Mandated vs. Voluntary Adaptation to Natural Disasters: The Case of U.S. Wildfires

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Despite escalating losses in climate-related disasters, adoption of protective technologies and behaviors is limited by risk misperception, externalities, and insurance market frictions. One response to these market failures is to mandate these investments. We measure the effect of California's wildfire building codes on own and neighboring structure survival using comprehensive data on US homes exposed to wildfires since 2000. Differences across jurisdictions and vintages reveal remarkable resilience effects of building codes. Codes also increase survival of neighboring homes by reducing structure-to-structure spread. We then develop and estimate a model of social benefits of mandatory building standards vs. other adaptation policies.

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Take-up of many socially desirable investments and behaviors is thought to be limited due to positive externalities, imperfect information, or other frictions. One response to such barriers is to provide information and subsidies to encourage voluntary adoption. An alternative approach is to *mandate* desired outcomes through minimum standards or bans on alternatives. Mandates ensure wide adoption but may compel some individuals to choose options they would have preferred to avoid even if fully informed and fully accountable. This tension between increased adoption benefits and potentially inefficient adoption exists across a broad range of policies, including energy efficiency standards; regulation of smoking, drinking, and other health behaviors; consumer financial protections; and product and worker safety regulation.[1]

The same tradeoff urgently confronts policies intended to guide adaptation to worsening climate-related disasters. Worldwide natural disaster losses averaged $218 billion per year during 2016–2020, a 60% increase in real terms over the preceding 30 years.[2] Efficient investment in adaptation is essential in the face of these escalating risks. Yet efficient take-up of protective technologies and behaviors appears to be hindered by a variety of market frictions: spatial spillovers of mitigation benefits, insurance market imperfections, post-disaster relief programs that soften ex-ante incentives, and widespread misperception of risks and benefits.[3] In response, some hazard-prone jurisdictions have encouraged resilience through information campaigns and voluntary incentives.[4] Other policymakers have instead implemented adapta-

1. See, for example, Jacobsen and Kotchen (2013) and Allcott and Taubinsky (2015) on energy efficiency; Cawley and Ruhm (2011) on health behaviors; Campbell et al. (2011) on financial regulation; and Viscusi (1992) on product and worker safety regulation.

2. Loss data are from Munich RE and are in 2020 dollars.

3. There is growing evidence of these frictions. Benefits of mitigation against some hazards are thought to extend to neighboring properties (Shafran 2008; Lueck and Yoder 2016; Costello, Quérou, and Tomini 2017). Monitoring costs and other insurance market imperfections mean that mitigation behaviors are not accurately reflected in insurance prices (Kunreuther and Michel-Kerjan 2011; California Department of Insurance 2018; Wagner 2022). Public disaster relief programs may reduce private incentives for property protection (Kousky, Luttmer, and Zeckhauser 2006; Deryugina 2017; Baylis and Boomhower 2023). Hedonic studies imply that homeowners do not accurately perceive disaster risks and thus the value of protective investments (Hallstrom and Smith 2005; Gallagher 2014; McCoy and Walsh 2018; Bakkensen and Barrage 2021).

4. Examples in the U.S. include the Ready campaign and Ready.gov website; the Community Rating System under the National Flood Insurance Program; the StormReady, Hurricane Protection Week, and

tion mandates—for example, construction standards that require disaster-resistant building materials.[5] In the contest between these approaches, the stakes are high. Well-targeted investments in resilience may prevent vast losses. But such investments are costly and the net benefits of adaptation mandates depend on how accurately regulators judge the effectiveness of the required actions, the level of the hazard, and individual risk preferences. Moreover, implementing mandatory standards is often politically challenging. Do the additional benefits justify the expenditure of time and political capital? Despite these important questions, there is little empirical evidence about outcomes under a mandated disaster resilience regime compared to a counterfactual of voluntary take-up.

This paper considers the case of wildfire building codes in California, a high-profile adaptation mandate. California suffered nearly $50 billion in wildfire property losses alone between 2017 and 2021 (Paci, Newman, and Gage 2023). The state also has among the strictest wildfire building codes in the world. We provide the first comprehensive evaluation of the effect of these codes on own-structure survival and on neighbor spillovers via structure-to-structure fire spread. We then embed these empirical estimates in an economic model to calculate net social benefits of wildfire building codes as a function of local wildfire hazard and number of close neighbors. Finally, we use the model and data to compare the welfare effects of building codes, voluntary mitigation subsidies, and a no-policy alternative.

This analysis takes advantage of a new dataset that includes property-level data for the majority of U.S. homes exposed to wildfire between 2003 and 2020, where "exposed" is defined as being included within the burn perimeter of a wildfire incident. We compiled the data by requesting post-incident damage censuses from numerous emergency management

National Tsunami Hazard Mitigation programs; the Firewise USA program; and the Community Wildfire Protection Plan program.

5. Florida has construction standards for hurricane winds and codes exist in various regions for winter storms and non-weather disasters such as earthquakes and tsunamis (Federal Emergency Management Agency 2020). In flood-prone areas, U.S. federal rules require homes to be elevated and some localities have imposed even stricter requirements. California, Utah, Nevada, and Pennsylvania have statewide wildfire building standards while in other states, notably Colorado, wildfire codes have been adopted at the local level (Insurance Institute for Business and Home Safety 2019). Australia, New Zealand, France, and Italy also have wildfire building codes (Intini et al. 2020).

agencies and individual county assessors. We merged these lists of damaged homes to assessor data for the universe of (destroyed and surviving) homes inside wildfire burn areas. The data show that even during catastrophic wildfires, more than 50% of exposed homes survive unscathed (wildfire damage outcomes are typically bimodal, with homes either undamaged or totally destroyed). One of the key advantages of the new data is the ability to observe and learn from these surviving homes. The property-level loss information also distinguishes the wildfire data from floods and other disasters where loss data are typically available at the zip code or Census tract level. In addition to the new loss data, the empirical work also leverages emerging tools in spatial analysis, including high-resolution aerial imagery and precise "rooftop" geocoding of structure locations.

The empirical design makes use of rich variation in building code requirements across space and over time. The complex nature of building regulation in California creates a patchwork of wildfire standards across localities. We also observe fires in other states that do not have wildfire building codes. In all of these places, we observe homes built before and after changes in California's codes. This identifying variation yields credible counterfactual predictions for how homes would have performed in the absence of California's standards. Our preferred statistical model is a fixed effects regression that compares the likelihood of survival for homes of different vintages located on the same residential street segment during the same wildfire event. These sub-street fixed effects allow us to compare groups of homes that experience essentially identical wildfire exposures.

We find remarkable vintage effects for California homes subject to the state's wildfire standards. A 2008 or newer home is about 13 percentage points less likely to be destroyed than a 1990 home experiencing an identical wildfire exposure (a 34% reduction from the sample average of 39%). There is strong evidence that these effects are due to state and local building code changes—first after the deadly 1991 Oakland Firestorm, and again with the strengthening of wildfire codes in 2008. The observed vintage effects are highly nonlinear, appearing immediately for homes built after building code changes. There are no similar effects in jurisdictions without wildfire building codes.

We also find that code-induced mitigation benefits neighboring homes, consistent with reduced structure-to-structure spread. These neighbor effects are in keeping with anecdotal reports of home-to-home spread as an important factor in urban conflagrations (Cohen 2000; Cohen and Stratton 2008; Cohen 2010).[6] Our results imply that, all else equal, the presence of a pre-code neighbor less than 10 meters away (within the distance fire experts refer to as the home ignition zone) increases a home's likelihood of destruction during a wildfire by about 1.7 percentage points (4.4%). This benefit is even larger when homes have multiple close neighbors.

We separately investigate whether the codes had an effect on housing market outcomes. Using data on home sales and construction near borders of code-required areas across California, we show that neither home sale prices nor growth rates in code-required areas changed differentially in response to the building codes. This indicates that the welfare impacts of the building mandates are driven primarily by the costs and benefits of the mitigation requirement.

Finally, we develop an economic model that identifies the conditions under which mandated adaptation improves welfare. We use the model, our empirical results, and additional data on homes in wildfire hazard areas throughout California to understand (1) where universal mitigation investment is cost-effective and (2) the relative welfare properties of building codes, voluntary mitigation subsidies, and a no-policy alternative. With respect to (1), our calculations find that wildfire building codes deliver unambiguously positive benefits in the most fire-prone areas of the state, especially where homes are clustered closely together and thus create large risk spillovers. In areas with more moderate wildfire risk, building standards for new homes can also be justified given reasonable assumptions about household

6. We are also aware of at least one insurance company which will not sell homeowners insurance to homes located next to a home with a wood roof in high-risk areas (Allstate Indemnity Company 2018).

risk aversion, the completeness of insurance, future increases in wildfire hazard, and/or co-benefits of building codes (such as reductions in public expenditures on wildland firefighting). On the other hand, the costs of retrofitting existing homes to meet current wildfire building standards are substantial and our analysis suggests full retrofits are only economic in areas with extreme wildfire hazard.

Turning to the policy comparison, we find that building codes with a degree of geographic targeting similar to those employed in practice increase welfare by about 20% relative to the no-policy alternative. We also find that welfare under building codes is only slightly lower than under an idealized perfectly-differentiated mitigation subsidy. These favorable welfare results are driven by two features of disaster risk that have not been sufficiently appreciated in the literature. First, unlike many goods where regulators might apply bans or mandates, key determinants of the returns to mitigation investment are readily observable. This includes the level of wildfire risk and the number of close neighbors affected by risk spillovers. Second, the spatially continuous nature of disaster risk means that these factors are highly spatially correlated within neighborhoods. This combination of factors makes it feasible to target adaptation mandates to areas where almost all, if not all, homeowners will be made better off by investment.

These results are broadly relevant to natural disaster management. The standards-based approach that we study nearly halves loss risk when structures are exposed to the hazard. Moreover, our cost-benefit calculations imply that low take-up in the absence of standards is unlikely due to a lack of cost-effectiveness. These facts can inform policies to mitigate other risks like floods, hurricanes, tornadoes, and heat waves where voluntary adaptation investment may also be limited by risk misperception and other potential market failures.[7]

7. Risk misperception is an important theme across disaster types: see Kunreuther and Michel-Kerjan (2011) for an overview of cognitive biases in disaster risk mitigation. Spillovers and other market failures differ across hazards. While direct physical risk spillovers are most pronounced for fires, other types of spillovers affect all disasters. Fu and Gregory (2019) document after-the-fact amenity spillovers from post-flood relocation decisions, which presumably correlate with damage levels and thus ex-ante mitigation. Large-scale public investments to manage flooding can also create longer-distance hydrologic externalities that may justify coordination of mitigation investments (Hummel et al. 2021; Bradt and Aldy 2023).

This work also has immediate implications for wildfire policy. Our results imply there are gains to be realized from strengthening building codes in other states and countries to match California's, and that a home's adherence to wildfire building standards has benefits not just for itself but for its neighbors as well. This evidence is relevant to recent proposals in Colorado, Oregon, Washington, and other states.[8] Meanwhile, California is moving to expand the geographic coverage of designated wildfire hazard zones and reduce the ability of local jurisdictions to opt out of recommended standards. Separately, new California legislation from 2020 provides financial incentives for retrofits of existing homes in wildfire-prone areas.[9] The law specifically calls for support of "cost effective" retrofits, a concept for which the evidence in this study is essential. Additionally, policymakers are confronting pressing issues of insurance rate reform in response to mounting wildfire losses. One key debate is the degree to which individual investments improve structure survival and should thus be rewarded through regulated insurance discounts (California Department of Insurance 2018), such as California's Safer From Wildfires program. This paper's evidence on the effectiveness of such investments during real wildfires bears directly on this question.

Our work builds on previous studies of natural hazard mitigation. For wildfires, a number of engineering and forestry studies describe the effects of construction materials and vegetation management on structure resilience (Gibbons et al. 2012; Syphard et al. 2012; Syphard, Brennan, and Keeley 2014; Alexandre et al. 2016; Syphard, Brennan, and Keeley 2017; Kramer et al. 2018; Syphard and Keeley 2019). Our paper focuses on the effects of a mandatory mitigation policy, while these previous studies measure the effectiveness of specific mitigation technologies (e.g., survival of homes whose owners did vs. did not choose to install fire-resistant exterior siding).

Engineering studies on building requirements for homes exposed to natural disasters typically calculate the value of building codes through modeling and simulation (e.g., Federal

8. See, e.g., Profita, Cassandra. "The Labor Day Fires Burned Towns and Homes. Oregon Has a Plan to Avoid a Repeat." Oregon Public Broadcasting, September 7, 2021.

9. See California's Senate Bill 63 (2021–2022). and Assembly Bill 38 (2019–2020).

Emergency Management Agency 2020). Two empirical studies on the related topic of hurricanes do consider building codes, with conflicting results. Dehring and Halek (2013) is a small case study of several hundred homes during Hurricane Charley in 2004. Simmons, Czajkowski, and Done (2018) study aggregate zip-code level data on annual insurance claims by homes built in different decades to infer benefits of hurricane building codes in Florida. In contrast, our study uses highly granular property- and event-level loss data for a large sample of wildfires covering several states. In the flood context, Ostriker and Russo (2022) provide complementary evidence that building codes can reduce damages by forcing homeowners to build elsewhere or to elevate their homes. Their finding that targeting mandates towards high-risk areas echoes what we see in our setting. Finally, our work is methodologically related to a separate literature in economics on building codes and household energy consumption (Jacobsen and Kotchen 2013; Levinson 2016).

This study makes three main contributions. First, we provide the first comprehensive evaluation of the causal effects of wildfire building codes on own- and neighboring-structure survival. The causal estimates we document leverage a quasi-experimental empirical design, where previous work is primarily descriptive or relies on regression adjustment. The estimates of risk spillovers between neighbors make use of detailed information on the spatial relationships between nearby homes. Beyond the wildfire context, this result improves our understanding of disaster resilience under standards-based vs. voluntary policies. Second, by embedding the empirical estimates in an economic model, we calculate the net social benefits of building codes and compare these to the benefits of alternative adaptation policies. Third and finally, we compile and make available a comprehensive dataset of structure-level outcomes in wildfires across several states that, to our knowledge, is the most complete accounting in existence. This new dataset will enable future work on the economics of catastrophic wildfire risk.

The rest of the paper proceeds as follows. Section 1 discusses structure survival in wildfires and California's history of building code updates. Section 2 describes the data and spatial analysis. Section 3 outlines the empirical strategy, and Section 4 presents the results. Section 5 develops the model of net social benefits of building codes and other policies and Section 6 concludes.

1 Wildfire Building Codes in California and Other States

> *"Unlike a flash flood or an avalanche, in which a mass engulfs objects in its path, fire spreads because the requirements for combustion are satisfied at locations along the path... A wildland fire cannot spread to homes unless the homes and their adjacent surroundings meet those combustion requirements."* Jack D. Cohen, *Journal of Forestry*, 2000.

Established forestry and engineering evidence supports the importance of the so-called home ignition zone in determining structure resilience to wildfires. The home ignition zone includes the home itself as well as an area extending 30 meters away from the structure. Fire scientists emphasize the elimination of flammable materials inside this zone (e.g., Cohen 2000, 2010; Calkin et al. 2014). This guidance applies to both vegetation around the home ("defensible space") and the construction of the home itself, especially the roof.

Among U.S. states, California has gone the furthest in mandating take-up of wildfire resilience investments by property owners.[10] However, the application of these codes varies throughout the state. In jurisdictions where CAL FIRE provides firefighting services (State Responsibility Areas or SRAs), the state directly determines building standards. Within incorporated cities and other jurisdictions with their own fire departments (Local Responsibility Areas or LRAs), local governments have historically had greater control over code requirements. The development of the modern standards began with the Oakland Hills Firestorm of 1991, which killed 25 people and caused $1.5 billion in property damage. The

10. Efforts to adopt statewide codes in Oregon, Colorado, and Washington are ongoing, but continue to face substantial opposition (Sommer 2020; Booth 2023; Washington State Building Code Council 2024).

tragedy led to a series of legislative actions during the mid-1990s that required more fire-resistant roofing and maintenance of vegetation immediately adjacent to the home. The first of these was the so-called Bates Bill of 1992 (Assembly Bill 337). Among other changes, the Bates Bill encouraged stronger building standards in LRAs by requiring CAL FIRE to produce maps of recommended Very High Fire Hazard Severity Zones (VHFHSZ). In LRAs, local governments could then choose whether or not to adopt these recommended hazard maps, and thus the accompanying building standards. This designation process unfolded over several years, with hundreds of local governments adopting or rejecting CAL FIRE's proposed VHFHSZ maps at different times. According to Troy (2007), 151 of 208 local governments (73%) either adopted the VHFHSZ regulations or claimed to have promulgated equally strong rules.[11]

On the heels of the Bates Bill, Assembly Bills 2131 (1992) and 3819 (1994) increased requirements for ignition-resistant roofs. Roofing materials are rated Class A, B, C, or unrated.[12] Starting in 1995, the new codes required Class B or C roofs on newly-constructed or re-roofed homes throughout California. In 1997, the requirement increased to Class A roofs for high-hazard areas (a substantial improvement in fire resistance). However, the stronger Class A standard only applied in SRAs and the subset of LRAs where local governments had adopted state-recommended VHFHSZ maps. Finally, Assembly Bill 423 in 1999 simplified enforcement of the new roofing codes by outlawing the use of unrated roofing materials throughout the state.

The collective effect of these mid-1990s building code reforms was to substantially increase

11. For a detailed qualitative study of the determinants of local VHFHSZ adoption decisions, see Miller, Field, and Mach (2020).

12. These ratings are earned through laboratory testing; for example, the Class A test involves placing a 12-inch by 12-inch burning brand on the roof material under high wind conditions for 90 minutes. A roof's fire resistance depends on the roof covering material (shingles, tiles, concrete, etc.), the particular flame retardants with which that material may be treated, and the way that the roof components are assembled. Thus, there is not a one-to-one mapping between roof covering material and rating class, though untreated wood shingles are always "unrated". For a detailed discussion of roof classifications, see S. L. Quarles et al. (2010) and ASTM International Standard E108, "Standard Test Methods for Fire Tests of Roof Coverings."

the fire resistance of roofs on newly-constructed homes. The roofing requirements also applied to existing homes, but only at the time of roof replacement. Homeowners who replaced more than 50% of the roof surface in a single year were in principle obligated to comply.

California strengthened its wildfire codes again in 2008 with the so-called Chapter 7A standards of the California Building Code. These requirements apply to all homes built in 2008 or later in all SRAs and in LRAs where proposed VHFHSZ designations have been accepted. The codes apply to many dimensions of new homes. Roofs must be rated class A or B, eaves and exterior siding must be fire resistant, vents must covered by a fine wire mesh to resist ember intrusion, windows and doors must resist fire for at least 20 minutes, and decks and other building appendages must be built of noncombustible or otherwise fire-resistant materials. Chapter 7A also includes additional requirements for defensible space, which also applied to existing and new homes. However, in practice, the primary point of enforcement for these codes was at the time of new construction; enforcement effort for existing homes appears to have been limited (see e.g., Maclay 1997).

In summary, there were two key periods of change in California wildfire building codes: the mid-1990s and 2008. The strictness and uniformity of the codes varied across SRAs and LRAs. Codes were applied directly as written in SRAs, while local decisionmakers exercised final control over the adoption of state-recommended codes in LRAs.

This study also considers wildfires in Arizona, Colorado, Oregon, and Washington. None of these had statewide wildfire building standards at the time of the included fires (Insurance Institute for Business and Home Safety 2019). Some local governments—particularly in Colorado—have adopted local standards that include a diverse mix of rules about roofs, other construction materials, and/or defensible space. Our empirical analysis excludes a small number of fires in the comparison states that overlap jurisdictions with local wildfire building standards.[13] While the non-California homes in this study are not subject to

13. These are the 2012 Waldo Canyon Fire, 2013 Black Forest Fire, and 2018 Mile Marker 117 Fire in El Paso County, Colorado (S. Quarles et al. 2013) and the 2012 High Park Fire and 2020 Cameron Peak Fire

mandatory standards, they, like the California homes, are targeted by a range of information and incentive programs that seek to increase voluntary home hardening. Programs active in these states include FireWise USA (National Fire Protection Association), the Community Wildfire Protection Plan program (United States Forest Service and Department of Interior), the Fire Adapted Communities Coalition (numerous public agencies and NGOs), the Ready, Set, Go! program (International Association of Fire Chiefs), and numerous other initiatives.

2 Data and Spatial Analysis

This section describes the construction of the database of wildfire damages, property tax assessment information, and the spatial analysis used to identify structure locations and distances to neighboring homes.

2.1 Homes and Damage Data

2.1.1 Damage Inspection Data

We sought to assemble as comprehensive a database as possible of administrative records for homes destroyed or damaged by wildfire in the United States. For recent wildfires in California, this information is managed by CAL FIRE. For earlier California fires and for fires in other states, we contacted individual county assessors (who track these damages in order to update property tax assessments) and other agencies to request historical records of structure damages. To our knowledge, the resulting database is the most complete accounting of U.S. homes lost to wildfire.[14] In general and consistent with the case study observations in Cohen (2000), the vast majority of damaged structures are fully destroyed (i.e., partial

in Larimer County, Colorado (Larimer County 2020).

14. Comparing to incident-level aggregate data in ICS-209 incident reports (St. Denis et al. 2023), we calculate that the wildfires in our data represent 70% of all of the wildfire-destroyed residential structures across the entire United States between 2003 and 2020 (the time period of our sample). Restricting to only the five states in our sample (CA, AZ, CO, OR, and WA), we capture 85%. Restricting further to only incidents between 2010 and 2020, we capture 93%.

damage outcomes are rare). We thus follow the literature and focus on "destroyed" as our primary outcome.

California 2013–2020: In California, the CAL FIRE Damage Inspection (DINS) database is a census of destroyed and damaged homes following significant wildfire incidents during 2013–2020. The data include street address and assessor parcel number (APN); limited structure characteristics; and for some fires, an additional sample of undamaged homes. The damage variable has four levels: destroyed ($> 50\%$ damage), major (26–50%), minor (10–25%), and affected (1%–9%). Of these, "destroyed" is the most commonly reported damage category and the only category that appears consistently across all fires.

California 2003–2013: Data for pre-2013 wildfires in California come from two sources. For the 2003 and 2007 San Diego fire storms, we received damage assessment data from San Diego County. For other counties, CAL FIRE staff provided us with a large collection of unformatted historical damage assessment reports that we compiled and standardized to be usable for research.

Other States: Using ICS-209 incident reports, we identified 15 counties in western states other than California with large numbers of structures lost to wildfire since 2010. We then contacted county assessors in each of these counties to request damage data. We received usable structure-level damage data from 10 of these 15 counties, including 9 of the top 10 by structures destroyed.[15] Among these areas, we conduct an extensive search to rule out locations where wildfire building codes were in place prior to the relevant wildfire incident.[16]

15. Specifically, homes in following non-California counties are represented in our data (in order of structures destroyed): Jackson (OR), Okanogan (WA), Marion (OR), Lane (OR), Grand (CO), Lincoln (OR), Jefferson (OR), and Yavapai (AZ).

16. We document that process in Appendix Section A.7. As a result, we do not include homes located in El Paso County and Larimer County, since they both had county-wide wildfire building codes prior to the wildfire incidents we study.

2.1.2 Property Tax Assessment Data

We merge the damage records to comprehensive assessment data for all U.S. homes from the Zillow ZTRAX database. The ZTRAX data include information on year built, effective year built (in the case of remodels), building square footage, and other property characteristics. The merge from damage data to ZTRAX uses assessor parcel numbers, and we validate the accuracy of this merge by comparing street addresses across the two datasets. We restrict the data to include only single family homes, which account for most properties inside the wildfire perimeters in our sample.[17] For each incident, we merge the damage data to the most recent historical assessment data from the pre-fire period (assessments are conducted annually or every other year in nearly all counties). In other words, we merge to the population of single family homes that existed immediately prior to the start of the fire. Appendix Table A5 documents the full list of wildfire incidents in the dataset, the number of single family homes inside of each perimeter, and the share destroyed.

2.2 Spatial Analysis and Dataset Construction

2.2.1 Identifying Structure Rooftop Locations

This study uses the physical locations of the homes in the data in two ways. First, homes must be spatially assigned to building code jurisdictions and to wildfire burned areas. Second, the measurement of spillovers across properties requires precise distances between neighboring structures. The street address-based geocoding methods typically used in academic research are not sufficiently detailed for this second purpose, which requires accurate structure locations at a meter scale. We solved this challenge by combining several spatial datasets to identify precise rooftop locations. First, we limit the population of ZTRAX homes to all homes in zip codes where at least one home was destroyed. We then merge these ZTRAX

17. In the CAL FIRE Damage Inspection data, which include comprehensive information on structure category, single residences represent 91% of all destroyed structures (excluding minor structures such as sheds). Of all single residences, single family homes represent 82% of the data. The remainder are primarily manufactured or mobile homes, which are not subject to California's wildfire building codes.

records to parcel boundary maps from county assessors using assessor parcel numbers. This yields a parcel polygon for each home. We then use comprehensive building footprint maps from Microsoft to identify the largest structure overlaying each parcel.[18] We call this location the "footprint location." Figure 1 shows an example for Redding, California in the area of the 2018 Carr Fire. Gray lines are parcel boundaries from the Shasta County Assessor. Blue polygons are building footprints. The purple and yellow markers show the assigned rooftop locations for each structure. Yellow markers show homes that are reported as destroyed in the damage data.

This rooftop geocoding method generates highly accurate locations, but it is dependent on the availability of high-quality parcel boundary GIS data. In areas where such data are not available (representing 7% of homes in the final analysis dataset), we instead geocode home locations using the ESRI StreetMap Premium geolocator, a commercially-available address-based product. Our quality checking shows that these locations (henceforth "address-based locations") are generally reliable to the parcel level but not always to the structure rooftop level. Appendix Section A.5 describes the geocoding in more detail.

2.2.2 Validating Locations and Damage Reports

We quality check the calculated property locations and the damage report data using high-resolution aerial imagery from NearMap. The base image in Figure 1 shows an example. The detailed imagery allows us to manually confirm the accuracy of structure locations, which closely coincide with the blue building footprints in the figure. In addition, the NearMap imagery includes post-fire surveys for many of the incidents in our database. Figure 1 illustrates how destroyed properties are readily visible in these surveys, which allows us to confirm the accuracy and completeness of the damage data. Appendix Table A3 reports accuracy rates in a random sample of homes. For damage reports, 99% of reported outcomes match the

18. The Microsoft U.S. Building Footprints Database is publicly available at https://github.com/microsoft/USBuildingFootprints.

ground truth imagery. For rooftop locations, 98% of the assigned structure locations are on top of the structure rooftop in the ground truth imagery (with 99%+ accuracy in densely developed areas). Locations that rely on street address based geocoding tended to be accurate to the parcel but not always to the actual structure rooftop—about 75% of these assigned locations are on top of the structure rooftop in the ground truth imagery.

2.2.3 Spatial Merge to Wildfire Perimeters, Code Jurisdictions, and Topography

We restrict the dataset to homes located within final wildfire perimeters (plus a 10-meter buffer). Depending on the state and time period, these digital perimeter maps come from the California Forest and Range Assessment Program (FRAP), the Monitoring Trends in Burn Severity (MTBS) dataset, or the National Interagency Fire Center (NIFC).

We merge the homes data to spatial data on fire protection responsibility (SRA vs. LRA) and designated fire hazard (FHSZ) that together determine building codes in a given location in California. We use historical GIS maps provided by CAL FIRE to assign homes to code regimes according to the codes in effect when the home was built.[19] Section 3.1 below discusses the assignment of homes to code regimes in more detail.

For each home, we also compute measures of topography and wildfire hazard: we obtain ground slope, elevation, and vegetation type from 30m resolution grids provided by LANDFIRE (Rollins 2009), and we compute a measure of wildfire hazard using data from the United States Forest Service (USFS) Wildfire Risk to Communities project (Scott et al. 2020). We refer to this measure as "wildfire hazard." It represents the annual probability of moderate to severe wildfire exposure for each home and is calculated as the product of Burn Probability (the total annual wildfire probability) and Flame Length Exceedance Probability 4 (conditional on any fire, the probability that the fire will reach moderate or

19. For SRA/LRA boundaries, the historical map data include updates in 1990, 1996, 2003, 2005, and annually from 2010–2020. For FHSZ, the historical map data include updates in 1985, 1998, 2007, and 2008.

greater threat status). This measure represents a snapshot of topography and wildfire risk from 2013 and 2014, prior to most of the wildfires experienced by homes in the dataset.

2.2.4 Calculating Distances Between Neighboring Homes

We construct two measures of distance between homes. The first is the minimum distance between the building footprint polygons associated with the two structures (henceforth the "wall-to-wall" distance). This measure is only available for homes where we assign locations based on building footprints. The second metric uses the distance between assigned point locations, which are available for all homes in the dataset. We call this metric the "centroid-to-centroid" distance because these points are meant to correspond to the center of the roof.[20] The wall to wall distance is our preferred measure because it more accurately captures space between homes and because the footprint-geocoded locations are more accurate than the address-based location points (Appendix Table A3). Our main estimates of neighbor spillovers use the restricted sample of homes for which wall to wall distances are available. For robustness, we also show specifications that use centroid-to-centroid distances and the full sample of homes.

We identify potentially at-risk neighbors for each home in the final dataset. We define potentially at-risk neighbors as those within 200 meters (using centroid-to-centroid distances) of the focal home and included the given fire perimeter (plus the buffer). Panel (b) of Figure 1 shows two examples. Each image shows wall-to-wall distances (in meters) from the structure marked "0". Appendix Figure A3 summarizes the distribution of number of neighbors at various distances.

20. When using centroid-to-centroid distances, we add 20 meters to the measured distance to ensure comparability with the wall-to-wall distances, since 30 meters in centroid-to-centroid distance roughly corresponds to 10 meters in wall-to-wall distance. The median building footprint area in the sample is 260 m^2. A hypothetical circular roof would thus have a radius of 9.1 meters and the centroid-to-centroid distance between two such homes would be 18.2 + wall-to-wall distance.

2.2.5 Data Summary

The final dataset includes 45,093 single family homes exposed to 104 wildfires in California, Arizona, Colorado, Oregon, and Washington between 2003 and 2020. This sample includes only homes for which the key variables are observed.[21] 39% of these were destroyed. Appendix Table A1 provides descriptive statistics. Appendix Figure A1 shows the distribution of year built and average share destroyed by year built for the full dataset.

