Induced development in risky locations: fire suppression and land use in the American West*

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DRAFT (not for citation or attribution)

June 22, 2012

* Kousky and Olmstead are Fellows at Resources for the Future (RFF). We gratefully acknowledge grant support from the Joint Fire Science Program (JFS 10-3-01-33) through the U.S. Forest Service (USFS), and we thank Krista Gebert at USFS for advice, Benjamin Sleeter at the U.S. Geological Survey for data, and Roger Sedjo at RFF for helpful input. Seminar participants at Harvard University, University of Pennsylvania, University of Maryland, University of California, San Diego, Colby College, Arizona State University, RFF, Virginia Tech, Pennsylvania State University, the Society for Benefit-Cost Analysis Annual Conference, and the 2011 AERE Summer Conference provided useful comments. All remaining errors are our own.

Abstract

We examine whether federal wildfire suppression in the United States induced land development in high-risk areas from 1970-2000. To identify the effects of suppression on development, we exploit a temporal shift in federal policy due to major 1988 fires in Yellowstone National Park, as well as spatial variation, since the impact of the shift varied regionally. Results suggest that federal fire suppression on public land has increased development nearby. The impacts we estimate are large in percentage terms, but small in absolute acreage. Reducing the likelihood of natural hazard occurrence causes unintended behavioral responses, which may increase future hazard exposure.

JEL Classification: Q23, Q24, Q28, Q54, H42

Keywords: land-use change, induced development, fire suppression, federal policy, subsidy, natural experiment

1. Introduction

Government policies that alter the relative returns to holding land in various uses change the distribution of those uses over the landscape. For example, two decades ago, economists demonstrated that much of the conversion of land from forested wetlands to agriculture in the Mississippi Delta region was attributable to federal flood control and drainage projects, which made marginal land profitable to convert, and intensified agricultural activity on land already converted (Stavins and Jaffe 1990). Such behavioral responses are often unintended and can significantly alter benefit-cost calculations for federal investments. While economists have long argued on theoretical grounds that disassociating the benefits and costs of locating in hazardous areas could spur inefficient levels of development, there is very little empirical work on whether federal investments in risk reduction induce more development in hazardous areas. Quantifying such a response is necessary for complete program evaluation.

This paper considers whether federal wildfire suppression in the western United States has increased development in areas benefiting from such efforts. Millions of American homeowners have developed new properties in the forests, grasslands, and shrublands of the American West in the past few decades. In 2000, 12.5 million U.S. homes were within the wildland/urban interface (WUI), suburban and exurban areas where homes and other structures mingle with fire-prone vegetation – a 52 percent increase over 1970. A majority of these (65 percent) are in what ecologists describe as high-severity fire regime classes (Theobald and Romme 2007). In California's San Diego County, alone, three out of four homes built since 1990 are within the WUI. Looking forward, U.S. land in developed uses is expected to increase by 70 million acres between 2003 and 2030, with the largest fraction converted from forests (Alig and Plantinga 2004). Little attention has been paid to the role of public policies at the federal or any other level in drawing people and structures into these regions, which have high amenity values but are also prone to wildfire.

We investigate whether federal fire suppression efforts have acted as an implicit subsidy, inducing development in risky areas. This implicit subsidy comes from the fact that, while the benefits of development in fire-prone regions are enjoyed by landowners, the costs of fire suppression, when fires occur, are borne by taxpayers at large.[1]

The federal government has played a significant role in wildland fire suppression since 1910.[2] Recent trends in public sector spending on fire suppression are striking. During the decade ending in 1980, the U.S. Forest Service (USFS), which receives about 70 percent of the funds appropriated by Congress for wildfire preparedness and operations, spent $340 million per year, on average, fighting fires; for the decade ending in 2005, expenditures averaged $685 million per year, with annual expenditures in three of those years exceeding $1 billion (Calkin *et al*. 2005).[3] A recent audit of USFS fire suppression costs suggests that the most significant cause of the increase in suppression expenditures is the agency's "efforts to protect private property in the wildland urban interface" (U.S. Department of Agriculture 2006). New development increases suppression costs because it is more difficult, dangerous, and costly to fight fires when people and structures must be protected.[4]

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[1]Landowners in fire-prone regions may grow to feel entitled to such subsidies. In the aftermath of the Station Fire north of Los Angeles in 2009, a homeowner quoted in the *Los Angeles Times*, in an article critical of cost-cutting at the U.S. Forest Service, notes: "If their main concern was balancing the budget instead of saving homes, there's something wrong with those priorities" (Pringle 2009).

