A City on Fire?
Housing Market Evidence of Wildfire Costs

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Abstract

Using a unique dataset containing over 2 million sales transactions for the Los Angeles and San Diego Basins, we investigate the pathways through which wildfires affect real estate prices, including the loss of visual amenities and proximity to burn scars. We further explore how two information signals affect homeowners' wildfire risk beliefs. First, we test whether homeowners, in a high risk zone, update their risk beliefs in response to a wildfire event. Second, we take advantage of an exogenous update in the risk zone to test how risk zone assignment affects risk beliefs. Throughout the paper, our main identification strategy takes advantage of a rich repeat sales dataset to control for house and neighborhood time-invariant unobservables. Findings reveal a 4.2% to 5.0% decrease in the price of properties with a burn scar view within 2km. We do not find evidence that wildfires or a change in the risk zone assignment significantly affect the value of high-risk properties. These findings suggest such signals may not convey novel information to homeowners.

JEL codes: Q51, Q54, Q58, R31

Keywords: hedonic pricing model, repeat sales, natural disasters, wildfires, viewshed, risk beliefs

1 Introduction

Climate change is arguably the largest market failure our world has seen (Stern, 2008). Impacts from climate change can come through multiple channels, such as more temperature extremes, weather fluctuations, and natural disasters. These changes in turn can affect trade and economic growth (Burke et al., 2015; Costinot et al., 2016; Hsiang et al., 2017), health (Deschênes and Moretti, 2009; Deschênes and Greenstone, 2011), agricultural output (Schlenker et al., 2006; Burke and Emerick, 2016; Fisher et al., 2012), and adaptation to extreme weather events (Hsiang and Narita, 2012). In particular, natural disasters have been growing, both in frequency and severity. The number of billion-dollar disasters is on an upward trend, with cumulative costs exceeding $300

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billion in 2017 in the United States—a new annual record (NOAA, 2018). For example, large wildfires have increased by around 500% over the last 30-40 years and climate change is likely the primary driver.[1] The economic costs for natural disasters are predicted to increase rapidly as new development expands in risk-prone areas. These areas are sought after, in part due to high amenity values, e.g., beach-front communities or wilderness areas, and in part to development pressure and infringement on the wildland or floodplains adjacent to urban centers. Yet, recent research suggests that the economic costs of natural disasters have been previously understated (Deryugina, 2017). Understanding these economic costs is critical for society to make appropriate investments to mitigate and reduce exposure to natural disaster damages. In addition, policymakers' ability to convey accurate risk information to local communities is essential to minimize development in risky areas beyond the socially optimal level. Two important policy questions emerge. First, what are the total costs of natural disasters, including property damages and amenity losses such as neighborhood quality, vistas, and wilderness access. Second, do residents update their risk beliefs in response to natural disasters or assignment to high risk zones?

To answer these questions, our paper focuses on wildfires in southern California, a region with frequent high-severity wildland-urban fires.[2] We assemble a uniquely large dataset of real estate transactions that contains over 2 million observations and spans seven southern California counties over 16 years. Every property is geo-coded and linked to a detailed wildfire history, neighborhood characteristics, and environmental amenities. A viewshed analysis is conducted in ArcGIS to precisely identify which properties' viewshed intersects with the burn scars perimeters. Two identification issues are present when estimating the effects of wildfires on property prices: 1) disentangling the various pathways through which wildfires affect property prices (burned vistas, proximity to burn scars, updated risk beliefs), and 2) constructing valid counterfactuals since properties that experience a wildfire may be systematically different from those that do not. Using a series of quasi-experimental methods, we estimate and disentangle the multi-faceted effects that wildfires have on real estate prices: burned vistas, proximity to burn scar, and risk beliefs updating. We take advantage of the large number of repeat sales in our dataset to control for house and neighborhood time-invariant unobservables. Focusing on within-property variation is important because model dependency and correlated unobservables may confound identification as properties impacted by a wildfire likely differ from properties not impacted. Using stringent samples restrictions to identify various treatment effects (burned vistas, proximity to burn scars, updated risk beliefs), we compare treated properties to similar control properties to minimize concerns about different unobservable trends. We explore the effects of two information signals on homeowners' risk beliefs. First, we test

[1] Resources Radio Podcast broadcast on December 4th, 2018 with Dr. Wibbenmeyer from Resources for the Future.

[2] In 2017, the Tubbs Fire destroyed over 5,000 structures, while the Thomas Fire set the record for the largest fire in California's history. Besides, fire suppression expenditures for the year 2017-2018 have reached over $770 million (California Department of Forestry and Fire Protection, 2018b).

whether being located in the high fire risk zone affects how homeowners update their risk beliefs after a wildfire. Second, we take advantage of an exogenous change in the risk zone to compare the value of properties newly assigned to the risk zone relative to their neighbors that did not experience a change in risk status.

We find that a burn scar view located within 2km lowers home values by 4.2% to 5.0% during the first year post-fire, while a burn scar view located between 3km and 4km reduces home values by 1.9% to 3.2%. The effects of burn scar views on home values are attenuated beyond one year, which may be explained by the fast regeneration of the shrub vegetation in southern California and/or homeowners' myopic behavior. Furthermore, once controlling for burn scar view, we find that proximity to a burn scar does not significantly affect property prices. We do not find evidence that, absent direct disamenity effect, wildfires significantly affect the value of riskier properties. It may be because homeowners are inattentive or myopic, or because of the potential offsetting effects of a wildfire on the changes in homeowners' subjective fire probability and expected losses. Somewhat surprisingly, we do not find evidence that a change in the risk zone assignment affects property prices, suggesting that the risk zoning fails to convey accurate risk information to homeowners. Overall, our results are consistent across a series of empirical specifications and robustness checks.

Our findings are relevant to policymakers to implement socially optimal public policies that balance the cost of government intervention with the avoided damages from intervention. The annual cost of US federal wildfire suppression and prevention programs is now exceeding $3 billion—and is predicted to keep rising (Hoover et al., 2015). Recent years have witnessed some of the worst wildland-urban fires in California's history. For example, the 2018 Camp Fire is now the most destructive fire on record with over 15,000 lost structures (and estimated insured losses of $7.5 to $10 billion (RMS)). While the cost of government intervention is relatively easy to monitor, the net effect of wildfires on society is more difficult to assess. Each wildfire affects the provisioning of local amenities for an entire region with a great number of properties impacted and considerable welfare implications. Improving our knowledge of the costs of wildfires on society is a pressing issue as both the wildland-urban interface has been developing rapidly (Radeloff et al., 2018)[3] and wildland-urban fires are predicted to continue to increase in frequency and severity with climate change (Westerling et al., 2006; Schoennagel et al., 2017).

Our findings have important policy implications on risk signalling and risk perceptions. Risk beliefs affect demand for housing in high risk areas, private mitigation actions, and preferences for public policies, which can mitigate natural disasters and reduce disaster management costs. Natural disasters are ideal for understanding the updating of risk beliefs because they are likely

[3]According to the International Association of Wildland Fire (2013), there are approximately 46 million homes in the United States on the wildland-urban interface, defined as properties adjacent to fire-prone public land. These homes correspond to an estimated $9.2 trillion in property value at risk (using the 2017 Zillow Home Value Index for the median American home of $200,000). Of the approximatively 13.6 million homes in California, 3.6 million are located in the wildland-urban interface (Martinuzzi et al., 2015).

exogenous shocks to property owners. Choosing a risky location, as in Bakkensen and Barrage (2017) and Baylis and Boomhower (2018), could be evidence that these homeowners ignore risk signals and take on too much risk. The social cost of miscalculating these risks could be extremely high in some regions, for example, if adaptation involves mass resettlement of population (Bogardi and Warner, 2009).

Recent studies model the hedonic price function formation accounting for the updating of natural disaster risk beliefs. Bakkensen and Barrage (2017) show that coastal homeowners select into risky locations for the coastal amenities, but also have lower risk beliefs. Gibson et al. (2018) model risk beliefs updating in response to both flood risk and insurance price signals. They find that updated floodplain maps have a considerable effect on housing prices after Superstorm Sandy. Baylis and Boomhower (2018) show how government fire suppression programs subsidize development in high fire risk areas, leading to moral hazard. A number of hedonic valuation studies investigates the effect of natural disasters on risk beliefs. One of the fundamental problems is to disentangle changes in homeowners' risk beliefs from natural disaster damages, which requires being able to identify which homes experience disaster damages (e.g., Hallstrom and Smith (2005); Bin and Landry (2013); McCoy and Zhao (2018)).[4]

A large body of literature examines the impact of wildfires on property prices (mostly) in low-population density and forested areas, such as Colorado and Montana. Using a data cross-section, Stetler et al. (2010) find that properties in northwest Montana within 5km of a burn scar sell for 14% less than those 20km away, but the effect becomes insignificant once focusing on properties for which the burn scar is not visible.[5] This finding suggests that the loss of visual amenity may be an important component of the total effect and that homeowners may only update their risk beliefs when burn scars are visible. The study most similar to ours is McCoy and Walsh (2018). Using a difference-in-differences approach, they investigate how wildfires affect risk salience in the Colorado Front Range. They find that proximity to a burn scar reduces home values by 11.6% in the first three years following a wildfire, while a burn scar view results in a 6.6% price drop in the first three years. Studies in forested regions find evidence that home prices decrease temporarily after a nearby wildfire or public disclosure campaign of risk ratings (Loomis, 2004; Donovan et al., 2007; McCoy and Walsh, 2018). For example, McCoy and Walsh (2018) find that location in a high-risk area leads to a 12.3% loss in home value in the first year after a fire.

This paper makes three contributions to the literature. It is the first large-scale study in a heavily urban geographical area: the Los Angeles and San Diego basins. This metropolitan area is

[4]For example, Hallstrom and Smith (2005) use a clever quasi-experimental design by examining the effect of Hurricane Andrew on changes in risk beliefs in a near-miss county in Florida—thus absent any storm damage.

[5]Our paper also relates to the literature on viewsheds. Mountain views have been shown to provide considerable environmental amenities that can be capitalized in property values (Benson et al., 1998; Paterson and Boyle, 2002; Cavailhès et al., 2009; Wasson et al., 2013). Considering the lasting visible evidence of damage and destruction that wildfires leave in their wake, it may not be surprising that views of burn scars are a critical part of the disamenities related to wildfires.

a particularly relevant case study because of the high number of homes at risk and relative high value of these homes. Wildfires in the region are highly frequent and destructive (with an upward trend) due to a dry Mediterranean climate and landscape dominated by shrubs that can ablaze and regenerate every few years (Miller and Safford, 2012). Second, we assemble a unique dataset of all the repeat sales properties affected by wildfires in the Los Angeles and San Diego Basins between 2000 and 2015. Such data position us well to investigate how two types of risk information signals affect homeowners' subjective wildfire probabilities and expected losses. Complementing this dataset with a detailed viewshed analysis, we cleanly estimate homeowners' willingness to pay for various amenities related to mitigation of wildfires, specifically through the destruction of scenic views and proximity to burn scars. Third, our paper contributes to the quasi-experimental literature on eliciting unbiased households' values for school quality, environmental amenities or risk (e.g., Black (1999); Chay and Greenstone (2005); Greenstone and Gallagher (2008); Muehlenbachs et al. (2015); Haninger et al. (2017)). Our repeat sales framework combined with a difference-in-differences approach restricted to neighboring properties on each side of the risk zone allows us to control for both time-invariant and time-varying unobservables that biases cross-sectional analysis.

The remainder of the paper is structured as follows. The next section describes the data sources and viewshed analysis. Section 3 motivates the identification strategy. Section 4 discusses the results. Section 5 presents the conceptual model of risk beliefs updating. The final section concludes.

2 Data

To capture all the properties likely affected by wildfires, we selected zip codes located within a 30km bandwidth of the national forests surrounding the Los Angeles and San Diego basins. Those zip codes 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. We start with a dataset of 2,187,007 unique properties. Single family residence sales (excluding mobile homes) and arms-length transactions of owner-occupied properties account for 1,215,523 observations. Properties with missing sale price as well as those sold more than once within the same year or sold in the same year as built are also dropped to eliminate potential house flippers and made-to-order homes (1,070,639 remaining observations). We deflate all prices using the Consumer Price Index from the U.S. Bureau of Labor Statistics. We then further drop observations with sale prices in the bottom and top 1%, and properties with the top 1% of bedrooms, bathrooms, and square footage. Of the remaining 1,022,072 properties, 439,796 are repeat sales in our 16-year time period. To construct our repeat sales dataset, we keep properties that sold more than once between 2000 and 2015 (in practice, exactly twice since CoreLogic only contains information up to the prior sale). To eliminate potential outliers and reduce the likelihood that a property experienced significant renovation in-between sales, we drop properties whose price change across transactions is in the top and bottom percentiles and whose transactions took place more than 10 years apart.