3 Empirical Strategy

This section describes the empirical design used to measure the effect of wildfire building codes on structure survival. To fix ideas, Figure 2 provides an example of the merged dataset for the 2018 Woolsey Fire in Los Angeles County. The green and purple markers indicate locations of surviving and destroyed single family homes inside the final fire perimeter. The street map data give a sense of development density. The intensity of losses varies significantly within the burned area. Near Malibu, a large share of affected homes were lost. Further north, however, there are several areas where most homes inside the fire perimeter escaped destruction. These differences reflect varying fire conditions, firefighter response times, landscape vulnerability, structure characteristics, and potentially numerous other factors. This heterogeneity adds noise to empirical analysis of structure survival. It may also introduce bias if year built or other structure traits vary similarly within burned areas. We address these challenges using an empirical design that compares the likelihood of survival for homes of different vintages on the same residential street during the same wildfire. We attribute these vintage effects to building codes by comparing vintage effects across jurisdictions with and without wildfire building codes.

21. The key variables are those we use in our preferred specification, Equation (1): whether the home was destroyed, vintage, jurisdiction, period (year built), ground slope, and location.

3.1 Treatment Groups

Our empirical design focuses on homes in three types of jurisdictions. The first are homes in the SRA, where compliance with California building codes was mandatory and fully determined by maps released by CAL FIRE. The second are homes within LRAs that were ever recommended by CAL FIRE as VHFHSZ (henceforth, "LRA-VHFHSZs"). This group includes all proposed local VHFHSZ regardless of whether local governments accepted the designation. There is no centralized database that records local VHFHSZ adoption decisions, but Troy (2007) reports high rates of adoption.[22] In principle, however, it's possible that the maps used by LRAs to determine building code requirements could vary slightly from the original recommendation maps provided by CAL FIRE. For that reason, we consider the LRA-VHFHSZ homes as an "intent-to-treat" treatment group.

The third group of homes are those located in jurisdictions without wildfire building codes (henceforth, "No-codes") that were included in wildfire incidents during our sample period. These jurisdictions are locations in Arizona, Colorado, Oregon, and Washington without any state or local wildfire building codes. As discussed in Section 2, we are able to collect data for 10 of the 15 counties with the most destroyed homes outside of California, so this group includes the majority of available observations within the universe of possible comparison homes.[23] The bottom panel of Appendix Table A1 reports the number of homes in each of the treatment groups.

22. In addition, historical news accounts show that cities that rejected the official VHFHSZ designation often still adopted the underlying code requirements in the recommended areas. This seems to have been an attempt to achieve the state-recommended resilience requirements while avoiding the VHFHSZ label due to fears about its effect on property values (Sullivan 1995; Snyder 1995; Stewart 1995; Yost 1996; Grad 1996). One state fire official's response: "We didn't care if they called it a nuclear-free zone, as long as they adopted the regulations" (Maclay 1997).

23. A small number of homes built in parts of California that were not included in California's wildfire building code zones have also experienced wildfires. We use this group in sensitivity checks as an alternative comparison group. Our preferred specifications focus on the non-California comparison homes for two reasons. First, the within-California comparison areas contain relatively few recently-built homes. Second, the ability of local governments to amend the proposed VHFHSZ maps prior to adoption may have led to some municipalities requiring to-code construction in areas not included in the proposed VHFHSZ designations from CAL FIRE, such that the treatment status of these comparison homes may be somewhat ambiguous to the researcher.

3.2 Identification

The empirical design takes advantage of variation across home vintages and code jurisdictions. We first estimate the difference in likelihood of destruction when confronted with a wildfire (henceforth, "wildfire resilience") for pre- and post-code homes in regulated regions. To isolate the effect of construction practices as opposed to other factors that might vary with home vintage (such as the intensity of the wildfire upon reaching a given neighborhood), our preferred specification includes spatially granular fixed effects that compare nearby homes on the same residential street ("sub-street" fixed effects) during the same wildfire incident. Intuitively, this research design treats every sub-street as its own natural experiment, where all of the homes in each sub-street are on similar terrain and are equally exposed to the same wildfire, but—for homes in code-required jurisdictions—face different building requirements depending on whether they were constructed before or after the codes were in place.

These localized comparisons rule out bias from omitted variables that might differ across neighborhoods or communities. As one example, this sub-street design neutralizes potential differences in firefighting effort due to characteristics of the fire-threatened community as discussed in Baylis and Boomhower (2023) and Plantinga, Walsh, and Wibbenmeyer (2022), since these differences are determined by average characteristics over a broad area.

We then ask whether the measured improvements in wildfire resilience for regulated California homes are the result of the state's building codes. The relevant identification assumption is that no other important determinants of wildfire resilience changed at the same time as the California building code changes that we observe. To support this assumption, we marshal several lines of evidence. First, we use an event study analysis to examine whether the time pattern of observed improvements in California lines up with changes in building code policies. Second, we consider the evolution of wildfire resilience for homes built over the same time periods in other states without wildfire building codes. This additional evidence, which makes use of a novel West-wide wildfire outcomes dataset that we compiled for this purpose, speaks to the question of whether something else about the way U.S. homes were built changed exactly at the same time that California changed its wildfire building codes. In the Appendix, we also consider the evolution of wildfire resilience for homes built in parts of California that were never subject to California's wildfire building codes. This final line of evidence speaks to the question of whether something else changed about the way that homes were built *only in California* at the same time that California introduced its building codes.

3.3 Own-structure Survival

3.3.1 Flexible Vintage Effects

We begin the regression analysis with the following binned regression model for home i on street s exposed to wildfire incident f. We estimate this model separately for the SRA, LRA-VHFHSZ, and No-codes groups.

$$\text{Destroyed}_{isf} = \sum_{v=v_0}^{V} \beta_v \text{Vintage}_i^v + \gamma_{sf} + X_i \alpha + \epsilon_{isf} \tag{1}$$

The outcome Destroyed_{isf} is an indicator variable equal to one for destroyed homes and zero otherwise. Vintage_i^v represents indicator variables equal to one if house i's year built falls into bin v. The vintage bins vary in size depending on the availability of data in a given jurisdiction, and the omitted bin is the vintages just prior to the implementation of the codes. The main parameters of interest are the coefficients β that correspond to these vintage bins. These give the effect of each vintage on probability of survival when exposed to wildfire. In our preferred specification, we use sub-street fixed effects γ_{sf}, which include separate indicator variables for groups of 25 homes on the same street within fire perimeter f. We interact the sub-street fixed effects with jurisdiction indicators to account for the small number of incidents that cross jurisdictional boundaries. We also estimate models with incident fixed effects, which compare homes inside the same wildfire perimeter and models with street fixed effects, which compare groups of homes on the same street.

The additional control variables X_i include controls for wildfire vulnerability at the home site. These include, at minimum, ground slope and vegetation type. For some alternative specifications we also include elevation and property characteristics (lot size, building square footage, number of bedrooms), though this reduces the number of observations used in the analysis since these variables are not perfectly observed throughout our sample.

3.3.2 Pooled Regression

We summarize the overall effects of the wildfire building standards using a regression model that pools jurisdictions and vintages. We divide the sample into three vintage categories indexed by v: homes built before 1998 (the omitted category), between 1998–2007, and 2008 and onwards. The latter two periods correspond to the end of the mid-1990s roofing reforms and the introduction of the Chapter 7A requirements. Jurisdictions are indicated by Jurisdiction_i^j, which capture whether the home was built in the SRA, an LRA-VHFHSZ, or in a No-codes jurisdiction.

$$\text{Destroyed}_{isf} = \sum_{v=v_0}^{V} \sum_{j=j_0}^{J} \beta_{vj} \text{Vintage}_i^v \times \text{Jurisdiction}_i^j + \gamma_{sf} + X_i \alpha + \epsilon_{ijf} \tag{2}$$

The six β_{vj} estimated in this regression are the coefficients of interest, and they capture the change in destruction probability between the pre-1998 homes in each jurisdiction and homes built in future later years. This specification emphasizes temporal changes within jurisdiction and is directly comparable to the finer bin estimates provided by Equation (1).

3.4 Structure-to-structure spread

To measure the effect of code-driven mitigation on likelihood of structure-to-structure spread in code-required jurisdictions, we also estimate the effect of building vintage on likelihood of survival for neighboring homes in SRA and LRA-VHFHSZs.

$$\text{Destroyed}_{isf} = \sum_{d=1}^{D} \rho_d \text{Pre-code}_{id} + \sum_{d=1}^{D} \phi_d \text{Post-code}_{id} + \gamma_{sf} + X_i\alpha + \epsilon_{isf} \tag{3}$$

As in Equation (1), this specification controls for own year of construction and sub-street fixed effects. The additional regressors Pre-code$_{id}$ and Post-code$_{id}$ are the number of neighbors within various distance bins d that were built before and after wildfire building codes for each home i. Homes are considered post-code in 1998 in the SRA and in the year the jurisdiction was first recommended as a VHFHSZ in LRA-VHFHSZs. The coefficients ρ_d and ϕ_d for $d = 1, ..., D$ give the effect of these neighbors on own-structure survival. Our preferred specification uses 10-meter bins of wall-to-wall distance. The sample is restricted to California homes (since we can only reliably calculate exact home locations for California homes) built in code-required jurisdictions (since there are too few homes in no-code jurisdictions within California to estimate neighbor effects by distance bin). We further drop condominiums and townhomes to focus on detached single family homes.

This regression identifies the causal effect of code-induced mitigation by neighboring homes if the code regime for neighboring homes is uncorrelated with other determinants of structure- and neighborhood-level risk. This assumption is bolstered by the sub-street fixed effects, which focus on highly local variation, and by comparing between homes with pre- or post-code neighbors immediately adjacent versus those with neighbors farther away. The motivation behind this approach is that even within these narrow comparisons and even after controlling for own age, the age of a home's neighbors may still be correlated with other wildfire risk factors. Properties located 50 to 100 meters away are outside of the 30-meter home ignition zone and so present more limited direct ignition threat, but should otherwise be subject to the same potential omitted variables as directly adjacent homes.

4 Results

4.1 Own-Structure Survival

4.1.1 Graphical Evidence

Figure 3 shows the average rate of destruction for SRA homes in our sample according to year of construction. About 35% of exposed homes built prior to the mid-1990s were destroyed during the fires in our sample. These destruction probabilities begin to fall for homes built after the mid-1990s, decreasing quickly to about 20%. This sharp improvement in resilience corresponds in time to the post-Oakland Firestorm building reforms. Appendix Figure A2 shows that homes built before and after the building code changes are otherwise comparable. The introduction of building codes does not coincide with changes in site-level predictors of fire risk, like ground slope, or in structure characteristics such as building square footage.

There is also some evidence in Figure 3 that homes built before about 1980 may be less likely to be destroyed than homes built just prior to the roof requirements. This could reflect the fact these older homes are more likely to have been re-roofed at least once after the mid-1990s and complied with the requirement for ignition-resistant materials at roof replacement. This pattern would imply a replacement cycle of about 30-40 years. Actual data on roof service lifetimes is scarce, but this period is within the range proposed by the National Association of Home Builders and other sources (National Association of Home Builders 2007). To the extent that some pre-building code homes may be re-roofed with code-compliant materials, our estimates of building code effects are conservative.

Figure 4 presents the vintage effect estimates from Equation (1). The top panel shows homes in SRA, where WUI building codes are mandatory. The markers show estimates and 95% confidence intervals for two-year vintage bins. The omitted bin is 1987-1988, so that these estimates can be interpreted as percentage-point differences in likelihood of destruction relative to a 1987 home. The vintage effects are flat prior to about 1993, and then begin to decrease sharply during the 1995–1999 period. The point estimates suggest additional reductions in loss probability following the adoption of the Chapter 7A codes in 2000. This additional reduction could reflect the increased stringency of the revised structural requirements in the Chapter 7A reforms and the introduction of compliance with vegetation management regulations, although the small number of homes in those bins leads to somewhat noisy vintage estimates. The overall difference in loss probability between a 1987 home and a post-2008 home is about 15 percentage points.

The middle panel shows homes in LRAs that CAL FIRE recommended for Very High Fire Hazard Severity Zone (VHFHSZ) designation. These jurisdictions again show flat trends in resilience prior to the 1991 Oakland Firestorm and subsequent Bates Bill. After the Bates Bill takes effect, the figure shows steady improvements that persist for about 12 years. The slope of these improvements appears more gradual than in SRAs, which is consistent with varied timing of adoption of the recommended codes across hundreds of individual municipalities. The post-2008 estimates are again noisy but imply further improvements in resilience following adoption of the Chapter 7A building codes.

Finally, the bottom panel of Figure 4 shows vintage effects for homes in jurisdictions not subject to California's codes. This includes fires in Arizona, Colorado, Oregon, and Washington with no state or local wildfire building codes. There are relatively few homes in this group, so we use wider ten-year vintage bins to increase precision. Unlike the top two panels, there is little evidence of improved resilience for homes built since the mid 1990s in these jurisdictions.

4.1.2 Regression Estimates

Table 1 summarizes the effects of wildfire building codes on structure resilience. We report separate estimates for the SRA, LRA-VHFHSZ, and No-codes groups. The various group by time period estimates can be interpreted as percentage point differences in likelihood of destruction relative to a pre-1998 home in the same jurisdiction.[24] Column (1) shows a specification with incident fixed effects. These incident dummies absorb fire-specific differences in severity and preparedness. In this specification, SRA homes built during 1998–2007 or 2008–2016 perform 12.6 percentage points and 15.8 percentage points better, respectively. A similar pattern exists for LRA-VHFHSZs, with improvements of 6.7 percentage points for 1998–2007 homes and 10.7 percentage points for 2008–2016 homes. Except for 1998–2007 LRA-VHFHSZ homes, all of these estimates are statistically distinguishable from zero at conventional levels. Similar improvements are not apparent in the No-codes comparison group, where homes built in the latter two vintage categories do not experience statistically significant changes in destruction rates. This is further evidence that observed improvements in the regulated jurisdictions are due to building codes as opposed to other time-varying factors. The regression also includes controls for topography and vegetation. As expected, slope steepness at the home site increases vulnerability. A home on a 10 degree slope would be five percentage points less likely to survive than an otherwise-identical home on flat ground.

The remaining columns of Table 1 show specifications with various street-by-incident fixed effects. These more granular fixed effects adjust for potential differences in fire intensity or home characteristics within the burned area. Column (2) reports estimates from a specification that controls for street fixed effects, identifying differences in destruction rates across homes of different vintages but on the same street. Again, we find that homes built in SRA and LRA-VHFSHZ jurisdictions after the California building code mandates came into effect experience large declines in destruction, while non-California homes show no change over the same period.

Column (3) reports the preferred specification using sub-street fixed effects: here we examine differences in destruction rate for homes of different vintages within sets of 25 homes on the same street. We find that SRA homes experience declines of 8.9 percentage points (on a mean of 39 percentage points) if they were built between 1998 and 2007, and 13.1 percentage

24. Note that group-specific means are absorbed by the fixed effects in each specification.

points if they were built between 2008 and 2016. LRA homes experience analogous declines of 7.1 percentage points and 11.1 percentage points. By contrast, homes outside of California do not experience statistically significant changes in destruction rate. The R^2 with sub-street fixed effects is notably higher than with incident fixed effects (0.66 vs 0.39), implying that these fixed effects remove variation in fire severity and other factors within incidents that might otherwise threaten identification.

Even with this rich set of fixed effects, differential trends in home construction across space could lead to either home or location characteristics shifting differentially across locations over the period we study. Appendix Table A7 tests for meaningful changes in the covariates using the standardized mean difference approach described in Imbens and Rubin (2015). We do not see meaningful differential changes in slope, elevation, lot size, bedrooms, or wildfire hazard. However, homes in codes-required jurisdictions have increased in square footage slightly more during this time period than homes in jurisdictions where there are no codes in place. If larger homes—having a large surface area for ignition—are more likely to burn in wildfires, then this imbalance would cause the effects of mandatory building codes we document in columns (1)–(3) to be biased towards zero (i.e., our estimates would be conservative).

To account for any covariate imbalances directly, column (4) adds lot size, number of bedrooms, home square footage, lot elevation, and wildfire hazard as additional controls. These characteristics are missing for about 20% of homes, which shrinks the sample in this column. Nevertheless, the point estimates increase slightly in magnitude relative to column (3) for homes in areas with codes, and remain statistically insignificant and close to zero for areas without them. Appendix Table A6 reports all estimated coefficients for the specifications.

4.1.3 Additional Results and Sensitivity Checks

Appendix Section B documents an extensive series of additional results and sensitivity checks on the main findings. We summarize these checks here and reserve complete descriptions for the Appendix. In general, the findings are insensitive to alternative choices of estimation samples, included control variables, and estimation approaches.

Alternative estimation samples. Appendix Section B.1.1 considers several sensitivity checks on the main estimates that use different sets of homes as the estimation sample. First, we report specifications using the small number of California homes that were not subject to building codes as comparison homes. Second, we estimate a specification that includes only homes built before 2015 and fires occurring after 2015, and drop homes that are included in more than one fire perimeter. Across these alternatives estimation choices, the qualitative findings remain the same: post-code homes in jurisdictions where codes were required survive at considerably higher rates than pre-code homes, while the estimates for homes in the No-codes group remain close to zero.

Differential firefighting effort. We consider whether targeted firefighting effort could explain part of the resilience effects that we measure. As discussed previously, because our empirical design compares outcomes for homes on the same sub-street, it would need to be the case that suppression effort is not only targeted broadly across threatened neighborhoods, but also *within* groups of nearby homes on the same street. Columns (6) and (7) of Appendix Table B1 test for this possibility by restricting the sample to only homes in very large wildfires, where the ability of firefighters to protect individual homes is extremely limited.[25] We find that the estimated resilience improvements from building codes are similar when we include only homes involved in fires that threatened more than 1,000 homes or where fire managers reported severe personnel constraints. In the spirit of completeness, the same appendix table also shows that the results are unaffected by dropping homes located less than 50 meters

25. For example, the 2018 Camp Fire in Paradise, CA was burning more than 80 acres *per minute* at one point (Simon 2018).

inside the edge of the final wildfire perimeter, which is another way of testing for highly localized targeting of firefighting effort.

External validity. Potential community-scale differences in firefighting effort and other factors could in principle raise questions of external validity. Is the sample of homes that actually experience wildfires representative of the broader population of homes at risk of wildfire? To help answer this question, Appendix Table A2 compares characteristics of homes that experienced a wildfire to other sets of potentially at-risk homes. We find that the main estimation sample is broadly similar on observables to three increasingly restrictive comparison groups: all California homes in census tracts with average wildfire hazard exceeding 0.1% (the sample we consider in Section 5), homes in areas subject to California's wildfire building codes, and homes in SRA jurisdictions. We interpret this as evidence that our findings are likely to be broadly applicable to homes in other at-risk areas in California.

Homes Contributing to Estimation with Fixed Effects. To better understand which homes are contributing to the estimation, Appendix Section B.1.2 documents how many homes are on streets or sub-streets with variation in exposure to building code regulations (and therefore contribute to identification with street and sub-street fixed effects). In brief, around half of the homes in our sample are on streets with both pre- and post-code homes. The subsample with within-street variation is not substantially different on observables.

Attenuation Due to Spillovers. Appendix Section B.1.3 considers the potential for attenuated estimates of own-home resilience due to the spillover effects in Section 4.2. We implement a regression that controls directly for the number of pre- and post-code near neighbors in the street fixed effects regression. Ultimately, the differences in the estimated building code effects across these approaches—sub-street fixed effects, incident fixed effects, and sub-street fixed effects directly controlling for spillovers—are small.

Alternative Statistical Specifications. Appendix Section B.1.4 presents a set of sensitivity tests that use the original estimation but vary the statistical specification used by introducing increasingly granular sets of fixed effects and alternative research designs. The variations on our preferred model include a specification that only includes fixed effects as controls, sub-street fixed effects that also account for side of street, propensity score matching, formal difference-in-differences, and an analogous logit model. For all of these specifications, we find quantitative estimates virtually identical to those shown above, and in many cases the point estimates are larger in magnitude than those we document here.

Neighborhood Estimates. Appendix Section B.3 describes and estimates a model that regresses average neighborhood-level outcomes on proportions of homes in that neighborhood built to code. We find slightly larger average effects when the model is estimated in this way, which is consistent with the main resilience estimates presented in Table 1 and suggestive of the additional within-neighborhood spillover benefits we consider in the following section.

4.2 Spillovers to Neighboring Properties

This section discusses the spillover benefits of code-induced mitigation to neighboring homes. Figure 5 shows regression results for Equation (3). These estimates focus on the more accurate wall-to-wall distances, and include only the sample of California homes for whom we can precisely identify neighbor distances using this metric. This sample focuses on homes in denser areas (at least 10 neighbors within a 200 meter radius; see Appendix Table A3) in fires since 2013 (for older incidents, it is more likely that parcel boundaries have changed since the fire). The top panel shows effects of the presence of any pre-code neighbors at various wall-to-wall distances. One or more pre-code neighbors within 10 meters increases own-structure loss probability during a wildfire by about 2.4 percentage points.[26] One or more pre-code neighbors between 10–20 meters increases own-structure loss probability by about 0.8 percentage points, though this estimate is only narrowly statistically distinguishable

26. Appendix Figure A3 summarizes the distribution of neighbors by distance. On average, homes in the sample have 0.43 neighbors with walls between 0 and 10 meters away, and 82% of those neighboring homes are built in the pre-building codes era.

from zero. Neighbors within 20–30 meters increase risk by 0.7 percentage points, but the confidence interval on this estimate is large and includes zero.

The remaining pre-code home effects are statistical zeroes beyond 30 meters. Notably, this is similar to the distance of 30 meters that wildfire managers consider to be the home ignition zone—the distance within which flammable material presents a risk of structure ignition (Cohen 2000, 2010; Calkin et al. 2014).[27] The near-zero estimates beyond 30 meters bolster the validity of the research design. If our estimates for the nearest neighbors were biased by omitted predictors of resilience that co-vary within neighborhoods, one would expect that bias to also appear in estimates for homes another few dozen meters away (Figure 1b provides a visual illustration of these distances).

The bottom panel shows the estimates for post-code neighbors. The confidence intervals for these estimates are wider since we observe fewer post-code homes. However, the point estimates suggest that the presence of close neighbors built under WUI building codes does not substantially increase own-structure loss probability. Focusing solely on the magnitudes of the point estimates, the presence of a post-code neighbor within 0–10 meters increases the risk of own-home loss by 1.0 percentage points (less than half the 2.4 percentage points we estimate for pre-code neighbors) but zero is well within the 95% confidence interval.

There is also no implied effect of further-away post-code neighbors on own survival, offering additional placebo evidence to support the identifying assumptions behind this regression. Appendix Figure B2 documents sensitivity checks on these estimates. We show there the pattern of estimates we describe for pre- and post-code neighbors is very similar when using the less precise centroid-to-centroid distances on full set of California homes. We also show that spillover risk from pre-code neighbors is slightly higher for homes implicated in more destructive incidents (defined as incidents where more homes were at risk). Spillover risk in

27. The larger spillover effects for the very nearest neighbors within the home ignition zone are also consistent with the scientific literature, which identifies a few meters around a structure as particularly critical for structure survival.

these incidents remains close to zero for post-code neighbors, suggesting that increasingly destructive fires could magnify the importance of building codes in reducing neighbor-to-neighbor ignition.[28]

Table 2 reports regression estimates for each additional neighbor within 10 meters of the focal home. These estimates provide a key input for the exercise in Section 5, where we consider the net benefits of replacing pre-code homes with post-code homes, which include the spillover benefits those replacements confer on their neighbors.[29] Column (1) considers nearby pre- and post-code neighbors as determined by wall-to-wall distances. Each additional pre-code neighbor increases own-structure loss risk by 1.7 percentage points. The estimated effect of each additional post-code neighbor is 0.6 percentage points and not statistically different from zero. Column (2) shows the same regression using the centroid-to-centroid distances, which are less precise but allow us to include all California homes. The estimated risk posed by additional pre-code neighbors is slightly larger in this specification, and the estimates for post-code neighbors are smaller at 0.2 percentage points and not statistically different from zero.

Columns (3) and (4) repeat the same exercise, but replace the counts of pre- and post-code neighbors with indicators for having one or two (or more) nearby neighbors. These estimates show that the effect of one additional pre-code neighbor roughly doubles the likelihood of own-home destruction in both columns, going from 1.9 percentage points to 3.3 percentage points for the sample of homes for which we have wall-to-wall distances, and 1.8 percentage points to 3.7 percentage points for the sample of homes for which we have centroid-to-centroid distances. We do not see the same doubling of effect size for post-code neighbors, which is

28. Another likely determinant of whether a neighboring home ignites is the direction of local winds that could transport burning material between homes. We do not include wind direction in these specifications for two reasons. First, winds during major conflagrations are highly localized, variable, and difficult to predict or measure. Second, and more importantly, the unpredictability of local winds during wildfires means that there is little that homeowners or policymakers can do to account for these factors in building codes or ex-ante mitigation investment decisions.

29. For the sake of parsimony, this exercise focuses on the effects of neighbors within 10 meters since they represent both the largest and the most precisely estimated spillover effects in Figure 5.

consistent with post-code homes having limited spillover effects on their neighbors.

4.3 Broader Housing Market Effects of Wildfire Building Codes

In this section, we present evidence on the housing market impacts of wildfire building codes to complement the results on structure resilience in Section 4.1 and Section 4.2. In principle, building codes could affect housing demand in regulated areas positively if homeowners are aware of and attentive to the resilience benefits that we document. They could also reduce demand if the codes prohibit aesthetic features that homebuyers value highly (such as cedar siding). These demand effects could also manifest at the neighborhood level through the kinds of housing externalities demonstrated by Rossi-Hansberg, Sarte, and Owens (2010), Fu and Gregory (2019), and others. On the other side of the market, building codes could in principle reduce the supply of new housing if additional required investments substantially increase overall construction costs.[30]

To understand the degree to which local housing markets were impacted by the presence of wildfire building codes, Appendix Section C directly examines whether home sale prices and housing growth rates responded to the implementation of building code mandates. To do so, we test for changes in homes sales and growth rates among the larger set of California homes that face some wildfire hazard. Appendix Tables C2, C3, and C4 show that jurisdictions where wildfire building codes were implemented did not experience meaningful changes in transaction prices or home growth rates after the codes came into effect. It does not appear that California's wildfire building codes led to meaningful shifts in the local housing market. We speculate that the relatively subtle aesthetic changes required by the mandate and the limited understanding by buyers of its resilience benefits help explain the lack of housing market response.

30. Ostriker and Russo (2022) document such an effect for home elevation requirements in floodplain areas, finding that one quarter of the reduction of flooding damages comes from housing relocation away from the requirements, while the rest is due to directly reduced damages. In our setting, the cost of mitigation is considerably lower—as we discuss in Section 5.3, estimates of the total additional cost of to-code construction are generally low relative to the average value of homes.

5 Net Social Benefits of Adaptation Policies

This section turns to the topics of cost effectiveness and welfare. We explore two questions: (1) for which homes are the code-required investments cost-effective? and (2) given frictions in takeup, what are the welfare effects of building standards and other possible policies to encourage adoption? Section 5.1 develops a model of hazard mitigation investment. Section 5.2 derives the welfare properties of policies to encourage investment in this model, focusing on building code mandates and subsidies. Section 5.3 discusses how we parameterize the model using our empirical estimates and values from the literature. Section 5.4 presents results showing where mitigation investment is cost effective and Section 5.5 compares the welfare effects of various policies to encourage mitigation.

5.1 Model of Natural Hazard Mitigation Investment

Homeowners indexed by i face a risk p_i of a natural disaster in their neighborhood.[31] Conditional on a disaster occurring, they incur loss L with probability q. A homeowner's overall probability of loss is then $p_i q$. They can invest in mitigation (e.g., disaster-resistant construction) at cost m, which reduces their overall probability of loss to $p_i(q - \tau)$. For ease of exposition, this section presents a static model. Our actual calculations discount future benefits over the life of the home at 5% per year.

We say that mitigation by homeowner i is privately cost-effective if the reduction in expected

31. Here we build a model that is empirically tractable while capturing the key economic tradeoffs of a hazard mitigation mandate. Existing theoretical work, including the model of strategic mitigation of a public bad described in Costello, Quérou, and Tomini (2017), considers a richer framework. These more general treatments include strategic interactions between agents who make repeated defensive investments against a mobile public bad. We see these strategic interactions as less central in our application, where homeowners are making large one-time investments. Our model also differs by incorporating risk miscerception. Given the large empirical literature on misjudged disaster risk, we see this margin as likely important for determining real-world outcomes.

loss outweighs the mitigation cost.

$$p_i q L - \big[p_i(q-\tau)L - m\big] > 0 \Rightarrow$$
$$p_i \tau L > m$$

This measure is likely a lower bound on the true private cost-effectiveness of mitigation, given anecdotal evidence that many households are not fully insured against disaster losses (e.g., Klein 2018). The Appendix describes an expected utility calculation that accounts for additional benefits from reduced exposure to uninsured risk (Appendix Section D.5). We focus on the lower bound here because it is more conservative and requires fewer assumptions about the utility function, the completeness of insurance, initial wealth, and other parameters.

In this benchmark version of the model, homeowner decisions to mitigate or not will align with a social planner whose goal is to maximize social net benefits. The following sections add elements that drive a wedge between some homeowners' choices and what the social planner would choose.

5.1.1 Adding Spillovers

Now we assume that mitigation by i creates spillover benefits η for close neighbors. We adopt an empirically tractable specification where investment by i reduces neighbor j's loss probability during a disaster, so that the decrease in expected loss for each affected neighbor j is $p_j \eta L$. Letting q_j represent the neighbor's baseline loss probability in a disaster, the spillover benefit is $p_j q_j L - p_j(q_j - \eta)L = p_j \eta L$.[32] A social planner would then choose that i

32. Richer models could allow for more heterogeneity in spillover benefits. An example is Fu and Gregory 2019, which finds that amenity spillovers from post-Hurricane Katrina resettlement were nonlinear in the average resettlement rate within a one-mile radius. In comparison, our model restricts to a constant neighbor externality since A) few homes have more than one or two neighbors in the relevant distance, limiting the empirical scope for nonlinearity according to takeup rate; B) the evidence in Table 2 is broadly consistent with linear spillovers, in that it does not reject equal benefits for the first and second neighbor that mitigate; and C) the limited historical sample of wildfire-exposed homes means that estimation of more complex spillover functions is limited by statistical power.

mitigate if:

$$p_i \tau L + \sum_j p_j \eta L > m$$

In our empirical application, affected neighbors are close in space (tens of meters). Since there is little meaningful variation in p over such short distances, we simplify the calculation and notation by replacing p_j with p_i and letting δ_i be the number of close neighbors for home i. We can then write that mitigation is cost-effective for society if the following conditions hold:

$$\begin{aligned} p_i \tau L + \delta p_i \eta L &> m \Rightarrow \\ p_i L(\tau + \delta_i \eta) &> m \end{aligned} \tag{4}$$

We define the break-even level of fire risk p_i where the social planner is indifferent between a homeowner investing and not as $\tilde{p} \equiv \frac{m}{L(\tau+\delta_i \eta)}$.