[2] Many state governments also spend significant resources fighting wildfires. Annual emergency fire suppression expenditures made by the California Department of Forestry and Fire Protection (CalFIRE) averaged $51.5 million per year, 1979-1989; and $258 million per year, 1998-2008 – more than a fivefold increase in less than three decades. See http://www.fire.ca.gov/fire_protection/downloads/Summaryfirecosts05_06.pdf. For the 2006 fire season, the State of Wyoming budgeted $1.2 million for suppression and other expenditures, but spent $30 million (McKinley and Johnson 2007).

[3] All fire suppression expenditures are expressed in constant 2008 dollars.

[4] Some of the increase in suppression expenditures is also due to an observed increase in large western wildfires (incidence and acres burned), attributed by natural scientists to climate change, changes in grazing and logging

The devastating effects of wildfires in developed areas have drawn significant public attention in recent years. Most recently, the 2011 fires in Texas, fueled by the state's worst drought in a century, burned nearly 4 million acres of land, consumed more than 7,000 structures, and took several lives. The 1991 Oakland/Berkeley Hills fire, the worst in California's history, killed 25 people, injured 150, and caused an estimated $1.5 billion in other damages (Carle 2002). Large wildfires affecting residential areas are also common in other high-growth states such as Nevada, Arizona, New Mexico, Colorado, and Washington.[5] The subject is of international importance, as well. In February 2009, more than 200 people were killed when brushfires in Australia burned out of control through residential areas. Forest fires in southern Greece in summer 2007 killed 84 people. Russia and Israel also have experienced significant fires that intrude on residential areas, resulting in human mortality.

We conduct an econometric analysis of U.S. land-cover trends to estimate the impacts of fire suppression policies on land development, controlling for other factors. The methods hinge on our ability to identify the effects on development of changes in the intensity of public fire suppression activities. Our identification strategy exploits temporal and spatial variation in federal fire suppression efforts between 1970 and 2000, relying primarily on a natural experiment – a major change in federal fire suppression policy that took place as a result of severe fires affecting Yellowstone National Park in 1988. We draw on a database of land cover changes over four periods from the U. S. Geological Society (USGS) that economists have not yet employed for econometric analysis. Our results suggest that during periods when the federal government has intensified its expected suppression efforts on public lands, private development

practices, and the effect of these two forces in combination (Westerling et al. 2006, Johnston et al. 2009, Calkin et al. 2005, Prestemon et al. 2002).

[5] The 2010 fire season included Colorado's most expensive wildfire on record, the Fourmile fire, which destroyed 169 homes near Boulder, CO with total insurance claims of $217 million as of September 20, 2010.

has accelerated on nearby land that would benefit from that suppression. We find small impacts in terms of absolute acreage, but large impacts in percentage terms.

The next section places our paper in the context of related research. Section 3 discusses the relevant policy changes that we use in our identification strategy. Section 4 presents the data and Section 5 provides an overview of our models. Section 6 presents results. We discuss remaining identification concerns in Section 7 and policy implications in Section 8. Section 9 concludes.

2. Related literature

Our work on this issue is related to several questions in the economics literature. Fire suppression is an implicit public subsidy to local private land owners, driving a wedge between private benefits and costs in household and firm location decisions. Similarly, subsidized insurance and federally-funded risk reduction for natural hazards such as floods and droughts can disassociate benefits and costs and have been demonstrated to influence patterns of residential and agricultural land development. Subsidies that reduce crop insurance premium rates below actuarially fair levels may increase the amount of land farmers cultivate; agricultural disaster payments have a similar effect (Goodwin *et al*. 2004, Wu 1999). The theoretical possibility of induced development was an early concern regarding areas protected by federal flood control projects (Krutilla 1966), and empirical economic research suggests that such projects induced significant conversion of land from forested wetlands to agriculture in the Mississippi Delta region (Stavins and Jaffe 1990). Examination of coastal areas suggests that the availability of flood insurance through the National Flood Insurance Program may also spur development (Cross 1989; Cordes and Yezer 1998).