The California Fire Resource and Assessment Program (FRAP; frap.fire.ca.gov) provide spatial data on wildfires, the Fire Hazard Severity Zones, and the wildland-urban interface (WUI). The California Department of Forestry and Fire Protection (CAL FIRE) produces maps of significant fire hazard, called Fire Hazard Severity Zones (FHSZ), which we will refer to generically as "risk zones" hereinafter. These maps are generated using an ember diffusion model developed at the University of California, Berkeley, that takes into account the physical attributes of the area, including vegetation type, topography, local climate and wind directions. The maps focus on hazards and do not account for private risk mitigating actions on a given property, e.g., fuel reduction and defensible space. As a result, homeowners do not have the ability to influence their assignment to the risk zone.[6] By law, sellers have to disclose the property's risk zone status to the buyer at the time of sale. While early maps were in place since 2000, new maps (expanding the risk zone) were implemented in 2008.

The wildfire data contain information on perimeters, area burned, and start and containment dates. We discard fires smaller than 50 acres because they are likely not large enough to affect local amenities or risk beliefs. Thus, our analysis includes 251 fires between 1998 and 2015. Burn scars range between 51 to 270,686 acres (with median and mean sizes of 695 and 5,634 acres, respectively; Table 1).[7] Our analysis includes some of the largest wildfires in California's history. For example, the 2003 Cedar Fire (271k acres, i.e., the largest fire in our dataset; San Diego County) is the second largest in California's history after the 2017 Thomas Fire, followed by the 2007 Witch Fire (162k acres; San Diego County), and the 2009 Station Fire (161k acres; Los Angeles County). It is noteworthy that the Cedar and Witch fires partially overlapped (by over 40,000 acres) despite being only 4 years apart. It illustrates the short fire interval existing in southern California, which contrasts with that of most forested areas in the rest of the western United States.

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/data). Spatial data on primary roads come from the US Data Catalog (catalog.data.gov). The 2010 census tract boundaries and census characteristics come from the American Community Survey and include median household income, race, and ethnicity—which we use in Appendix E to examine changes in neighborhood composition.

All properties are geo-coded to obtain exact latitude and longitude coordinates and link them to aforementioned spatial data. In ArcGIS, we calculate slope and elevation as well as distances

[6] Risk zones are managed by the state (State Responsibility Area) or municipalities (Local Responsibility Area).

[7] McCoy and Walsh (2018) use a 500-acre minimum fire size threshold, while Stetler et al. (2010) use a 10-acre threshold.

Table 1 Wildfire characteristics in our sample

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

to burn scars, nearest national forest, nearest state or local park, and nearest primary road.[8] To focus our analysis on the effect of a single wildfire event on sales one or two years post-fire, we drop properties that experience a second fire in the five years prior to the sale.[9]

2.1 Visual amenity and proximity effects

Because the human eye would have trouble distinguishing burned from unburned shrubs from more than a few kilometers away, we restrict the analysis to repeat sales properties for which one of the sales occurred within 4km of a burn scar.[10] Due to the fast regeneration of shrubs, we further focus our analysis on the first and second years post-fire. Summary statistics for the repeat sales properties that sold once within 4km and during the first two years post-fire (with and without a burn scar view) are shown in Table 2 and the properties are depicted in Figure 1.[11]

[8]Properties located on national forest land are excluded from the analysis due to concerns of belonging to different markets. We further discard properties that lie on a wildfire perimeter or within a 50m buffer outside the perimeter to ensure we exclude properties most exposed to structural damage by the fire. Note that (at least until recently) wildfires in California were not associated with large numbers of homes destroyed. For example, between 2000 and 2015, 16,761 structures (including both residential and commercial) were lost in the state of California (California Department of Forestry and Fire Protection, 2018a).

[9]Because there is a lag between the time the sale is recorded and the time the price of the property is negotiated and agreed upon by the buyer and seller, we consider a sale as post-fire when it is recorded more than 60 days post-fire (Mueller and Loomis, 2014). Results are robust to using a 90-day lag.

[10]McCoy and Walsh (2018) find that a 5km threshold is appropriate in their Colorado setting with forests and burned trees visible from farther away than shrubs.

[11]Table A1 shows that repeat sales properties on average sell for slightly less than properties in the full sample. However, they do not appear to differ from the non-repeat sales properties based on other property, neighborhood, or wildfire characteristics (e.g., distance and view of burn scar). This alleviates concerns that properties selling multiple

Table 2 Summary characteristics of the repeat sales properties that sold during the first two years post-fire for different distance bins from the burn scar

	0-2km distance bin				2-4km distance bin			
	No view		Burn scar view		No view		Burn scar view	
	Means	(sd)	Means	(sd)	Means	(sd)	Means	(sd)
Sale price (k$2015)	504.88	(278.67)	515.54	(278.96)	457.71	(263.23)	433.70	(228.00)
Age	26.20	(20.61)	27.79	(21.81)	25.08	(20.28)	29.32	(23.19)
Living area (k sqft)	2.17	(0.86)	2.01	(0.77)	2.15	(0.80)	1.95	(0.72)
# bedrooms	3.55	(0.84)	3.45	(0.79)	3.55	(0.81)	3.42	(0.80)
# bathrooms	2.70	(0.86)	2.59	(0.81)	2.67	(0.78)	2.47	(0.77)
Swimming pool (0/1)	0.25	(0.43)	0.19	(0.39)	0.21	(0.41)	0.18	(0.38)
Dist. green space (km)	0.54	(0.50)	0.47	(0.44)	0.60	(0.60)	0.56	(0.51)
Elevation (m)	258.79	(167.40)	274.60	(174.72)	288.60	(160.83)	307.59	(186.96)
Slope	5.88	(5.79)	3.51	(3.90)	4.05	(4.59)	2.36	(3.11)
FHSZ (0/1)	0.23	(0.42)	0.17	(0.37)	0.16	(0.37)	0.05	(0.21)
WUI (0/1)	0.81	(0.39)	0.80	(0.40)	0.72	(0.45)	0.51	(0.50)
Dist. main road (km)	1.76	(1.17)	1.38	(1.19)	1.50	(1.28)	1.27	(1.06)
Dist. burn scar (km)	1.36	(0.46)	1.12	(0.56)	3.28	(0.54)	2.97	(0.55)
Days since fire	421.31	(199.00)	424.96	(205.77)	444.52	(203.55)	436.93	(208.58)
Median hh. income (k$)	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)
Years between sales	4.86	(2.16)	4.86	(2.13)	4.82	(2.21)	4.79	(2.17)
# of unique properties	1087		4199		6117		6261	
# of census tracts	184		442		705		702	
# of fires	80		107		157		129	

Properties with a view of the burn scar are on average slightly older, smaller, at higher elevation, in less wealthy and more ethnically diverse neighborhoods, and closer to the burn scar than properties without a burn scar view. The closer to the burn scar perimeter, the likelier it is that a property has a burn scar view, as is illustrated by the larger number of treated properties (with view) relative to the controls (without view) in the 0-2km bin than in the 3-4km bin.

Following the methodology employed in much of the literature on visual (dis)amenities, e.g., wildfire (Stetler et al., 2010; McCoy and Walsh, 2018), wind turbines (Gibbons, 2015), or shale gas development (Muehlenbachs et al., 2015), we use 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 how far a 5-foot tall person can see from the property in a 4km radius. We then intersect each property's 4km-radius viewshed with burn scar footprints from the prior two years. Because the Digital Elevation Model only takes into account the bare earth, considerable measurement error may be associated with our burn scar view variable. To resolve part of this imprecision, we collected Light Detection and Ranging (LiDAR) data to construct a Digital Surface Model (DSM) that captures structures on the earth such as buildings and trees. One limitation of this approach is that LiDAR data are only available for three counties—San Diego, San Bernardino,

times in our time period experience a different wildfire history from the non-repeat sales properties.

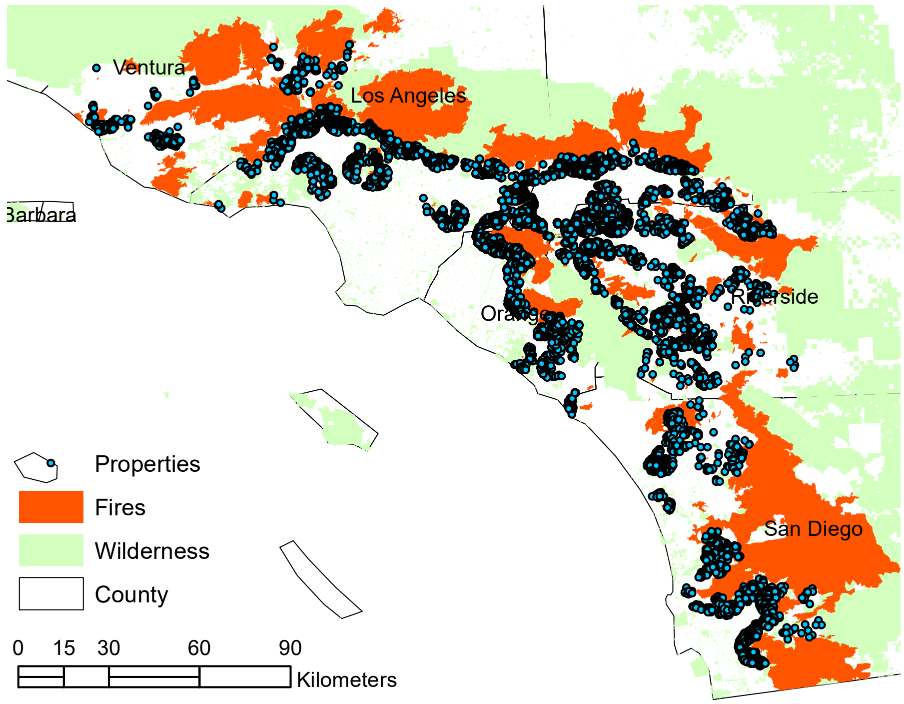


Figure 1 Wildfire perimeters between 1998 and 2015 and repeat sales properties sold within 4km of burn scar and during the first two years post-fire.

and Riverside counties.[12]

Figure 2 demonstrates the distribution of properties that are treated with a view of burn scars compared to the properties without a view for four fires in our sample (top and middle panels use the standard DEM, while the bottom panels rely on the LiDAR DSM). As expected, as properties are closer to the burn scar it becomes more likely that properties also have a view of the burn scar. Homes with a view of the burn scar tend to be clustered together, which highlights the need to control for spatial variables that are correlated with the burn scar that are time invariant, such as distance to amenities. The repeat sales approach we employ controls for time invariant unobservables that would likely contribute to confounding identification of the effects of proximity to and view of burn scars.

2.2 Risk perception effect

To isolate the effect of the two risk information signals, we need properties that do not experience the disamenity effect from wildfires. Thus, we select for this analysis properties at least 5km away from a burn scar (in the spirit of Hallstrom and Smith (2005) and McCoy and Walsh (2018)). The motivation for this 5km threshold is to create some buffer beyond the 4km amenity effect we find evidence for.

[12]We are not aware of other valuation studies using finer-resolution, LiDAR data to explore the effect of measurement error in the visual amenity variable.