5.1.2 Adding Risk Misperception

A large literature has consistently shown that households substantially underestimate disaster risks. Surveys that compare homeowners' beliefs about wildfire risk to expert assessments of the same properties consistently find substantial downward bias in beliefs (Brenkert-Smith et al. 2019; J. Meldrum et al. 2019b, 2019a). Studies using property transaction data show that home prices fall after nearby fires or wildfire information campaigns, consistent with biased beliefs about wildfire risk beforehand (Loomis 2004; Donovan, Champ, and Butry 2007; McCoy and Walsh 2018). Similar evidence for flood and other disaster risks points in the same direction (Gallagher 2014; Wagner 2022; Bakkensen and Barrage 2021). Suppose that the homeowner underestimates their true risk of disaster by a factor θ such that $0 \leq \theta \leq 1$.[33]

33. Homeowners could also underestimate the effectiveness of mitigation τ or the extent of their losses L, or they could be inattentive to their true risk (i.e., the risk would not be salient). In this model the effect of

Now, i invests if:

$$\theta p_i \tau L > m \tag{5}$$

There are two differences between the homeowner's—Equation (5)—and the planner's—Equation (4)—choices to mitigate: the homeowner does not take into account spillover benefits of mitigation or correctly anticipate the size of their expected loss. This means that the threshold value of p_i that determines investment is higher than the social planner would choose. We define the level of fire risk where homeowners are indifferent between investing and not as $\hat{p} \equiv \frac{m}{\theta \tau L}$, which is larger than $\tilde{p}$. Put another way, homeowners who underestimate their own risk and/or do not internalize spillovers may choose not to mitigate when such investment would be socially efficient.

5.2 Policies to Encourage Mitigation

Given risk misperception and/or spillovers, policies that encourage mitigation may yield social benefits. The theoretically ideal bundle of policies for addressing a combination of externalities and internalities would be a perfectly effective information intervention combined with a Pigouvian tax to address externalities (see, e.g., Allcott, Mullainathan, and Taubinsky 2014). For both political and practical reasons, such a combination of policies is unlikely to be feasible for addressing disaster risk. We consider two second-best policies that are frequently implemented in practice: building codes and subsidies.

5.2.1 Building Codes

Total welfare under a building code that mandates mitigation is

$$\sum_i \left[p_i(\tau + \delta_i \eta)L - m\right]$$

these would be equivalent.

Recognizing that homeowners facing $p_i > \hat{p}$ mitigate regardless, the change in welfare due to the policy is the sum of the net benefits of adoption by homeowners with $0 \leq p_i \leq \hat{p}$. It is instructive to write this in three parts:

$$\underbrace{\sum_{i|p_i \in [\tilde{p}, \hat{p}]} [p_i \tau L - m]}_{\text{(A)}} + \underbrace{\sum_{i|p_i \in [0, \hat{p}]} p_i \delta_i \eta L}_{\text{(B)}} - \underbrace{\sum_{i|p_i \in [0, \tilde{p}]} [m - p_i \tau L]}_{\text{(C)}}$$

Part (A) is private net benefits to homeowners who benefit from mitigating but fail to invest due to risk misperception; (B) is external benefits to neighbors; and (C) is private net costs to homeowners for whom mitigation is not socially cost-effective. This formulation captures the inherent tradeoff of a mandate: regulators must weigh the benefits of correcting internalities and externalities against the costs of forced adoption by homes with negative social net benefits.

Disaster mitigation differs importantly from many markets where standards have been evaluated, including consumer goods, personal finance, and health (see citations in Footnote 1). Two primary determinants of social net benefit—local disaster hazard and the number of close neighbors—are readily observable. This creates scope for targeted mandates that may limit the amount of forced inefficient adoption. The benefits of such targeting depend on the geographic granularity with which the regulator can define code areas, and the variance of risk within those spatial units. In our empirical application of wildfire risk—and for other disasters like hurricanes, floods, and earthquakes—disaster risk is highly spatially correlated.

5.2.2 Subsidies

A popular alternative policy is to subsidize mitigation. We consider the second-best optimal subsidy to address internalities and exernalities in Allcott and Taubinsky (2015). This subsidy equals the sum of the average undervaluation and the average externality for the marginal adopter in the efficient outcome.[34] In our setting, this equates to

$$s = E_{i|p_i(\tau+\delta_i\eta)L=m}[(1-\theta)p_i\tau L + \delta_i\eta L]$$

This amount represents the average shortfall in willingness to pay for households just at the margin of efficient adoption. Ignoring for a moment the cost to finance the subsidy payments, total welfare under the subsidy is

$$\sum_i 1[\theta p_i L + s \geq m][p_i(\tau + \delta_i)L - m]$$

This subsidy corrects behavior on average, but does not necessarily yield the first-best outcome for all homes. Differences in δ_i across homes marginal to the policy lead to under- or overadoption for some households. The total fiscal cost of the subsidies provided is

$$s \sum_i 1[\theta p_i L + s \geq m]$$

As with mandates, the observability of disaster risk and number of neighbors creates scope for targeted subsidies. We also consider a differentiated subsidy that pays each efficient adopter the exact amount by which their private perceived WTP falls short of m. This perfectly differentiated subsidy is

$$s_i = \begin{cases} 0 & p_i(\tau + \delta_i\eta)L < m \\ max(0, m - \theta p_i L) & p_i(\tau + \delta_i\eta)L \geq m \end{cases}$$

The perfectly targeted subsidy yields the first-best level of mitigation investment, with total fiscal cost of subsidies equal to $\sum_i s_i$.

34. In empirical applications, the discrete nature of the value function maximization problem implies that this formula may differ slightly from the optimal subsidy calculated using, for example, a grid search over possible subsidy levels. Simulations with our data find that this difference is negligible in our setting.

5.3 Empirical Implementation

We parameterize our model using data on a large sample of California homes in areas of wildfire hazard, empirical results from our study, and values from the literature. We calculate Equation (4) for each home, which allows us to identify where mitigation (i.e., wildfire-resistant construction) is cost-effective. We then calculate Equation (5) for each home, which allows us to evaluate takeup in the absence of a mandate or subsidy policy. Finally, we compute the empirical analogues of the welfare expressions for building codes and subsidies in Sections 5.2.1 and 5.2.2.

This exercise uses a sample of 1.1 million homes in wildfire-prone areas throughout California. Unlike the empirical analysis of building code effects, which uses homes located inside historical wildfire perimeters, the net benefits calculation considers homes throughout California, not just those that faced a wildfire. Specifically, we include all single-family homes in California for which we can obtain key home characteristics information (year built, square footage) and that are located in a census tract where home average wildfire hazard is at least 0.1%.[35] The rest of this section summarizes the calibration of model parameters. We reserve more comprehensive descriptions for Appendix Section D.

Wildfire hazard (p_i): We identify annual wildfire hazard probability p_i for each home in the sample using the same definition as in Section 2: it is the product of Burn Probability (the total annual wildfire probability) and Flame Length Exceedance Probability 4 (conditional on any fire, the probability that the fire will reach moderate or greater threat status), both obtained from Scott et al. (2020).

Voluntary takeup without building codes: Detailed data on wildfire preparedness for homes outside building code jurisdictions is not systematically collected or tracked.[36] We conducted

35. As a result of these restrictions, 50 of 58 California counties are included in the sample. Imperial, Inyo, Kings, San Francisco, Sutter, and Yolo are excluded because they do not have any tracts with home average wildfire hazard greater than 0.1%. Del Norte and Mendocino are excluded because the assessment data do not include square footage or year built information for those homes.

36. Remote sensing methods using aerial or satellite imagery are not yet capable of accurately determining

a comprehensive review of survey studies of wildfire home hardening (see Appendix Table A4). Our benchmark assumption about adoption in the absence of building codes is based on professional assessments of 1,474 Colorado homes in Champ et al. (2020). Of those, 40% use building materials that would comply with California codes in at least two of three areas: roof, exterior siding, and deck.

Own mitigation benefits (τ): Our estimates of the effect of codes on structure survival in Section 4 can be seen as intent-to-treat estimates of the effect of mitigation investment. Given the above assumption about voluntary takeup, the standard Wald estimator gives τ as the ratio of the reduced form estimates and the difference in take-up rates between jurisdictions where codes are and are not required.[37] Using our estimate from Table 1, column (2), we divide by one minus the voluntary adoption rate: $\tau = \frac{0.133}{0.6} = 0.22$.

Number of neighbors (δ_i): We measure each home's number of neighbors directly. We count neighbors within 30 meters of centroid to centroid distance, which roughly corresponds to 10 meters of wall-to-wall distance (see footnote 20) and is less computationally demanding in this larger sample.

Neighbor benefits (η): Spillover benefit η is calculated similarly to τ, but is the effect of having a nearby post-code neighbor instead of a pre-code neighbor. Our estimate of the effect is therefore 0.017 - 0.006 = 0.011 (Table 2, column (1)). We divide by one minus the voluntary adoption rate: $\eta = \frac{0.011}{0.6} = 0.018$.

Losses (L_i): Assumed losses L_i for a home destroyed by wildfire include rebuilding costs, contents of the home, alternative living costs during rebuilding, debris removal, and hazardous waste cleanup. Per-square-foot rebuilding, contents, and alternative living arrangements costs come from the FEMA Hazus model (Federal Emergency Management Agency 2021). Hazus provides loss cost estimates at the Census block level in 2018 dollars per square foot.

the fire readiness of hard-to-observe building components like siding, eaves, and vents. This may change in coming years, as this is a major area of current investment.

37. See e.g., Angrist and Pischke (2009) p. 127-133. This calculation assumes perfect compliance by homes subject to codes and a homogeneous effect of mitigation on structure survival.

These costs vary based on local wages and the mix of construction classes (economy, average, custom, and luxury) in a given block. Appendix Section D.4 shows that the 5th and 95th percentile of per-square foot losses are roughly \$200 and \$350, with a mean of \$260. Total cost L_i is the square footage for home i times these per-square-foot costs, plus a garage rebuilding cost adjustment (also provided by Hazus at the Census block level) and a provision for hazardous waste disposal / site cleanup as described in the appendix.

Mitigation costs (m_i): Our main estimate of m_i comes from Headwaters Economics (2018), which uses construction estimating tools to calculate the cost of complying with California's Chapter 7A wildfire code. Overall, the study reports zero cost difference between code-compliant and standard designs because one aspect of code-compliant construction—exterior siding—is substantially *less* expensive than standard designs. These savings offset increased costs in other areas. Our main estimate of m_i ignores the reported savings from siding since the failure to adopt this less expensive but more resilient option in the baseline scenario may indicate aesthetic preferences that offset the lower construction costs. After making this one adjustment to the Headwaters Economics (2018) estimate, the resulting code compliance cost is \$15,660 (in 2018 dollars) for a 2,500-foot reference home. In scenarios where we hold reconstruction and mitigation costs constant, we set square footage to 2,500 to match this source as closely as possible. In scenarios where we allow reconstruction and mitigation costs to vary across homes, we convert to a per square foot mitigation cost of $\frac{15{,}660}{2{,}500} = 6.22$.

We also report results for alternative cost estimates from the National Association of Home Builders (NAHB), which reports "low" and "high" scenarios of \$7,868 and \$29,429 (Home Innovation Research Labs 2020). We deflate the NAHB estimates to 2018 dollars to get \$7,634 and \$28,553. Finally, our retrofit scenario uses Headwaters Economics' estimate of \$62,760 to fully replace roofing and exterior walls on an existing home.

5.4 Where is Mitigation Cost-Effective?

This section documents the cost-effectiveness of wildfire hardening investments across at-risk areas across California. This section first considers cost effectiveness of wildfire hardening investment for a home of constant size and rebuilt cost, given varied levels of wildfire risk and number of close neighbors. We then incorporate heterogeneity in size and rebuilding costs.

5.4.1 Constant Rebuilding and Mitigation Costs

Figure 6 shows census tract averages of annual wildfire hazard and number of near neighbors. The wildfire hazard reaches strikingly high levels: several tracts face event probabilities above 2% per year, implying a significant wildfire exposure every 50 years on average. The color scale shows the social benefit of mitigation investment in each census tract following Equation (4). These estimates assume a typical new home based on the 2,500-foot considered in Headwaters Economics (2018). The dashed black line shows a threshold for positive net benefits of building standards assuming a typical home based on the 2,500-foot cost estimates considered in Headwaters Economics (2018). Homes to the right of this line have lower expected costs with mitigation investments than without. The threshold bends left as the number of neighbors increases due to the spillover benefits of mitigation. For a home with zero near neighbors, i.e., a home that does not have other homes near enough for wildfire risk to spill over to its neighbors, the break-even annual wildfire hazard is 0.40%. This falls to 0.37% with 1 near neighbor and 0.30% with 4 near neighbors. Appendix Section D.4 documents average cost-effectiveness using the alternative mitigation cost scenarios given in Table 3.

These cost effectiveness estimates are a lower bound on the net benefits of mitigation investments. One reason for this is that many homeowners are not fully insured for wildfire losses. Even for properties covered by homeowners insurance, Klein (2018) reports that coverage limits for wildfire-destroyed properties are often up to 50% below actual monetary losses. Through the lens of a standard expected utility model, the fact that homeowners are not fully insured makes the possibility of a wildfire loss more costly than in the risk neutral model—and thus makes the net benefits of mitigation investment higher than the risk-neutral cost effectiveness estimates as long as homeowners have any non-zero degree of risk aversion.[38] Table 3 reports break-even annual wildfire probabilities for a home with 2.5 near neighbors (the mean among the all-California sample we use in this section) based on the expected utility model in Appendix Section D.5, which incorporates measures of risk aversion in the homeowner's utility function. Although this model requires additional strong assumptions, these back-of-the-envelope numbers depict how risk aversion might affect benefits. For example, if mitigation costs $15,660, a homeowner with a coefficient of relative risk aversion of 5 and an insurance policy covering two thirds of total losses would be better off investing in mitigation wherever the annual probability of a damaging wildfire exceeds 0.29%.[39]

Table 3 also reports results using other estimates of mitigation cost. The zero net cost estimate from Headwaters Economics (2018) leads to positive benefits for any level of hazard. The two additional estimates from Home Innovation Research Labs (2020) bracket the main cost estimate. Finally, the estimated retrofit cost of $62,760 results in much higher break-even hazard levels for existing homes. This kind of full retrofit to existing homes appears to generate positive benefits only for a handful of areas with extreme fire hazard. We note that a partial retrofit, i.e., a retrofit undertaken during other renovations, would likely imply lower mitigation costs and therefore be cost-effective in more areas.

38. We take the positive empirical fact of partial insurance as given. This empirical fact is sufficient to establish that the risk neutral cost effectiveness measure is a lower bound on the net benefits of mitigation investments. The question of *why* homeowners are not fully insured for wildfire risk is a critical question for future work (e.g., Boomhower et al. 2024).

39. Studies of the property insurance market generally report high implied levels of relative risk aversion. Cohen and Einav (2007) and Sydnor (2010) examine deductible choices in auto and homeowners insurance respectively and find double-digit values for the mean household across a variety of specifications. Evidence from other markets suggests values closer to the low single digits (e.g., Gertner 1993; Chetty 2006).

Beyond risk aversion, wildfire-safe construction likely has additional benefits that are not included in our calculations. These include reductions in public expenditures on firefighting during large wildfires (Baylis and Boomhower 2023), reduced need for public assistance to fire victims (Deryugina 2017), avoided emotional and psychological distress, and less need for public safety power shutoffs that interrupt electricity service during high fire-risk periods.[40] Projected increases in future wildfire probabilities throughout North America suggest that the benefits of mitigation will continue to grow.[41]

5.4.2 Heterogeneity in Rebuilding and Mitigation Costs

The cost-effectiveness estimates in the preceding section assume a 2,500 square foot newly-constructed home based on the model home in Headwaters Economics (2018). This section introduces heterogeneity in home size and per-square-foot rebuilding costs. Appendix Table D1 describes the variation in these factors within the homes in our sample of California homes. Per-square-foot rebuilding costs from the FEMA Hazus model vary across Census blocks due both to differences in local labor costs and differences in the typical quality of construction (economy, luxury, and so on). The 5th, 50th, and 95th percentiles of per-square-foot rebuilding costs ("Losses per sf") in our sample are 198, 249, and 354. As discussed previously, square footage also varies across homes, with more recently constructed homes being larger on average. Appendix Table D2 also reports the pairwise correlations between per-square-foot rebuilding costs, square footage, annual wildfire hazard, and number of neighbors.

Figure 7 shows the distribution of structure-level cost-effectiveness using individualized loss value and up-front investment assumptions that depend on both the location and size of the home. Each of the box-and-whiskers plots in the figure summarizes the distribution of

40. For a systematic review of catastrophic wildfire costs, see Feo et al. (2020).

41. Risk Factor, an open source model of fire risk produced by the non-profit First Street Foundation, predicts roughly a doubling in the annual probability of homes in wildland areas facing a major wildfire threat in the next thirty years. See https://riskfactor.com/ for details.

individual cost-effectiveness estimates for homes in a given range of wildfire hazard. The left-hand panel shows the distribution of net benefits for hardening investments made at the time of construction. For each home, we calculate the net benefits from hardening a newly-constructed home with the observed square footage and local rebuilding cost factors. In the highest-hazard areas (annual wildfire hazard above 0.75%), mitigation is cost-effective for the full range of observed losses and mitigation costs. For homes with annual wildfire probability between 0.5% and 0.75%, mitigation is cost-effective for over $\frac{3}{4}$ of homes. In the lower wildfire hazard bins, relatively few homes pass this risk-neutral cost-effectiveness test. These more detailed, heterogeneity-inclusive results are broadly consistent with the constant-cost analysis in Section 5.4.1, which found that hardening for a typical home was cost-effective in areas with wildfire risk above 0.33%. The right-hand panel shows the distribution of net benefits for the same group of 1.1 million homes, under the assumption that hardening improvements are made as retrofits to already-built homes. Because of the much higher cost of retrofitting homes, few homes pass the cost-effectiveness test in this case.

In summary, our empirical estimates and model calculations suggest that investments in wildfire-safe construction yield unambiguous net benefits in the most fire-prone areas of California, especially when homes are clustered closely together such that there are large risk spillovers. For areas with lower fire hazard, the sign of net benefits is more sensitive to modeling choices and the assumed co-benefits of mitigation. Further work on the cost-effectiveness of wildfire mitigation measures in low- and moderate-hazard areas is an important area for research, particularly as overall wildfire probabilities continue to rise.

5.5 Comparing Policies to Encourage Mitigation

This section considers the welfare effects of policies to increase wildfire-safe construction. We consider two categories of policies that are frequently implemented in practice—building codes and subsidies—as well as a no-policy alternative. In each case, we use the model and data to calculate the relevant welfare expressions from Section 5.2. This exercise requires one additional assumption, which is the degree of risk misperception θ. While the literature strongly points to $\theta < 1$, the exact size of the behavioral bias is less clear. Our main approach to calculating θ is to find the degree of misperception that matches the implied voluntary takeup rate for properties in our sample (given our other assumptions) to the 40% takeup rate in Champ et al. (2020). This yields $\theta = 0.5$, which we use throughout the policy comparisons presented here. The appendix documents how alternative assumptions about θ impact the policy comparisons. Our calculations can be easily updated in the future as better data on private demand for home hardening investments become available.

Table 4 reports welfare measures under a range of policies. The metric of central interest is the column labeled "Social Net Benefit", which is the sum of all homeowners' net benefits under a given policy. The remaining columns summarize total private benefits ("Private Net Benefit"), spillover benefits to neighbors ("External Benefit"), the share of all households who adopt ("Total Adoption Rate"), the share of all households who adopt and in doing so create negative social net benefits ("Inefficient Adoption Rate"), and the total subsidy payments ("Fiscal Cost").[42]

Panel A of Table 4 establishes some benchmarks for comparison. Given our calibrated mitigation costs, benefits, and degree of risk misperception, the total net benefit from home hardening investments in the absence of any policy would be about \$3.9 billion. About 10% of the homes in our sample of wildfire-threatened California homes would adopt. The second row shows that a perfect standard where the regulator mandated adoption by all efficient adopters would yield \$5.1 billion in net benefit and a takeup rate of 29%. The third row considers an idealized perfectly-differentiated subsidy that achieves the first-best outcome by paying each efficient adopter (each home where social net benefit exceeds cost) the exact amount by which their perceived private value lags the cost of adoption (if any). This idealized differentiated subsidy would require payments of about \$900 million.

42. Appendix Section D.3 gives more details on the construction of these metrics.

Panel B compares outcomes under a range of building code policies, all of which involve zero fiscal cost. The “Hazard only” row shows a policy where building code is targeted only on wildfire hazard. This type of policy mimics proposed or adopted building standards for wildfire, flood, and other hazards.[43] We consider a policy that requires adoption wherever the annual probability of wildfire exposure exceeds 0.4%, the welfare-maximizing hazard cutoff in our data for such a policy. Total welfare is about $100 million less than under the ideal benchmarks. The total adoption rate is about 1.3 percentage points lower, and 1.8% of households are induced to adopt despite negative net benefits. These differences arise because of differences in rebuilding cost across homes with identical levels of wildfire hazard. At the same time, these differences are relatively small. Compared to the no-policy alternative, the hazard-based building code achieves welfare gains of $1.15 billion and the idealized standard or subsidy achieves welfare gains of $1.23 billion.

The remaining rows in Panel B impose different geographic restrictions on building codes. We assume that for practical implementation purposes, the building standard must be applied to all or no homes within contiguous spatial units. In each case, we assume the regulator mandates takeup in spatial units where the total net benefits of universal adoption are non-negative. A building code applied at the county level would significantly reduce welfare compared to no policy. The total takeup rate is 38%, with slightly over half of those being inefficient adopters where the costs of adoption exceed social benefits. This result occurs because the handful of counties where the standard is imposed include large numbers of inframarginal adopters, who would have mitigated regardless. The primary effect of the standard is to induce takeup among large numbers of households with negative social net benefits.

More geographically tailored mandates do better. Standards at the Census block or street

43. Mapmakers at the California Department of Forestry and Fire Protection define code boundaries based on assessed wildfire risk. See https://osfm.fire.ca.gov/divisions/community-wildfire-preparedness-and-mitigation/wildfire-preparedness/fire-hazard-severity-zones/fire-hazard-severity-zones-map/ for examples. For flood, building requirements often apply to homes in the 100-year flood plain.

level increase welfare compared to the no-policy alternative. Targeting codes by census block or street yields social net benefits 25% larger than under no policy. These policies achieve about 95% of the welfare level under the idealized house-level standard or subsidy. Intuitively, the number of inefficient adopters is higher when codes are applied coarsely (e.g., at the county level) because the variance of disaster risk and number of neighbors is larger in these broad geographies than within smaller units. Appendix Table D3 illustrates this point by decomposing variance in wildfire risk within and across counties, Census blocks, and streets. The large majority of variation in wildfire risk is between as opposed to within blocks or streets.

The bottom rows of the table consider second-best subsidy policies. A fully uniform subsidy, where the regulator chooses the fixed subsidy amount that maximizes net social benefits, yields about $300 million less in net benefits than a hazard-only building code, and requires around $1.5 billion in subsidy payments. A per square foot subsidy, where the regulator offers a fixed amount that scales with home size, produces better results: total welfare under this second-best subsidy is about $70 million higher than under the hazard-based building codes. However, the per square foot subsidy also implies a fiscal cost of $2.2 billion in transfers and an attendant efficiency cost determined by the relevant cost of public funds, which we do not directly model here. Moreover, unlike the building code policy, the regulator must know the level of risk misperception θ in order to implement the correct second-best subsidy.

Appendix Section D.7 includes additional discussion on four related topics, which we summarize here. First, we compare welfare gains under each policy when spillover benefits are included vs. excluded, showing that (a) spillover benefits to neighbors contribute meaningfully to the total welfare improvement but (b) the large private benefits rooted in risk misperception mean that spillovers are not necessary for policy interventions to improve welfare. Second, we show that a more sophisticated regulator can slightly improve the building code standard policy by accounting for inframarginal voluntary takeup when drawing code boundaries. Third, we show how welfare outcomes change as we vary the inattention parameter θ. This sensitivity exercise shows that standards are preferred to no policy for $\theta < 0.95$, that this welfare gap is quite large when θ is small, and that standards and no policy yield similar welfare outcomes for $\theta \geq 0.95$. The welfare difference between standards and a uniform subsidy varies little with respect to θ (ignoring again the fiscal cost of subsidies and attendant cost of financing the transfers). Finally, we consider the implications of potential correlation in risk misperception and wildfire hazard. In general, we find that a building code mandate outperforms a no-policy counterfactual irrespective of correlation between misperception and risk, and the corrective policies are particularly valuable when high-hazard homeowners are more likely to underestimate their true risk.

To summarize, realistically granular wildfire building codes achieve welfare outcomes that beat a no-policy counterfactual and are equal or close to those under highly-targeted subsidies, even without accounting for the cost of raising public funds. The reason this occurs is that many of the key margins of heterogeneity that determine the net social benefits of takeup are directly observable by the regulator—especially the occurrence probability of a disaster. Moreover, the spatially correlated nature of disaster risk means that building standards can be targeted to contiguous areas where the risk faced by most homes exceeds the break-even level.

6 Conclusion

Efficient investment in adaptation is essential in the face of rapidly accelerating disaster losses. Yet efficient take-up of protective technologies and behaviors is thought to be constrained by spatial externalities, insurance market failures, misperception of risk, and other frictions. The pressing question facing researchers and policymakers is how to best respond to these market barriers. One suite of policies focuses on increasing voluntary take-up through information or subsidies. Another option is to mandate certain investments in hazard areas.

These policies may differ substantially in their effects and their political acceptability.

This study contributes evidence on the effects and net economic benefits of a mandatory adaptation policy. We provide the first comprehensive empirical evaluation of California's strict wildfire building codes. The analysis uses a new dataset of property-level data on U.S. homes destroyed by wildfire that was created for this study. The new data combine post-fire damage assessment records collected from numerous local and state agencies with property assessment data for all homes, destroyed or surviving, that were within the perimeter of the fire. This combination has three important advantages: it collects and harmonizes previously disparate damage data; it contains a complete record of homes that survive as well as homes that are destroyed; and unlike data for floods and other losses, it is reported at the individual property level. Beyond this study, the new data will enable additional important research on disaster losses.

The empirical analysis in this study is bolstered by our ability to observe differences in building code regimes over time, across jurisdictions within California, and between California and other states. The empirical strategy isolates the effect of building code changes using a fixed effects design that compares outcomes for pre- and post-code homes on the same residential street. This approach narrows the comparison to homes experiencing essentially identical wildfire exposures.

The results show that compared to reliance on voluntary action alone, California's wildfire building codes reduced average structure loss risk during a wildfire by about 15 percentage points, or about a 40% reduction. They also reduced the risk to a close neighbor's home by about 2 percentage points, a 6% reduction. These striking results imply materially different levels of resilience in communities with and without such codes. Moreover, the spatial externalities provide a rationale for public policy intervention even if homeowners were fully informed and rational about wildfire risk.

Having documented these large resilience benefits, we then show how the empirical results can be embedded in an economic model that accounts for mitigation costs, spatial spillovers, and risk preferences. We use our results and other values from the literature to provide a back-of-the-envelope approximation of the minimum annual wildfire risk at which universal mitigation generates positive net benefits. In the most fire-prone areas of California, the calculation shows large net benefits of building codes for new homes. Given the high cost of fully retrofitting existing homes to modern standards, full retrofits do not pass a benefit-cost test in most areas. An important task for future research is to identify individual low-cost investments that can cost-effectively improve the resilience of existing homes in high hazard areas.

Finally, we use the model and data to compare the welfare effects of alternative policy approaches to encourage adaptation. The observability of wildfire risk and number of neighbors, along with spatial correlation of disaster risk within neighborhoods, mean that wildfire building codes targeted with a realistic degree of granularity compare favorably to idealized policy interventions such as a perfectly differentiated mitigation subsidy.

Other longer-run welfare effects resulting from broader mandates or subsidies outside of the scope of our model are likely salutary as well. Improved overall resiliency should result in wildfires growing more slowly within affected neighborhoods and increase the ability of fire management professionals to effectively target resources to areas of greatest need, both of which should reduce the overall social costs of wildfires. Mandated investment could also help mitigate existing insurance market failures: if wildfire-safe construction can be taken for granted due to regulation, the market may be better able to set prices that match risk (Boomhower 2023). In the context of particularly high levels of risk, "managed retreat" policies that either restrict future development or incentivize the relocation of current homes may be socially beneficial. We view the assessment of such policies as fruitful areas for future work, although this paper provides suggestive evidence that even areas of substantial wildfire risk can be efficiently occupied if structures are sufficiently resilient.

In summary, the data show that an adaptation mandate substantially improved resilience to wildfires and a cost-benefit exercise suggests that low take-up without standards is more likely driven by market failures than by fully-informed individual decisionmaking. These results are immediately applicable to policy debates in the U.S., Canada, Australia, the European Union, and other jurisdictions that are seeking to respond to escalating wildfire risk. More broadly, these findings should be of interest to policymakers and researchers confronting other hazards like floods, hurricanes, and heat waves where voluntary take-up of self-protective investments seems to be constrained by similar barriers. As climate change continues to increase disaster losses, improving our understanding of the role of public policy and market incentives in shaping adaptation is increasingly urgent.

7 Data Availability

Code replicating the tables and figures in this article can be found in Baylis and Boomhower (2025) in the Harvard Dataverse, https://doi.org/10.7910/DVN/A8MB7B.