Another way to frame this problem starts from the position that fire suppression is a public good. It is non-excludable (within the affected region) and, to some extent, non-rival in consumption (though with a limited budget, suppression to protect residential and commercial development may displace suppression for other purposes, or suppression for the same purpose in other regions).[6] While a small literature models economically optimal public fire suppression effort (see, e.g., Prestemon *et al*. 2001, Yoder 2004, Mercer *et al*. 2007), we know of no prior research examining the potentially important link between suppression and development. An understanding of induced changes in development from suppression is necessary to fully evaluate the benefits and costs of fire management policies and to determine optimal levels of fire suppression.[7]

The problem of development in the fire-prone WUI is made even more critical by the anticipated impacts of climate change. Wildland fires are expected to increase in frequency and intensity, and policy responses to this change in extreme events must be chosen in light of the potential feedbacks between suppression and development. This issue is not just limited to wildfires. Other natural disasters, such as extreme precipitation, droughts, and possibly hurricanes are also predicted to be impacted by climate change (IPCC 2011). Many climate adaptation measures (such as seawalls and levees) may have unintended impacts similar to the land development changes we observe in response to fire suppression, drawing additional population and assets into harm's way. Lessons learned on the ties between land development

[6] A large private landowner may internalize the costs of wildfire risk by engaging in its own suppression activities (Lueck 2012). Small-scale landowners may, likewise, contract to share responsibility for suppression. Such contracts, like all Coasian solutions, are less likely to arise among a large number of small private landowners, or in any other case in which transaction costs are high. If fire suppression is an impure public good (offering some private benefit, as well as pure public benefits), then, in theory, federal suppression activities could either crowd in or crowd out private suppression (Kotchen 2005).

[7] If development responds positively to protection (as we examine empirically here) and government increases protection when there is more development, there could be increasing returns to both protection and development, and choosing protection levels ignoring this interaction could, in theory, lead to a suboptimal equilibrium (Kousky, Luttmer, and Zeckhauser 2006).

and public policy regarding fire risk could thus prove useful in thinking about disaster mitigation more broadly, both in terms of current conditions, in which significant populations are already at risk, and for natural hazard policy strategies going forward.

3. Policy background and identification strategy

Panel data on annual federal fire suppression expenditures are readily available (at least for the USFS, the agency in which the most significant expenditures occur). The simplest approach to testing our hypothesis might be to regress some measure of land development on suppression expenditures. However, this approach would generate biased estimates, due to the endogeneity of suppression expenditures. Development in the WUI increases fire incidence (Cardille *et al*. 2001), as well as the costs of suppression, conditional on fire occurrence, since firefighting is complicated by development (Gill and Stephens 2009). Thus, while our hypothesis is that suppression expenditures induce development, development also increases suppression expenditures. In addition, many of the factors that make a parcel of land fire prone also make that land appealing to develop (e.g., arid climate, forested landscape, and location on a high ridge). Thus, even in the absence of reverse causality, amenity values – unless perfectly controlled for in the estimation strategy – would be correlated with fire suppression and the unexplained portion of development conversion, biasing estimates.

Seeking an experimental solution to these sources of endogeneity, we turned to the history of federal fire policy. Between approximately 1910 and the late 1970s, public land management policy reflected the conventional wisdom that fire was a hazard that had to be stopped in all circumstances, lest it destroy valuable natural resources. Public service messages beginning in 1944 used “Smokey the Bear,” a character symbolizing the importance of fire prevention and suppression. From 1933 through 1978, federal agencies officially followed the so-called "10am policy": they attempted to extinguish any wildland fire, no matter how it started, by 10:00 on the morning after the fire was detected (Carle 2002). If they did not succeed, they continued their effort, with a new goal of extinguishing the fire by 10:00am on the following morning, continuing in this manner until the fire was out.

An important exception to this rule concerned fire-prone pine forests in southeastern states. Forest managers in the Southeast adopted the 10am policy along with the rest of the country in 1933. However, ecologists had recognized since the early 1900s that fire was an essential part of some forest and grassland ecosystems, including southern forests of longleaf, shortleaf, loblolly, and slash pine – these systems were not just fire-prone, but fire-dependent.[8] This point was controversial, and U.S. federal policy toward wildland fire did not, at first, reflect the emerging consensus that some fires should be allowed to burn, or, alternatively, agencies might use "prescribed burns" (setting controlled fires) to simulate the effects of natural fire, under conditions of reduced risk (Carle 2002, Gorte 2006). As such, rapid buildup of excess fuel in southern pine forests under the total suppression policy caused a series of catastrophic fires in the late 1930s and early 1940s. In 1943, southern forest managers seceded from the standard federal policy, a reversal referred to by fire management agencies as the "Treaty of Lake City," as the southern policy was finalized at the Ocala National Forest in Lake City, Florida (Carle 2002). USFS authorized controlled burning in national forests with longleaf and slash pine in August 1943, and by April 1946, 580,000 acres on southern federal land had experienced controlled burns under the revised policy (Carle 2002).