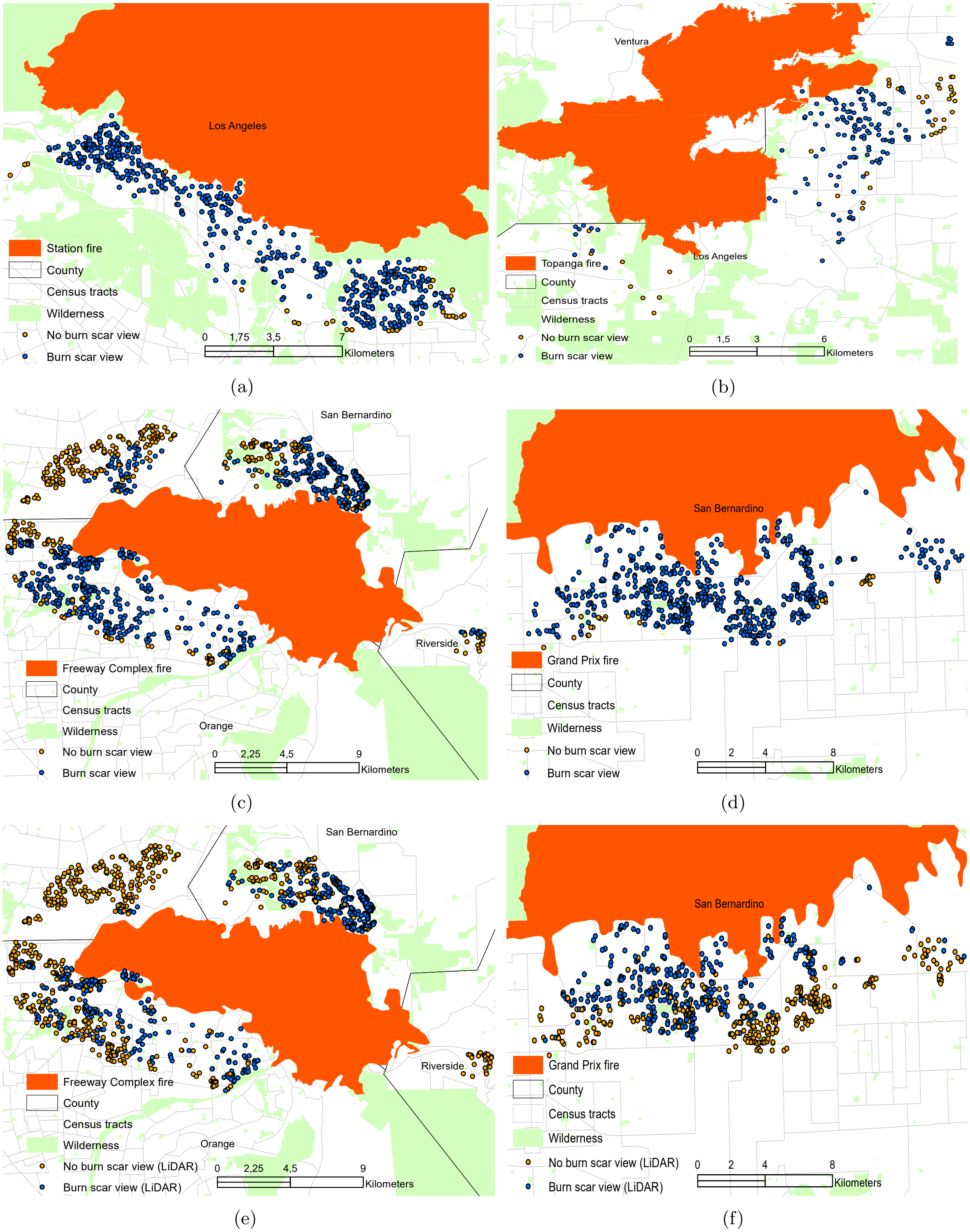
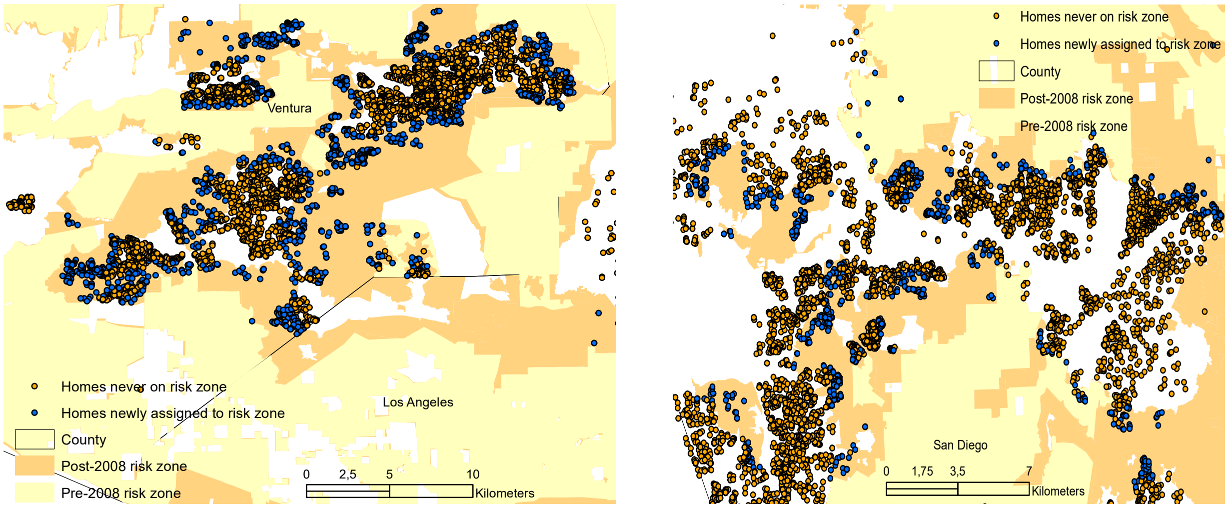


Figure 2 Properties with or without burn scar view sold within 4km and two years post-fire for: a) the 2009 Station Fire in Los Angeles County, b) the 2005 Topanga Fire in Ventura County, c) the 2008 Freeway Complex Fire in Orange County, and d) the 2003 Grand Prix Fire in Los Angeles County. LiDAR data are used to construct the viewshed for: e) the 2008 Freeway Complex Fire and f) the Grand Prix Fire.



(a) (b)

Figure 3 Properties always off the risk zone and properties newly assigned to the risk zone in 2008. Examples in a) Ventura County and b) San Diego County.

To test whether location on the high fire risk zone affects how homeowners update their risk beliefs after a wildfire, we restrict the analysis to repeat sales properties near the risk zone for which one sale took place during the first two years post-fire but at least 5km from the burn scar.[13] To investigate the effect of the change in the risk zone (absent any wildfire), we focus on repeat sales properties near the risk zone (either inside or outside) that sold before and after the 2008 rezoning. Figure 3 shows the distribution of two subsamples of properties newly assigned to the risk zone and neighboring properties never on the risk zone. (The effect of the 2008 rezoning for our entire study area is depicted in Figure A1.) Summary statistics for the property samples used for these two risk information signals are shown in Appendix A (Tables A2 and A3). On average, properties on the risk zone are slightly more expensive, newer, larger, at higher elevation, on steeper slopes, more likely on the wildland-urban interface, in wealthier and less ethnically diverse neighborhoods relative to properties outside the risk zone.

3 Empirical strategy

We use the hedonic pricing method to value the effect of wildfires on the changes in housing attributes (Rosen, 1974). The change in attributes that results from a wildfire affects the comparative prices of houses with these attributes and can measure the disutility of having a view of a burn scar or living in a high-risk area. First, we estimate the average treatment effect of wildfire disamenities, isolating the burn scar view effect from the proximity effect. Specifically, we estimate the effect of having a burn scar view on the sales prices of treated properties (average treatment

[13]To study this first information signal, we omit properties that changed risk zone status in 2008.

effect on the treated; ATT), holding constant other effects that vary with the proximity to the burn scar. Second, we investigate the ATT of burn scar proximity, while controlling for burn scar view. Last, we investigate the ATT effect of two types of information signals on changes in risk beliefs, absent disamenity effect: an exogenous updating of the risk zone map and the differential effect of a wildfire event on properties across the risk zone.

ATT is subject to biases if the properties that received treatment are systematically different from those that did not. For example, homes located near burn scars may be older and smaller than the average home farther away, or may be located in neighborhoods that experience different amenity levels, e.g., access to the wilderness. Failure to control for an unobservable that is correlated with both the treatment and home price will lead to biased estimates. The fundamental issue is that we do not observe the counterfactual for treated observations, e.g., the price of a property if that same property did not have a burn scar view. Our main empirical strategy takes advantage of our repeat sales properties to control for time-invariant property and neighborhood unobservables that may be correlated with both the treatment and home prices. Next, the empirical strategy lays out our approach to recover unbiased ATT of burn scar view, proximity, and risk beliefs updating.

3.1 Effect of burn scar view

To identify the effect of burn scar views on property values, one must control for proximity effects such as lost access to recreation sites, and changes in risk latency that may confound the burn scar view estimate. By construction, comparing treated properties to control properties that are located in the same distance bin from the burn scar will pin down most of the proximity effects.[14] Running separate models for different distance bins from the burn scar allows us to capture the heterogeneous effect of the visual disamenity over space. The thinner the bin, the more heterogeneity we allow, but the fewer the number of observations and the potentially less precise our estimates. (We test multiple bin widths and show results for the 2km-bin width in Section 4 and relegate results for the 1km-bin width to Appendix B.)

Using the repeat sales model we estimate equation (1) where careful selection of our sample of property sales determines β_j, the estimated ATT effect of burn scar view across the first and second years post-fire $j = \{1, 2\}$.

$$\ln p_{it} = \sum_j (\beta_j View_{jit} + \gamma_j View_{jit} \times Large_{jit}) + \lambda_i + \mu_{it} + \epsilon_{it}. \tag{1}$$

In this equation the dependent variable is the natural log of property i's sale price at time t. λ_i are property specific fixed effects, μ_{it} are temporal and spatial fixed effects and/or trends. To investigate

[14] Despite not having insurance data, it is likely that insurance premium updating in the aftermath of a wildfire is largely determined by the proximity to the fire along with other property and neighborhood characteristics that are controlled for in our repeat sales approach.

potential treatment heterogeneity in the burn scar view intensity, we consider the effect of large burn scar views (above 10 acres) on property values, i.e., γ_j. The hypothesis is that properties with large burn scar views may be impacted more severely than properties from which the visible burn scar is small.

Importantly, by using properties without a view of the burn scar as controls we can estimate the effect of burn scar view within the same distance bin. Conditional on the trends for prices for homes with and without a burn scar view being identical, our estimate provides an unconfounded estimate of the effect of view on the price of a property, holding the proximity effect constant.[15] In addition, the within-property variation from the repeat sales approach allows us to hold the physical risk constant. Because the repeat sales approach relies on time variation, it is critical to control for the potential heterogeneity in temporal shocks across the region. For example, macro-level housing shocks could drive price changes and confound the effect of wildfires.[16] Thus, we rely on time varying fixed effects 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, which are more flexible (but also soak up more of the variation).[17]

3.2 Effect of burn scar proximity

We focus our analysis on repeat sales properties for which one of the sales is affected by a wildfire and define the treatment group as properties located within K-km of the burn scar, while the control group consists of properties located between the K-km threshold and 4km. Our empirical model (2) allows for heterogeneity of the proximity effect K across the first and second years post-fire $j = \{1, 2\}$, while controlling for properties that have a burn scar view $View_{jit}$.

$$\ln p_{it} = \sum_j (\beta_j K_{jit} + \gamma_j View_{jit} + \delta_j K_{jit} \times View_{jit}) + \lambda_i + \mu_{it} + \epsilon_{it}. \tag{2}$$

The parameters β_j reflect the ATT effect of proximity over time. We control for property and neighborhood time-invariant unobservables λ_i, and local and macro shocks μ_{it} through year-by-quarter fixed effects and quadratic county trends, or county-by-year and quarter fixed effects.

Section 4 shows results for K ranging from 1km to 3km. As a robustness check, Appendix C depicts results running separate regressions for properties that have a burn scar view and those that do not. This selection of properties provides another way to estimate the effect of proximity to wildfire burn scars, holding constant burn scar view.

[15] In practice, we discard a small number of properties that experience fires across the two sales so that it is straightforward to assign the property to a single distance bin within 0 and 4km.

[16] We are not concerned about housing booms because housing supply is inelastic in the region due to the presence of steep-sloped terrain (Green et al., 2005; Saiz, 2010). Saiz (2010) reports MSA-level elasticities for Los Angeles-Long Beach, Riverside-San Bernardino, and San Diego are 0.63, 0.67, and 0.94, respectively.

[17] Due to the large number of census tracts, we cannot afford to control for temporal shocks that vary at the census tract level by year.

3.3 Effect of information signals on risk beliefs

To investigate how homeowners update their risk beliefs, we consider two types of information signals. First, we look at how a wildfire event may differently impact properties located in a high fire risk area relative to neighboring properties sharing similar local amenities but just outside the risk zone.[18] We restrict the analysis to the repeat sales properties for which one of the sales took place within two years post-fire but at least 5km from a burn scar (in practice between 5km and 15km; results are similar for 5km to 12km). We argue that the disamenities associated with burn scars (including view and proximity) would be greatly diminished at such distances but that the impact of risk information signal would still be relevant due to the spatial and temporal distributions of wildfires in southern California. We differentiate the effect of risk latency by using the risk zone as defined by the California Fire Hazard Severity Zones. This zoning is likely a good measure of risk latency since the designations are publicly known, disclosed at the time of sale, and based on objective hazard measures uncorrelated with homeowners' risk mitigating actions.

Our quasi-experimental design combines a repeat sales approach with a difference-in-differences framework. In regression (3), the repeat sales approach has the benefits of controlling for time-invariant unobservables at the property and neighborhood levels, through λ_i, while the difference-in-differences framework deals with time-variant unobservables within a close proximity of the risk zone boundary, conditional on the parallel trend assumption between the treated ($RiskZone_{it} = 1$) and control groups ($RiskZone_{it} = 0$) holding in the pre-fire period ($PostFire_{it} = 0$). The estimated ATT effect of wildfires on risk beliefs updating is β.

$$\ln p_{it} = \beta RiskZone_{it} + \gamma PostFire_{it} + \delta RiskZone_{it} \times PostFire_{it} + \lambda_i + \mu_{it} + \epsilon_{it}. \quad (3)$$

μ_{it} controls for year-by-quarter fixed effects and linear county trends or county fixed effects. To alleviate concerns of unobservable trends that may vary across space, we focus the analysis on homes within 1 or 2km on each side of the risk zone boundary so that we compare properties just inside the risk zone with those just outside.[19]

Our second type of information signal analysis explores the effectiveness of risk zoning policies on homeowners' risk beliefs updating β. We take advantage of a 2008 exogenous update in the risk zone to compare the value of properties newly assigned to the risk zone relative to their neighbors that did not experience a change in risk status. We focus the analysis on the repeat sales properties that change risk zone assignment across the two sales $\Delta RiskZone_{it} = 1$, i.e., that become assigned to the risk zone in the most recent sale.[20] Our quasi-experimental design again consists of a combined

[18]Our quasi-experimental design is in spirit akin to a regression discontinuity design on the risk zone boundary.