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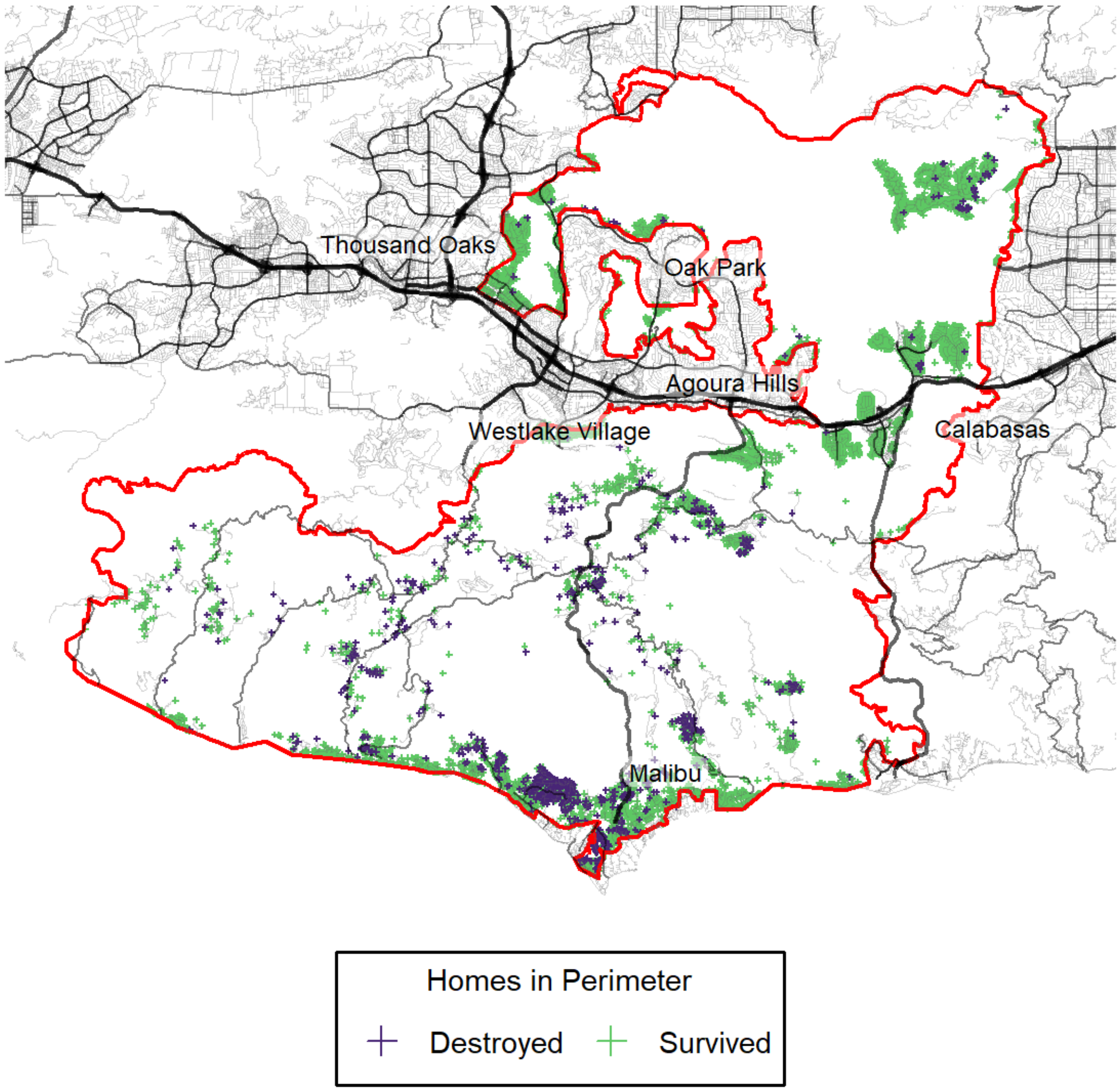
Figure 1: Building and Validating the Dataset

(a) Roof Locations and Damage Reports

(b) Distance Between Structures

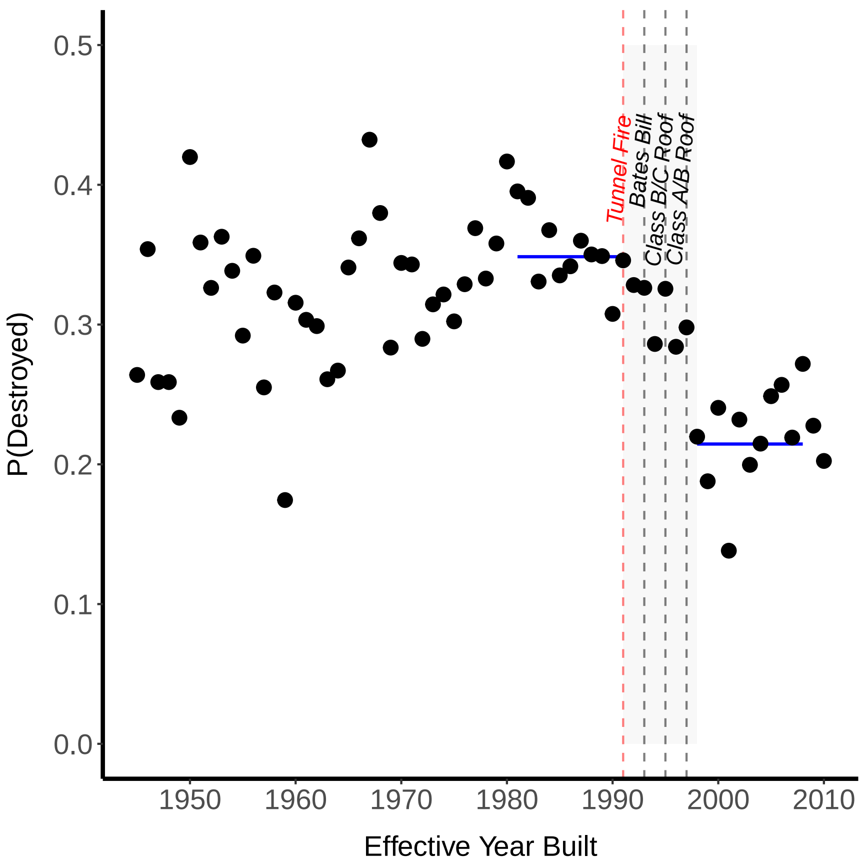
Notes: Maps showing home roof locations, damage indicators, and distance between structures. Panel (a): Homes affected by the Carr Fire (2018). Markers are geocoded structure locations. Circular markers are structures reported as destroyed in the damage inspection data; Square markers are all other homes in the data. The background image is aerial imagery before and after the Carr Fire from NearMap. Building shapes and parcel outlines are the building footprint data and assessor parcel boundary data used to identify structure locations (see text for details). Panel (b): Examples of calculated distances between structure walls. Images are pre-fire aerial imagery of homes affected by the Thomas Fire (2017) and Tubbs Fire (2017). Figure shows the wall-to-wall distance from the structure marked '0' to the other homes.

Figure 2: Structure-level Outcomes in the Woolsey Fire



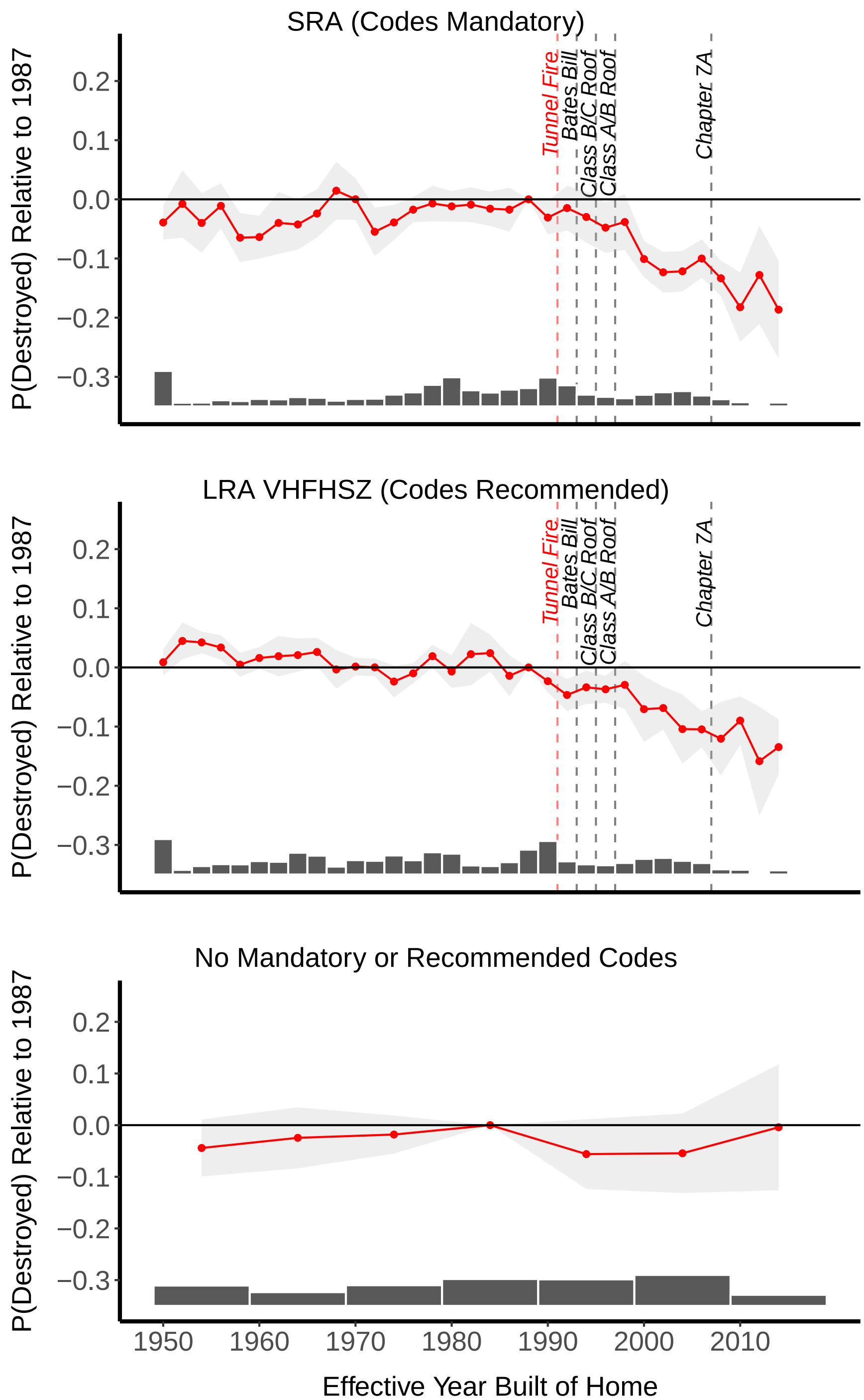
Notes: Map of single family homes in Los Angeles and Ventura Counties that were included within the Woolsey Fire perimeter in 2018. This map is an example of the merged inspection, assessor, and fire perimeter data for one fire in our dataset. Markers indicate the locations of all single family homes included in the assessment data that were located inside the final Woolsey Fire perimeter. Dark-shaded homes are reported destroyed in damage inspection data; light-shaded homes are all remaining homes in the assessment data. Blank areas within the fire perimeter indicate that single family homes were not present at the time of the fire; these are mostly mountainous state park and national recreation areas. Street map data are from Open Street Map.

Figure 3: Share Destroyed by Year Built in Mandatory Code Jurisdictions



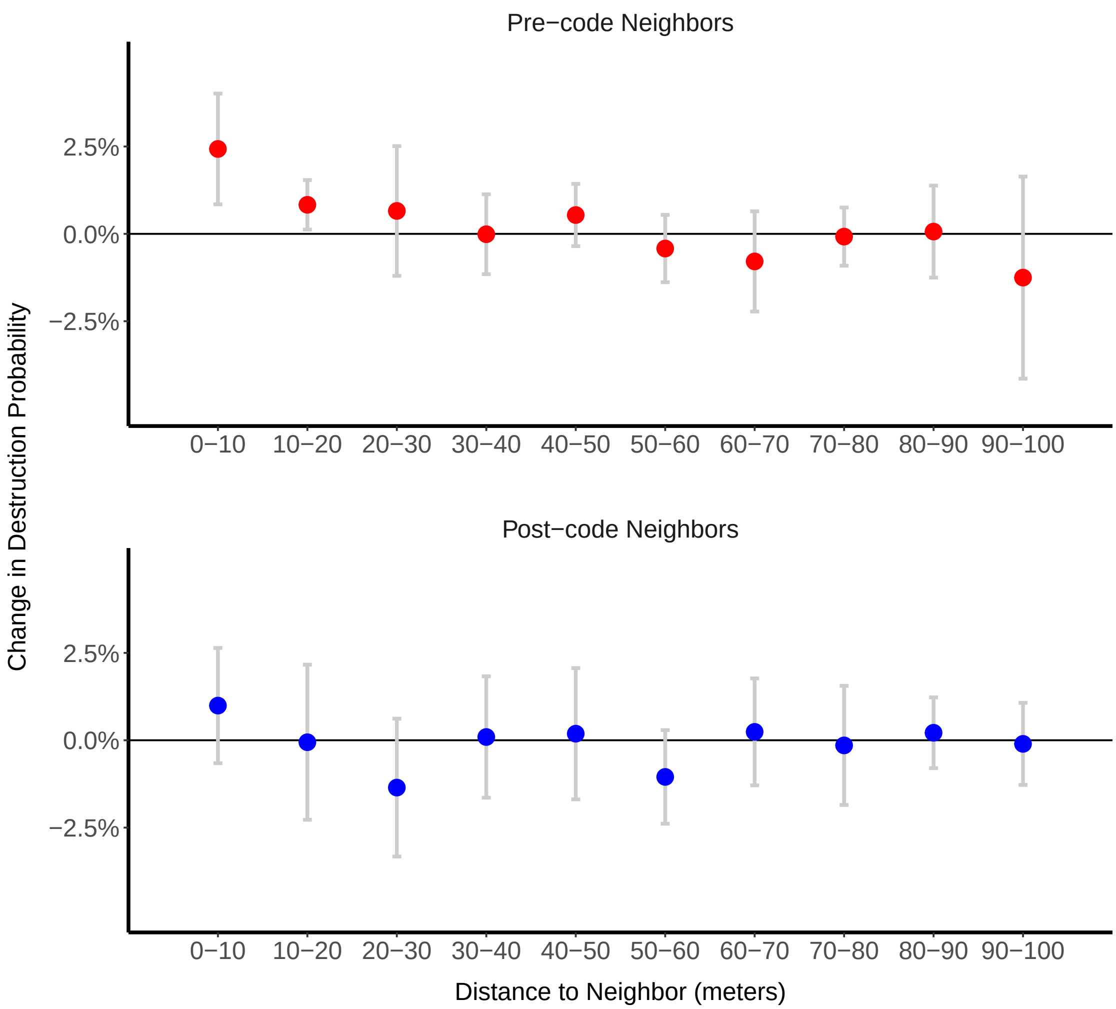
Notes: Figure shows the share of homes inside wildfire perimeters that were destroyed, according to the year that the home was built. The sample is limited to homes in State Responsibility Areas. The two horizontal lines show ten-year averages.

Figure 4: Estimated Vintage Effects by Building Code Jurisdiction



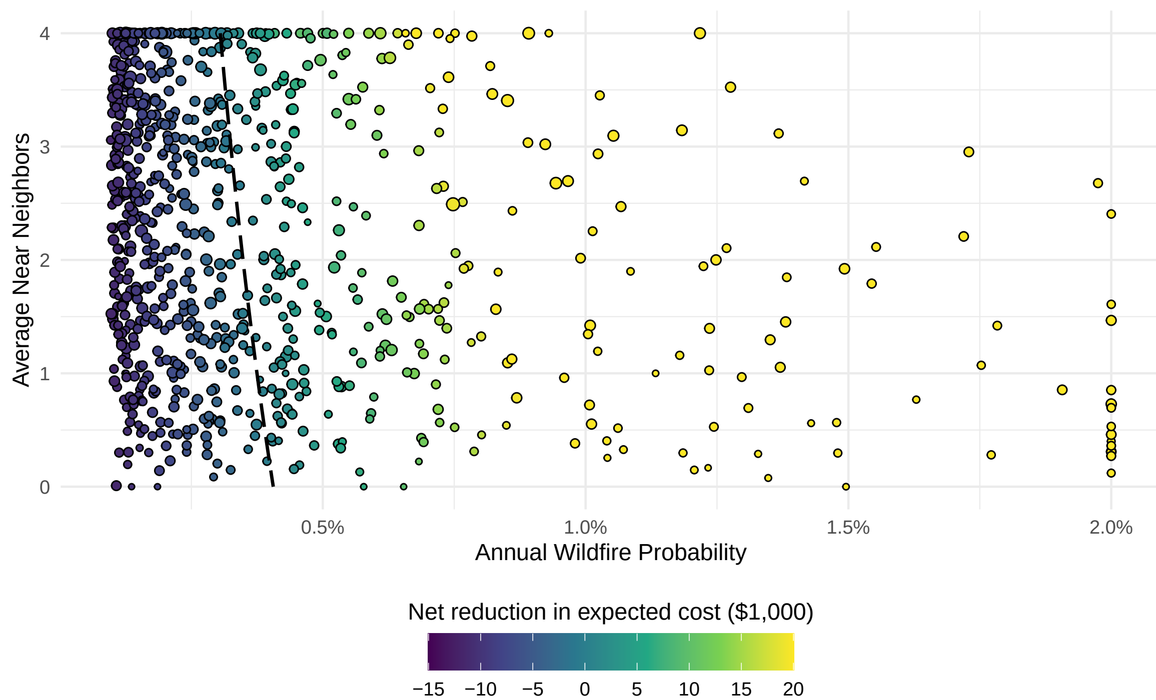
Notes: Figure plots point estimates and 95% confidence intervals from event study regressions of home destruction on year built, split by jurisdiction. The outcome variable is an indicator for Destroyed and the bin indicators group homes into either two- or ten-year bins of effective year built. Each regression includes sub-street by incident fixed effects and other controls described in the text. Top panel shows homes in the SRA and uses two-year bins. Middle panel shows homes in state-recommended LRA-VHFHSZs and uses two-year bins. Bottom panel shows homes in No-codes jurisdictions in AZ, CO, OR, and WA and uses ten-year bins. Standard errors are clustered by incident. The histogram below each panel shows the relative number of observations in each bin.

Figure 5: Building Code Neighbor Effects by Distance



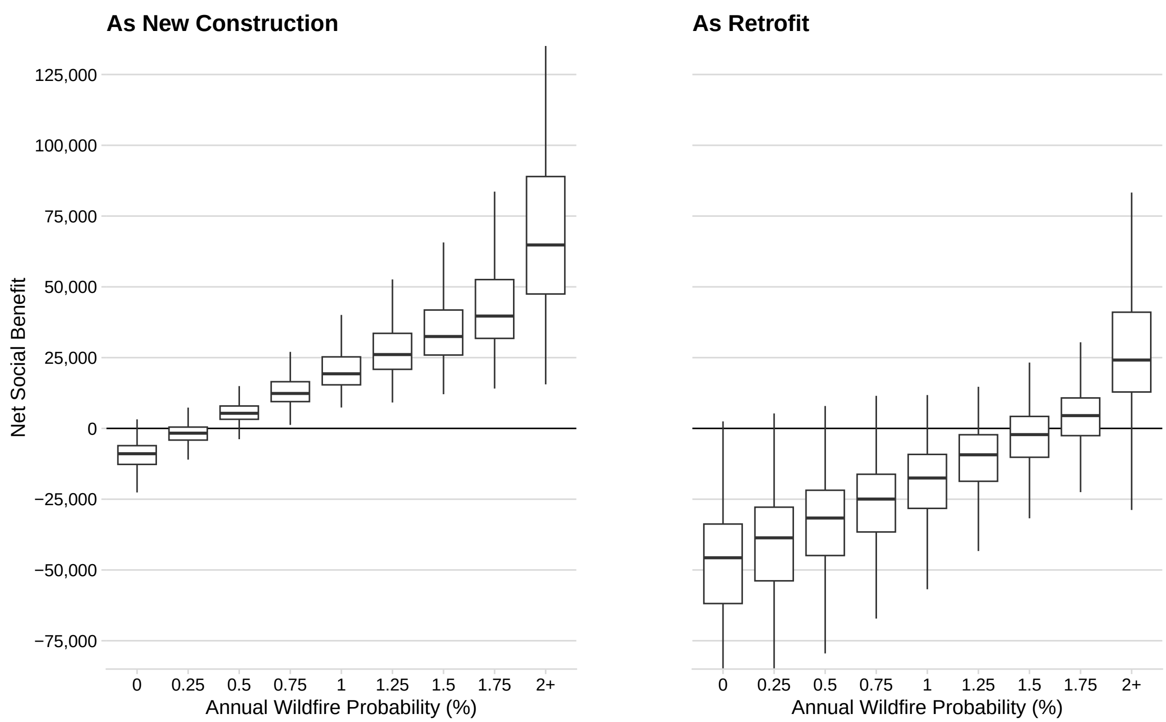
Notes: Figure shows coefficients and 95% confidence intervals from a single regression of an indicator for Destroyed on the presence of pre- and post-code neighbors at various distances. The top panel shows estimates for indicator variables for the presence of one or more neighbors built without wildfire building codes. The bottom panel shows estimates for indicator variables for the presence of one or more neighbors built after wildfire building codes. The regression also includes own year built (in four year bins), street by incident fixed effects, and topographic controls. Distance to neighboring home is wall-to-wall distance. See text for details.

Figure 6: Mitigation Cost-Effectiveness by Fire Hazard and Number of Neighbors



Notes: Figure plots census tract averages of the cost-effectiveness of mitigation by annual probability of a damaging wildfire and number of close neighbors for California homes in high wildfire hazard areas. Marker color indicates average net benefits in the census tract using the cost-effectiveness measure, which is a conservative lower bound on total net benefits. Markers represent zip-code averages of annual wildfire hazard from Scott et al. (2020) and represents a snapshot as of 2014. Number of neighbors is the number of homes within a 30-meter centroid to centroid distance. Marker size is proportional to number of homes in the census tract. The dashed line shows a threshold for zero net reduction in expected cost. See text for discussion and alternative scenarios.

Figure 7: Heterogeneity in Cost-effectiveness of Home Hardening



Notes: Figure shows box plots of net social benefits (cost-effectiveness) of mitigation for the all-California sample of 1.1 million homes facing some wildfire risk. Net social benefits are calculated using home-specific wildfire hazard, replacement costs, and mitigation costs (see text for details). Each box is homes with similar annual wildfire hazard. Labels on horizontal axis indicate left side of interval of wildfire probability, e.g., "0.25" indicates homes with wildfire hazard between 0.25% and 0.5%. Whiskers indicate 5th and 95th percentiles and boxes show median and interquartile range. Left panel uses central estimate of mitigation costs for newly constructed homes ($15,660) and right panel uses estimate of retrofit costs ($62,760), both from Headwaters Economics (2018), adjusted for the size of home.

Table 1: Building Code Effects on Own Destruction

	Incident FE	Street FE		
	(1)	(2)	(3)	(4)
1998–2007 × SRA	-0.126***	-0.088***	-0.089***	-0.091***
	(0.021)	(0.009)	(0.008)	(0.010)
2008–2016 × SRA	-0.158***	-0.133***	-0.131***	-0.139***
	(0.032)	(0.021)	(0.019)	(0.021)
1998–2007 × LRA-VHFHSZ	-0.067	-0.082***	-0.071***	-0.070**
	(0.042)	(0.028)	(0.023)	(0.027)
2008–2016 × LRA-VHFHSZ	-0.107*	-0.122***	-0.111***	-0.109***
	(0.060)	(0.035)	(0.026)	(0.029)
1998–2007 × No-codes	0.0005	-0.045	-0.038	-0.029
	(0.056)	(0.031)	(0.025)	(0.027)
2008–2016 × No-codes	-0.028	0.004	0.005	0.032
	(0.046)	(0.039)	(0.044)	(0.050)
Ground slope (degrees)	0.004***	0.005***	0.004***	0.004***
	(0.001)	(0.0007)	(0.0007)	(0.0006)
Fuel model indicators	✓	✓	✓	✓
Additional controls				✓
Incident FE	✓			
Street FE		✓		
Sub-street FE			✓	✓
Observations	45,093	45,093	45,093	36,176
R^2	0.39	0.62	0.66	0.67
Dependent variable mean	0.39	0.39	0.39	0.44

Notes: Table shows estimates and standard errors from regressions of home destruction on vintage by jurisdiction indicators. The outcome variable is an indicator for Destroyed. The included home vintage categories represent homes with effective year built from 1998–2007 and 2008–2016 and the excluded category is homes with effective year built before 1998. SRA (State Responsibility Areas) and LRA-VHFHSZ (Local Responsibility Areas in Very High Fire Hazard Severity Zones) are treatment groups where wildfire building codes applied after 1998, while No-codes is the comparison group of homes in other states without building codes. Incident fixed effects are dummies for each wildfire. Street fixed effects includes separate dummies for each street-by-incident. Sub-street fixed effects use separate dummies for each group of 25 adjacent homes on the same street-by-incident. Fuel model indicators are dummies for Anderson fire behavior fuel models. Additional controls include lot size, home square footage, number of bedrooms, elevation in meters, and wildfire hazard. Appendix Table A6 reports all estimated coefficients. Standard errors are clustered by incident.

Table 2: Building Code Neighbor Effects on Own Destruction

	(1)	(2)	(3)	(4)
Pre-code Neighbors	0.017**	0.018***		
	(0.007)	(0.006)		
Post-code Neighbors	0.006	0.002		
	(0.005)	(0.007)		
1 Pre-code Neighbor			0.019***	0.018***
			(0.007)	(0.005)
2+ Pre-code Neighbors			0.033**	0.037***
			(0.015)	(0.013)
1 Post-code Neighbor			0.012	0.006
			(0.008)	(0.011)
2+ Post-code Neighbors			0.005	0.0004
			(0.011)	(0.013)
Topography	✓	✓	✓	✓
Year built bins FE	✓	✓	✓	✓
Fuel model indicators FE	✓	✓	✓	✓
Sub-street FE	✓	✓	✓	✓
Distances	Walls	Centroids	Walls	Centroids
Observations	20,757	40,779	20,757	40,779
R^2	0.72	0.66	0.72	0.66
Dependent variable mean	0.47	0.39	0.47	0.39

Notes: Table shows estimates of the effect of having pre- or post-code nearby neighbors on own-home destruction. The outcome variable is Destroyed, an indicator for home destruction. The coefficients in columns (1) and (2) represent the effect of having one additional pre or post-code nearby neighbor, while the coefficients in columns (3) and (4) are the effect of having either one or two or more nearby neighbors. The "Distances" row indicates the method of distance calculation and the subset of homes used, where "Walls" distances calculate a nearby neighbor as any home with a wall-to-wall distance of 10 meters or less and are limited to the subset of homes for which we have accurate home footprints. "Centroids" distances calculate nearby neighbors as any homes with adjusted centroid-to-centroid distances of 10 meters or less (see Footnote 20 for description of adjustment) and use the larger set of homes for which we have parcel boundaries and centroids. Both samples include only homes in code-required jurisdictions in California. Standard errors are clustered by incident.

Table 3: Break-even Hazard under Risk Aversion and Alternative Costs

	Insured %	100	67		33	
		CRRA	$\gamma = 2$	$\gamma = 5$	$\gamma = 2$	$\gamma = 5$
Cost Estimate ($)	Source	Break-even Wildfire Hazard (%)				
Panel A. New Home						
0	HE-Low	0.00	0.00	0.00	0.00	0.00
7,634	NAHB-Low	0.16	0.16	0.14	0.13	0.08
15,660	HE	0.34	0.32	0.29	0.27	0.18
28,553	NAHB-High	0.61	0.59	0.54	0.50	0.35
Panel B. Retrofit						
62,760	HE	1.35	1.30	1.24	1.18	0.98

Notes: Table shows estimated minimum annual wildfire probability for which building standards yield positive net benefits under various assumptions about cost, share of losses insured, and risk aversion. Probabilities are reported as percentages (e.g., 0.33% per year). For partial insurance scenarios, γ is the coefficient of relative risk aversion. Calculations assume 2.5 near neighbors. See text for details of these calculations. Source HE represents Headwaters Economics (2018) and NAHB represents Home Innovation Research Labs (2020).

Table 4: Welfare Under Various Adaptation Policies

Policy	Social Net Benefit ($M)	Private Net Benefit ($M)	External Benefit ($M)	Total Adoption Rate	Inefficient Adoption Rate	Fiscal Cost ($M)
Panel A. Benchmarks						
No policy	3,872	3,546	326	10.2	0.0	0
Perfect standard	5,106	4,454	652	29.2	0.0	0
Perfect subsidy	5,106	4,454	652	29.2	0.0	886
Panel B. Building Codes						
Hazard only	5,025	4,393	632	27.9	1.8	0
County	2,628	2,044	584	37.9	19.7	0
Census block	4,849	4,219	629	28.9	3.5	0
Street	4,898	4,267	631	28.7	3.1	0
Panel C. Subsidies						
Uniform	4,704	4,153	550	27.7	4.9	1,508
Per square foot	5,097	4,460	638	29.1	0.8	2,211

Notes: Table shows total social net benefits from investment in wildfire-resistant construction under various policies. Sample includes approximately 1.1 million homes in areas of moderate to very high fire risk throughout California. Each row is a different adaptation policy described in Section 5, and each column is a welfare metric. Social Net Benefit is the total net benefit under the given policy, Private Net Benefit is the total net benefit of mitigation for each person who mitigates, and External Benefit is the total benefits to neighbors of homes that mitigate. Total Adoption Rate is the percent of all households who mitigate under the policy, Inefficient Adoption Rate is the percent of all households who mitigate in spite of having negative social net benefits under the policy, and Fiscal Cost is the total amount of subsidy paid from the regulator to mitigating homeowners.

Online Appendix to "Mandated vs. Voluntary Adaptation to Natural Disasters: The Case of U.S. Wildfires"

Patrick Baylis and Judson Boomhower

A Additional Data and Supporting Information

This appendix section includes additional exhibits describing the main dataset of homes used in the paper and supporting information on how the dataset was collected and checked for errors. It also describes how we estimate the degree of additional takeup that would have occurred in the absence of codes, which jurisdictions outside of California also have comparable wildfire building codes, the full set of wildfire perimeters included in the dataset, provides full sets of coefficient estimates for the main results, and documents balance checks of covariates across vintages for homes in the sample.

