0.047	0.057
Census tract FE	✓	✓	✓	✓	✓	✓
Trends	✓		✓		✓	
Yr×Qtr	✓		✓		✓	
County×Yr×Qtr		✓		✓		✓

Note: Robust standard errors clustered at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table C4 Difference-in-differences regressions of the effect of the new risk designation on the composition of buyers inside and outside the new risk designation neighborhoods pre and post policy change in 2007. Data source: HMDA. All specifications include census tract fixed effects and, either quadratic county trends and year by quarter fixed effects, or county by year by quarter fixed effects. (Results are robust to restricting the analysis to 1, 2, 3, or 4 year(s) around the time of the policy change in 2007.)

	Income		White		Hispanic	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: 0-500m					
NewDesignation×Post	-10.88	3.877	-0.118	-0.126	-0.034	-0.044
	(15.26)	(17.74)	(0.082)	(0.089)	(0.033)	(0.040)
N	4247	4247	4247	4247	4247	4247
R^2_{adj}	0.011	0.013	0.013	0.014	0.048	0.046
	Panel B: 500m-1km					
NewDesignation×Post	104.4	132.9**	-0.177	-0.051	-0.065	0.117
	(67.60)	(66.76)	(0.252)	(0.306)	(0.121)	(0.106)
N	2601	2601	2601	2601	2601	2601
R^2_{adj}	0.027	0.022	0.022	0.032	0.058	0.078
Census tract FE	✓	✓	✓	✓	✓	✓
Trends	✓		✓		✓	
Yr×Qtr	✓		✓		✓	
County×Yr×Qtr		✓		✓		✓

Note: Robust standard errors clustered at census tract level. * p<0.1, ** p<0.05, *** p<0.01.

Table C5 Difference-in-differences regressions of the effect of the burn scar view on the composition of buyers inside and outside the burn scar view and no burn scar view neighborhoods within two year of a fire. Data source: HMDA. All specifications include census tract fixed effects and, either quadratic county trends and year by quarter fixed effects, or county by year by quarter fixed effects.

	Income		White		Hispanic	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: 0-2km					
View×PostFire	-2.975	-2.576	0.017	0.016	0.005	-0.001
	(3.723)	(3.629)	(0.018)	(0.019)	(0.014)	(0.014)
N	19093	19093	19097	19097	19097	19097
R^2_{adj}	0.031	0.030	0.007	0.010	0.034	0.039
	Panel A: 2-4km					
View×PostFire	0.235	0.742	0.021*	0.017	0.008	0.004
	(1.565)	(1.620)	(0.011)	(0.011)	(0.009)	(0.009)
N	38596	38596	38602	38602	38602	38602
R^2_{adj}	0.036	0.038	0.019	0.020	0.051	0.053
Census tract FE	✓	✓	✓	✓	✓	✓
Trends	✓		✓		✓	
Yr×Qtr	✓		✓		✓	
County×Yr×Qtr		✓		✓		✓

Note: Robust standard errors clustered at census tract level. * p<0.1, ** p<0.05, *** p<0.01.