[8] One of the first published indications that extinguishing all wildland fires could cause ecological harm was a study suggesting that longleaf pine forests in the American Southeast could not survive without periodic fire, which promoted new seedlings and enabled the trees to compete with other species (Chapman 1932).

In the meantime, many decades passed during which the policy of total fire suppression remained in place on federal lands in the rest of the United States. New Deal programs over seven years around the Great Depression funded the construction of thousands of fire towers, almost 100,000 miles of fire roads and trails, and 4.1 million man-hours of fire suppression (Carle 2002).

Over time, however, research in the natural sciences produced overwhelming evidence of the downsides of total suppression. In particular, ecologist Harold Biswell's work from the 1940s through the early 1960s suggested that, where fires were suppressed over long periods in the ponderosa pine forests of California, the resulting fuel buildup generated far more destructive "crown fires," rather than the slow-burning understory fires that occurred pre-suppression (Biswell 1989). Several federal agencies, most notably the National Park Service and then USFS, eventually began experimenting with so-called "fire management" policies that allowed some "let burns" and prescribed burns. This shift took place slowly during the early and mid-1970s, and in 1978, total fire management became the policy on all federal lands, officially replacing the 10am policy, and bringing western forest management practices regarding suppression into alignment with southeastern practices (Carle 2002).

In the Summer and Fall of 1988, more than 1 million acres in Yellowstone National Park were affected by a set of massive fires. The largest of these fires was ignited by a carelessly tossed cigarette. There is wide scientific agreement that such major fires were important to the Yellowstone ecosystem, had occurred naturally every 200-400 years, and were overdue. Nonetheless, at least one of the major fires in Yellowstone was an escaped prescribed burn by park managers, and the public was outraged. A *New York Times* front-page headline on September 22, 1988 claimed: "Ethic of protecting land fueled Yellowstone's fires." Politicians followed the public outcry. Western Congressmen and Senators signed a petition to the president opposing all "let it burn" policies and asking for a permanent abandonment of this approach (Rothman 2005). The Yellowstone fires thus resulted in a backlash against the federal shift to fire management, with an immediate effect on policy.

While the fires were still burning, the National Park Service director stopped prescribed burning on all NPS lands (Rothman 2005). Right after the fires, the federal government established a moratorium on fire management, to remain in effect until each park or management area had revised its fire management plan to account for concerns that arose as a result of the Yellowstone fires (Elfring 1989). These actions essentially re-established total fire suppression as federal policy for a few years post-Yellowstone. As an example of the impact, prescribed burning by the National Park Service fell from 32,135 acres per year between 1983 and 1988 to 3,708 acres per year between 1990 and 1994. The effects of the Yellowstone fire waned slowly after 1989. By 1991, 11 out of 20 national parks had revised their plans and were back to fire management, which was again the dominant policy nationwide by the early to mid-1990s (Carle 2002).[9] The return to fire management in the early 1990s was finalized by a 1995 report noting that federal fire policy would shift from placing a priority on protecting structures to equally valuing private property and federal resources, with protecting life (including that of firefighters) as the highest goal (Gorte 2006). This is noteworthy for our analysis, in that the shift back to fire management was not only a shift away from suppression but a shift away from protecting property as a primary objective. In 2000, an escaped prescribed burn destroyed National Lab buildings and 239 homes in Los Alamos, New Mexico, but despite significant media coverage,

[9] The shift back to fire management was influenced by continued scientific evidence supporting a more pragmatic view of the ecological role of fire, as well as several high-profile firefighter deaths during suppression efforts during the 1994-1995 fire season.
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this did not result in a policy shift, the way the Yellowstone fires did in the late 1980s – the tables had finally turned, perhaps for good.

The temporary shift back to complete fire suppression due to the Yellowstone fires was focused in the west. Managers of fire-dependent land in the southeast departed from their western counterparts in their response to the policy change, as they had in the 1940s. This was due to many factors, including a more entrenched culture of fire management, less federal land, and different ecosystems. Burn statistics clearly show that the southeast was not affected by the temporary return to total suppression. Post-Yellowstone, many more acres burned in the Southeast compared with the West, and many more prescribed burns were undertaken. For example, in 1990, just over 2,000 acres of prescribed burns occurred in the West but over 70,000 acres were burned in the Southeast, despite the fact that federal land holdings in the west are vastly larger than in the southeast (Rothman 2005). As another indicator of the differing approaches and cultures, in 1990, Florida passed a bill authorizing and promoting the continued use of prescribed burning. Following its passage, seven other southeastern states passed similar laws.