A.1 Descriptive Statistics

Table A1 reports descriptive statistics for the analysis dataset used in the paper. This dataset reflects only those homes used in the preferred specification, i.e., California homes within wildfire perimeters and located in areas where codes became required (the SRA or LRA-VHFHSZs) and comparison homes drawn from wildfire perimeters outside of California where to-code construction is not required by building codes.

Table A1: Descriptive Statistics

	Mean	SD	P5	Median	P95
Panel A. Variables (Homes = 45,093)					
Destroyed	0.394	0.489	0	0	1
Year built	1979	19.5	1946	1981	2006
Ground slope (degrees)	7.36	5.74	1	6	19
Lot size (acres)	4.58	25.7	0.16	0.734	18.4
Square feet (thousands)	2.17	1.3	0.812	1.85	4.57
Bedrooms	3.04	1.09	1	3	5
Elevation (meters)	455	395	73	378	1165
Wildfire hazard (%)	0.498	0.708	0.0145	0.264	2.11
Panel B. Homes by Jurisdiction and Vintage					
Jurisdiction	Vintage	N			
SRA	Before 1998	20,553			
SRA	1998–2007	3,817			
SRA	2008–2016	828			
LRA-VHFHSZ	Before 1998	14,087			
LRA-VHFHSZ	1998–2007	2,515			
LRA-VHFHSZ	2008–2016	474			
No-codes	Before 1998	1,974			
No-codes	1998–2007	601			
No-codes	2008–2016	244			

Notes: Table of descriptive statistics for the main sample. Each observation is a single home inside one of the wildfire perimeters we study. Panel A reports variable distributions for key variables, and Panel B counts the number of homes in each jurisdiction and vintage. Destroyed is an indicator for whether the home burned in the incident. Year built is the home's year of construction or most recent major renovation. Ground slope is the angle on which the home sits, measured from LANDFIRE. Lot size is the size of the parcel in acres. Home square footage is the size of the building in thousands of square feet. Bedrooms is the number of bedrooms. Total assessed value is the total assessed value in thousands of dollars. Elevation is the elevation of the parcel of the parcel in meters, measured from LANDFIRE. Wildfire hazard is the likelihood of a moderate or larger wildfire occurring in that location (Scott et al. 2020). Jurisdiction is SRA for homes in California's State Responsibility Area, LRA-VHFHSZ for homes in one of California's Local Responsibility Areas rated as having Very High Fire Hazard Severity, and No-codes for homes outside of California and in areas without a wildfire building code in place at the time of the wildfire.

A.2 Comparison of Samples

To understand the degree to which the resilience estimates in the main paper could be extrapolated to other locations, Table A2 compares homes in the main sample to the larger set of at-risk homes in California.

Homes in the "Main" sample are those in the estimation sample used in Section 4, i.e., homes that were inside the wildfire perimeter of one of the incidents we study. "At-Risk California

Homes" are the 1.1 million California homes we use the cost-effectiveness simulations in Section 5; these are homes in tracts with average wildfire hazard greater than 0.1%. These homes tend be similar along most dimensions, though average parcel slope, lot size, and wildfire hazard are slightly lower in comparison to the main sample. Further restricting to only the 0.8 million homes in areas where codes are required (SRA and LRA-VHFHSZ jurisdictions) within this sample increases the comparability of the samples. Finally, restricting even further to the more than the 0.5 million homes in SRA jurisdictions yields the highest degree of comparability with the main sample.

From this we conclude that the set of homes we study, i.e., those that were actually included in a wildfire perimeter, are reasonably similar to other homes in California facing high wildfire risk, and in particular to other homes located in jurisdictions where codes are required. The resilience estimates we document are likely to be applicable to the broader set of at-risk California homes, and particularly so to the subset of those homes in code-required jurisdictions.

Table A2: Comparison of Homes Across Samples

	Main	At-Risk California Homes		
		All	Code areas	SRA
Year built	1979 (19)	1983 (22)	1981 (22)	1982 (22)
Ground slope (degrees)	7.4 (5.7)	4.9 (4.8)	5.9 (5)	6.1 (5.1)
Lot size (acres)	4.6 (26)	3 (170)	3.6 (199)	5.1 (239)
Square feet (thousands)	2.2 (1.3)	2.2 (1.4)	2.2 (1.5)	2.1 (1.6)
Bedrooms	3 (1.1)	3.3 (1)	3.2 (1.1)	3 (1.1)
Elevation (meters)	455 (395)	514 (461)	573 (496)	661 (546)
Wildfire hazard (%)	0.5 (0.71)	0.38 (0.52)	0.45 (0.56)	0.53 (0.62)

Notes: Table compares structure, lot, and topographic characteristics between single family homes in wildfire perimeters versus other samples of homes within California. Table cells are means and standard deviations (in parentheses) for each variable in each sample. "Main" (45,093 homes) represents the set of homes used in the home resilience estimations in Section 4. Within the "At-Risk California Homes", "All" (1,071,033 homes) is the set of homes used in the policy simulations in Section 5 and is all single family homes in California in census tracts with average wildfire risk of 0.1% or greater. "Code areas" (762,364 homes) is the subset of at-risk homes that are in SRA or LRA-VHFHSZ areas. "SRA" (524,408 homes) is the subset of code area homes in the SRA.

A.3 Additional Summary Data

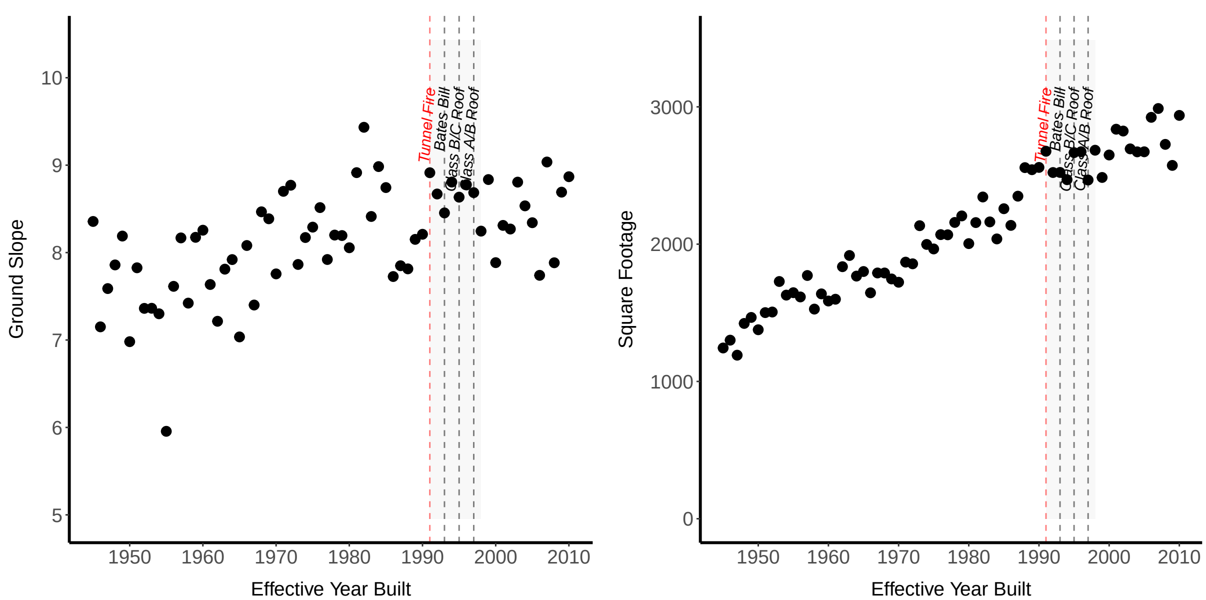
The top panel of Figure A1 shows the average probability of destruction by effective year built of home, while the bottom panel is a histogram of the number of homes, also by effective year built. Figure A2 reports averages of ground slope and home square feet for to-code homes by effective year built for homes in code-required jurisdictions in California. The figure illustrates the timing of relevant building code legislation to show that while home characteristics continued to change over time, there are no discontinuous jumps in these characteristics when the building codes took effect.

Figure A1: Year Built and Probability of Destruction – All Fires

Notes: Figure shows average probability of destruction by effective year built for all homes in the main sample, which is all single-family homes inside of observed wildfire perimeters. Red markers are share of homes of each vintage that are reported as destroyed in damage data.

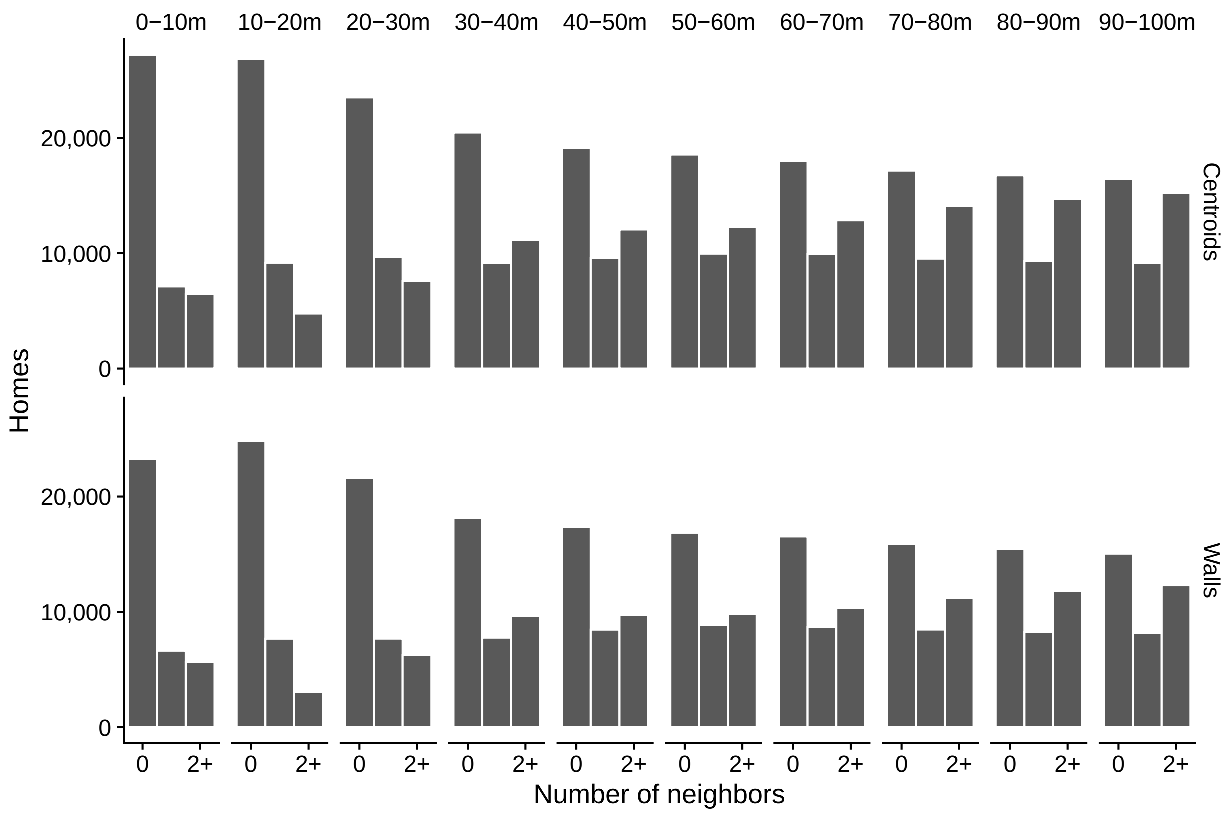
Figure A2: Other Characteristics by Year Built in Mandatory Code Jurisdictions

(a) Ground slope (degrees) (b) Square feet



Notes: Figure shows average levels of ground slope and home square footage by effective year built for homes in code-required areas in California. Panel (a) is ground slope at the home site from LANDFIRE. Panel (b) is building square footage measured from county assessor data.

Figure A3: Number of Neighbors by Distance



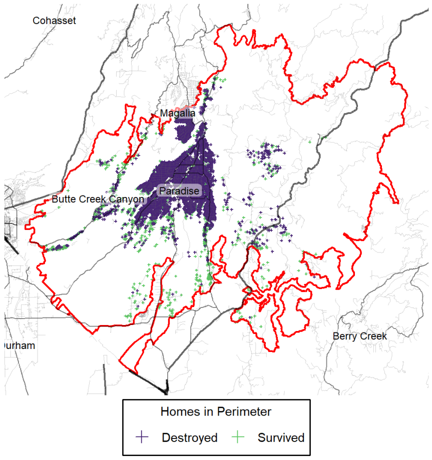
Notes: Figure shows the distribution of counts of neighbors at various distances. Each three-bar histogram counts the number of homes within the given distance ring noted on top (e.g., 10–20m) that have 0, 1, or 2+ neighbors. The top panel counts neighbors using centroid-to-centroid distances, and the bottom panel counts neighbors using wall-to-wall distances.

Figure A3 shows the number of neighbors by distance for homes in the sample. The top panel documents neighbors as counted by centroid-to-centroid distances, and the bottom as counted by wall-to-wall distances. Each three-bar histogram counts the number of homes with 0, 1, and 2+ homes within 0-10m, 10-20m, and so on.

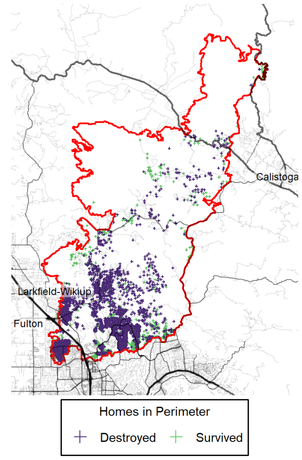
A.4 Additional Maps

This section includes additional maps visualizing the data sources we use. Figure A4 shows incident-wide maps of home outcomes for selected fires. Figure A5 reproduces 2019 maps of state, local, and federal responsibility areas in California, as well as Fire Hazard Severity Zones.

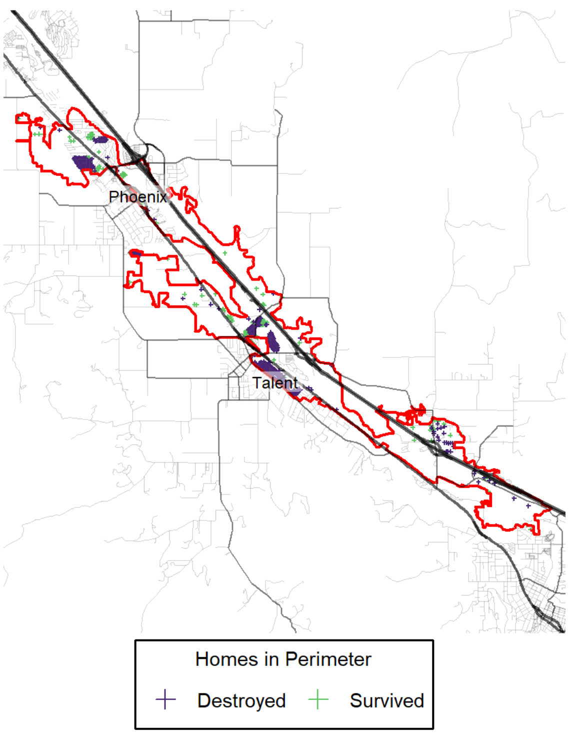
Figure A4: Additional Incident Maps



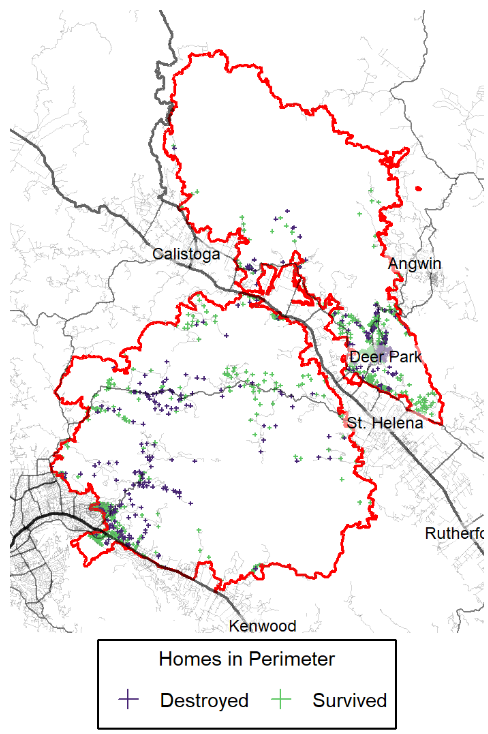
(a) Camp Fire (CA, 2018)



(b) Tubbs Fire (CA, 2017)



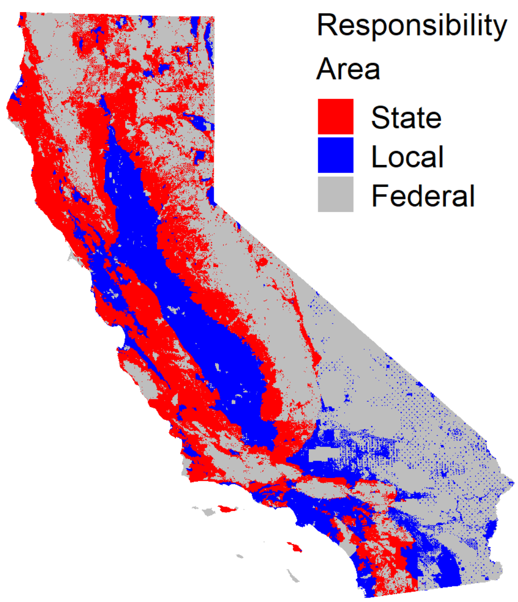
(c) Almeda-Obenchain (OR, 2020)



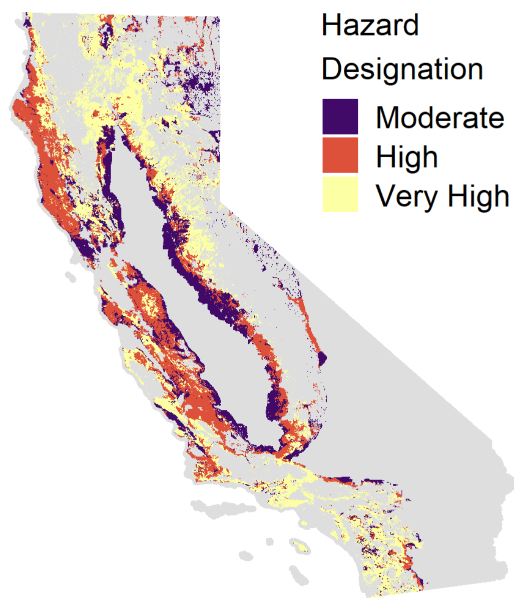
(d) Glass Fire (CA, 2020)

Notes: Maps of single family homes inside wildfire perimeters for four wildfires in the dataset. These maps are examples of the merged inspection, assessor, and fire perimeter data for the fires in our dataset. Markers indicate the locations of all single family homes included in the assessment data that were located inside the final wildfire perimeters (perimeters in red). Purple homes are reported destroyed in damage inspection data and green homes are all remaining homes in the assessment data. Blank areas within the fire perimeter indicate that single family homes were not present at the time of the fire; these are mostly mountainous state park and national recreation areas. Street map data are from Open Street Map. Best viewed in color.

Figure A5: Responsibility Areas and Fire Hazard Severity Zones in 2019



(a) Responsibility Areas



(b) Fire Hazard Severity Zones

Notes: Maps of California Responsibility Areas and Fire Hazard Severity zones as of 2019. Responsibility areas indicate jurisdictional responsibilities for wildfire management. The areas where the state is responsible is the SRA, areas where local municipalities are responsible are LRAs, and areas where the federal government is responsible is the FRA. Since the FRA is mostly public lands, there are very few homes in this area. Fire Hazard Severity Zones are designed by CAL FIRE and indicate wildfire hazard across the state.

A.5 Geocoding and Ground-truthing Homes and Damage Data

This section provides additional detail on how we geocode the location of homes in the main sample.

A.5.1 Rules for Choosing Street Address-based Locations

Section 2.2.1 explains how building footprints and parcel boundary GIS data are used to assign structure rooftop locations for the majority of homes in the data (87%). For the remaining 13%, street-address based geocodes from the ESRI premium geolocator are used. We use ESRI geocodes whenever any of these conditions exist for a given property:

1. Accurate parcel GIS boundary data are not available for the county.
2. The merge on assessor parcel number (APN) between all homes in a given incident and the parcel boundary data yields a merge rate below 95% (an indication of inconsistently-formatted APNs). For such incidents, all homes are located using ESRI locations.
3. A single parcel polygon contains 4 or more building footprint shapes. In our testing, this condition often indicates formerly-large parcels that were subdivided and developed subsequent to the date of the parcel boundary data and prior to the wildfire.

For the address-based geocoding, we used ESRI ArcGIS StreetMap Premium[44] with the "USA" locator. Geocoding was based on street address (PropertyFullStreetAddress from ZTrax), zip code (PropertyZip from ZTrax), county name, and state. Geocoding was performed on May 26, 2022 using the current version of the StreetMap Premium locator on that date.

A.5.2 Ground Truthing with Aerial Imagery

To ensure the quality of the location and damage assessment data, we evaluated a random sample of homes by hand using high resolution aerial images from NearMap as ground truth (as in Figure 1). For each wildfire, we randomly chose 1 home (if any existed) with more than 10 neighbors within 200 meters and 1 home (if any existed) with fewer than 10 neighbors within 200 meters. We downloaded the NearMap image tiles containing these homes. Each downloaded image tile contains potentially other homes as well. We evaluated all homes in each image (stopping at 20 homes if there were more than 20 in one image).

Rooftop Locations. We assessed rooftop location accuracy using only *pre-fire* imagery (incidents with no available pre-fire imagery are excluded from the count). Rooftop locations were considered accurate if the assigned location was on top of the structure roof visible in the NearMap image. Even small deviations from the parcel rooftop were coded as errors, due to the need for accuracy in the neighbor analysis. Because we suspected that our footprint-based method would yield more accurate locations than street address-based geocoding, and that more rural areas would have poorer accuracy, we assessed accuracy separately for in-

44. https://doc.arcgis.com/en/streetmap-premium/get-started/overview.htm.

cidents geocoded with either method; and for areas with more or less than 10 neighbors located within 200 meters of a home.

Damage Reports. We assessed damage report accuracy using imagery taken within 365 days after the wildfire. If no imagery in this time window was available, the incident is excluded from the count. Damage reports were considered accurate if the reported structure outcome (Destroyed/Survived) matched the state of the structure visible in the NearMap imagery.

Table A3 reports on the error rates for both rooftop locations and damage reports.

Table A3: Accuracy of Rooftop Locations and Damage Reports

	Error (%)	Homes	Images
Panel A. Location Error Rates			
Footprint Geocodes, 10+ within 200 m	0.8	252	18
Footprint Geocodes, 0–9 within 200 m	6.9	72	19
Address Geocodes, 10+ within 200 m	27.5	51	3
Address Geocodes, 0–9 within 200 m	12.5	16	5
Panel B. Damage Error Rate			
All	1.1	556	86

Notes: Table of error rates among geocoding approaches. Error is the percent of homes with incorrect rooftop locations (i.e., the geocoding location does not sit on the rooftop of the comes) or damage outcomes (i.e., a home is marked as destroyed when it is not, or vice-versa), as determined by manual examination of NearMap imagery.

Location error rates are lowest for geocoding done with building footprints for buildings with at least 10 homes within 200 meters. They're higher for buildings in sparser areas, and for geocoding done with street address rather than footprint locations. Given the strict requirement for rooftop location accuracy, even the relatively high error rates for address geocoding are reasonable. Still, we focus on footprint-based geocoding in dense areas when estimating spillover effects from neighbors to ensure location precision.

A.6 Voluntary Mitigation Takeup Without Building Codes

Our calculations in Section 5 require an estimate of the share of homes that would have been voluntarily built to these codes in the absence of a legal requirement to do so. Detailed data on wildfire preparedness characteristics are not tracked or measured for most of the at-risk homes in our data, meaning we do not have the data to assess this voluntary takeup rate for the full sample. Available tools to remotely assess these characteristics (e.g., through aerial or satellite imagery) are not yet capable of accurately determining the fire readiness of hard-to-observe building components like siding, eaves, and vents (this may change in coming years, as this type of remote assessment is a major area of current investment).

To calibrate the average rate of voluntary take-up in the absence of wildfire building codes, we conducted a comprehensive review of the survey literature on wildfire home hardening in the United States. We identified 20 studies that report numeric takeup rates for home hardening investments in jurisdictions with no wildfire building codes. Appendix Table A4 summarizes the findings of these studies.

For our benchmark assumption about the overall average rate of adoption in the absence of building codes, we use survey data from Champ et al. (2020), which synthesizes datasets from several of the studies we survey. Based on professional risk assessments of 1,474 homes in wildfire hazard areas in western Colorado, that study finds that 40% of homes use building materials that would comply with California's wildfire building codes in at least two of these three areas: roof, exterior siding, and deck.

Table A4: Non-CA Fire Readiness

Citation	Location	Homes	Siding	Vents	Deck	Veg.	Topog.
Brenkert-Smith, Champ, and Flores (2012)	Boulder and Larimer Counties, CO	747	19%	24%			
Champ and Brenkert-Smith (2016)	Boulder and Larimer Counties, CO	442	22%	33%			
J. R. Meldrum et al. (2015)	Ouray County, CO	256	35%		2%	28%	9%
Schulte and Miller (2010)	Clear Creek County, CO	195	23%				
Stasiewicz and Paveglio (2022)	Oreille County, WA	770	31%				
Olsen et al. (2017)	Central Oregon	284	55%				
J. Meldrum et al. (2019a)	Archuleta County, CO	209	28%		17%	56%	39%
Brenkert-Smith et al. (2022)	Grand County, CO (Kremmling)	70	18%		16%	31%	19%
Brenkert-Smith et al. (2022)	Grand County, CO (Hot Sulphur Springs)	57	35%		39%	18%	21%
Brenkert-Smith et al. (2022)	Grand County, CO (Grand Lake)	454	17%		4%	26%	4%
Brenkert-Smith et al. (2022)	Grand County, CO (Grand)	292	13%		5%	37%	38%
Brenkert-Smith et al. (2022)	Grand County, CO (East Grand)	289	28%		3%	26%	17%
Brenkert-Smith et al. (2019)	La Plata County, CO	913	20%		16%	6%	50%
J. Meldrum et al. (2019b)	Montezuma County, CO	481	65%		43%	57%	27%
Brenkert-Smith et al. (2020)	Methow, WA	155	3%		8%	23%	30%
Brenkert-Smith et al. (2020)	Wenatchee Heights, WA	181	13%		14%	52%	50%
Brenkert-Smith et al. (2020)	Squilchuck Valley, WA	201	13%		12%	57%	62%
Brenkert-Smith et al. (2020)	Forest Ridge, WA	68	25%		18%	41%	99%
Goolsby et al. (2022)	Teton County, WY	725	34%		5%	38%	28%
J. Meldrum et al. (2022)	Grant County, CO	352	16%		6%	4%	23%

Notes: Table summarizes fire readiness characteristics reported by studies that surveyed homes outside of California and in areas where wildfire building codes were not in place at the time of the survey. Siding is the percentage of surveyed homes with wildfire-resistant siding. Vents is the percentage of surveyed homes with screened vents. Deck is the percentage of homes with non-flammable decks. Veg. indicates having heavy vegetation in the neighborhood of the home. Topog. is the percentage of homes within 50 feet of dangerous topography.

A.7 Wildfire Building Codes Outside of California

As we describe in the main text, outside of California and Utah there are no fully implemented statewide wildfire building standards. However, a handful of counties and cities in other states have enacted their own local codes. We consulted several sources to ensure that the non-California homes in the dataset did not face similar building codes to those in place in California.

Specifically, we examined multiple sources of information to determine the presence or absence of codes. We first checked IBHS Wildfire Codes and Standards 2019 reference guide, which gives information on which state and county adoptions of wildfire codes. Any homes counties that are listed in that guide as including wildfire building codes are excluded from our regression sample. We also examined each county's building codes reference website and documents to identify whether they included wildfire specific building standards.

This processed resulted in identifying two Colorado counties with homes in our damaged homes dataset – El Paso and Larimer – that had wildfire building codes in place prior to wildfire incidents for which we had gathered data. We therefore excluded the High Park (2012), Waldo Canyon (2012), Black Forest (2013), MM 117 (2018), and Cameron Peak (2020) wildfires from our data.

A.8 Full List of Wildfires in the Dataset

Table A5 reports the full list of wildfires in the main dataset. For each fire, we give the state, year, number of single family homes destroyed, exposed homes (homes inside the wildfire perimeter), and the share of exposed homes that were destroyed. These counts differ from reported counts of "structures lost" from incident reports because we focus on single family homes and do not include outbuildings (sheds, detached garages, and other miscellaneous structures).