[19]Reducing the distance threshold minimizes concerns about varying trends, with the tradeoff of reducing the number of observations and precision of our estimate.

[20]We drop properties that ever experienced a fire within the 5 years prior to a sale, so as to isolate the effect of the 2008 rezoning in the absence of fire events.

repeat sales and difference-in-differences approaches. We show evidence of the common trends in pre-rezoning prices in Appendix D. Regression (4) shows the property fixed effects λ_i, treatment group $\Delta RiskZone_{it}$, post treatment $PostRezoning_{it}$, and spatial and temporal fixed effects and/or trends μ_{it}.

$$\ln p_{it} = \beta \Delta RiskZone_{it} + \gamma PostRezoning_{it} + \delta \Delta RiskZone_{it} \times PostRezoning_{it} + \lambda_i + \mu_{it} + \epsilon_{it}. \quad (4)$$

Thanks to the relatively larger number of observations impacted by the rezoning (relative to the fire event signal), we are able to narrow our analysis to homes within 250m, 500m, or 750m on each side of the risk zone boundary to reduce concerns of unobservable trends varying over space. Figure 3 shows the distribution of properties in Ventura and San Diego Counties as an example of the sample of data used to identify the effects of new risk zone assignment.

4 Results

This section presents and discusses the disamenity effects and risk beliefs updating results. First, we estimate the burn scar view effect holding constant the proximity effect. Second, we identify the effect of burn scar proximity controlling for the burn scar view. Last, we estimate the effect of two information signals on risk beliefs updating for properties on each side of the risk zone (in the absence of disamenity effects).

4.1 Effect of burn scar view

Table 3 suggests that having a view of a burn scar decreases prices from 4.2% to 5.0% for properties within 2km of a burn scar in the first year post-fire. The effect is in general attenuated the farther a property is from the burn perimeter, with home values reduced by 1.9% to 3.2% between 3 and 4km. The subscripts 1 and 2 on coefficients in Table 3 refer to the year post-fire for which a coefficient is reported (e.g., View_1 indicates the coefficient for a property with a burn scar view sold in the first year post-fire). We do not find evidence of heterogeneity based on the size of the burned viewshed (γ_j). Properties selling during the second year post-fire show no or weak burn scar view effects. In Table 3, under the specification with year-by-quarter fixed effects and county-level quadratic trends, having a burn scar view causes a decrease in property prices of 4.2% in the first 0-2km bin and 1.9% in the 3-4km bin in the first year post-fire. The second year post-fire is only statistically significant for properties in the 3-4km bin. When allowing for the more flexible county-by-year-by-quarter fixed effects, the effect of the burn scar view is slightly higher in the 0-2km bin (-5.0%) and remains more persistent in the 3-4km bin (-3.2%) in the first year post-fire. A smaller effect further persists in the second year post-fire in the 3-4km bin (-2.6%). The attenuation in our estimate beyond one year is likely due to the fast regrowth of shrub vegetation and/or homeowners' myopia that another fire may ablaze in the area in the coming years. To put our estimates in perspective, a 5% decrease in home prices due to having a view of a burn scar corresponds to a loss of value of $28,300 for the average home in our sample (worth $566,000).

Table 3 Burn scar view estimates for the 0-2 and 3-4km bins

	0-2km bin		3-4km bin	
	(1)	(2)	(3)	(4)
View_1	-0.0419***	-0.0504***	-0.0194**	-0.0323***
	(0.0145)	(0.0131)	(0.0085)	(0.0079)
View_2	-0.0203	-0.0216	-0.0167**	-0.0259***
	(0.0145)	(0.0132)	(0.0075)	(0.0069)
$\text{View}_1 \times \text{Large}_1$	0.0066	0.0070	-0.0084	-0.0083
	(0.0184)	(0.0174)	(0.0141)	(0.0140)
$\text{View}_2 \times \text{Large}_2$	0.0023	-0.0090	0.0098	0.0043
	(0.0177)	(0.0162)	(0.0138)	(0.0124)
Quadratic county trends	Yes		Yes	
Year×Quarter	Yes		Yes	
County×Year×Quarter		Yes		Yes
N	10573	10573	24770	24770
R^2_{adj}	0.843	0.862	0.868	0.880

Note: Each specification includes Property fixed effects. Robust standard errors clustered at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

In Appendix B, we further refine the widths of the distance bins to increase the accuracy with which we control for proximity to elicit the effect of burn scar view. Results in Table B1 are qualitatively similar.

Overall, our estimates of the negative effect of burn scar view are consistent with those found in McCoy and Walsh (2018) and Stetler et al. (2010) (-6.6% and -2.6%, respectively), despite two distinctions (likely working in opposite directions): these studies consider views of forested areas and look at properties typically farther away (beyond 2km). Another distinction is our work finds that burn scar view effects are robust for the first year and likely attenuate after that year. In the hedonic literature it is common to assume a permanent change in an amenity level, e.g., air quality, such that estimates can be interpreted as the discounted flow of net benefits associated with the amenity change over the lifetime of the housing investment. In our setting, we examine a temporary change in an amenity, which is less common to interpret. We posit that home buyers may discount the risk signal or visual disamenities that come with a burn scar view over their expected time span of the effect. As the vegetation recovers, amenity values may be expected to rebound partially or completely after a number of years.

One potential concern with our estimates is that they could include a housing market supply side effect. If wildfires destroy a large enough number of homes, thus reducing market supply and increasing housing prices, our results likely underestimate the actual demand effect. Alternatively, if wildfires lead to more households leaving the neighborhood and, thus, more homes on the market, it may dampen home prices and bias upward our marginal willingness-to-pay for disamenities.

However, it would seem likely that any supply side effect last for longer than one year. In addition, we do not find consistent evidence of changes in neighborhood composition (Appendix E). Therefore, we suspect that we are identifying the demand effects in this analysis and not a response to supply shocks to the housing market.

Robustness checks

The results are robust to an array of specifications and sample definitions, including omitting sales during the first quarter post-fire and changing the definition of the burn scar view above a minimum size threshold, e.g., 0.1 or 0.5 acre (Appendix B).

Although the repeat sales approach is ideal for ensuring covariates balance between the treated observations (with view) and the controls (without view), one concern discussed in Kuminoff and Pope (2014) is the instability of the hedonic price function. One strategy to deal with the problem of temporal shifts in the hedonic price function is to use cross-sectional variation in prices through regression or matching methods for identification. We construct a cross-sectional dataset with treated properties (with view) and controls (without view). For our matching estimator to be unbiased, we critically rely on the ability to construct good matches and balance covariates between the control and treatment populations. One issue with the matching estimator presented in Appendix B.1.2 is that there are few controls (without view) relative to the number of treated observations (with view) in the bins close to the burn scar (the closer a home is to the burn scar, the more likely it is to have a view). The small pool of controls poses a problem as balance improves for some covariates but worsens for others. Overall, with our data the balance between covariates after matching hardly appears satisfactory (Table B3). Poor balance on observables raises concerns for poor balance on unobservables and omitted variable bias. We present the matching results, along with results from an entropy balancing approach that mitigates the balancing issues arising with matching, in Appendices B.1.2 and B.1.3.

Our estimates of a burn scar view may also be attenuated since the Digital Elevation Model assumes that views are not blocked by physical structures on the earth, such as buildings and trees. To identify how much this is an issue we run a separate Digital Surface Model viewshed analysis for three counties using LiDAR satellite data accounting for all physical structures on the ground; thereby assigning properties with less error to the treatment or control groups (Figure 2; bottom panels). The tradeoff is that LiDAR data are not available for all our study counties and therefore we face a reduction in the sample size and reduced power for an increase in accuracy of assignment to treatment. Results in Table 4 suggest a similar burn scar view effect in the first year post-fire, ranging from -2.6% to -3.3%. These results are not statistically different than the results in Table 3 at the 10% level and suggest that our main findings are robust to the definition of burn scar view by LiDAR.

Table 4 Burn scar view estimates for the 0-2 and 3-4km bins using LiDAR data

	0-2km bin		3-4km bin	
	(1)	(2)	(3)	(4)
View_1	-0.0263*	-0.0325**	-0.0267**	-0.0269**
	(0.0152)	(0.0139)	(0.0123)	(0.0117)
View_2	-0.0043	0.0100	-0.0222**	-0.0181*
	(0.0169)	(0.0146)	(0.0108)	(0.0109)
$\text{View}_1 \times \text{Large}_1$	-0.0062	0.0018	-0.0106	-0.0097
	(0.0206)	(0.0163)	(0.0196)	(0.0189)
$\text{View}_2 \times \text{Large}_2$	-0.0039	-0.0117	-0.0103	-0.0063
	(0.0172)	(0.0145)	(0.0188)	(0.0180)
Quadratic county trends	Yes		Yes	
Year×Quarter	Yes		Yes	
County×Year×Quarter		Yes		Yes
N	5658	5658	9248	9248
R^2_{adj}	0.882	0.896	0.873	0.884

Note: Each specification includes Property fixed effects. Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

4.2 Effect of proximity to burn scar

In Table 5 we present the repeat sales estimates for properties within K-km to the burn scar relative to those further away. We also interact the proximity measure with the binary indicator for a view of the burn scar. We find insignificance of proximity to a burn scar when controlling for view of a burn scar. The subscripts 1 and 2 on coefficients in Table 5 refer to the year post-fire for which a coefficient is reported (e.g., K_1 indicates the coefficient for properties within K-km of the burn scar sold in the first year post-fire). Table 5 (all columns) shows no effect of proximity with estimates that are both statistically and economically insignificant. Though the results show a robust price decrease of 2.4% to 3.8% for properties with a burn scar view and within 3km that sold during the first year after a fire. These results also attenuate some in the second year post-fire with price decreases of 1.2% to 3.0%. These results qualitatively support our previous viewshed results. Proximity to a burn scar is robustly not significant and the disamenity of being close to the burn scar is attributable to having a view of it. Our estimates of disamenity associated with proximity effects differ from those found in Mueller and Loomis (2014) and Loomis (2004). This may at least be partially due to the fact that we identify the effect of proximity controlling for the burn scar view effect.

4.3 Effect of information signals on risk beliefs

Overall, we do not find evidence that wildfire events or updates in the risk zone assignment significantly affect the value of high-risk properties. Table 6 presents the effect of wildfires on property prices located on the risk zone relative to those located just outside the risk zone, while absent disamenities related to burn scar proximity and view (since properties are over 5km away from the

Table 5 Proximity effect estimates within threshold K-km of the burn scar

	$K=1$		$K=2$		$K=3$	
	(1)	(2)	(3)	(4)	(5)	(6)
K_1	-0.001879	-0.0118	-0.0029	-0.0042	0.0110	0.0112
	(0.0190)	(0.0187)	(0.0125)	(0.0114)	(0.0108)	(0.0091)
K_2	0.0091	0.0173	0.0142	0.0137	0.0101	0.0110
	(0.0247)	(0.0246)	(0.0129)	(0.0118)	(0.0098)	(0.0088)
$View_1$	-0.0238***	-0.0359***	-0.0235***	-0.0361***	-0.0298***	-0.0382***
	(0.0071)	(0.0066)	(0.0076)	(0.0072)	(0.0091)	(0.0087)
$View_2$	-0.0127*	-0.0262***	-0.0171**	-0.0302***	-0.0158*	-0.0300***
	(0.0067)	(0.0063)	(0.0071)	(0.0066)	(0.0091)	(0.0085)
$K_1 \times View_1$	0.0072	0.0090	0.0059	0.0044	0.0030	-0.0040
	(0.0239)	(0.0236)	(0.0168)	(0.0159)	(0.0151)	(0.0137)
$K_2 \times View_2$	-0.0003	-0.0076	0.0025	0.0028	0.0005	0.0015
	(0.0260)	(0.0253)	(0.0158)	(0.0142)	(0.0129)	(0.0120)
Quadr county trends	Yes		Yes		Yes	
Year×Quarter	Yes		Yes		Yes	
County×Year, Quarter		Yes		Yes		Yes
N	35343	35343	35343	35343	35343	35343
R^2_{adj}	0.859	0.859	0.860	0.859	0.860	0.859

Note: Each specification includes Property fixed effects. Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

fire perimeter).[21] In general, we find that the effects of being in a high risk area after a wildfire occurs are statistically insignificant, with the exception of the specification in column (3) (2.7% with p-value=0.05)—which is difficult to explain unless other factors are changing simultaneously with the status update. Our results differ substantially from McCoy and Walsh (2018) who show a large price decrease of 12.3% for high risk properties selling in the first year after a wildfire in Colorado, based on physical characteristics like terrain and vegetation (the effect is not significant beyond the first year). The absence of significant decrease in property prices suggests homeowners do not act upon the information conveyed by the risk zone designation. This finding differs considerably from studies on changing risk beliefs after hurricane and flood events, which find considerable effects of risk updating occurring after a major natural disaster event (e.g., Hallstrom and Smith (2005); Gibson et al. (2018)). The frequency of natural disasters may impact how risk updating occurs. Flood events are more rare than wildfire events and a high frequency event may be less likely to move risk beliefs. It may be that homeowners are already perfectly informed about wildfire risks due to the high-frequency of fires in the region and, therefore, a new wildfire event does not generate novel information and risk beliefs updating. The high frequency of disasters may also lull homeowners to be inattentive to new information. Last, we are cautious about these findings because of the small sample sizes for this analysis. Relaxing the 2km sample restrictions around the risk zone boundary

[21] Table 6 includes more observations than featured in Table A2 due to the presence of repeat sales properties close to the risk zone boundary but that did not experience a wildfire. Those properties help identify the trends and fixed effects. Still, in Table 6 we employ less restrictive county-level linear trends and separate the county and year-by-quarter fixed effects because of the small number of observations on the risk zone with-in the 1km sample restriction.

does not bring in more treated properties as the risk zone is relatively thin (Figures 3 and A1). It does allow more control properties but the ratio of treated to control observations is already low (Table A2), and the concern is that control properties farther away are more likely to be affected by unobservable trends correlated with the risk status and the price, thus biasing our estimates.