This history of major changes in federal fire suppression policy suggests several potential natural experiments that might help to identify the impacts of suppression on development. First, if our hypothesis is correct, the drop in public fire suppression from the replacement of the 10am policy with total fire management in the 1970s, all else equal, may have slowed the rate of development on or near lands protected by federal fire suppression efforts. However, this first policy change, while potentially important from a welfare perspective, is simply not temporally "sharp" enough to exploit statistically. In addition, U.S. land-cover data prior to 1970 is not available on a large enough scale to support quantitative analysis of this shift.

Similarly, if fire suppression increased after the 1988 Yellowstone fires, our hypothesis would suggest an uptick in the rate of development, which would fall again when policy shifted again in the early 1990s. This development increase, however, should be limited to western states, as the southeast did experience a policy reversal back to total suppression, giving us an additional source of variation with which to test our hypothesis. The Yellowstone-related policy shift is also appealing for other reasons; it was sudden, unexpected, and plausibly exogenous to other trends in both land development and federal policy. It also occurred in the middle of the available panel data on U.S. land cover. It is, thus, an ideal candidate for a natural experiment, and we exploit it for this purpose.

Finally, our statistical approach exploits one additional source of spatial variation in the likely effects of federal fire policy shifts. Land closer to federal lands affected by fire suppression (those within the "umbrella of fire suppression efforts) should have experienced a more significant change in development incentives due to federal policy shifts. Not all federal land is equally affected by the federal suppression effort, however. Five federal agencies receive funds for fire suppression activities: the USFS, National Park Service (NPS), Bureau of Land Management (BLM), Fish and Wildlife Service, and Bureau of Indian Affairs. The most significant federal actors in managing and suppressing wildfire are the first three (USFS, NPS, and BLM); USFS, alone, receives 70 percent of Congressional appropriations for wildfire preparedness and operations.

How close must a parcel of land be to land managed by one of these agencies to benefit from suppression activities, should a fire occur? The maximum distance a firebrand (a piece of very light burning material that could ignite a fire) can fly ahead of a fire front is 2.4 km (California Fire Alliance 2001). Thus, at a minimum, private land within 2.4 km of USFS, NPS, and BLM lands would receive significant protection from fire contagion, even if federal fire suppression efforts were exerted only on public lands. In reality, federal firefighting effort often "chases" fires as they move from public to private land, so the umbrella of protection from fire contagion may actually stretch much further.

4. Data

Since our focus is development in fire-prone regions near federal lands, we obtained a GIS layer of federal lands from the U.S. National Atlas.[10] These lands are mapped in Figure 1. We used these data to identify the holdings of the three agencies that comprise the vast majority of federal expenditures on fire suppression, BLM, USFS, and NPS. Federal land holdings vary dramatically between the eastern and western United States. A substantial majority of western land is owned and managed by federal agencies; federal land as a percentage of total land area is almost 85 percent in Nevada, more than 50 percent in Utah, Oregon, and Idaho, and between 40 and 50 percent in California, Arizona, Wyoming, and New Mexico. In contrast, eastern states have very little federal land, and most of that is managed by the USFS.

We obtained land cover data from the USGS, which has recently developed the Land Cover Trends Database, in cooperation with the U.S. Environmental Protection Agency.[11] Although natural science and geography research has made use of these data, to our knowledge, they have not previously been used for economic or policy analysis. The full data comprise a set of randomly-selected sample blocks with an area of 100 km^2 (about 24,711 acres) from across

[10] See http://www.nationalatlas.gov/.
[11] We thank Benjamin Sleeter at the USGS for all of his assistance in obtaining these data. See Loveland *et al.* (2002) and Stehman *et al.* (2003) for a description and more information on the Land Cover Trends project. Additional information is available from the project's website: http://landcovertrends.usgs.gov.

the United States.[12] These 10 km by 10 km sample blocks are our spatial unit of observation, which we call "parcels" for the remainder of the paper. We combine these data with the National Atlas data on the location of federal lands. The parcels sampled for the full USGS Land Cover Trends database are depicted as gray squares in Figure 2.