Table A5: Full List of Fires and Single Family Home Counts

	State	Year	Destroyed	Exposed	Share Destroyed
California					
CZU Lightning Cmplx	CA	2020	419	1,279	0.33
North Complex	CA	2020	382	575	0.66
LNU Lightning Cmplx	CA	2020	342	997	0.34
Glass	CA	2020	249	774	0.32
Creek	CA	2020	185	686	0.27
Bobcat	CA	2020	38	187	0.20
BEU Lightning Cmplx	CA	2020	36	125	0.29
Slater	CA	2020	32	55	0.58
Zogg	CA	2020	19	53	0.36
Lake	CA	2020	7	28	0.25
Laura 2	CA	2020	6	12	0.50
Valley	CA	2020	4	72	0.06
Willow	CA	2020	3	11	0.27
Bond	CA	2020	3	94	0.03
Stagecoach	CA	2020	2	10	0.20
Sheep	CA	2020	2	56	0.04

Table A5: Full List of Fires and Single Family Home Counts *(continued)*

	State	Year	Destroyed	Exposed	Share Destroyed
Jones	CA	2020	2	13	0.15
SCU Lightning Cmplx	CA	2020	2	28	0.07
Gold	CA	2020	1	6	0.17
Quail	CA	2020	1	25	0.04
Blue Ridge	CA	2020	1	133	0.01
SQF Complex	CA	2020	1	2	0.50
August Complex	CA	2020	0	4	0.00
Branch	CA	2020	0	2	0.00
Pond	CA	2020	0	1	0.00
Kincade	CA	2019	28	100	0.28
Tick	CA	2019	16	605	0.03
Getty	CA	2019	8	128	0.06
Saddleridge	CA	2019	3	182	0.02
Mountain	CA	2019	2	8	0.25
Camp	CA	2018	8,141	10,118	0.80
Carr	CA	2018	658	1,496	0.44
Woolsey	CA	2018	648	6,648	0.10
Ranch	CA	2018	28	166	0.17
West	CA	2018	21	237	0.09
Klamathon	CA	2018	19	49	0.39
Holiday	CA	2018	9	32	0.28
Steele	CA	2018	8	15	0.53
Pawnee	CA	2018	6	22	0.27
Meyers	CA	2018	3	4	0.75
Cranston	CA	2018	3	52	0.06
Delta	CA	2018	2	18	0.11
Holy	CA	2018	1	142	0.01
Carder	CA	2018	1	1	1.00
Marsh	CA	2018	1	5	0.20
Silver	CA	2018	1	2	0.50
Creek	CA	2018	0	1	0.00
Tubbs Fire	CA	2017	2,066	2,667	0.77
Thomas	CA	2017	360	1,841	0.20
Nuns	CA	2017	333	1,247	0.27
Atlas	CA	2017	225	639	0.35
Cascade	CA	2017	58	213	0.27
Creek	CA	2017	25	593	0.04
Laporte	CA	2017	21	76	0.28
Lilac	CA	2017	20	292	0.07
Sulphur	CA	2017	17	29	0.59
Helena	CA	2017	13	26	0.50
Canyon 2	CA	2017	9	194	0.05
Ponderosa	CA	2017	8	14	0.57
Wall	CA	2017	6	35	0.17
Skirball	CA	2017	5	76	0.07
Detwiler	CA	2017	5	56	0.09
Estate	CA	2017	2	4	0.50
Laverne	CA	2017	2	9	0.22
Hill	CA	2017	2	8	0.25
Railroad	CA	2017	2	6	0.33

Table A5: Full List of Fires and Single Family Home Counts *(continued)*

	State	Year	Destroyed	Exposed	Share Destroyed
Mission	CA	2017	1	13	0.08
Cherokee	CA	2017	0	2	0.00
Canyon	CA	2017	0	24	0.00
Alamo	CA	2017	0	5	0.00
Stone	CA	2017	0	1	0.00
Erskine	CA	2016	46	515	0.09
Clayton	CA	2016	34	73	0.47
Soberanes	CA	2016	11	38	0.29
Grade	CA	2016	4	11	0.36
Goose	CA	2016	1	5	0.20
Chimney	CA	2016	0	8	0.00
Valley	CA	2015	763	1,538	0.50
Butte	CA	2015	144	407	0.35
Round	CA	2015	25	91	0.27
Rocky	CA	2015	9	40	0.22
Tassajara	CA	2015	2	7	0.29
Cocos	CA	2014	30	155	0.19
Clover	CA	2013	5	11	0.45
Silver	CA	2013	4	66	0.06
Shockey	CA	2012	5	9	0.56
Ponderosa	CA	2012	2	4	0.50
49er	CA	2009	41	73	0.56
Humboldt	CA	2008	47	174	0.27
Trabing	CA	2008	0	1	0.00
Witch	CA	2007	487	4,828	0.10
Grass Valley	CA	2007	150	410	0.37
Paradise2003	CA	2003	27	481	0.06
Other States					
Goodwin	AZ	2017	2	6	0.33
Yarnell	AZ	2013	79	212	0.37
EastTroublesome	CO	2020	270	569	0.47
Almeda-Obenchain	OR	2020	414	598	0.69
HolidayFarm	OR	2020	291	503	0.58
BeachieCreek-Santiam	OR	2020	208	340	0.61
EchoMountainComplex	OR	2020	121	251	0.48
ColdSprings	WA	2020	10	56	0.18
OkanoganComplex	WA	2015	6	100	0.06
CarltonComplex	WA	2014	10	81	0.12
Eagle Road Fire	WA	2014	2	103	0.02

A.9 All Coefficient Estimates

Table A6 is a version of Table 1 that reports all estimated coefficients for each model. These include ground slope, fuel model, lot size, home square footage, number of bedrooms, elevation in meters, and location wildfire hazard. Of these, increases in ground slope and elevation correlate with increased probability of destruction, and homes in some grassy areas and all timber areas face higher probabilities of destruction relative to homes elsewhere (the omitted category is homes in locations where the fuel model is rated "unburnable"). Notes omitted for brevity.

Table A6: Building Code Effects on Own Destruction (all coefficients)

	Incident FE	Street FE		
	(1)	(2)	(3)	(4)
1998–2007 × SRA	-0.126***	-0.088***	-0.089***	-0.091***
	(0.021)	(0.009)	(0.008)	(0.010)
2008–2016 × SRA	-0.158***	-0.133***	-0.131***	-0.139***
	(0.032)	(0.021)	(0.019)	(0.021)
1998–2007 × LRA-VHFHSZ	-0.067	-0.082***	-0.071***	-0.070**
	(0.042)	(0.028)	(0.023)	(0.027)
2008–2016 × LRA-VHFHSZ	-0.107*	-0.122***	-0.111***	-0.109***
	(0.060)	(0.035)	(0.026)	(0.029)
1998–2007 × No-codes	0.0005	-0.045	-0.038	-0.029
	(0.056)	(0.031)	(0.025)	(0.027)
2008–2016 × No-codes	-0.028	0.004	0.005	0.032
	(0.046)	(0.039)	(0.044)	(0.050)
Ground slope (degrees)	0.004***	0.005***	0.004***	0.004***
	(0.001)	(0.0007)	(0.0007)	(0.0006)
Fuel model indicators = Grass2	0.051**	0.050***	0.037***	0.046***
	(0.020)	(0.008)	(0.007)	(0.008)
Fuel model indicators = other	-0.015	0.013	0.010	0.021**
	(0.035)	(0.009)	(0.008)	(0.009)
Fuel model indicators = Shrub2	0.003	0.021	0.016	0.028**
	(0.036)	(0.013)	(0.013)	(0.012)
Fuel model indicators = Timber1	0.048	0.035***	0.023*	0.040***
	(0.032)	(0.013)	(0.012)	(0.011)
Fuel model indicators = Timber2	0.049*	0.037***	0.025***	0.042***
	(0.027)	(0.010)	(0.009)	(0.008)
Fuel model indicators = Timber3	0.089***	0.048***	0.036***	0.049***
	(0.026)	(0.011)	(0.009)	(0.008)
Fuel model indicators = Unburnable	-0.003	0.014*	0.008	0.022***
	(0.026)	(0.008)	(0.006)	(0.008)
Elevation (meters)				0.0007***
				(0.0001)
Wildfire hazard (%)				0.023
				(0.027)
Lot size (acres)				0.00002
				(0.00010)
Square feet (thousands)				-0.003
				(0.002)
Bedrooms				0.001
				(0.003)
Incident FE	✓			
Street FE		✓		
Sub-street FE			✓	✓
Observations	45,093	45,093	45,093	36,176
R^2	0.39	0.62	0.66	0.67
Dependent variable mean	0.39	0.39	0.39	0.44

A.10 Balance Across Vintages

We assess how covariates have changed over time by considering standardized mean differences (SMDs) between home vintages. Standardized mean differences, described in Chapter 15 of Imbens and Rubin (2015), are designed to identify practically meaningful differences in covariates, i.e., covariates that are sufficiently different across groups such that directly including them in the regression could be problematic. The Imbens and Rubin rule of thumb is that SMDs greater than 1 in absolute value may indicate problematic covariates. The formula for the SMD of a variable ΔX and treatment and control groups T and C is:

$$\Delta X = \frac{\overline{X_T} - \overline{X_C}}{\sqrt{(s_T^2 + s_C^2)/2}}$$

Where $\overline{X}$ indicates a mean and s is a standard deviation. Table A7 reports SMDs for control variables between homes built before codes (pre-1998) compared to those built after code came into place (1998–2007 and 2008–2016) in the SRA, LRA-VHFHSZs, and No-codes jurisdictions. To reflect the same variation we use in our preferred specification, each control variable is residualized, or demeaned, by sub-street. Each column is a covariate and each row is a different set of comparisons using either all vintage years of homes or a subset following the vintage definitions we use in the paper.

Table A7: Standardized Mean Differences in Covariates Across Vintage

Jurisdiction	Vintage	Slope	Lot size	Sqft	Bedrooms	Elevation	Hazard
SRA	1998–2007	0.01	0.00	0.36	0.16	0.06	−0.01
SRA	2008–2016	0.06	0.02	0.37	0.01	0.08	0.09
LRA-VHFHSZ	1998–2007	0.11	0.01	0.32	0.20	−0.03	0.01
LRA-VHFHSZ	2008–2016	0.13	0.09	0.38	0.24	−0.08	0.06
No-codes	1998–2007	−0.02	−0.04	0.16	0.06	0.10	0.05
No-codes	2008–2016	0.00	−0.06	0.08	−0.06	−0.01	0.12

Notes: Table shows standardized mean differences across vintages of residualized control variables between code-required jurisdictions (SRA/LRA-VHFHSZ) and No-codes jurisdictions. Each column is a control variable after it has been residualized by sub-street. Jurisdiction indicates the jurisdiction the home is located in, and vintage is the years of homes being compared to the pre-1998 homes. Cells are standardized mean differences, which are the difference in mean values divided by the pooled average standard deviation for that variable between homes built between 1998–2007/2008–2016 and pre-1998 homes.

All of the SMDs are less than 0.4, and most are close to 0. The largest residualized changes in terms of SMDs are for home square footage and indicate that homes in code-required jurisdictions may have grown at slightly faster rates than those in no-code jurisdictions. That all of these SMDs are well below one indicates that controlling for them via regression adjustment—as we do in column (4) of Table 1—is sufficient.

B Sensitivity Checks

This section includes sensitivity checks on the resilience estimates in Section 4.

B.1 Own-Structure Survival Sensitivity

Here we report on a range of sensitivity checks relating to the own-structure survival estimates in Section 4.1.

B.1.1 Alternative Estimation Samples

Appendix Table B1 documents the sensitivity of the estimates in Table 1 to alternative choices of estimation samples, i.e., which homes are included in the specifications. We discuss the motivation for each check and the findings in turn below.

CA Controls. The empirical design in the main text considers differences in destruction probability across vintages of homes built before and after the California's wildland building code changes. The comparison group in the main text is a set of homes outside of California that faced a major wildfire incident.

However, there are a small number of homes inside of California that may have also not faced codes but were nonetheless involved in a wildfire incident. These are homes located inside LRA jurisdictions in areas that were never designated as high risk. As we discuss in Section 3.1, since local jurisdictions were permitted to construct their own hazard maps for code enforcement and since those maps are not consistently available for all jurisdictions, there remains some uncertainty with respect to whether these homes did or did not face building code requirements at the time of construction. This factor, along with the limited number of homes in this category (only 37 homes were built in California no-code areas between 2008 and 2016), leads us to exclude California homes from the comparison group in the main text. For the sake of completeness, here we report estimates that include California homes in the comparison group.

Column (1) documents how the estimates change when only California homes are taken as the no-codes control group (i.e., where we replace the outside of California homes with the no-codes homes in California). Homes built between 1998 and 2007 in these areas without codes survive at very similar rates to pre-1998 homes, and homes built between 2008 and 2016 survive at a rate that is around 6 percentage points higher, but, as expected, this estimate is noisy and not statistically distinguishable from zero.

Combined Controls. Column (2) shows the estimates when both non-CA and within-CA controls are included in the estimation. The sample size is larger for this estimation and the estimates remain qualitatively similar. As expected, the standard errors for the No-codes homes shrink slightly, as more homes are included in this estimate.

Sample restrictions. Column (3) restricts the sample to a group of homes that is balanced on year built by using only fires after January 1, 2015 and homes built before 2015. This ensures that the composition of homes by year built is identical across incidents. It also speaks to possible concerns that more recently-collected data may have been more accurate or consistently coded. The estimates are similar to those we document in the main text: California's building codes reduced the likelihood that homes in code-required jurisdictions where destroyed in wildfire events. Column (4) restricts the sample to only homes that were not involved in multiple incidents during our sample period. In both cases, the estimates are virtually identical using either of these sampling restriction.

Controlling for edge effects. Our sample includes only homes that are either inside or within 10 meters of a wildfire perimeter. Wildfire perimeter definitions are defined post-fire and reflect the edges of burned areas. Here we consider the possibility that imprecision at the edge of a wildfire perimeter, firefighting effort, or other factors to selectively rule out or rule in homes in a way that impacts the main estimates. We test for this in Column (5) by restricting the sample to only homes that are more than 50 meters from the perimeter, ruling out edge effects by ensuring that all homes are well within the true perimeter. We find that the estimates are again unchanged.

Large and resource-limited fires. Even with the granularity of the included fixed effects, if fire management efforts are deliberately targeted towards homes that are perceived as more protectable (e.g., because they are built following wildfire building codes), it could inflate the protective effect of the codes. To examine this possibility, column (6) restricts the sample to homes in wildfires that put at least 1,000 homes at risk. Since the most destructive wildfires are less responsive to firefighter efforts, and in many cases homes facing such large incidents receive no protection at all. If targeting of fire management efforts is contributing to the building code effects we measure, the effects of being built under a code-required regime should be smaller for the most destructive fires. In fact, we see the opposite: if anything, the protective effects of post-code homes are more pronounced in large fires, though only slightly. This specification does not produce estimates for homes that did not face codes, since these incidents only took place in California and the preferred control group is homes outside of California.

Column (7) takes this comparison a step farther by restricting the set of incidents we examine to only those large fires where fire managers report resource constraints. To do so, we load the by-date incident situation report data from ICS-209 Plus (St. Denis et al. 2023) and identify the large incidents where fire managers reported "critical" resource needs that included crew.[45] Again, post-code homes survive at higher rates even among incidents where fire professionals are most resource-constrained. As before, only California incidents fall into this category, so only estimates for the SRA and LRA-VHFHSZ homes are reported.

45. This set of incidents includes Valley (2015), Thomas (2017), Camp (2018), Carr (2018), Woolsey (2018), and the LNU Lightning Complex (2020).

Table B1: Building Code Effects on Own Destruction, Alternative Estimation Samples

	CA Controls	Combined Controls	Balanced Panel	Single Incident	50m+ From Perimeter	1000+ Homes At Risk	Limited Resources
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1998–2007 × SRA	-0.089***	-0.089***	-0.089***	-0.091***	-0.094***	-0.099***	-0.101***
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)	(0.014)
2008–2016 × SRA	-0.131***	-0.131***	-0.120***	-0.129***	-0.135***	-0.142***	-0.159***
	(0.019)	(0.019)	(0.015)	(0.018)	(0.021)	(0.027)	(0.037)
1998–2007 × LRA-VHFHSZ	-0.071***	-0.080***	-0.074***	-0.070***	-0.075***	-0.071**	-0.083**
	(0.023)	(0.020)	(0.025)	(0.023)	(0.023)	(0.025)	(0.023)
2008–2016 × LRA-VHFHSZ	-0.112***	-0.133***	-0.111***	-0.110***	-0.118***	-0.111***	-0.133***
	(0.026)	(0.016)	(0.038)	(0.026)	(0.028)	(0.029)	(0.018)
1998–2007 × No-codes	-0.009	-0.028	-0.033	-0.037	-0.035		
	(0.046)	(0.023)	(0.026)	(0.025)	(0.025)		
2008–2016 × No-codes	-0.058	-0.006	0.012	0.012	0.035		
	(0.062)	(0.038)	(0.054)	(0.043)	(0.048)		
Ground slope (degrees)	0.004***	0.004***	0.004***	0.004***	0.004***	0.004***	0.003**
	(0.0007)	(0.0007)	(0.0006)	(0.0007)	(0.0007)	(0.0008)	(0.0009)
Fuel model indicators	✓	✓	✓	✓	✓	✓	✓
Sub-street FE	✓	✓	✓	✓	✓	✓	✓
Observations	46,035	47,090	38,241	44,434	40,684	32,659	22,638
R^2	0.67	0.66	0.65	0.66	0.65	0.67	0.66
Dependent variable mean	0.40	0.39	0.44	0.40	0.42	0.44	0.48

Notes: Table shows estimates and standard errors from various sensitivity tests on the main results presented in Table 1. The dependent variable is an indicator for home destruction. The first six rows are the coefficients for the vintage by jurisdiction interactions, where vintage indicates homes with an effective year built between 1998–2007 or 2008–2016 and jurisdiction is either SRA, LRA-VHFHSZ, or No-codes. The omitted vintage category is homes with an effective year built prior to 1998, so the coefficients represent the within-jurisdiction change in the probability of destruction relative to pre-1998 homes. Ground slope is a measure of the land slope of the home's parcel. Fuel model controls are dummy variables for the fuel model at the location of the home. Additional controls are include ground slope, lot size, home square footage, number of bedrooms, elevation, and wildfire hazard. The estimation sample row indicates which subset of homes are included in the given regression. For additional details on estimation samples, see directly preceding text in Appendix Section B.

B.1.2 Comparing Fixed Effects Specifications

Here we consider what share of streets include both pre- and post-code homes, and therefore contribute to identification with street or sub-street fixed effects. Appendix Figure B1 shows that about 50% of homes are located on streets with pre- and post-1998 homes, and that nearly all incidents continue to contribute to identification under all sets of fixed effects.

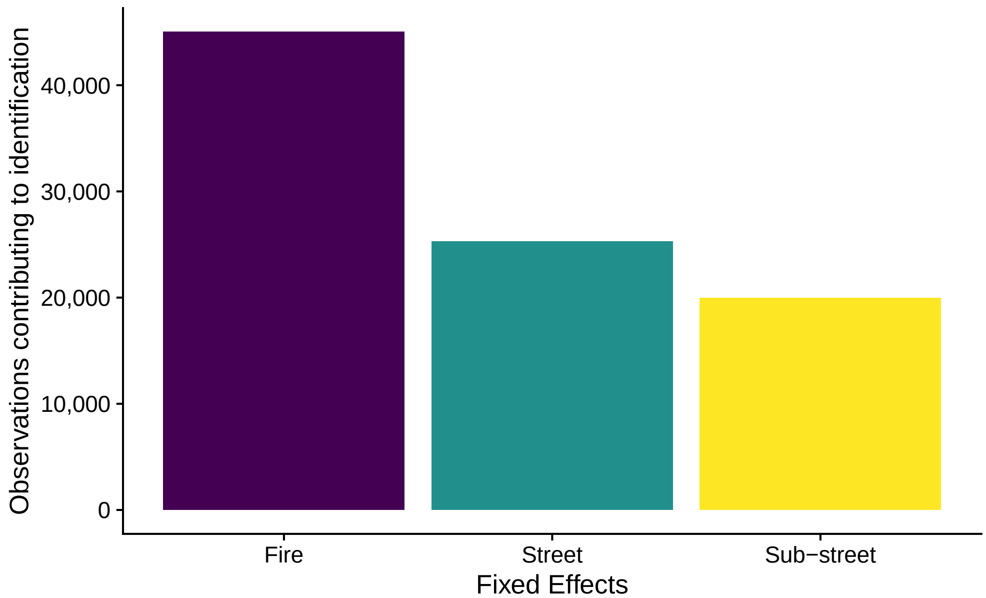
Appendix Table B2 then examines the differences in observable characteristics between the streets that do and do not have variation in exposure to building codes. The differences in observables between these homes and the remaining exposed homes are small in magnitude, though statistically significant in some cases.

B.1.3 Attenuation Due to Spillovers

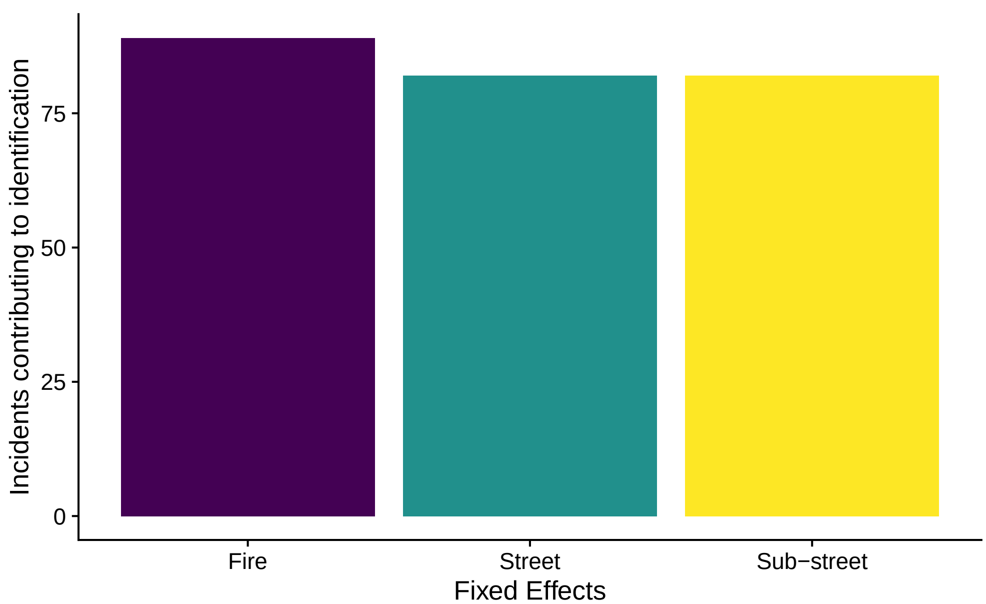
The street or sub-street fixed effects design could underestimate the effect of building codes due to the spillover benefits that we document in Section 4.2. If code-induced investments also benefit nearby pre-code homes, the difference in outcomes between post-code and pre-

Figure B1: Residual Variation in Year Built

(a) Number of Observations



(b) Number of Fires



Notes: Figures show the number of observations and number of fires that contribute to identification of the building code effects in the given fixed effects specifications. The bars in the top panel show the number of homes for which there is "within" variation in 1[Built 1998+], a dummy for construction after the introduction of California's wildfire building codes. The bars in the bottom panel show the number of unique incidents represented by these homes. "Fire" fixed effects reflect comparisons across homes in the same incident, "Street" across homes in the same street and in the same incident, and "Sub-street" across groups of 25 sequential homes (by address number) on the same street and in the same incident.

Table B2: Homes on Streets Without and With Both Pre- and Post-Code Homes

Variable	Without Variation	With Variation	t-statistic
Destroyed (%)	38 (48)	41 (49)	0.51
Ground slope	7.1 (5.7)	7.6 (5.7)	2.61
Elevation (m)	444 (403)	469 (385)	1.43
Wildfire hazard (%)	0.45 (0.66)	0.56 (0.76)	1.86
Lot size (acres)	4.3 (31)	4.9 (18)	-0.12
Square feet (1000s)	2.1 (1.2)	2.3 (1.5)	2.56
Bedrooms	3.1 (1.1)	3 (1.1)	-0.84

Notes: Table shows characteristics of homes split by whether those homes are sub-streets (groups of 25 homes on the same street) that have a mix of pre- and post-code homes ("With Variation") or do not ("Without Variation"). Means and standard deviations for each category of homes are given in the first two columns, and the t-value for a difference in means test (clustering by incident) is given in the third column. Destroyed is the % of homes destroyed in each group, ground slope is the slope of the ground of the home in degrees, elevation is its elevation in meters, wildfire hazard is the likelihood of a moderate or larger wildfire occurring in that location, lot size is the parcel lot size in acres, Square feet is the footprint of the home in 1000s of square feet, and bedrooms in the number of bedrooms in the home.

code homes will understate the true effect of codes on survival. This is a violation of the Stable Unit Treatment Value Assumption, or SUTVA (Rubin 1980). This attenuation could be exacerbated by the sub-street fixed effects, which by construction are focused on homes located relatively close to each other. Such reasoning might lead readers to prefer the Table 1, column (1) estimates with incident fixed effects.

In practice, as we show in Section 4.2, spillovers are highly localized and small compared to the own-resilience effects. In the spirit of exhaustiveness, Appendix Table B3 investigates the quantitative significance of SUTVA concerns by controlling directly for the number of pre- and post-code near neighbors in the sub-street fixed effects regression. Ultimately, the differences in the estimated building code effects across these approaches – incident fixed effects, sub-street fixed effects, and sub-street fixed effects directly controlling for spillovers – are small.

Table B3: Controlling for spillovers in own-effect estimates

	Walls		Centroids	
	(1)	(2)	(3)	(4)
1998–2007 × SRA	-0.088***	-0.087***	-0.088***	-0.088***
	(0.015)	(0.015)	(0.009)	(0.008)
2008–2016 × SRA	-0.074***	-0.073***	-0.133***	-0.133***
	(0.020)	(0.019)	(0.020)	(0.020)
1998–2007 × LRA-VHFHSZ	-0.074***	-0.073***	-0.070***	-0.068***
	(0.024)	(0.024)	(0.023)	(0.023)
2008–2016 × LRA-VHFHSZ	-0.118**	-0.119**	-0.111***	-0.109***
	(0.048)	(0.048)	(0.024)	(0.024)
Ground slope (degrees)	0.003***	0.003***	0.004***	0.004***
	(0.0008)	(0.0008)	(0.0007)	(0.0007)
Pre-code Neighbors		0.017**		0.019***
		(0.007)		(0.006)
Post-code Neighbors		0.006		0.002
		(0.005)		(0.007)
Fuel model indicators FE	✓	✓	✓	✓
Sub-street FE	✓	✓	✓	✓
Observations	20,757	20,757	40,779	40,779
R^2	0.72	0.72	0.66	0.66
Dependent variable mean	0.47	0.47	0.39	0.39

Notes: Table estimates whether a home is destroyed on treatment indicators for vintage and jurisdiction and the presence of nearby neighbors. Sample includes only California homes. Columns (1) and (3) reproduce specifications from Table 1 using street and sub-street fixed effects. Columns (2) and (4) add indicators for the prescence of pre-and post-code nearby neighbors (within 10m of wall-to-wall distance). Standard errors are clustered by incident.

B.1.4 Other Statistical Specifications

Table B4 documents the sensitivity of the results to alternative statistical specifications.

Columns (1) and (2) present alternative choices of controls and fixed effects relative to the preferred specification in column (3) of Table 1, which includes a fixed effect for each set of 25 homes on a street. Column (1) includes only the vintage-by-jurisdiction interactions without any other controls and column (2) adds the baseline controls (ground slope and fuel model indicators) but uses sub-street fixed effects that also differentiate by side of street. In both specifications, the estimates are similar for both code-required and no-code jurisdictions.

Propensity score matching. Column (3) implements a propensity score matching regression following D. E. Ho et al. (2007) and programmed using the R package `MatchIt`. Every home in code-required jurisdictions is matched (with replacement) to a home in an area without codes using a propensity score matching approach, where the propensity score is estimated using a probit regression of treatment on the effective year built, ground slope, and fuel model. When matching, year built for pre-1935 homes is rounded to five-year bins and year built for pre-1900 homes is rounded to 1900 to ensure matches for all homes in the treatment jurisdictions. We then re-estimate the model on the set of treatment and matched control homes using the preferred specification (column (3) in Table 1) and the propensity score weights estimated above. As in the other columns, the estimates remain qualitatively similar and, if anything, the difference between the code-required and no-code coefficients is more pronounced.

Traditional Difference-in-Differences. To directly estimate of building codes in SRAs and LRA-VHFHSZs relative to No-codes jurisdictions, in column (4) we replace the No-codes-vintage interactions (No-codes × 1998-2007 and No-codes × 2008–2016) with vintage indicators (1998–2007 and 2008–2016). The SRA-vintage and LRA-VHFHSZ-vintage interactions now represent the *change* in destruction probability between the given vintage and the pre-period relative to homes in the control group. Because the No-codes estimates are close to zero, the estimates are qualitatively similar to those presented in the the main text.

Logit. Column (5) estimates the effect of building codes on home survival using a logit model. For comparability to other specifications, the displayed coefficients are the average marginal effects of each of the focal variables at the means of all other variables. Relative to the estimates in the main text and in other columns of this table, the average marginal effects are slightly larger in magnitude. The reduction in the number of observations relative is an artifact of the logit estimation procedure when fixed effects are included, since any observations without variation in the outcome must be dropped from the estimation in order for the estimates to converge (Correia, Guimarães, and Zylkin 2021).[46]

46. See Appendix Section B.1.2 for a discussion of the effective sample size of the LPM estimates.

Table B4: Building Code Effects on Own Destruction, Alternative Specifications

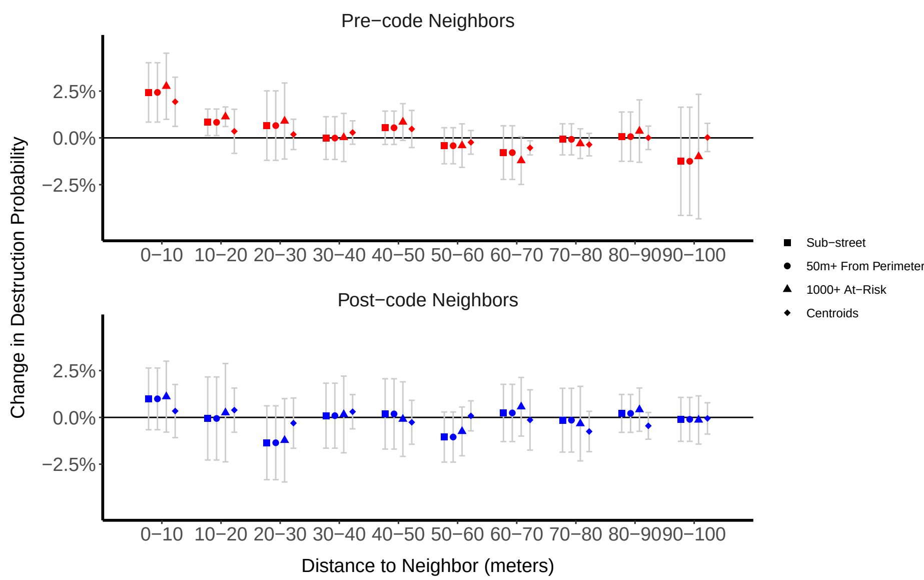
	No Controls (1)	Street Side (2)	Matching (3)	DID (4)	Logit (5)
1998–2007 × SRA	−0.089***	−0.092***	−0.090***	−0.051*	−0.141***
	(0.008)	(0.008)	(0.010)	(0.027)	(0.014)
2008–2016 × SRA	−0.129***	−0.131***	−0.138***	−0.135***	−0.205***
	(0.020)	(0.021)	(0.021)	(0.049)	(0.025)
1998–2007 × LRA-VHFHSZ	−0.067***	−0.082***	−0.072***	−0.033	−0.124***
	(0.024)	(0.026)	(0.026)	(0.034)	(0.024)
2008–2016 × LRA-VHFHSZ	−0.107***	−0.121***	−0.112***	−0.116**	−0.202***
	(0.028)	(0.033)	(0.028)	(0.052)	(0.038)
1998–2007 × No-codes	−0.038	−0.052	0.002		−0.042
	(0.025)	(0.034)	(0.039)		(0.042)
2008–2016 × No-codes	0.007	−0.030	0.025		0.013
	(0.045)	(0.040)	(0.057)		(0.067)
1998–2007				−0.038	
				(0.025)	
2008–2016				0.005	
				(0.044)	
Ground slope (degrees)		0.004***	0.004***	0.004***	0.007***
		(0.001)	(0.001)	(0.001)	(0.001)
Fuel model indicators		✓	✓	✓	✓
Sub-street FE	✓		✓	✓	✓
Sub-street × Side FE		✓			
Observations	45,093	45,093	36,380	45,093	21,810
R^2	0.66	0.71	0.67	0.66	
Dependent variable mean	0.39	0.39	0.43	0.39	0.49

Notes: Table shows estimates and standard errors from various sensitivity tests on the main results presented in Table 1. The dependent variable is an indicator for home destruction. The first six rows are vintage-jurisdiction interactions, where vintage indicates homes with an effective year built between 1998–2007 or 2008–2016 and jurisdiction is either SRA, LRA-VHFHSZ, or No-codes. The omitted period category is homes with an effective year built prior to 1998, so the coefficients represent the within-jurisdiction change in the probability of destruction relative to pre-1998 homes. Ground slope is a measure of the land slope of the home's parcel. Fuel model controls are dummy variables for the fuel model at the location of the home. Additional controls are include ground slope, lot size, home square footage, number of bedrooms, elevation, and wildfire risk. Each column is an alternative specification to those presented in the main text. For additional details on each specification, see directly preceding text in Appendix Section B.