Table 6 Effect of latent risk on risk beliefs' updating after a wildfire

	Sample restrictions around the risk zone			
	Within 1km		Within 2km	
	(1)	(2)	(3)	(4)
Risk zone×PostFire	0.0166	0.0059	0.0270**	0.0213
	(0.0143)	(0.0147)	(0.0136)	(0.0140)
Linear county trends	Yes		Yes	
Year×Quarter	Yes		Yes	
Year×Quarter, County FE		Yes		Yes
N	23283	23283	35221	35221
R^2_{adj}	0.717	0.714	0.763	0.760

Note: Each specification includes Property fixed effects. Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table 7 shows that rezoning does not significantly impact properties newly assigned to the risk zone. Our preferred sample definitions restrict the analysis to properties as close to possible to the risk zone boundary (250m; columns (1) and (2)) to alleviate concerns of unobservable trends varying over space across the treated and control properties. The null result of fire risk rezoning is surprising, but may indicate again that homeowners are not updating risk beliefs with this information. It is possible that the high frequency of fires in these areas affects risk perception updating of rezoning as well as wildfire events. In essence, homeowners do not feel that they gain new information from such a signal when the frequency of events is relatively high. One caveat with our repeat sales design is that we are focusing on within-property variation for properties that sold once prior and once past the 2008 rezoning. In practice, properties may be selling multiple years post 2008. Therefore, it may not be surprising that any immediate effect of the rezoning attenuates overtime.

4.4 Neighborhood composition

A potential concern with identifying the value of disamenities using temporal variation in prices, as we do with repeat sales, is the instability of the hedonic price function (Kuminoff and Pope, 2014). For example, if neighborhoods change in response to fire events, our disamenity estimate would simply capture a capitalization effect rather than the marginal willingness-to-pay, or change in surplus, associated with a change in environmental quality (Banzhaf, 2015). However, since wildfires appear to happen randomly over space and time across the wildland-urban interface surrounding the LA and San Diego basins, we do not expect a single wildfire event to result in large neighborhood changes. One way in which we may identify more systemically such shifts in the equilibrium of

Table 7 Effect of new risk zoning on risk beliefs' updating

	Sample restrictions around the risk zone					
	Within 250m		Within 500m		Within 750m	
	(1)	(2)	(3)	(4)	(3)	(4)
ΔRiskZone×PostRezoning	-0.0021	-0.0098	0.0169	0.0097	0.0227	0.0135
	(0.0313)	(0.0365)	(0.0234)	(0.0226)	(0.0193)	(0.0183)
Quadratic county trends	Yes		Yes		Yes	
Year×Quarter	Yes		Yes		Yes	
County×Year×Quarter		Yes		Yes		Yes
N	4120	4120	8144	8144	12350	12350
R^2_{adj}	0.735	0.753	0.770	0.788	0.774	0.789

Note: Each specification includes Property fixed effects. Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

the hedonic price function is through inspection of the demographics of the buyers over time in our study area. Following Bayer et al. (2016) and Haninger et al. (2017), we use data from the Home Mortgage Disclosure Act (HMDA) to capture the buyers mortgage application information. The HMDA data provide income, gender, race, and ethnicity of the applicant, as well as the loan amount and year, lender name, and census tract of the property. In Appendix E, we use these data to test whether the distributions of income, race, and ethnicity change after a wildfire. Overall, we do not find evidence that neighborhood composition is affected by burn scar view, proximity, or changes in risk zone assignment or wildfire events.

5 Model of risk beliefs updating

To shed light on our empirical results, we now present a conceptual model of risk beliefs updating. This model clarifies how subjective risk beliefs enter into home buyer's willingness to pay for housing and provides a interpretation of our null results. Following on Rosen (1974), Kousky (2010), and more directly on Gibson et al. (2018), we define the hedonic home price as $H(\mathbf{Z}, p)$, where $\mathbf{Z}$ denotes a vector of housing and neighborhood attributes, and p is a homeowner's subjective probability of a wildfire. We assume p is a function of the fire risk zone F and recent fire events E. Letting Y represent the homeowner's income and X the consumption of a numeraire good (with price normalized to 1), the homeowner's budget constraint is $Y = X + H(\mathbf{Z}, p)$. For simplicity, let us assume that insurance premiums are exogenous to a single wildfire event E and that all homes are fully insured against physical fire damages.[22] Yet, the loss of non-market amenities (visual or

[22]Following Proposition 103 passed in 1988, home insurance is heavily regulated in California (California Department of Insurance, 2018). Increases in insurance rates have to be approved by the California Department of Insurance. They cannot be driven by a single wildfire year, but instead must be based on long-term trends that look back at wildfire damages over the last 20 years (Daniels, 2017). In addition, in the United States fire damages are covered under regular home insurance. All banks require home insurance as a pre-condition for a mortgage. Virtually all homes in our dataset have a mortgage with a bank. Homeowners who have may not been able to obtain insurance in the regular market can purchase basic insurance under the 1968 California Fair Access to Insurance Requirements (FAIR) Plan.

proximity) or changes in risk would realistically not be insured. Let us assume that the subjective expected amenity losses, L, resulting from a wildfire depend on the fire risk zone F and recent fire events E. The state-dependent consumption levels are given by X_1 and X_0:

$$\begin{aligned} X_1 &= Y - H(\mathbf{Z}, p) - L \\ X_0 &= Y - H(\mathbf{Z}, p). \end{aligned} \tag{5}$$

Assuming a twice continuously differentiable, concave von Neumann-Morgenstern utility function U, the expected utility can be written:

$$EU = pU(X_1, \mathbf{Z}) + (1-p)U(X_0, \mathbf{Z}). \tag{6}$$

Substituting in (5) gives:

$$EU = p(F,E)U(Y - H(\mathbf{Z}, p(F,E)) - L(F,E), \mathbf{Z}) + (1 - p(F,E))U(Y - H(\mathbf{Z}, p(F,E)), \mathbf{Z}). \tag{7}$$

Now, we can examine the marginal effect of our two information signals on housing prices. A wildfire event generates an information signal E. Maximizing EU with respect to E, we solve for the marginal effect of a change in wildfire exposure on the hedonic price:

$$\frac{\partial H}{\partial E} = \frac{[U(X_1) - U(X_0)]\frac{\partial p}{\partial E} - p\frac{\partial U}{\partial X_1}\frac{\partial L}{\partial E}}{p\frac{\partial U}{\partial X_1} + (1-p)\frac{\partial U}{\partial X_0}}. \tag{8}$$

Applying the intermediate value theorem and a Taylor series expansion on (8) yields:[23]

$$\frac{\partial H}{\partial E} \approx -L[1 + (X_c - X_m)r(X_c)]\frac{\partial p}{\partial E} - p[1 + (X_c - X_1)r(X_c)]\frac{\partial L}{\partial E}, \tag{9}$$

where X_c denotes the point at which the marginal utility of consumption is equal to the expected value of marginal utility of consumption across fire and non-fire states, while X_m is the average marginal utility of consumption over the interval $[X_1, X_0]$. The Arrow-Pratt absolute risk aversion is denoted $r(X)$. The effect of a recent fire on homeowners' risk beliefs is *ex ante* ambiguous, $\frac{\partial p}{\partial E} \lesseqgtr 0$, e.g., it may be driven by greater saliency or alternatively reduction in fuel in the region. Similarly, a recent fire may lead homeowners to revise upward or downward their expected losses, i.e., $\frac{\partial L}{\partial E} \lesseqgtr 0$. As a result of these two effects, a wildfire may decrease, increase, or not affect property values through risk beliefs. Our empirical results showing the absence of an effect may correspond to 1) $\frac{\partial p}{\partial E} = \frac{\partial L}{\partial E} = 0$, 2) $\frac{\partial p}{\partial E} < 0$ and $\frac{\partial L}{\partial E} > 0$, or 3) $\frac{\partial p}{\partial E} > 0$ and $\frac{\partial L}{\partial E} < 0$. In the first case, homeowners

[23] Following the derivation in Gibson et al. (2018), there exists a point X_c on $[X_1, X_0]$ such that $\frac{\partial U}{\partial X_c} = p\frac{\partial U}{\partial X_1} + (1-p)\frac{\partial U}{\partial X_0}$, and a point X_m on $[X_1, X_0]$ such that $\frac{\partial U}{\partial X_m} = \frac{U(X_1) - U(X_0)}{X_1 - X_0} = \frac{U(X_1) - U(X_0)}{-L}$. Applying a Taylor series expansion yields $\frac{\partial U}{\partial X_m} \approx \frac{\partial U}{\partial X_c} + (X_m - X_c)\frac{\partial^2 U}{\partial X_c^2}$ and $\frac{\partial U}{\partial X_1} \approx \frac{\partial U}{\partial X_c} + (X_1 - X_c)\frac{\partial^2 U}{\partial X_c^2}$.

are already well informed about risk and do not revise their expected amenity losses after a fire. In the second and third cases, they revise their risk beliefs and their expected losses, with both effects cancelling each other out.

Conducting a similar analysis to examine the marginal effect of a change in risk zoning F on housing prices yields:

$$\frac{\partial H}{\partial F} = \frac{[U(X_1) - U(X_0)]\frac{\partial p}{\partial F} - p\frac{\partial U}{\partial X_1}\frac{\partial L}{\partial F}}{p\frac{\partial U}{\partial X_1} + (1-p)\frac{\partial U}{\partial X_0}}, \tag{10}$$

and the local approximation:

$$\frac{\partial H}{\partial F} \approx -L[1 + (X_c - X_m)r(X_c)]\frac{\partial p}{\partial F} - p[1 + (X_c - X_1)r(X_c)]\frac{\partial L}{\partial F}. \tag{11}$$

Thus, the effect of rezoning can affect housing prices through two channels: 1) the subjective fire probability ($\frac{\partial p}{\partial F} \geq 0$) and 2) the subjective expected amenity loss ($\frac{\partial L}{\partial F} \geq 0$), which are both non-decreasing. In our setting in which the rezoning does not affect property prices it must thus be that both effects are nil, suggesting that homeowners do not view the rezoning are conveying credible new information about the fire risk, and do not anticipate greater amenity loss from location on the risk zone.

6 Conclusions

Burned vistas are a critical pathway through which wildfires affect property values. We find that properties with a view of a burn scar located within 2km experience a 4.2% to 5.0% price drop relative to similar properties without a view. This effect attenuates with distance to the burn scar and over time. Our estimates are smaller and statistically weaker in the second year post-fire. In addition, once controlling for visual disamenity, properties located near a burn scar do not face a price loss relative to properties farther away, suggesting most of the disamenity value associated with burn scar perimeters occurs through the burned viewshed channel.

Furthermore, we do not find evidence that the two types of risk information signals we investigate affect homeowners' risk beliefs updating. Because wildfires may affect homeowners' risk beliefs updating through both changes in their subjective wildfire probability and losses, their net effect is *ex ante* ambiguous. Indeed, it is not clear how homeowners may interpret this signal. For example, they may believe that a recent fire may imply future fires in the region are now less likely (subjective probabilities are revised downward), but it may also lead them to reassess upward their subjective losses in case of a fire (due to greater risk salience). In our setting, we find that either both effects (subjective probabilities and losses) are nil or they move in opposite directions and cancel each other out. Unfortunately, our quasi-experimental design does not allow us to isolate each of these effects separately. Overall, our result contrasts with the findings of Hallstrom and Smith (2005) and Gibson et al. (2018) (among others) who find that hurricane and flood risk beliefs are updated with large natural disaster events. We must consider how hurricane/flood and fire risk beliefs may vary as major flood events are less frequent but often more destructive than fire events, which may impact how these signals are interpreted.