Each parcel includes approximately 27,889 observed pixels (each pixel is 60m x 60m). Though we know the precise location of each parcel, we do not know the location of each pixel within a parcel. Instead, we observe the number of pixels per parcel in each of the following land cover types: water, developed, mechanically disturbed, barren, forest, grassland/shrubland, agriculture, wetland, nonmechanically disturbed, and snow/ice.[13] The "developed" category includes land uses that are not strictly urban, such as low-density residential development and other less intensive uses where both vegetation and structures are present. The inclusion of such low density development is critical for our analysis, as it is typical of the WUI.[14] Satellite data and historical aerial photographs were used to determine land use changes for five periods: 1973-80, 1980-86, 1986-92, and 1992-2000. The fact the periods of observation are not of equal length (they range from six to eight years) will require that we control for the number years in each period in the analysis.

We do not use all of the parcels identified in Figure 2 in our statistical analysis. First, we define "western" parcels as those west of the 100th Meridian (Figure 3) which, in the United

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[12] Sample blocks were chosen using probability sampling based on ecoregions, or areas of similar ecosystems, with 30-40 sample blocks drawn from each of the 84 Level III Ecoregions in the lower 48 states. Level III is the third of four levels defined by a particular classification regime, the Omernik ecoregion system, which considers the spatial patterns of both the living and non-living components of the region, such as geology, physiography, vegetation, climate, soils, land use, wildlife, water quality, and hydrology. Level III is the most detailed level available nationally for this system of ecoregions.

[13] The fact that we can identify the precise location of each parcel makes these data different from other land-use data typically used by economists. For example, the U.S. Department of Agriculture's Natural Resources Inventory allows researchers to reference the county in which a parcel of land is located, but finer spatial location information is not available.

[14] In other federal land cover datasets, the related land use category is "urban," which would not include very low-density development.

States, roughly marks the boundary west of which few locations receive 20 inches of rainfall or more per year. It is also known as the “20-inch line” and is commonly considered the cartographic boundary between the arid west and the humid east. For models that investigate differential impacts of changes in suppression policy in the West vs. the Southeast we define “southeastern” parcels as those located in fire-prone ecoregions (areas of similar ecosystems mapped by the U.S. Environmental Protection Agency) of the southeast United States – those ecoregions dominated by fire dependent species such as longleaf, shortleaf, loblolly and slash pine. Figure 3 shades these ecoregions in red.[15] We also identify any parcel that is completely within federal land and drop it from our analysis, as private development is unlikely to occur or would be governed by different processes than we are examining (as an exception, we do not drop parcels on Bureau of Indian Affairs land, since private development does occur on Native American reservations and should be included in our analysis).

After restricting our sample to the West and Southeast, and dropping parcels entirely in federal land, we are left with 1053 parcels over 4 periods of observation, creating a panel of 4212 observations in models that include the full sample. We also use the federal lands data to identify parcels that have any portion within 2.4 km of BLM, NPS, or FS lands, or those within the “umbrella” of federal suppression activity, in accordance with our earlier discussions (as well as 1.2 km and 4.8 km as robustness checks).[16] Finally, we use a GIS layer of state boundaries from the US Census to control for non-time-varying state characteristics.[17]

We next needed to identify the relevant fire suppression policy that was in place – total suppression or fire management – during each of the time periods in the data. To do so, it is

[15] Figure 2 also shows that there are a handful of 20km by 20km parcels in the full USGS sample. These are not included in our analysis; the sampling regime was changed to 10km by 10km after these were already analyzed.
[16] This includes parcels that are partially within federal land.
[17] For parcels that cross state boundaries, we assign the state that contains the centroid of the parcel.

useful to refer to a timeline (Figure 4) that depicts the four periods in our data, along with the fire policy shifts essential to our identification strategy, described in Section 2. Two periods in the data (1980-1986, and 1992-2000) are clearly periods in which the relevant policy is fire management. During the first period, 1973-1980, the federal government was shifting toward the fire management policy, and away from total suppression. While the official switch took place with an announcement at the USFS headquarters in 1978, several sources note that it was implemented on the ground significantly earlier (Carle 2002, Gorte 2006). We identify this first period as fire management, but the fact that federal policy was in flux at this time leads us to also estimate more flexible models as a robustness check, discussed further in Section 5. The period 1986-1992 contains the Yellowstone event, and it is the only period in the sample for which fire suppression is the dominant federal policy. Since this period, like the first period, actually represents a mix of the two policies (though total suppression was the predominant policy for most of the period), this provides an additional reason for estimating more flexible models as a robustness check.