B.2 Spillovers to Neighboring Properties Sensitivity

Here we examine the sensitivity of the neighbor spillover estimates. Appendix Figure B2 reproduces Figure 5 in the main text with alternative sets of fixed effects and estimation samples. The spillover estimates are insensitive to the choice of street or sub-street fixed effects, excluding homes within 50 meters of the fire perimeter, and focusing only on the most destructive fires.

Figure B2: Building Code Neighbor Effects by Distance (Sensitivity)



Notes: Figure shows coefficients and 95% confidence intervals from a regressions of "Destroyed" on the presence of pre- and post-code neighbors at various distances and across four specifications. Distance to neighboring home is wall-to-wall distance. The top panel shows estimates for indicator variables for the presence of one or more neighbors built without wildfire building codes. The bottom panel shows estimates for indicator variables for the presence of one or more neighbors built after wildfire building codes. "Street FE" includes own year built (in four year bins), street by incident fixed effects, and topographic controls. "Sub-street FE" replaces the street by incident fixed effects with street by 25 homes by incident fixed effects. "50m+ From Perimeter" limits homes to only those 50 meters or more inside the wildfire perimeter. "1000+ At-Risk" limits the sample to homes implicated in fires that threatened at least 1000 homes.

B.3 Building Code Effects on Neighborhood Destruction

Here we document estimates from neighborhood-level regressions that estimate the effect of the proportion of post-code homes in the neighborhood on the average rate of home survival in that neighborhood. We estimate models where the outcome is average home survival in that neighborhood and the treatment variable is a continuous measure of the proportion of homes in that neighborhood that are post-code. The coefficient estimates then represent the change in average probability of survival for homes in the neighborhood when moving from 0% post-code homes to 100% post-code homes.

Because these specifications impose significant limits on statistical power, we pool all homes built in code-required jurisdictions (i.e., SRA or LRA-VHFHSZ) into a single treatment group and all post-code vintages (1998–2007 and 2008–2018) into a single post-code indicator for 1998–2016. Each column aggregates homes into neighborhoods following different possible definitions of what constitutes a neighborhood.

Table B5 shows the results. For comparison, column (1) shows an equivalent specification that is similar to the estimates in Table 1, but where we pool the indicator variables for code-required jurisdictions and vintages as described in the paragraph above. Columns (2)–(5) use proportions of home destruction within the given neighborhood as the outcome and proportions of post-code homes in code-required and no-code areas as the main independent variables. Column (2) uses census blocks as the definition of neighborhoods, (3) uses homes on the same street, (4) uses homes on the same sub-street (every 25 homes on the same street), and (5) uses homes both on the same street and in the same census block. In all cases, neighborhoods with in areas with a higher proportion of post-code homes survive at higher rates than neighborhoods with more new homes built in the absence of codes. These estimates are around 14%, similar in magnitude to the own-home effects we estimate in Table 1, though slightly larger. This difference is consistent with the additional spillover resilience benefits between neighbors we consider in Section 4.2. By contrast, the effect of having more homes built between 1998 and 2016 in areas where codes are not required is not statistically different from zero under any of these neighborhood definitions, and in all except the Census Block neighborhood case (a noisy estimate due to the relatively large areas census blocks cover), fairly close to zero.

Table B5: Building Code Effects on Neighborhood Rate of Destruction

	Homes (1)	Census Blocks (2)	Street (3)	Sub-street (4)	Street × Block (5)
1998–2016 × Code-required	-0.123***	-0.160*	-0.147**	-0.147**	-0.141**
	(0.037)	(0.085)	(0.068)	(0.067)	(0.060)
1998–2016 × No-codes	-0.004	0.109	0.023	0.008	0.011
	(0.051)	(0.105)	(0.092)	(0.079)	(0.073)
Ground slope	0.005***	0.006*	0.005*	0.006**	0.005**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)
Incident FE	✓	✓	✓	✓	✓
Observations	45,093	4,062	6,139	8,369	10,813
R^2	0.38	0.66	0.61	0.57	0.54
Dependent variable mean	0.39	0.31	0.37	0.37	0.37

Notes: Table shows estimates of building code effects on home destruction at the neighborhood level. Column (1) presents a benchmark specification where the observational unit is a single home, the outcome is home destruction, and the first two rows indicate if the home was built between 1998 and 2016 in either a code-required (SRA or LRA-VHFHSZ) or in a No-code jurisdiction. Columns (2)–(5) show estimates from specifications where the observational units are neighborhoods. Across columns (2)–(5), neighborhoods are defined as: groups of homes in (2) the same census block, (3) homes on the same street, (4) homes in the same group of 25 homes on the same street, and (5) homes on the same street and in the same census block. For columns (2)–(5), the outcome is the proportion of homes destroyed in that neighborhood. The first row is the proportion of homes built between 1998 and 2016 in code-required jurisdictions. The second row is the proportion of homes built between 1998 and 2016 in jurisdictions without codes. The omitted vintage category is homes with an effective year built prior to 1998, so the coefficients represent the within-jurisdiction change in the probability of destruction relative to pre-1998 homes. Ground slope is the average land slope of parcels in the neighborhood. Standard errors clustered by incident.

C Housing Market Effects of Building Codes

This appendix section tests for housing market impacts of wildfire building codes. If homeowners or their neighbors experience amenity losses as the result of being required to comply with wildfire building mandates, then the social net benefits of a mandate could be less than those we measure in the main text using the cost-effectiveness measure. This could occur if, for example, many homeowners would strongly prefer to live in a home with cedar roofing or in a neighborhood where future neighbors are not required to manage the vegetation near the homes. Alternatively, if the resilience benefits are capitalized into either the post-code homes' or their neighbors' home values, then home prices could increase to code-required areas.

In order to test for the possibility of own-home value changes, we examine whether parcel sale prices in code-required jurisdictions significantly declined after the introduction of the wildfire building codes. To test for neighboring home effects, we examine whether sale prices fell for already-constructed homes in newly code-required jurisdictions. Finally, we consider quantity changes: do areas in code-required jurisdictions experience differential changes in the growth rate of homes relative to no-code jurisdictions after the codes take effect.

To conduct these tests, we introduce a new sample that includes all California homes that are within a one kilometer border between state and local responsibility areas. We focus on homes within these border areas to ensure that the homes in code-required and no-codes jurisdictions are as comparable as possible to each other in terms of location, amenities, and other factors that determine prices in local housing markets. This section describes the construction of the sample, the cleaning of the transaction data for those homes, and finally the estimates of building codes' effect on housing market outcomes.

In general, we find that the introduction of building codes does not lead to significant losses (or gains) in home value for new homes or for existing neighbors, indicating that homeowners neither have a strong negative market response to the new building requirements, nor do they substantially capitalize the home protection benefits into the value of their homes.

C.1 Housing Market Sample

The additional sample of homes for the housing market regressions in this section come from the same data provider described in the main text. In contrast to the resilience regressions in the main text where we could only examine homes included inside of a wildfire perimeter, here we include the full set of single family homes in California that are within a one kilometer border between areas where to-code construction was and was not required. Doing so increases the statistical power of this research design substantially and allows us to examine whether homeowners across California more broadly saw changes in home values as a result of the building code mandate.

For all of the homes in this sample, we also obtain home sales data from county recorder files, also compiled by ZTRAX. This additional dataset provides property sales between 1994 and 2020. To ensure that the transactions we incorporate represent true market values for residences in our sample, we conduct an extensive cleaning process following the best practices described in Nolte et al. (2023). In brief, we first limit the sample of transactions to only include single family homes or similar and remove any transactions that are not deed transfers (e.g., mortgage foreclosures). Next we include transactions with both high quality document and loan types, where type quality is defined as in Nolte et al. (2023). We finally remove remaining transactions that are not "arms-length" by filtering out those where the provided data indicate an intra-family transfer, where the sale is flagged as not reflective of the local market, or where the sale price is $1,000 or less. We adjust sale prices for inflation to 2015 dollars using the California Home Price Index provided by the Federal Reserve Bank of St. Louis.

In total, the sample includes 1.4 million home sales for 0.7 million homes. Table C1 gives descriptive statistics for the dataset.

Table C1: Housing Market Sample Descriptive Statistics

	Mean	SD	P5	Median	P95
Panel A. Home sales (N = 1,363,899)					
Year built	1979	22.7	1934	1985	2006
Ground slope (degrees)	4.15	4.28	0	3	13
Lot size (acres)	0.589	72.7	0.084	0.176	0.786
Square feet (thousands)	2.14	0.951	1.01	1.96	3.82
Bedrooms	3.46	0.938	2	3	5
Elevation (meters)	269	247	34	206	579
Wildfire hazard (%)	0.117	0.241	0	0.033	0.521
Sold price (thousands)	662	665	188	536	1499
Panel B. Counts					
Homes	725,910				
Counties	48				
Census blocks	46,735				

Notes: Table of descriptive statistics for the housing market sample, which includes all single family home sales in California within one kilometer of an SRA/LRA border and where key assessment variables (year built and square feet) are available. Each observation is a single home sale. In the first panel, slope is the land slope in degrees of the parcel on which the home is located, elevation is its elevation in meters. Lot size is the size of the lot in acres, square feet is the square footage of the home itself, bedrooms is the number of total bedrooms, year built is the year the home was built or the most recent year of a major remodel (if one occurred), hazard is wildfire hazard, and sale price is the price the home sold for. The second panel summarizes the number of unique of homes, counties, and census blocks in the dataset.

C.2 Effects on Sale Prices for Post-Code Homes

We estimate hedonic regressions where we regress the logged sale price of home i on street s using forms of the following specification:

$$\log(\text{Sale Price})_{is} = \sum_{v=v_0}^{V}\sum_{j=j_0}^{J}\beta_{vj}\text{Vintage}_i^v \times \text{Jurisdiction}_i^j + X_i + \gamma + \epsilon_{is} \tag{6}$$

As before, we collapse home vintages into three bins denoted by v: homes built before 1998 (the omitted category), homes built between 1998 and 2007, and homes built between 2008 and 2016. The βs are the coefficients of primary interest: they capture differential changes in home prices in SRAs and LRA-VHFHSZs relative to no code jurisdictions. X_i is a vector of home parameters that includes ground slope, fuel model, lot size, home square footage, elevation, and wildfire hazard. γ represent fixed effects, which include street fixed effects,[47] census tract by vintage fixed effects, and sold period fixed effects. The inclusion of the tract-vintage fixed effects implies that the comparison of trends in home sale prices is a comparison of trends within tracts, which controls for any unobserved local trends in sale prices. The sold period fixed effects control for any sample-wide shifts in housing markets with indicator variables for homes sold before 1998, between 1998 and 2007, and between 2008 and 2016.

Table C2 documents the effect of building codes on home sale prices for post-code homes. Column (1) shows the estimates using the 1.3 million home sales that occurred within 1 km of an SRA/LRA border. The first four rows show the β coefficients of interest: the additional change in the logged sale prices of homes in code-required jurisdictions built relative to homes built in areas without codes. For all four jurisdictions and post-code vintages, we find no statistically different estimates for homes built in the code-required jurisdictions. Column (2) reports similar estimates using a more restrictive 500 meter buffer. Finally, our preferred sample in column (3) restricts to homes increasingly close to an SRA/LRA border, and for these specifications we again find no statistically significant differences for the prices of post-code home sales relative to no-code jurisdictions.

These estimates suggest that post-code homes in code-required areas did not experience substantial losses in value as a result of the mandate. That we can similarly rule out large positive effects implies that capitalization of the additional protection afforded by building codes was limited, which is consistent both with partial insurance protection and limited homeowner awareness of the large protective benefits of these improvements.

47. In contrast to the resilience estimates in the main text, we use street rather than sub-street fixed effects here since there are many fewer home sales on the same street, whereas in a wildfire we observe all affected homes on a given street.

Table C2: Building Code Effects on Sale Prices for Post-Code Homes

	Log sale price		
	(1)	(2)	(3)
SRA × 1998–2007	-0.021	-0.024	0.016
	(0.018)	(0.025)	(0.029)
SRA × 2008–2016	-0.023	-0.035	0.007
	(0.037)	(0.044)	(0.047)
LRA-VHFHSZ × 1998–2007	-0.018	-0.014	0.001
	(0.013)	(0.015)	(0.021)
LRA-VHFHSZ × 2008–2016	-0.010	-0.031	-0.041
	(0.019)	(0.023)	(0.028)
Ground slope (degrees)	0.0004*	0.0006**	0.0001
	(0.0002)	(0.0003)	(0.0003)
Fuel Model Controls	✓	✓	✓
Additional Controls	✓	✓	✓
Census tract-Vintage FE	✓	✓	✓
Street FE	✓	✓	✓
Sold period FE	✓	✓	✓
Border size	1 km	500 m	250 m
Observations	1,340,493	792,264	420,610
R^2	0.748	0.748	0.751
Dependent variable mean	13.2	13.2	13.2

Notes: Table shows estimates of logged home sale prices regressed on building vintage, split by jurisdiction. Specifications follow Equation (6), where the omitted category is homes in that are not subject to building codes, i.e., No-codes homes.

C.3 Effects on Sale Prices for Existing Homes in Code-Required Jurisdictions

The previous subsection established that homes built in code-required jurisdictions after the building codes took effect do not sell for any less (or more) than similar homes built in no-code jurisdictions. Here we investigate the possibility that the building code mandate could have had overall effects on perceived neighborhood quality. To do so, we focus on homes that had already been built when the codes came into effect. Changes in home values for pre-code homes in code-required jurisdictions could indicate additional spillover benefits or costs to homeowners that we don't capture in our measure of the reduction in neighbor risk. These types of spillovers would be analogous to neighborhood externalities such as those described in Rossi-Hansberg, Sarte, and Owens (2010) and Fu and Gregory (2019).

We estimate a model similar to the hedonic specification described above, but here we consider only homes that were already built by 1990, i.e., before the Oakland hills fire or the building code legislation that followed it. We then consider whether those already-built homes in code-required jurisdictions sold for different prices relative to those in no-code jurisdictions. Because this model focuses on homes that were built prior to the building code legislation, we can include parcel fixed effects to compare homes sold in these areas before and after the codes took effect. These fixed effects account for home characteristics and control flexibly for potential unobservables at the home level. Table C3 documents the findings.

Table C3: Building Code Effects on Sale Prices of Pre-Code Homes

	Log sale price		
	(1)	(2)	(3)
SRA × Sold 1998–2007	-0.031	-0.019	0.0008
	(0.020)	(0.020)	(0.028)
SRA × Sold 2008–2016	-0.034*	-0.026	-0.007
	(0.020)	(0.020)	(0.028)
LRA-VHFHSZ × Sold 1998–2007	-0.011	-0.005	-0.001
	(0.009)	(0.010)	(0.013)
LRA-VHFHSZ × Sold 2008–2016	-0.019*	-0.014	-0.011
	(0.010)	(0.011)	(0.013)
Fuel Model Controls			
Additional Controls			
Census tract-Vintage FE	✓	✓	✓
Census tract-Sold period FE	✓	✓	✓
Parcel FE	✓	✓	✓
Buffer size	1 km	500 m	250 m
Observations	652,223	378,526	201,875
R^2	0.839	0.836	0.833
Dependent variable mean	13.1	13.1	13.1

Notes: Table shows estimates of logged home sale prices regressed on sale year, split by jurisdiction. The omitted category is homes in that are not subject to building codes, i.e., the No-codes group in Table 1.

Column (1) uses the set of sales that includes all homes within one kilometer and controls for parcel fixed effects, restricting to only homes that were sold more than once. Here we find estimates that are close to zero, though estimates for LRA-VHFHSZ homes are marginally statistically different from zero. As before, we suspect this is more likely the result of differential housing market trends across code-required and no-code jurisdictions.

In column (2) we limit the sample to only consider homes within half a kilometer of the border; here we find point estimates that are small and not different from zero at conventional levels. In column (3), we finally restrict to only home sales within 250 m of an SRA/LRA border. This limits the sample to around 200,000 homes that we expect are most comparable to one another, which retaining variation in which of these existing homes were in code-required versus no-code jurisdictions. Again, the estimates are small in magnitude and statistical zeroes. We conclude that the effects on pre-code home values are close to zero.

C.4 Effects on Home Growth Rates After Building Codes

Having shown that home sale prices of both post-code and existing homes are generally unresponsive to the changes in building code policy we document, we now turn to the question of whether fewer homes were built in code-required jurisdictions. Given what we showed in the previous sections, a fall in the number of homes in code-required jurisdictions would be consistent with a combination of decreased housing demand (due to, say, undesirability of post-code homes) and increased housing cost. No change in the number of homes built would indicate that housing markets were generally unresponsive to the building code mandate.

Table C4 tests for changes in the number of homes built across code-required and no-code jurisdictions in California. As before, we consider homes within one kilometer (or less) of a border between code-required jurisdictions (SRA or LRA-VHFHSZ) and no-code jurisdictions (LRA areas not designated as VHFHSZ). We then measure the growth rate of homes in a given census block (b) and jurisdiction (j), which we calculate as:

$$\text{Home growth rate}_{bj} = \frac{\#\text{ Homes built 1998 or later}_{bj}}{\#\text{ Homes built before 1998}_{bj}}$$

We next estimate the following statistical model, where each observation is a set of homes in the same census block and jurisdiction:

$$\text{Home growth rate}_{bj} = \sum_{j=j_0}^{J} \beta_j \text{Jurisdiction}_{bj} + \phi_{\text{tract}} + X_{bj}\alpha + \varepsilon_{bj} \qquad (7)$$

No-code jurisdictions are the omitted category, so the β coefficients represent differences in growth rates between SRAs/LRA-VHFHSZs and no-code jurisdictions. ϕ_{tract} are census tract fixed effects, so these estimates compare blocks within the same tract. We remove any blocks that overlap SRA/LRA boundaries and weight the regressions by the number of pre-code homes in the census block, the denominator in the outcome variable. Table C4 documents the estimates across a range of alternative specifications.

Column (1) of Table C4 includes homes within a 1 km buffer and controls for census tract fixed effects and average home slope. The growth rates of SRA and LRA-VHFHSZ blocks are slightly higher than growth rates for no-code blocks, but the estimates are not statistically different from zero. Column (2) adds average home characteristics as control variables, and again the point estimates are statistical zeroes. Column (3) restricts the sample to only those blocks where there are at least 10 pre-code homes. Again, the estimates are similar.

Table C4: Building Code Effects on Home Growth Rates After Building Codes

	Home Growth Rate After Building Codes				
	(1)	(2)	(3)	(4)	(5)
SRA	0.008	-0.004	-0.001	-0.006	-0.0006
	(0.007)	(0.008)	(0.008)	(0.007)	(0.009)
LRA-VHFHSZ	0.004	-0.002	-0.0009	-0.004	0.004
	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)
Ground slope (degrees)	0.004***	0.001**	0.001**	0.001**	0.001*
	(0.0005)	(0.0005)	(0.0006)	(0.0006)	(0.0007)
Additional Controls		✓	✓	✓	✓
Census tract FE	✓	✓	✓	✓	✓
Border size	1 km	1 km	1 km	500 m	250 m
Sub-sample	All	All	Min. 10 pre-code	All	All
Observations	42,343	39,524	27,851	25,134	16,384
R^2	0.28	0.34	0.36	0.35	0.34
Dependent variable mean	0.08	0.09	0.07	0.08	0.07

Notes: Table shows estimates of changes in home growth rates before and after 1998, split by jurisdiction. Each observation is a set of homes in the same census block and under the same jurisdiction (SRA, LRA, or No-Codes) inside the given buffer. Only census blocks where all homes are in the same jurisdiction are included. Buffers are the area within the given buffer size (1 km, 500 m, or 250 m) of a border between a code-required area and a no-code area. The outcome variable, home growth rate after building codes, is the number of homes built 1998 or later divided by the number of homes built before 1998. The growth rate is Winsorized from above at the 95th percentile of observed finite values. SRA indicates homes in the State Responsibility Area, LRA-VHFHSZ indicates homes in a Local Responsibility Area rated as Very High Fire Hazard Severity, and the omitted category is homes in LRA areas where codes do not apply. Additional controls include average elevation, wildfire hazard, lot size, home square footage, and bedrooms for homes represented by this observation. Sub-sample "Min 10 pre-code" includes only sets of homes with at least 10 homes built before 1998. Standard errors are clustered by census tract.

Columns (4) and (5) finally demonstrate the effect of further narrowing the window around SRA/LRA borders to 500 and 250 m. As before the estimates remain small in magnitude and close to zero.

To summarize our findings in this section, we find little evidence to support significant housing market effects of California's wildland building codes. Home prices of both new (i.e., required to build to code) and existing (i.e., already built but in areas where future neighbors would be required to build to code) appear to be unaffected by the deployment of building codes in the mid-1990s. Finally, we do not see evidence that the quantity of homes built in these areas was significantly affected by the codes either.

D Calculating Net Benefits

This section provides additional details on the simulation exercise described by Section 5.3 in the main paper. We approach the simulation exercise in three steps, each of which is explained in detail in the following sections.

1. *Sampling at-risk homes.* We draw roughly one million homes located in wildfire-prone areas in California. (Section D.1)
2. *Calculating per-home cost effectiveness.* Using the model, data from each home, estimates from our empirical specifications, and parameters from the literature, we use a version of Equation (3) to calculate the social cost-effectiveness of each home's mitigation choice. (Section D.2)
3. *Social net benefit calculation.* For each policy below, we report the total social net benefits and several related measures. These are the numbers in Table 4. (Section D.3)
   (a) *No mitigation policy* (benchmark policy). Simulate mitigation choices under our benchmark scenario of no mitigation policy using Equation (4). (Section D.3.1)
   (b) *Building codes.* We simulate the social planner's optimal choice for location-specific building standards at different spatial units (e.g., a county), which enforce mitigation for homeowners in those locations. (Section D.3.2)
   (c) *Mitigation subsidies.* We simulate the social planner's optimal subsidy choice and subsequent mitigation choices by homeowners. (Section D.3.3)

D.1 Sampling At-Risk Homes

Unlike the empirical analysis of building code effects, which uses homes that experienced historical wildfires, the net benefits calculation considers homes sampled from *all* California homes in fire hazard areas. Specifically, we include all single-family homes in California located in a census tract where average annual wildfire risk equals or exceeds 0.01%. This yields a sample of 1.1 million homes in wildfire hazard areas, representing 50 counties and 62,484 census blocks. Table D1 includes complete summary statistics.

Table D1: California-Wide Sample Descriptive Statistics

	Mean	SD	P5	Median	P95
Panel A. All (Homes = 1,071,033)					
Year built	1983	22.3	1941	1987	2011
Square feet (thousands)	2.18	1.4	0.9	1.96	4.04
Neighbors in 30m	2.5	2.11	0	2	6
Wildfire hazard (%)	0.382	0.518	0.0112	0.21	1.34
Losses per sf	260	49.9	198	249	354
Panel B. Pre-1998 (Homes = 749,224)					
Year built	1973	19.6	1935	1978	1995
Square feet (thousands)	1.91	1.4	0.832	1.74	3.5
Neighbors in 30m	2.34	2.07	0	2	6
Wildfire hazard (%)	0.367	0.505	0.00952	0.2	1.29
Losses per sf	261	50.4	198	249	357
Panel C. 1998 and later (Homes = 321,809)					
Year built	2005	5.02	1999	2004	2016
Square feet (thousands)	2.79	1.21	1.4	2.62	4.71
Neighbors in 30m	2.87	2.16	0	3	7
Wildfire hazard (%)	0.418	0.543	0.0168	0.235	1.43
Losses per sf	259	48.8	198	249	354
Panel D. Areas represented					
Counties	50				
Census blocks	62,484				

Notes: Table of descriptive statistics for the California-wide sample, which includes all single family homes in California in a census block where homes face an average annual wildfire hazard greater or equal to 0.01% and where key assessment variable (year built and square feet) are available. Each observation is a single home in California in assessment year 2018. Year built is the home's year of construction. Square feet is the square footage of the home. Neighbors is the total count of neighbors with centroids less than 30m from the focal home. Hazard is shorthand for wildfire hazard, which is the estimated annual probability the home will face a high severity fire. Losses per sf is the dollar value of losses for the home per square foot. First three panels summarize all homes, homes built before 1998, and homes built 1998 and later. Fourth panel summarizes distinct numbers of counties and census blocks represented by the data.

The correlation matrix in Table D2 summarizes the pairwise correlations between home size, number of neighbors, per-square-foot rebuilding costs, and annual wildfire hazard. For the most part, correlations between these variables are slight. Homes with fewer neighbors tend to have slightly higher wildfire risk, but this relationship is not exceptionally strong.

Table D2: Pairwise correlations between fire hazard and cost variables

	Correlation			
	(1)	(2)	(3)	(4)
(1) Square feet (1000s)	1	.	.	.
(2) Neighbors in 30m	-.05	1	.	.
(3) Wildfire hazard (%)	.02	-.17	1	.
(4) Losses per sf	.05	.00	-.08	1

Notes: Table of pairwise correlations for key variables in the California-wide sample, which includes all single family homes in California in a census block where homes face an average annual wildfire hazard greater or equal to 0.01% and where key assessment variable (year built and square feet) are available. Each observation is a single home in California in assessment year 2018. Square feet is the square footage of the home. Neighbors in 30m is the total count of neighbors less than 30 meters from the focal home (measured using centroid-to-centroid distances). Wildfire hazard the estimated annual probability the home will face a moderate to high severity fire. Losses per sf is the dollar value of losses for the home per square foot.

D.2 Calculating Per-Home Cost Effectiveness

For each home, we calculate the social net benefits of mitigation as the total benefits of mitigation (to the homeowner and their neighbors), minus the costs (borne by the homeowner) of mitigating. This measure is captured by Equation (3), which we rearrange and reiterate here.

$$p_i L(\tau + \delta_i \eta) - m$$

$p_i \in [0, 1]$ is the probability of the home experiencing a wildfire, L are the losses from home destruction, τ is the reduction in own-home destruction probability mitigation provides, δ_i is the number of nearby neighbors i has, η is the reduction in neighboring-home destruction probability, and m is the cost of mitigation. We list detailed sources for each variable below, expanding on the overview in the main text.

- p_i: We identify each home's annual wildfire hazard p_i using data from the United States Forest Service (USFS) Wildfire Risk to Communities project. This measure captures the annual probability of moderate to severe wildfire exposure (Scott et al. 2020). We use the product of Burn Probability (the total annual wildfire probability) and Flame Length Exceedance Probability 4 (conditional on any fire, the probability that the fire will reach moderate or greater threat status).
- L: Assumed losses L for a home destroyed by wildfire include rebuilding costs, belongings and contents of the home, alternative living costs while the home is rebuilt, and

costs for debris removal and hazardous waste cleanup. The rebuilding, belongings and contents, and alternative living arrangements costs all come from the FEMA Hazus model (Federal Emergency Management Agency 2021). Hazus provides Census-block specific loss cost estimates in 2018 dollars per square foot. For debris removal (which is borne by homeowners) and hazardous waste cleanup (borne by governments), we add a total of $150,000.[48] Appendix Table D1 shows the distribution of total loss costs per square foot in the sample used for cost-effectiveness and welfare calculations.

- Voluntary takeup rate: To calibrate the average rate of voluntary take-up in the absence of wildfire building codes, we conducted a comprehensive review of the survey literature on wildfire wildfire home hardening in the United States. We identified 20 studies that report numeric takeup rates for home hardening investments in jurisdictions with no wildfire building codes (see Appendix Table A4 for more details on this process). For our benchmark assumption about the overall average rate of adoption in the absence of building codes, we use survey data from Champ et al. (2020). Based on professional risk assessments of 1,474 homes in wildfire hazard areas in western Colorado, that study finds that 40% of homes use building materials that would comply with California's wildfire building codes in at least two of these three areas: roof, exterior siding, and deck. We accordingly assume takeup rate in the absence of codes is 40%.

- τ: The empirical results in Section 4 allow us to calculate τ, the effects of mitigation on own home survival, subject to one additional assumption. The reduced form estimates of the effect of building codes on structure survival can be seen as intent-to-treat estimates of the effect of mitigation investment. Given an average rate of voluntary take-up for the bundle of mitigation measures in the building code, the standard Wald estimator gives τ as the ratio of the reduced form estimates and the difference in take-up rates in the codes and no-codes jurisdictions.[49] The effectiveness of mitigation own-home survival is derived from our regression estimate of Equation 1 in Section 4. We examine how much the probability of own-structure loss for homes built in the SRA after 2008 relative to homes built before 1998. This estimate is $-\beta_{2008\text{–}2016} = 0.133$ (Table 1, column (2), row 2). To compute τ, which is the reductions in risk p_i relative to *no* takeup, we divide $\beta_{2008\text{–}2016}$ by the proportion of homeowners who do not voluntarily mitigate in these jurisdictions. Following the same benchmark assumption above that 40% of these homes are voluntarily mitigating (and therefore 60% are not), we compute $\tau = -\frac{\beta_{2008\text{–}2016}}{0.6} = \frac{0.133}{0.6} = 0.22$.