The second information signal we consider (risk zoning) is of direct relevance to policymakers since it is a common management tool to inform local residents of underlying natural disaster risks. The question is whether this tool is effective. Our findings suggest homeowners do not pay attention or do not find relatively small changes in the risk zoning credible as they neither update their subjective wildfire probability or expected losses. It is important to point out that our study examines a relatively marginal change in the risk zone assignment and, thus, we cannot make inferences about the effect of designating a new risk zone.

Yet, taken together these findings are concerning as the risk zone assignment and wildfire events are not conveying new information about risks. This may lead to a continuation of over development and greater demand for housing in high-risk areas without due weight to the increased risks from future natural disasters. The absence of beliefs updating may indicate a failing of the risk zone to adequately inform homeowners of increased wildfire risks, or that beliefs are slow to change. It may also indicate that property owners who choose to live in designated high-risk areas ignore new information about wildfire risks, consistent with Bakkensen and Barrage (2017). These explanations are somewhat troublesome for policymakers since efficient natural disaster policies presumably require homeowners to update their risk beliefs about natural disaster to align the long-term costs of mitigation and relief with the demand to live in such areas. As climate change effects intensify and development in risky areas, such as the wildland-urban interface, continues, policymakers must face the challenge of how to convey increases in natural disaster risk to homeowners. More research is needed to understand how risk information can be effectively conveyed to property owners.

Last, our estimates come from a uniquely large dataset for the Los Angeles and San Diego basins in southern California. This is advantageous and important for our quasi-experimental design and wildfire policy as this region offers a prime empirical application to quantify wildfire disamenities in a large metropole area subject to frequent wildfires. As the increase in the severity and frequency of wildfires continues, there is much interest in how to weigh the costs and benefits of prevention and mitigation strategies in the wildland-urban interface. Our estimates of the disamenities of wildfires, via reduced marginal willingness-to-pay for key property attributes (including burned vistas) suggest significant costs of wildfires—in addition to that of damaged properties and businesses. Such estimates are critical to conduct a cost-benefit analysis of government fire protection programs.

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A Additional figure and summary statistics

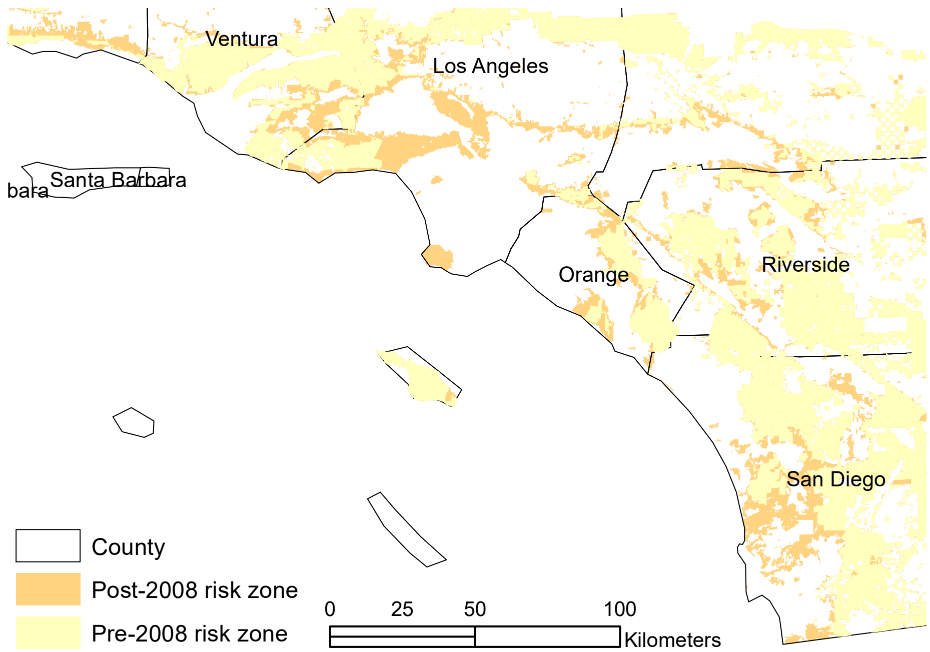


Figure A1 California wildfire risk zone, including the 2008 risk zone expansion.

Table A1 Summary characteristics of properties (full sample vs. repeat sales) sold within two years post-fire. (Statistics for repeat sales properties are shown for the most recent sale.)

	0-2km distance bin				2-4km distance bin			
	All		Repeat sales		All		Repeat sales	
	Means	(sd)	Means	(sd)	Means	(sd)	Means	(sd)
Sale price (k$2015)	565.55	(306.31)	513.35	(278.90)	497.98	(286.22)	445.57	(246.32)
Age	26.91	(21.38)	27.47	(21.58)	29.69	(21.94)	27.22	(21.90)
Living area (k sqft)	2.12	(0.80)	2.04	(0.80)	2.01	(0.76)	2.05	(0.77)
# bedrooms	3.50	(0.80)	3.47	(0.80)	3.43	(0.81)	3.48	(0.81)
# bathrooms	2.65	(0.84)	2.61	(0.82)	2.52	(0.80)	2.57	(0.78)
Swimming pool (0/1)	0.23	(0.42)	0.20	(0.40)	0.22	(0.41)	0.20	(0.40)
Dist. green space (km)	0.51	(0.49)	0.48	(0.45)	0.57	(0.53)	0.58	(0.55)
Elevation (m)	283.90	(186.51)	271.35	(173.35)	293.52	(182.18)	298.21	(174.79)
Slope	4.28	(4.58)	4.00	(4.46)	3.50	(4.14)	3.20	(4.00)
Risk zone (0/1)	0.20	(0.40)	0.18	(0.38)	0.10	(0.30)	0.10	(0.31)
WUI (0/1)	0.83	(0.38)	0.80	(0.40)	0.64	(0.48)	0.62	(0.49)
Dist. main road (km)	1.50	(1.23)	1.46	(1.19)	1.39	(1.17)	1.38	(1.18)
Dist. burn scar (km)	1.17	(0.54)	1.17	(0.55)	3.10	(0.57)	3.12	(0.56)
Burn scar view (0/1)	0.80	(0.40)	0.79	(0.40)	0.53	(0.50)	0.51	(0.50)
Median hh. income (k$)	88.98	(28.00)	84.61	(26.07)	82.58	(27.87)	79.82	(25.20)
% white	72.76	(13.54)	69.32	(13.84)	71.06	(14.60)	68.61	(14.56)
% hispanic	27.39	(19.12)	32.40	(21.55)	30.77	(18.95)	34.26	(19.71)
# of unique properties	19910		5286		40493		12378	
# of census tracts	689		492		1299		982	
# of fires	194		120		262		164	

Table A2 Summary characteristics of the repeat sales properties that sold during the first two years post-fire for different distance thresholds from the risk zone boundary (properties are referred to "on" or "off" the risk zone)

	Within 1km				Within 2km			
	On		Off		On		Off	
	Means	(sd)	Means	(sd)	Means	(sd)	Means	(sd)
Sale price (k$2015)	719.51	(313.08)	593.81	(286.61)	731.95	(311.78)	565.27	(291.83)
Age	32.00	(25.20)	41.87	(24.30)	30.94	(24.92)	44.85	(24.34)
Living area (k sqft)	2.19	(0.88)	1.80	(0.74)	2.24	(0.88)	1.70	(0.69)
# bedrooms	3.43	(0.92)	3.25	(0.82)	3.44	(0.93)	3.18	(0.82)
# bathrooms	2.86	(1.00)	2.39	(0.88)	2.86	(0.99)	2.26	(0.86)
Swimming pool (0/1)	0.17	(0.38)	0.16	(0.36)	0.16	(0.37)	0.14	(0.35)
Dist. green space (km)	0.42	(0.54)	0.43	(0.35)	0.49	(0.63)	0.40	(0.31)
Elevation (m)	159.10	(97.88)	135.08	(94.84)	176.90	(116.31)	115.42	(89.79)
Slope	4.53	(4.41)	3.59	(3.84)	4.71	(4.50)	3.09	(3.54)
WUI (0/1)	0.93	(0.25)	0.71	(0.46)	0.94	(0.24)	0.54	(0.50)
Dist. main road (km)	1.72	(1.10)	1.30	(1.04)	1.79	(1.11)	1.20	(1.02)
Dist. burn scar (km)	11.64	(2.58)	11.19	(2.82)	11.27	(2.88)	11.06	(2.88)
Median hh. income (k$)	105.07	(35.98)	79.91	(28.08)	104.30	(35.16)	75.56	(28.19)
% white	78.84	(13.08)	68.73	(21.60)	79.18	(13.11)	66.95	(20.71)
% hispanics	19.99	(18.55)	28.61	(21.59)	19.18	(17.83)	34.14	(24.60)
Years between sales	4.39	(1.84)	4.57	(2.28)	4.63	(2.05)	4.62	(2.32)
# of unique properties	303		1022		337		1889	

Table A3 Summary characteristics of the repeat sales properties that sold post updating of the risk zone map for different distance thresholds from the risk zone boundary (properties are referred to "newly on" or "always off" the risk zone)

	Within 250m				Within 750m			
	Newly on		Always off		Newly on		Always off	
	Means	(sd)	Means	(sd)	Means	(sd)	Means	(sd)
Sale price (k$2015)	747.33	(318.13)	674.76	(292.81)	647.54	(246.38)	657.65	(290.52)
Age	20.26	(13.82)	30.68	(19.89)	17.76	(13.17)	29.91	(20.71)
Living area (k sqft)	2.48	(0.91)	2.08	(0.80)	2.55	(0.83)	2.06	(0.78)
# bedrooms	3.73	(0.78)	3.49	(0.79)	3.70	(0.74)	3.47	(0.79)
# bathrooms	3.05	(0.95)	2.68	(0.85)	3.09	(0.85)	2.66	(0.84)
Swimming pool (0/1)	0.26	(0.44)	0.21	(0.41)	0.24	(0.43)	0.21	(0.40)
Dist. green space (km)	0.39	(0.35)	0.42	(0.39)	0.40	(0.38)	0.43	(0.36)
Elevation (m)	270.65	(106.97)	203.15	(98.41)	251.46	(103.56)	198.38	(92.46)
Slope	6.02	(4.79)	4.47	(3.88)	5.45	(3.98)	4.37	(3.90)
WUI (0/1)	1.00	(0.00)	0.96	(0.19)	1.00	(0.00)	0.91	(0.28)
Dist. main road (km)	2.15	(1.41)	1.59	(1.21)	1.92	(1.34)	1.44	(1.13)
Median hh. income (k$)	107.90	(25.39)	92.69	(29.41)	107.73	(23.39)	90.32	(27.88)
% white	81.62	(9.21)	79.26	(13.85)	80.88	(9.34)	79.15	(13.51)
% hispanic	16.91	(10.95)	20.62	(14.56)	15.95	(9.32)	21.89	(15.23)
Years between sales	6.61	(2.04)	3.38	(1.69)	6.55	(2.16)	3.72	(2.04)
# of unique properties	570		1775		1268		4905	

B Additional burn scar view results

Table B1 Burn scar view estimates for each 1km bin

	0-1km bin		1-2km bin		2-3km bin		3-4km bin	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$View_1$	-0.0313	-0.0501**	-0.0522***	-0.0634***	-0.0280**	-0.0484***	-0.0197**	-0.0316***
	(0.0245)	(0.0216)	(0.0159)	(0.0159)	(0.0131)	(0.0137)	(0.0097)	(0.0095)
$View_2$	-0.0092	-0.0151	-0.0286*	-0.0289*	-0.0334***	-0.0454***	-0.0105	-0.0265***
	(0.0225)	(0.0203)	(0.0165)	(0.0165)	(0.0111)	(0.0106)	(0.0097)	(0.0094)
$View_1 \times Large_1$	0.0027	0.0164	0.0125	0.0123	0.0021	0.0056	-0.0443**	-0.0349*
	(0.0262)	(0.0237)	(0.0212)	(0.0218)	(0.0177)	(0.0190)	(0.0202)	(0.0205)
$View_2 \times Large_2$	0.0049	-0.0111	0.0043	-0.0059	0.0142	0.0102	0.00450	0.0091
	(0.0268)	(0.0206)	(0.0194)	(0.0195)	(0.0170)	(0.0156)	(0.0176)	(0.0184)
Qd cty tr	Yes		Yes		Yes		Yes	
Year×Qtr	Yes		Yes		Yes		Yes	
Cty×Yr, Qtr		Yes		Yes		Yes		Yes
N	4048	4048	6525	6525	9928	9928	14842	14842
R^2_{adj}	0.857	0.868	0.839	0.843	0.859	0.858	0.875	0.871

Note: Each specification includes Property fixed effects. Robust standard errors clustered at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

B.1 Robustness checks

B.1.1 Repeat sales

Due to potential concerns about the properties that sold immediately after a fire, we omit properties that sold during the first quarter after a fire (Table B2).