Summary statistics are reported in Table 1. The average amount of land converted to "developed" from forest, grassland and shrubland per parcel-period (D_{it}) is small – about 39 pixels (34.7 acres) in the West, and almost 85 pixels (75.7 acres) in the Southeast. Though conversion varies significantly over time and space, zeros predominate in the data for our dependent variable. As the second row of Table 1 indicates, only about 24 percent of the parcel-period observations in the west and about 50 percent in the southeast have any conversion. In 1973, there were about 716 pixels developed per parcel, on average. By 2000, this had risen to about 946 developed pixels per parcel, or 3.3 percent of the average parcel area.

The length of an observation period (*years in period*) ranges from six to eight years. The variable *base development* describes the number of pixels in developed uses at the beginning of a period, which is about 800, on average, for the full panel. The variable *nearfed* is set equal to one for parcels with some portion within 2.4 kilometers of land managed by the USFS, NPS, or BLM – our indicator for being within the federal fire suppression umbrella. Of the western parcels, 64 percent have some portion within 2.4 kilometers of land managed by these agencies, whereas in the Southeast only 23 percent do.[18] The *firemgt* variable is set equal to one in three out of four periods in the sample – all but the Yellowstone period (1986-1992). Table 1 also demonstrates that the parcels dropped from the full sample because they were completely within federal land represented about 20 percent of the available USGS sample parcels west of the 100th Meridian or within the red-shaded southern pine forest ecoregions in Figure 3.

Finally, sample parcels lie within many different states (Table 2), but for the western sample, about two-thirds of parcels are contained in the top five states: California, Oregon, Washington, Idaho, and Arizona. The 100th Meridian crosses three states (Kansas, Oklahoma, and South Dakota) in which too few parcels lie west of this marker to allow the estimation of a state fixed effect in the econometric models, 34 parcels in these states are dropped from the western sample. In the southeast, Arkansas, Florida, and Alabama have the greatest number of parcels. There are three southeastern states (North Carolina, South Carolina and Maryland) that do not have enough parcels sampled within southern pine forest ecoregions to allow the inclusion of a state fixed effect, thus 13 parcels in these states are dropped from the southeastern sample. When we combine the western and southeastern parcels in the full-sample models, we are able to

[18] The proportion with 2.4 kilometers of land managed by each agency add up to more than 0.79 because some parcels lie within this distance of land owned by more than one agency.

add Oklahoma back in, since it has some parcels in each geographic group, though almost all of them (18 out of 20) are in the southeastern sample.

5. Econometric models

We estimate a set of panel data models to identify the effects of federal fire suppression policy on land development, at first restricting the analysis to the 877 western parcels in the data. The basic model is Eq. (1), in which D_{it} is the total number of pixels of land converted to developed uses in parcel *i* between period *t-1* and period *t*, on land cover at risk of fire (forest, grassland, and shrubland).

$$D_{it} = \beta_1 yrs\_in\_period_t + \beta_2 base\_devel_{it} + \beta_3 base\_devel_{it}^2 + \beta_4 nearfed_i + \beta_5 firemgt_t + \beta_6 (nearfed_i * firemgt_t) + \beta_7 y_t + \sum_{s=1}^{S} \theta_s s_s + \sum_{s=1}^{S} (\omega_s \, s_s * y_t) + u_i + \varepsilon_{it} \qquad (1)$$

We control for the length of the observation period ($yrs\_in\_period_t$), and a quadratic function of the amount of development in the parcel at the beginning of each period ($base\_devel_{it}$). The linear term accounts for the possibility that some development draws more development in the parcel, so we expect $\beta_2>0$. Since parcels are limited in size, eventually development will approach the maximum possible level; this, along with possible negative congestion externalities (for households locating in the WUI who may be motivated in part by the rural feel of such parcels), suggests $\beta_3<0$. We control for state heterogeneity in land development policies and general economic growth using a set of state fixed effects (*s*), a time trend (y_t), and interactions of state fixed effects and the time trend. The error term includes both

u_i (a fixed effect in most models) to capture heterogeneity among land parcels, and the standard econometric error term, ε_{it}. When u_i is a fixed effect, we cannot identify β_4 or θ.

The independent variable of greatest interest is the interaction between $nearfed_i$ and $firemgt_t$. Our main hypothesis suggests $\beta_6<0$; all else equal, when federal fire management policy is in place (relative to a policy of total suppression), there will be less conversion to developed uses on fire-prone parcels near federal lands. We also control independently for each of the two halves of this interaction term. Including $nearfed_i$ will account for any direct impact on land development incentives of being close to federal land (in models where this variable is not absorbed by a parcel FE), and the same is true for changes in fire suppression policy through inclusion of $firemgt_t$.