- δ_i: We calculate the number of nearby neighbors δ_i directly from our data as the number of neighbors within 10 meters of i. We compute neighbor presence using the centroid-to-centroid distances adjusted to match wall-to-wall distances as noted in footnote 20, since centroid distances are less computationally demanding to calculate in this large

48. For cleanup and debris removal costs, see Klein (2018); Lewis, Sukey, "Cleaning Up: Inside the Wildfire Debris Removal Job That Cost Taxpayers $1.3 Billion." *The California Report*, July 19, 2018; and Bizjak, Tony, "State's Effort to Clean Up After the Camp Fire is Off to a Rocky Start", *Sacramento Bee*, January 13, 2019.

49. See e.g., Angrist and Pischke (2009) p. 127-133. This calculation assumes perfect compliance by homes subject to codes and a homogeneous effect of mitigation on structure survival.

sample of homes.

- η: The reduction in neighboring-home probability of destruction resulting from mitigation η is estimated in a similar way to τ. We compute the neighboring home effect from our Equation (2) estimates: it is our estimate of the effect of having one pre-code neighbor with walls within 10 meters of the origin home minus the effect of having one post-code neighbor with walls within 10 meters of the origin homes $-\rho_{10m} = 0.017 - 0.006 = 0.011$ (Table 2, column (1), row 1 minus row 2). To compute η, which is the reduction in neighboring home risk relative to no takeup, we divide $-\rho_{10m}$ by the proportion of homeowners who do not voluntarily mitigate in these jurisdictions to get $\eta = -\frac{-\rho_{10m}}{0.6} = \frac{0.011}{0.6} = 0.018$.

- m: Our main estimate of mitigation costs m come from Headwaters Economics (2018). That study uses construction estimating tools from R.S. Means to calculate the additional cost to build a home that complies with California's Chapter 7A wildfire code. Overall, that study reports zero cost difference between code-compliant and standard designs. This counter-intuitive result arises because one aspect of code-compliant construction (exterior siding) is substantially *less* expensive than standard designs. These savings offset increased costs for roofing, landscaping, and other areas. Our main estimate of code compliance costs ignores savings from code-compliant siding on the theory that owners would make this choice even without standards. To arrive at our overall mitigation cost, we compute the difference in siding costs as $0 but otherwise use the cost differences they provide for other building materials. In all other cases, fire-resistant construction is somewhat more expensive, which yields an overall additional cost $15,660. In the Headwaters (2018) report, this amounts to ignoring the "Siding" line item in Table 5.1. We also report results using alternative cost estimates from the National Association of Home Builders (NAHB). Their estimated wildfire code compliance costs for newly-built California homes include a low scenario of $7,868 and a high scenario of $29,429 (Home Innovation Research Labs 2020). We deflate the NAHB estimates to 2018 dollars to get $7,634 and $28,553. [50] Finally, we show a "retrofit" scenario based on Headwaters Economics' estimate of $62,760 to fully replace roofing and exterior walls on an existing home. Since homes vary in size, we rescale these mitigation costs using the model home square footage of 2,500 square feet. In other words, writing m_{2500} as the mitigation cost for the model home, we compute $m = m_{2500} \times \frac{\text{Square feet}}{2500}$.

D.3 Calculating Social Net Benefits

For each policy option discussed below, we calculate the empirical analogue of the welfare equations described in the theoretical model in Section 5.2. We calculate the social net benefits of mitigation as the sum of the homeowner-specific cost effectiveness measure from the previous section for all homeowners who mitigate:

50. These are costs to meet the International Wildland Urban Interface Code, which is similar to the Chapter 7A code. In the low scenario, we ignore $3,839 of gross savings from code-compliant siding as we do for Headwaters Economics (2018).

$$\text{Social Net Benefit: } \sum_i 1[\text{Mitigate}]_i \times \left[p_i L(\tau + \delta_i \eta) - m\right]$$

$1[\text{Mitigate}]_i$ is an indicator for whether i mitigates (either by choice or as the result of a mandate). Social Net Benefit is our primary measure of the welfare value of each policy choice, and is given as the first numeric column in Table 4. We also calculate several related measures of welfare, all of which are also columns in Table 4:

- *Private Net Benefit* is the total benefits accrued to homeowners who mitigate under the policy, net of mitigation costs, and does not include spillover benefits to neighbors: $\sum_i 1[\text{Mitigate}]_i \times \left[p_i L\tau - m\right]$.
- *External Benefits* are the additional spillover benefits to neighbors of homeowners who mitigate: $\sum_i 1[\text{Mitigate}]_i \times \left[\delta_i \eta\right]$.
- *Total Adoption Rate* is the percentage of homeowners who mitigate: $\sum_i 1[\text{Mitigate}]_i / N$.
- *Inefficient Adoption Rate* is the number of homeowners who mitigate but do so at a net social loss: $\sum_i 1[\text{Mitigate}]_i \times 1[p_i L(\tau + \delta_i \eta) - m < 0]/N$.
- *Subsidy Paid* is the total amount of subsidies paid to homeowners, which is nonzero only for the subsidy policies: $\sum_i s_i \times 1[\text{Subsidy Paid}]_i$.

D.3.1 Benchmarks

No Policy. We begin by estimating how many homeowners would voluntarily undertake mitigation without a government policy. In our model, homeowners may make non-socially optimal choices for two reasons: first, because they underestimate their own wildfire risk, and second, because they do not internalize the spillover benefits of their mitigation choice to their neighbors. We can accordingly write the mitigation choice for i as based on the perceived private benefit calculation we assume homeowners make (this rearranges Equation (4) in the main paper):

$$1[\text{Mitigation}]_i = 1[\theta p_i \tau L > m]$$

Aside from θ, all of the parameters are given as above. $\theta \in [0, 1]$ is the degree of under-perception of risk. While the literature strongly points to $\theta < 1$, the exact size of the bias is less clear. Our main approach to calculating θ is to find the degree of misperception that matches the implied voluntary takeup rate for properties in our sample (given our other assumptions) to the 40% takeup rate in Champ et al. (2020). This yields $\theta = 0.5$. We also show results for a range of other assumptions in Appendix Figure D2.

Perfect Standard. We calculate the first-bench benchmark as a "perfect standard", i.e., a policy where the social planner requires all homeowners who generate positive social net benefits from mitigation to do so. Under this policy, the mitigation decision for i is determined by:

$$1[\text{Mitigation}]_i = 1[p_i L(\tau + \delta_i \eta) - m]$$

By construction, the only homeowners who will mitigate are those who are required to do so. All other homeowners will not mitigate, since their private net benefit is strictly less than the social net benefit.

Perfect Subsidy. Similar to the perfect standard, the benchmark perfect subsidy policy achieves the first-best outcome, although it requires transfers to some homeowners. In this scenario, the social planner identifies the homeowners who should mitigate (from society's perspective) but will not do so on their own, i.e., homeowners whose social net benefits are positive but private net benefits are negative. She then offers each of these homeowners exactly the difference between their private net benefits and their costs of mitigation to induce them to mitigate. This perfectly targeted subsidy s_i^* is the vector of subsidies such that:

$$s_i^* = max(0, m - \theta p_i L\tau) \ \ \forall\, i \text{ s.t. } p_i L(\tau + \delta_i \eta) - m \geq 0$$

D.3.2 Building Codes

Our first set of comparison policies assumes a social planner who can accurately estimate the net benefits of all of the homeowners' choices, but is required (for political or administrative reasons) to enforce building standards at a given spatial unit, e.g., a county. In other words, for each spatial unit, the social planner can only choose whether to enforce the standard (requiring *all* homeowners in that unit to mitigate) or not. Homeowners in spatial units without a standard are still free to mitigate voluntarily.

We begin with building code policy targeted only on wildfire probability. This type of policy is meant to simulate common map-based approaches to regulating building standards for wildfire, floods, and other hazards. These policies use models of location-specific risk to identify locations where standard will be required. For our simulation, we first identify the minimum wildfire hazard cutoff that maximizes net social welfare. The regulator then requires mitigation by all homes facing wildfire risk higher than the cutoff.

Next, we consider building code policies where the regulator is required (for, say, reasons of political expediency) to apply the building code mandate to all homes within the same spatial units. In our simulations, we consider three possible spatial units: county, census block, and street. These increasingly fine spatial units reflect differing political and administrative capacities to set spatially-varying building codes. Note that because we limit the sample of homes in the simulation to those in census tracts with at least some minimal amount of fire risk, this policy does not require mitigation by homes in areas where there is zero plausible chance of a wildfire.

D.3.3 Mitigation Subsidies

Our final set of policies are subsidies for mitigation. In these policies, the homeowner is offered a subsidy s_i and then is free to make a voluntary mitigation choice. From the homeowner's perspective, the choice to adopt is now determined by:

$$1[\text{Mitigation}]_i = 1[\theta p_i \tau L + s_i > m]$$

The social planner then chooses the subsidy to maximize net social benefits (we assume the subsidy funded by a lump sum tax on all homeowners, and therefore neither adds to nor subtracts from overall welfare). We consider two types of subsidies: a fixed uniform subsidy offered to all homeowners who mitigate, and a subsidy that offers a fixed amount per square foot of the home.

Mathematically, we can write the optimal fixed uniform subsidy s^* as the solution to the following problem:

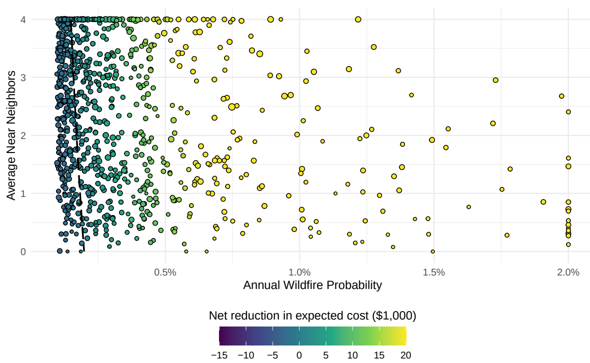
$$s^* = \arg\max_s \sum_i 1[\theta p_i L\tau + s > m]_i \times \left[p_i L(\tau + \delta_i \eta) - m\right]$$

The logic of the above expression is that the social planner wants to set a subsidy s that maximizes net benefits by encouraging homeowners who would not otherwise mitigate to do so. The drawback of this uniform subsidy is that it will also encourage some homeowners whose social net benefits are negative to adopt. Allcott and Taubinsky (2015) shows that, in the continuous case, the second-best optimal uniform subsidy equals the sum of the average undervaluation for consumers marginal to the subsidy and the average externality for consumers marginal to the subsidy. We calculate the per square foot subsidy similarly, but adjust for the size of the home.

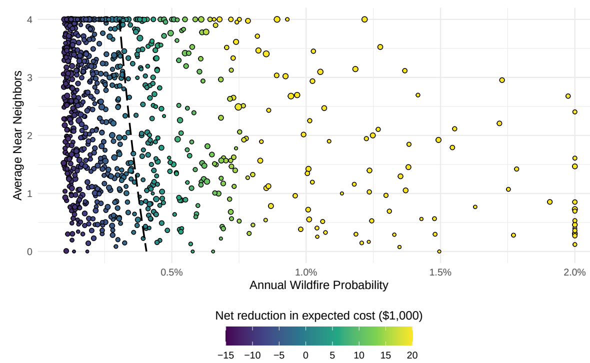
D.4 Alternative Cost-Effectiveness Estimates

Figure 6 presents average mitigation cost-effectiveness by census tract using the preferred mitigation cost estimate of \$15,660 from Headwaters Economics (2018). Appendix Figure D1 reproduces that figures using alternative scenarios with the alternative mitigation cost estimates given in Table 3.

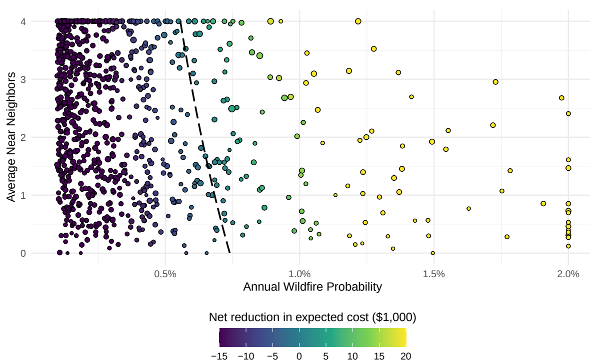
Figure D1: Mitigation Cost-Effectiveness by Fire Hazard and Number of Neighbors, Alternative Cost Scenarios



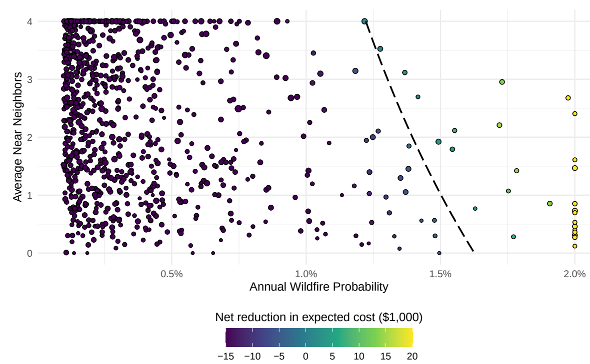
(a) \$7,634



(b) \$15,660 (Baseline)



(c) \$28,553



(d) \$62,760 (Retrofit)

D.5 Expected Utility and Cost Effectiveness

A more complete measure, *expected utility benefit*, accounts for the additional insurance benefits from mitigation due to the reduced probability of bearing uninsured losses. This section derives the expected utility benefit of universal mitigation relative to no mitigation, which is enough to identify the break-even wildfire hazards in Table 3.

We assume frictionless property insurance markets that offer coverage at actuarially fair annual premia. The coexistence of uninsured risk exposure and actuarially fair premiums reflects uninsurable losses (for example, mental and emotional distress) and/or household myopia.

Since insurance premiums are actuarially fair in the model, insurers break even by construction and utility differences are fully represented in household expected utility.[51] In the absence of mitigation investment, expected utility for a homeowner with utility function U and initial wealth (permanent income) w_0 is,

$$p_i q U(w_0 - L^N - k) + (1 - p_i q)U(w_0 - k) \tag{8}$$

Under a universal mitigation mandate, this same homeowner's private expected utility after undertaking mitigation is,

$$p_i(q - \tau)U(w_0 - L^N - \tilde{k} - m) + [1 - p_i(q - \tau - \delta\eta)]U(w_0 - \tilde{k} - m) \tag{9}$$

where $k = p_i L^I$ and $\tilde{k} = p_i(q - \tau - \delta\eta)L^I$, the actuarially fair insurance premia without and with mitigation. We express expected utility for the gambles in (8) and (9) in terms of certainty equivalents CE^0 and CE^M respectively. The net expected utility benefit across households can then be expressed as $\sum_{i=1}^{N} CE_i^M$ - CE_i^0, or the sum of households' willingness to pay for the mitigation gamble as opposed to the no-mitigation gamble.

Implementing the expected utility calculation requires strong assumptions about households' risk aversion, permanent income, ability to smooth across time periods, and other factors. Households have a constant relative risk aversion (CRRA) utility function $U(c) = \frac{c^{\gamma-1}}{\gamma-1}$. Permanent income is \$1,000,000. We further simplify the calculation of the expected utility measure by using a two-period model where households choose their mitigation investment at time 0 and then consume their wealth net of realized wildfire costs in period 1. To mirror the 40-year horizon of the risk-neutral cost effectiveness calculation, we discount period 1 costs using the mean of annual discount factors over 40 years ($\frac{1}{40}\sum_{t=1}^{40}\frac{1}{(1+0.05)^t}$). Future work could relax these simplifications by embedding our estimates into a life cycle model of consumption, savings, and mitigation which would capture savings behavior and differential ability to smooth losses according to age and other factors. Such a model goes well beyond the goal of the exercise in this section, which is to benchmark the degree to which uninsured loss exposure might affect the broad benefits of mitigation mandates.

51. Insurers are beginning to reward property-level mitigation, in part because regulators are beginning to require them to do so. The California Department of Insurance recently (October 2022, see here: https://www.latimes.com/california/story/2022-10-17/state-to-mandate-insurance-discounts-for-wildfire-mitigation) passed a regulator requiring insurers to provide discounts for homeowners who mitigate risk through structural hardening and/or the clearing of defensible space. We expect that competitive pressures and the availability of new technology to monitor home characteristics and predict loss risk will ensure that insurers should find it profitable to factor these investments into premiums.

D.6 Within vs. Between Variance in Wildfire Hazard

Appendix Table D3 helps explain the targeting benefit of granular building code areas. The table decomposes total variance in wildfire hazard p_i for homes in the data into between- and within-unit variances. At the county level, there is substantial within-unit variance, explaining the poor targeting properties of county-level standards. At the Census block group or street level, there is little within-unit variation in wildfire hazard.

Table D3: Variance Decomposition for Wildfire Risk

Grouping	Units	Non-singletons	Total	Within	Between	Within (%)
County	50	1,071,033	0.268	0.241	0.027	90.0
Census block	51,132	1,059,681	0.263	0.019	0.244	7.3
Street	72,968	1,059,745	0.262	0.017	0.245	6.6

Notes: Table showing distribution of within and between variation in wildfire risk for different levels of geographic aggregation. Wildfire risk is the product of burn probability and the likelihood of flame length exceeding four feet in the case of a fire. Grouping is the unit of geographic aggregation. Units are the number of unique units for the given grouping. Non-singletons are the number of non-single homes within each grouping (e.g., homes on streets where there are at least two total homes). Total is total variance of the wildfire risk variable among that set of homes, within is the variance within homes in the same grouping, and between is variance across groupings. Variances are multiplied by 10,000 for readability. Within (%) is the percent of within variation relative to the total.

D.7 Additional Welfare Results

This section expands on the findings in Section 5.

D.7.1 Private vs. External Benefits from Adaptation Policies

The total welfare gain from each policy is the sum of increased private benefits for adopters and increased external benefits. The first two rows of Table 4 in the main text compare welfare outcomes under no policy vs. an ideal standard or subsidy. Relative to the no-policy case, the ideal policy yields $908 M of additional private benefit for adopters and $326 M of additional spillover benefits. Spillover benefits are similarly important to the total welfare benefits of all of the policy scenarios considered in Table 4. At the same time, the presence of private risk misperception creates a role for policy interventions even in the absence of neighbor spillovers.

Appendix Table D4 shows welfare outcomes in a scenario where we assume zero spillover benefits. Eliminating spillover benefits reduces the first-best adoption rate from 29.2% to 26.6%. However, building code mandates still substantially improve welfare. An idealized building standard yields $929 million in welfare gains, all of which is private benefit to adopters. This private welfare gain is slightly larger than the $908 M in private welfare gains in Table 4 in the main text. The difference stems from the fact that when there are spillover benefits, the social planner mandates adoption for marginal homes where *total* net benefit is positive but *private* net benefit is negative. Thus, total welfare is higher but private gains are slightly lower than in the no-spillover case in Appendix Table D4.

Table D4: Welfare outcomes with no spillovers

Policy	Social Net Benefit ($M)	Private Net Benefit ($M)	External Benefit ($M)	Total Adoption Rate	Inefficient Adoption Rate	Fiscal Cost ($M)
Panel A. Benchmarks						
No policy	3,546	3,546	0	10.2	0.0	0
Perfect standard	4,475	4,475	0	26.6	0.0	0
Perfect subsidy	4,475	4,475	0	26.6	0.0	701
Panel B. Building Codes						
Hazard only	4,409	4,409	0	25.4	1.6	0
County	2,044	2,044	0	37.9	20.7	0
Census block	4,246	4,246	0	26.2	3.1	0
Street	4,292	4,292	0	26.1	2.8	0
Panel C. Subsidies						
Uniform	4,164	4,164	0	24.9	3.9	1,219
Per square foot	4,475	4,475	0	26.6	0.0	1,859

Notes: Table reports the same welfare calculations as in Table 4, except that spillover benefits of investment are assumed to be zero for all homes.

D.7.2 Building Codes Targeted on Marginal Net Benefit

Appendix Table D5 considers a more sophisticated regulator who accounts for voluntary takeup when deciding which spatial units should be subject to standards. The first three table rows are identical to the building codes scenarios in Table 4 in the main text. These calculations assume that the regulator imposes standards in all spatial units where the total net benefits of adoption are positive. The next three rows consider an alternative targeting rule where the regulator imposes standards in all spatial units where the marginal net benefits of the standard are positive. In other words, in these rows, the regulator accounts for inframarginal adopters who invest in mitigation even in the absence of the policy.

For the County level standards, such targeting improves total welfare by about $1.2 billion, since it substantially reduces the number of inefficient adopters resulting from the building code. For the street and Census Block level standards, this change slightly improves net benefit and reduces total adopters (since some streets or Census Blocks are no longer subject to the standards). At the same time, targeting codes on marginal benefit is more informationally-demanding, since it requires the regulator to know the misperception parameter θ.[52]

52. This second scenario appears more logical within the context of this model, but is perhaps less relevant to real world applications of wildland building codes, where takeup of mitigation technologies appears to be more related to idiosyncratic factors and less predictably related to individual wildfire risk.

Table D5: Building Codes Targeted on Average vs Marginal Benefit

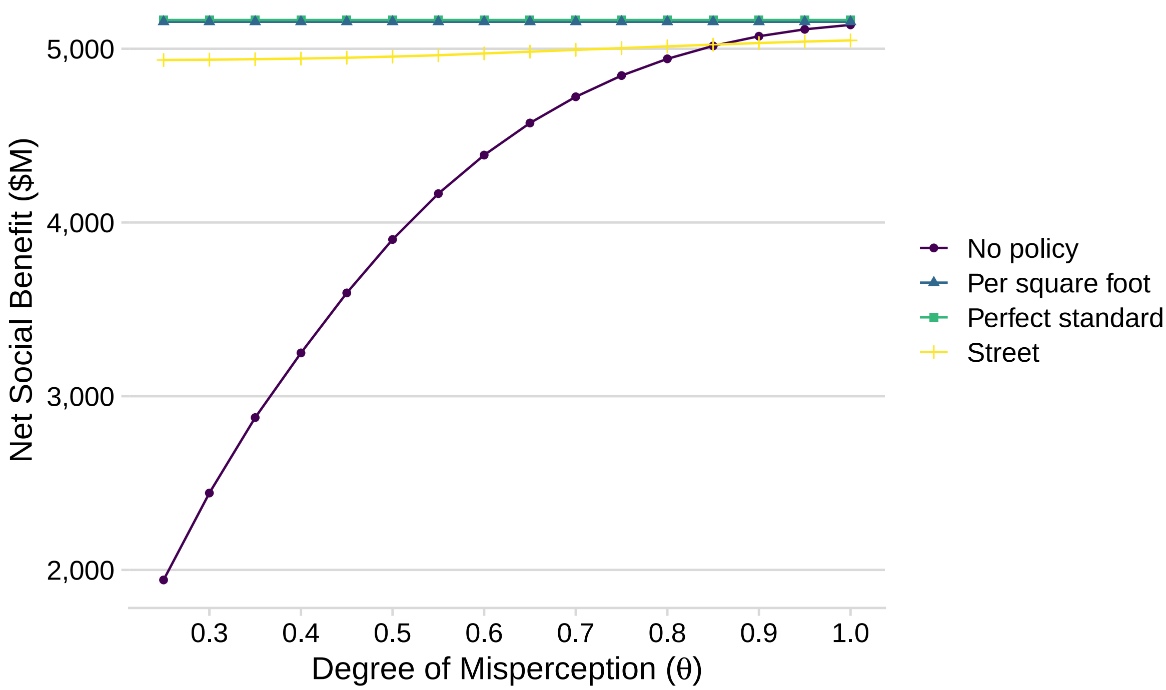
Policy	Social Net Benefit ($M)	Private Net Benefit ($M)	External Benefit ($M)	Total Adoption Rate	Inefficient Adoption Rate	Fiscal Cost ($M)
Panel A. Targeting Average Benefit						
County	2,628	2,044	584	37.9	19.7	0
Census block	4,849	4,219	629	28.9	3.5	0
Street	4,898	4,267	631	28.7	3.1	0
Panel B. Targeting Marginal Benefit						
County	3,897	3,550	348	12.7	1.1	0
Census block	4,869	4,249	620	28.1	3.0	0
Street	4,916	4,291	625	27.9	2.5	0

Notes: Rows 1–3 are copied from Table 4 in main text. Rows 4–6 consider analogous building code policies that account for inframarginal adopters when deciding where to impose codes.

D.7.3 Alternative Levels of Risk Misperception

Appendix Figure D2 shows total social net benefit under different policies, given various levels of risk misperception. Standards are preferred to no policy for $\theta < 0.95$. Standards and no policy yield similar welfare outcomes for $\theta \geq 0.95$. The welfare difference between standards and a uniform subsidy varies little with respect to θ (ignoring again the fiscal cost of subsidies and attendant cost of financing the transfers).

Figure D2: Policy Effects Under Varied θ



Notes: "Standard" scenario is a building code applied at the street level. Subsidy scenario is the second-best optimal uniform mitigation subsidy. Ideal adoption corresponds to adoption by all households with positive net social benefits of adoption (i.e., the same as the idealized differentiated subsidy).

D.7.4 Systematic Heterogeneity in Risk Misperception

This section allows for heterogeneity in private risk misperception that is correlated with wildfire risk. We explore a range of scenarios that vary the size and the sign of the covariance between risk misperception and house-level wildfire risk. In each case, we simulate a distribution of risk misperception by drawing the parameter θ_i for household i from a triangular distribution with lower limit $a = 0.2$, upper limit $b = 0.75$, and mode $c_i = a + \frac{a+b}{2} + \gamma \underline{p}_i$, where $\underline{p}_i = min(p_i, 1\%) - 0.5\%$. The parameter γ controls the relationship between risk misperception and fire risk (top-coded at 1% per year to minimize the influence of outliers). We explore values of γ from -50 to 50. When $\gamma = 50$, the mode of the risk misperception distribution ranges from 0.225 for the lowest-risk homes to 0.725 for the highest-risk homes. When $\gamma = -50$, this flips so that the lowest-risk homes have a mode of 0.725 and the highest-risk homes have a mode of 0.225. When $\gamma = 0$, the mode is 0.475 for all homes.

Appendix Table D6 shows the results. Panel A shows social net benefit. The largest effects of correlated risk misperception are for the no policy scenario. In the absence of corrective policies, welfare outcomes are particularly bad when high-hazard homes are assumed to be systematically more biased in terms of their beliefs about wildfire risk. This larger bias depresses private demand for hardening investments in the areas where they would have the highest return. In comparison, the welfare effects of a building code or a uniform subsidy are less dependent on the size and sign of correlated risk misperception.

Table D6: Correlated Risk Misperception

γ	No policy	Perfect standard	Building code (Hazard only)	Building code (Census block)	Subsidy (Per square foot)
Panel A. Social Net Benefit ($M)					
-50	3,066 (3.76)	5,106 (0.00)	5,025 (0.00)	4,844 (0.38)	4,893 (1.66)
-25	3,378 (3.54)	5,106 (0.00)	5,025 (0.00)	4,846 (0.34)	4,942 (1.05)
0	3,624 (3.09)	5,106 (0.00)	5,025 (0.00)	4,849 (0.34)	4,962 (0.89)
25	3,804 (2.79)	5,106 (0.00)	5,025 (0.00)	4,853 (0.37)	4,974 (0.83)
50	3,932 (2.71)	5,106 (0.00)	5,025 (0.00)	4,856 (0.35)	4,983 (0.66)
Panel B. Adoption Rate (%)					
-50	7.4 (0.01)	29.2 (0.00)	27.9 (0.00)	28.9 (0.00)	28.2 (0.08)
-25	8.5 (0.01)	29.2 (0.00)	27.9 (0.00)	28.9 (0.00)	28.5 (0.07)
0	9.6 (0.01)	29.2 (0.00)	27.9 (0.00)	28.9 (0.00)	28.6 (0.07)
25	10.5 (0.01)	29.2 (0.00)	27.9 (0.00)	29.0 (0.00)	28.7 (0.07)
50	11.2 (0.01)	29.2 (0.00)	27.9 (0.00)	29.0 (0.00)	28.8 (0.06)

Notes: Table shows means and standard deviations of policy outcomes from 200 simulations where individual risk mispercepation is systematically correlated with wildfire risk. In each simulation, individual risk misperception θ_i is drawn from a triangular distribution with lower limit 0.2 and upper limit 0.75. Panel A gives social net benefit, the total net benefits of all households under the given policy in millions of dollars, and Panel B reports on adoption rates under the given as a % of the overall population. The first column, γ, reports on the strength of the correlation between risk misperception and wildfire hazard. Each subsequent column is a different policy alternative from Table 4. No policy is the benchmark policy (no required mitigation). Perfect standard is a policy where the regulator requires mitigation by only the homes where their mitigation has positive social net benefits. Building code (Hazard only) is a policy where the regulator considers only individualized wildfire hazard and average costs of mitigation and expected losses to decide which homes should mitigate. Building code (Census block) is a policy where the regulator applies the building code requirement uniformly within each census block. Subsidy (per square foot) is the policy where the regulator offers the same per square foot subsidy to all homeowners if the mitigate.

E Software Used in Analysis

The empirical analysis in this paper makes use of the statistical software R (R Core Team 2023), the integrated development environment RStudio (Posit team 2024) and the following R packages:

- cowplot (Wilke 2024)
- data.table (Barrett et al. 2024)
- fixest (Berge 2024)
- fst (Klik 2022)
- kableExtra (Zhu 2024)
- mapview (Appelhans et al. 2023)
- marginaleffects (Arel-Bundock 2023)
- MatchIt (D. Ho et al. 2023)
- modelsummary (Arel-Bundock 2024)
- nngeo (Dorman 2023)
- osmdata (Padgham et al. 2023)
- qs (Ching 2023)
- raster (Hijmans 2023)
- scales (Wickham, Pedersen, and Seidel 2023)
- sf (Pebesma 2023)
- stringdist (van der Loo 2023)
- tidyverse (Wickham et al. 2019)
- viridis (Garnier 2024)

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