Table B2 Burn scar view estimates for the 0-2 and 3-4km bins, dropping sales during the first quarter post-fire

	0-2km bin		3-4km bin	
	(1)	(2)	(3)	(4)
$View_1$	-0.0411**	-0.0499***	-0.0192**	-0.0304***
	(0.0159)	(0.0143)	(0.00905)	(0.00849)
$View_2$	-0.0240*	-0.0247*	-0.0183**	-0.0267***
	(0.0144)	(0.0129)	(0.00750)	(0.00691)
$View_1 \times Large_1$	0.00211	0.00255	-0.00245	-0.00637
	(0.0203)	(0.0176)	(0.0151)	(0.0149)
$View_2 \times Large_2$	0.00331	-0.00847	0.0101	0.00491
	(0.0178)	(0.0160)	(0.0138)	(0.0126)
Quadratic county trends	Yes		Yes	
Year×Quarter	Yes		Yes	
County×Year×Quarter		Yes		Yes
N	9299	9299	22124	22124
R^2_{adj}	0.842	0.862	0.868	0.880

Note: Each specification includes Property fixed effects. Robust standard errors clustered at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

To complement the repeat sales specification that relies on temporal variation, we turn to two empirical strategies (matching and entropy balancing) that exploit variation across properties sold at the same time.

B.1.2 Nearest Neighbor matching

Matching techniques can reduce model dependence by balancing covariates among control and treated groups such that assignment to treatment appears random based on observables (Abadie and Imbens, 2006, 2011). Matching prunes observations from the original dataset so that the remaining data show better covariates balance. A growing number of studies have used nearest-neighbor matching (NNM) techniques to infer the average capitalized value of environmental amenities, including lake community amenities, shale gas development, and brown field remediation, e.g., Abbott and Klaiber (2013); Muehlenbachs et al. (2015); Haninger et al. (2017), among others. Yet, one important pitfall of matching techniques such as NNM is that they may not improve the balance of *all* covariates, and may even worsen the balance of potential confounders. As a result, it may be difficult to determine whether matching has actually reduced model dependence and estimates bias (Diamond and Sekhon, 2013).

We employ NNM to recover the average capitalized value of burn scar views by comparing treated properties with a burn scar view to similar control properties without a burn scar view in the same distance bin to the burn scar. The effect of the burn scar view treatment results from averaging across the home value differences for matched treated and control pairs. One approach to evaluate whether NNM unambiguously reduces model dependence for our data is to compare how the empirical distributions of the covariates compare across the matched control and treated groups, e.g., as discussed in Abbott and Klaiber (2013). From Table B3, we can see that while NNM has improved the balance on elevation and slope, covariates balance has worsened for age, number of bedrooms, and distance to green space (i.e., national forest or local or state park). Furthermore, distance to the burn scar, which is a key variable that can potentially confound burn scar view and proximity effects, is hardly satisfactorily balanced between the matched treated and control groups. In addition, the closer the property is to the burn scar, the likelier it is to have a burn scar view and, therefore, the higher the proportion of treated properties (with burn scar view) relative to controls. As a result, the probability of a successful match, i.e., a treated property matched with (at least) two controls, is lower near the burn scar (only 90% in bin 0-2km). This further raises concerns about the internal validity of NNM estimates for distance bins close to the burn scars.

Table B3 Covariates balance for the full and matched samples for the 0-2km bin

	Full sample				Matched sample			
	Treated		Control		Treated		Control	
	Mean	(sd)	Mean	(sd)	Mean	(sd)	Mean	(sd)
Age	27.64	(21.71)	27.50	(20.57)	27.55	(21.78)	26.66	(20.57)
Living area (k sqft)	2.07	(0.79)	2.18	(0.84)	2.10	(0.80)	2.22	(0.84)
# bedrooms	3.48	(0.79)	3.50	(0.81)	3.49	(0.80)	3.53	(0.82)
# bathrooms	2.62	(0.83)	2.68	(0.88)	2.65	(0.84)	2.71	(0.88)
Swimming pool (0/1)	0.23	(0.42)	0.26	(0.44)	0.23	(0.42)	0.26	(0.44)
Risk zone (0/1)	0.16	(0.37)	0.20	(0.40)	0.20	(0.40)	0.24	(0.43)
WUI (0/1)	0.81	(0.39)	0.86	(0.35)	0.84	(0.37)	0.87	(0.34)
Elevation (m)	291.09	(192.74)	260.76	(171.29)	275.19	(190.33)	273.48	(172.56)
Slope	3.62	(3.77)	6.13	(5.96)	3.88	(4.05)	6.56	(6.18)
Dist. green space (km)	0.51	(0.48)	0.50	(0.44)	0.49	(0.48)	0.55	(0.53)
Dist. road (km)	1.43	(1.22)	1.54	(1.14)	1.51	(1.25)	1.60	(1.23)
Dist. burn scar (km)	1.11	(0.55)	1.36	(0.45)	1.12	(0.55)	1.34	(0.45)
Observations	16020		3890		14442		3826	

NNM estimates of burn scar view are presented in Table B4. We find no significant effect of burn scar view in the 0-2km bin and a positive significant effect in the 3-4km bin. We are concerned that these estimates are biased based on the poor balancing of covariates across treated and control groups and therefore prefer the entropy balancing preprocessing of the data, which we describe next.

Table B4 Burn scar view nearest-neighbor matching estimates by distance bin to the burn scar

	0-2km	3-4km
View	-0.017	0.013**
	(0.012)	(0.006)
N	18268	40318

Note: Matching using two nearest neighbors based on the Euclidean metric with exact matching on county and sale year. Soft match on age, square footage, #bedrooms and baths, swimming pool, risk zone, and WUI dummies, elevation, slope, distances to the nearest green space, primary road, and burn scar, and census tract's median household income, %white and hispanic. Robust standard errors in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

B.1.3 Entropy balancing

As a result of our concerns with strong model dependence arising from matching estimators, we use entropy balancing, a new method developed in political science by Hainmueller (2012). Entropy balancing is a data preprocessing technique that generates a set of weights (one weight for each observation in the control group) so that the distributions of the relevant covariates in the treated and weighted control groups are identical for the sample moments specified by the researcher (up to the third moment). To reduce information loss, entropy balancing solves for the set of weights that satisfies the balance conditions for the selected covariates while minimizing the departure from the uniform base weights. It is important to note that observations are neither matched nor pruned but simply weighted (unlike with NNM or propensity score matching techniques). Any estimation technique can then be employed using the treated and entropy weighted control groups, such as fixed effects to control for remaining unobservables. This method in conjunction with our spatial and time fixed effects regression identification strategy provides a way to estimate the specific disamenity of burn scar views when matching techniques do not sufficiently resolve covariate balancing issues. It is of particular importance in the presence of limited sample sizes and is salient to the state of the literature as matching has been used heavily in the hedonic pricing literature since it provides distinct advantages over other statistical techniques.

Table B5 illustrates the covariates balance between the treated and entropy weighted control groups. The assignment to the burn scar view treatment appears as close to possible to random based on observables.[24] Note that we do not lose (prune) any observations either in the treated or control group.

Our preferred model specification, which uses the entropy weights that balance the set of covariates listed in Table B5, builds on equation (1) and is written as:

$$\ln p_{it} = \sum_{j}(\beta_j View_{jit} + \gamma_j View_{jit} \times Large_{jit}) + \mathbf{Z}_i'\boldsymbol{\omega} + Census_{it} + \mu_{it} + \epsilon_{it}. \qquad (12)$$

The dependent variable represents property i's sale price at time t. Our set of structural property-specific controls, $\mathbf{Z}_i$, include: square footage and age, indicator variables for number of bathrooms and bedrooms; a variable indicating if a property has a swimming pool, geographic characteristics including distance to the nearest burn scar and nearest green space, elevation, and slope. $Census_{it}$

[24] We find that entropy balancing on the third moment is unnecessary in our data as the control and treated variables have a good level of balance on the third moment without additional specification.

Table B5 Covariates balance with entropy balancing on first and second moments for the 0-2km bin

	Treated		Control Pre-balancing		Control Post-balancing	
	Mean (sd)		Mean (sd)		Mean (sd)	
Age	27.64	21.71	26.59	20.46	27.64	21.71
Living area (k sqft)	2.07	0.79	2.22	0.84	2.07	0.79
# bedrooms	3.48	0.79	3.53	0.81	3.48	0.79
# bathrooms	2.62	0.83	2.71	0.88	2.62	0.83
Swimming pool (0/1)	0.23	0.42	0.26	0.44	0.23	0.42
Risk zone (0/1)	0.16	0.37	0.23	0.42	0.16	0.37
WUI (0/1)	0.81	0.39	0.87	0.34	0.81	0.39
Elevation (m)	291.09	192.74	272.54	172.14	291.10	192.74
Slope	3.62	3.77	6.50	6.17	3.62	3.77
Dist. green space (km)	0.51	0.48	0.55	0.53	0.51	0.48
Dist. road (km)	1.43	1.22	1.61	1.23	1.43	1.22
Dist. burn scar (km)	1.11	0.55	1.35	0.45	1.11	0.55
Observations	16020		3890		3890	

denote census tract fixed effects controlling for time-invariant neighborhood unobservables. μ_{it} represent either quadratic county-level trends and year-by-quarter fixed effects or county-by-year-by-quarter fixed effects.

We find in Table B6 that the effect of view of a burn scar is similar to our main finding. These results reveal that properties with a view of a burn scar located within 2km of a burn scar experience a 1.6% to 1.9% price drop in the first year post-fire relative to similar properties without a view. Properties that are farther away, up to 4km, do not appear to be affected by having a view of a burn scar. The burn scar view effect attenuates in the second year post-fire with a price drop of 1.0% in the first 2km bin (p-value=0.1). These results comport well with the repeat sales model results but does not suffer from the concerns about hedonic price equilibrium shifts.

Table B6 Entropy balancing burn scar view estimates for each 2km bin

	0-2km bin		3-4km bin	
	(1)	(2)	(3)	(4)
View_1	-0.0190***	-0.0163***	-0.0029	-0.0037
	(0.0060)	(0.0063)	(0.0050)	(0.0053)
View_2	-0.0104*	-0.0103*	-0.0068	-0.0047
	(0.0061)	(0.0061)	(0.0050)	(0.0048)
$\text{View}_1 \times \text{Large}_1$	0.0052	0.0027	-0.0027	-0.0100
	(0.0089)	(0.0089)	(0.0108)	(0.0102)
$\text{View}_2 \times \text{Large}_2$	0.0083	-0.0081	0.0177*	-0.0005
	(0.0090)	(0.0089)	(0.0098)	(0.0086)
Quadratic county trends	Yes		Yes	
Year×Quarter	Yes		Yes	
County×Year×Quarter		Yes		Yes
N	19910	19910	40499	40499
R^2_{adj}	0.915	0.917	0.910	0.913

Note: Each specification includes entropy weights and Census tract fixed effects. Covariates are balanced on the first and second moments using entropy balancing. Covariates include: age, square footage, #bedrooms and baths, swimming pool, risk zone, and WUI dummies, elevation, slope, distances to the nearest green space, primary road, and burn scar. Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

C Additional proximity effect results

Table C1 Proximity effect estimates within threshold K-km of the burn scar and *without* a view

	$K=1$		$K=2$		$K=3$	
	(1)	(2)	(3)	(4)	(5)	(6)
K_1	-0.000266	-0.0122	0.000969	-0.00623	0.0155	0.00645
	(0.0198)	(0.0199)	(0.0120)	(0.0104)	(0.0101)	(0.00841)
K_2	0.00389	-0.00291	0.0136	0.0134	0.00948	0.00995
	(0.0238)	(0.0183)	(0.0126)	(0.0106)	(0.00950)	(0.00767)
Quadr county trends	Yes		Yes		Yes	
Year×Qtr	Yes		Yes		Yes	
County×Year×Qtr		Yes		Yes		Yes
N	14413	14413	14413	14413	14413	14413
R^2_{adj}	0.859	0.877	0.859	0.877	0.859	0.877

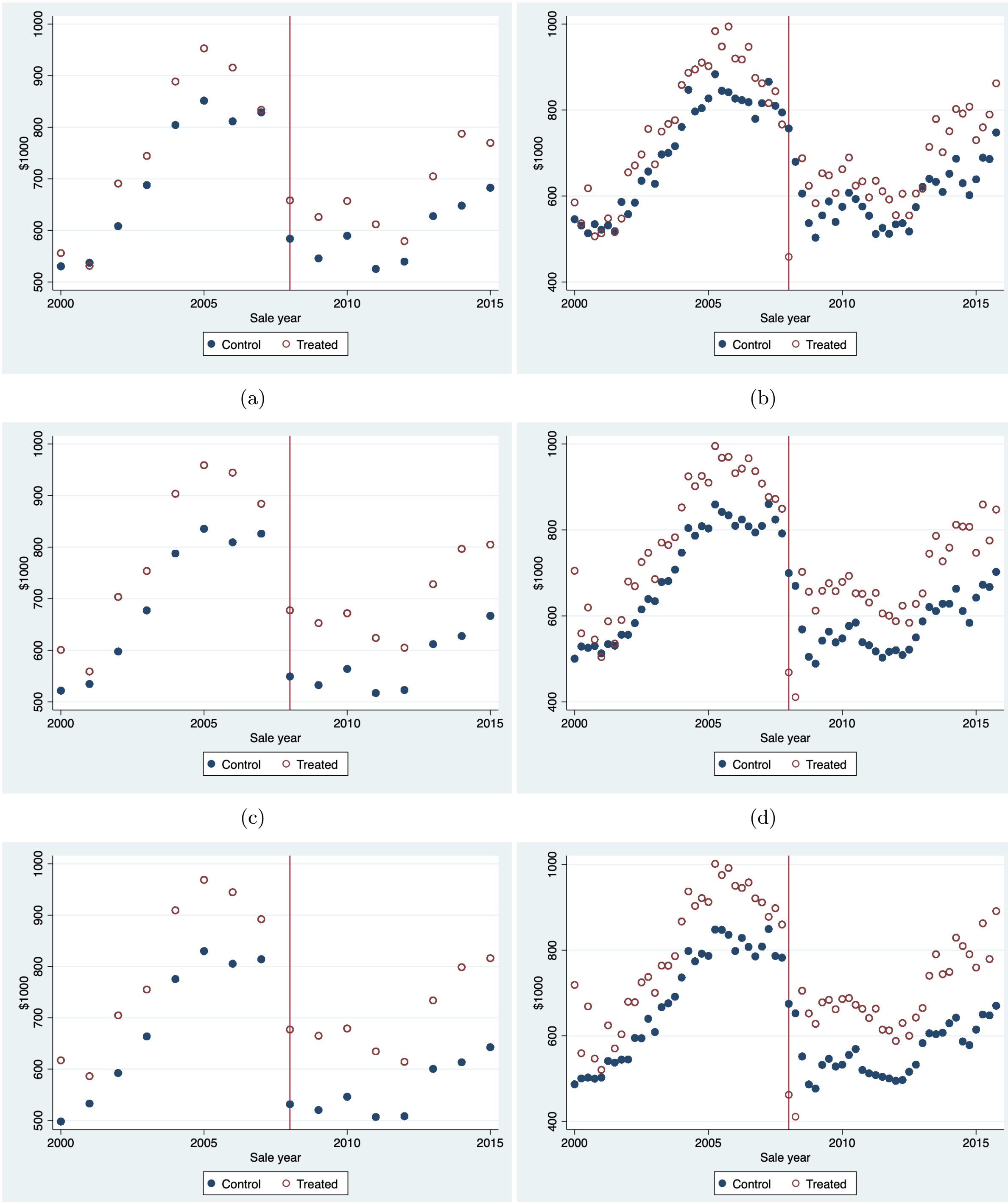
Note: Each specification includes Property fixed effects. Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table C2 Proximity effect estimates within threshold K-km of the burn scar for properties *with* a view

	$K=1$		$K=2$		$K=3$	
	(1)	(2)	(3)	(4)	(5)	(6)
K_1	-0.00241	-0.00420	-0.00398	-0.00524	-0.00439	-0.0113
	(0.0153)	(0.0141)	(0.0105)	(0.00961)	(0.00871)	(0.00828)
K_2	0.0105	0.00756	0.0162	0.0156	0.00588	0.00481
	(0.0151)	(0.0133)	(0.0100)	(0.00961)	(0.00836)	(0.00839)
Quadr county trends	Yes		Yes		Yes	
Year×Qtr	Yes		Yes		Yes	
County×Year×Qtr		Yes		Yes		Yes
N	14413	14413	14413	14413	14413	14413
R^2_{adj}	0.859	0.877	0.859	0.877	0.859	0.877

Note: Each specification includes Property fixed effects. Robust clustered standard errors at the census-tract level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

D Effect of the 2008 rezoning on wilfdire risk beliefs



(a) (b) (c) (d) (e) (f)

Figure D1 Visual evidence supporting the common trends assumption. The left panels show average yearly home prices, and rights panels average quarterly prices for the repeat sales properties newly assigned to the risk zone in 2008 (treated group) and those that always remained outside the risk zone (control group). The top panels include properties within 250m from the risk zone boundary, while the middle and bottom panels include properties within 500m, and 750m from the boundary, respectively.

E Composition of buyers in the market

Using data from the Home Mortgage Disclosure Act (HMDA), we test whether the distributions of income, race, and ethnicity change after a wildfire. We adopt a difference-in-differences framework for sales within two years pre- and post-fire to identify if treated properties are experiencing shifting demographics at the neighborhood level relative to control properties.[25]

For the burn scar view and proximity treatments, we start with the properties that sold within two years pre- and post-fire within 4km of a burn scar (119,815 observations). We drop observations with no mortgage year, no loan amount, no lendername, or indications that the lender was a private lender (108,932 remaining observations). Matching on mortgage year, lender name, loan amount and type, county, and census tract, leads to 64,230 matches. Keeping properties with unique matches, we end up with 57,699 properties, or a 53% matching success rate.[26] Table E1 shows that the distributions of income, race, and ethnicity do not significantly change across properties with or without a view of the burn scar during the first two years after a wildfire. Overall, results for the proximity to a burn scar, presented in Table E2, show little effect of wildfire proximity on demographics, with the exception of small decreases in white (-2.5% to -2.6%) and hispanic (-1.7% to -1.9%) within 2km. Yet, these results are only significant for the within 2km threshold and not for the within 1km and 3km thresholds, raising questions about their robustness. Taken together, Tables E1 and E2 provide evidence that our repeat sales model may not be subject to significant shifts in the equilibrium hedonic price function due to sorting and changes in preferences as detectable through demographics. Thus, we can have greater confidence in the point estimates reported in Tables 3 and 5 representing willingness to pay.

Table E1 Composition of buyers in the burn scar view and no-view markets

	0-2km bin		3-4km bin	
	(1)	(2)	(3)	(4)
Panel A: Income				
View×PostFire	-2.975	-2.576	0.235	0.742
	(3.723)	(3.629)	(1.565)	(1.620)
N	19093	19093	38596	38596
R^2_{adj}	0.0306	0.0298	0.0356	0.0381
Panel B: White				
View×PostFire	0.0166	0.0155	0.0205*	0.0172
	(0.0184)	(0.0192)	(0.0106)	(0.0107)
N	19097	19097	38602	38602
R^2_{adj}	0.00713	0.00973	0.0188	0.0200
Panel C: Hispanic				
View×PostFire	0.00548	-0.000777	0.00838	0.00461
	(0.0136)	(0.0138)	(0.00923)	(0.00925)
N	19097	19097	38602	38602
R^2_{adj}	0.0342	0.0394	0.0513	0.0534
Quadr county trends	Yes		Yes	
Year×Qtr	Yes		Yes	
County×Year×Qtr		Yes		Yes

Note: Each specification includes Census tract fixed effects. Robust standard errors clustered at census tract level. * p<0.1, ** p<0.05, *** p<0.01.

[25] In all cases, results are qualitatively similar for analyses run separately for the first or second year post-fire.

[26] Our HMDA-CoreLogic matching success rate compares favorably with those of Bayer et al. (2016) and Haninger et al. (2017).

Table E2 Composition of buyers near and away from the burn scar

	$K=1$		$K=2$		$K=3$	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Income			
$K\times$PostFire	1.444	0.961	2.723*	2.278	3.658**	3.325**
	(2.421)	(2.317)	(1.551)	(1.555)	(1.523)	(1.547)
N	57689	57689	57689	57689	57689	57689
R^2_{adj}	0.0363	0.0374	0.0361	0.0372	0.0362	0.0373
			Panel B: White			
$K\times$PostFire	-0.0233*	-0.0181	-0.0259***	-0.0250***	-0.0133	-0.0148*
	(0.0126)	(0.0121)	(0.00904)	(0.00897)	(0.00871)	(0.00869)
N	57699	57699	57699	57699	57699	57699
R^2_{adj}	0.0153	0.0169	0.0154	0.0170	0.0153	0.0169
			Panel C: Hispanic			
$K\times$PostFire	-0.0186*	-0.0138	-0.0187**	-0.0174**	0.00112	0.00168
	(0.00995)	(0.0100)	(0.00740)	(0.00752)	(0.00733)	(0.00732)
N	57699	57699	57699	57699	57699	57699
R^2_{adj}	0.0494	0.0523	0.0494	0.0524	0.0494	0.0524
Quadr county trends	Yes		Yes		Yes	
Year$\times$Qtr	Yes		Yes		Yes	
County$\times$Year$\times$Qtr		Yes		Yes		Yes

Note: Each specification includes Census tract fixed effects. Robust standard errors clustered at census tract level. * p<0.1, ** p<0.05, *** p<0.01.

For the effect of wildfire events on risk beliefs, we start with the properties that sold within two years pre- and post-fire within 5 and 15km of a burn scar and within 2km of the risk zone boundary (72,892 observations). After cleaning the data, matching as above and dropping duplicates, we end up with 33,995 properties, or a 52% matching success rate. In Table E3 we find no effect on income or hispanic but an increase in the number of white buyers after a wildfire in the high risk zone (9% to 10% within 1km of the risk zone). It is difficult to understand this finding considering that we do not find evidence of large or positive demographic shifts in close proximity to the wildfires where it would be more likely that treatment would affect sorting behavior by demographics.

For the effect on the 2008 rezoning on risk beliefs, we start with the properties whose sale was not affected by wildfires and within 750m of the risk zone boundary. After cleaning the data, matching as above and dropping duplicates, we obtain a 50.2% matching success rate. Table E4 shows no evidence of changes in neighborhood composition before and after the rezoning. [27]

[27] The results are robust to restricting the analysis to 1, 2, 3, or 4 year(s) around the 2008 rezoning.

Table E3 Composition of buyers inside and outside the risk zone after a fire

	Within 1km		Within 2km	
	(1)	(2)	(3)	(4)
	Panel A: Income			
Risk zone×PostFire	-2.996	-1.537	-0.316	-0.507
	(7.000)	(7.485)	(6.297)	(6.707)
N	7691	7691	12358	12358
R^2_{adj}	0.0158	0.0181	0.0149	0.0151
	Panel B: White			
Risk zone×PostFire	0.0914***	0.0991***	0.0503*	0.0599**
	(0.0306)	(0.0324)	(0.0301)	(0.0303)
N	7691	7691	12358	12358
R^2_{adj}	0.0130	0.0146	0.0128	0.0146
	Panel C: Hispanic			
Risk zone×PostFire	0.00382	0.0121	-0.000757	0.0110
	(0.0210)	(0.0218)	(0.0195)	(0.0197)
N	7691	7691	12358	12358
R^2_{adj}	0.0441	0.0454	0.0418	0.0486
Quadr county trends	Yes		Yes	
Year×Qtr	Yes		Yes	
County×Year×Qtr		Yes		Yes

Note: Each specification includes Census tract fixed effects. Robust standard errors clustered at census tract level. * p<0.1, ** p<0.05, *** p<0.01.

Table E4 Composition of buyers inside and outside the risk zone after the 2008 rezoning

	Within 250m		Within 500m		Within 750m	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Income					
Risk zone×PostRezoning	-26.81	-1.664	-26.80	-17.96	-15.52	-10.90
	(37.06)	(41.08)	(33.91)	(29.95)	(31.56)	(28.31)
N	3919	3919	5865	5865	7309	7309
R^2_{adj}	0.0249	0.0506	0.0264	0.0489	0.0316	0.0425
	Panel B: White					
Risk zone×PostRezoning	0.0252	0.0501	0.0215	-0.0299	0.0256	-0.00828
	(0.0654)	(0.0842)	(0.0523)	(0.0664)	(0.0502)	(0.0609)
N	3919	3919	5865	5865	7309	7309
R^2_{adj}	0.00346	0.00524	0.00385	0.00230	0.00489	0.00519
	Panel C: Hispanic					
Risk zone×PostRezoning	-0.0547	-0.0284	-0.0194	-0.0592	-0.0165	-0.0463
	(0.0696)	(0.0627)	(0.0514)	(0.0481)	(0.0540)	(0.0492)
N	3919	3919	5865	5865	7309	7309
R^2_{adj}	0.00760	0.0165	0.0113	0.0230	0.0134	0.0210
Quadr county trends	Yes		Yes		Yes	
Year×Qtr	Yes		Yes		Yes	
County×Year×Qtr		Yes		Yes		Yes

Note: Each specification includes Census tract fixed effects. Robust standard errors clustered at census tract level. * p<0.1, ** p<0.05, *** p<0.01.