Since we are looking at land-use change at a relatively fine spatial scale, there is significant censoring at zero – no conversion to developed uses takes place during a particular period on many parcels. Thus, models other than ordinary least squares (OLS) will likely be better fits for our data. In our case, the dependent variable zeros are actual zeros, not non-observable responses, so a censored regression model is one reasonable choice. We estimate a trimmed least absolute deviations censored regression model with fixed effects due to Honoré (1992).[19] Development conversion can also be modeled as a count process; our dependent variable (pixels that convert to development) is a non-negative integer-valued count, left-skewed, with a large proportion of zeros and a long right tail. For the count-data approach, we estimate a Poisson fixed-effects model (Hausman *et al.* 1984). A two-part hurdle model pairing a linear probability model on the 0/1 conversion probability with an OLS model for the positive counts could be appropriate if the decision to convert land to development is not influenced by the choice of how

[19] This estimator is somewhat unsatisfying, as it cannot be used to obtain estimates of marginal effects. These aspects of Tobit-type models in the panel setting are frequently discussed (Deaton 1997, Angrist and Pischke 2009). Lacking a better solution, we include the panel censored regression models for comparison.

much to convert (Jones 1998).[20] In our opinion, this is unlikely – the choices are likely simultaneous rather than sequential. However, we include the two-part model for the sake of comparison and completeness. Thus, our approach will supplement the simple linear model described in Eq. (1) with censored regression, Poisson, and hurdle models, all with the same basic specification of the relationship between the dependent and independent variables. Because of its ability to include a parcel fixed effect and estimate robust standard errors, we will generally interpret the Poisson model results as the main results.

We also estimate a second set of models using a more flexible approach. To do this, we use an indicator variable for each period (excluding the Yellowstone period, representing total fire suppression), and interact each of these with $nearfed_i$, as in Eq. (2). The coefficients of interest in these models are the σ_t. If all of the coefficient estimates σ_t are negative, this would be consistent with our having correctly classified each period as either a fire management period, or a total suppression period, in estimating models of the form in Eq. (1). In addition to allowing us to make this comparison, the more flexible approach reveals any differences in the magnitude of the effect of federal policy shifts across periods, relative to the Yellowstone period. As for Eq. 1, we also estimate censored regression, Poisson, and hurdle models for this more flexible specification.

$$D_{it} = \beta_1 yrs\_in\_period_t + \beta_2 base\_devel_{it} + \beta_3 base\_devel_{it}^2 + \beta_4 nearfed_i + \sum_{t=1}^{T}(\sigma_t t_t * nearfed_i) + \beta_5 y_t + \sum_{s=1}^{S}\theta_s s_s + \sum_{s=1}^{S}(\omega_s s_s * y_t) + u_i + \varepsilon_{it} \qquad (2)$$

[20] A probit model is another obvious choice for the first stage of a two-part hurdle model, but this would not allow for the inclusion of a parcel fixed effect.

Finally, we incorporate the southeastern parcels in the data as an additional test of our hypothesis. To do this we define a new variable, SE_i, which is set equal to one if the parcel is located in one of our southeast pine forest ecoregions shaded in red in Figure 3. We then estimate the base models as in Eq. (1), including the non-linear models described above, adding a triple-difference term that interacts $firemgt_t$, $nearfed_i$, and SE_i . We do the same for the σ_t terms, $nearfed_i$, and SE_i for the Eq. (2) models. This final approach is essentially a set of falsification tests, and for the results to be consistent with our main hypothesis, the coefficients on the three-way interaction terms should be statistically insignificant (while those on the double interactions representing the western parcels remain significant and negative), since the temporary policy switch due to the Yellowstone fires did not influence the management of forests in the Southeast as it did in the West.

6. Results

The first results we report are for the models described in Eq. (1) and Eq. (2), estimated only on the western parcels in our sample. As an additional test of our main hypothesis, we then add in the southeastern parcels, looking for a differential impact of the shifts in federal fire suppression policy on these two regions.

6.1 Models estimated for parcels west of the 100th Meridian

Table 3 reports results from estimating Eq. (1) for the western parcels. The dependent variable is the number of pixels per year converted to developed uses from forest, grassland, and shrubland since the previous period. While useful as a starting point, the linear models in Table 3 do not account for the fact that D_{it} is most often equal to zero and strongly right-skewed, and so this is not our preferred specification. In column (1), u_i is a fixed effect, and it is a random effect in column (2). Standard errors are robust and clustered by parcel. The coefficient estimate of greatest interest is *nearfed*firemgt*. Our hypothesis suggests that this coefficient should be negative – all else equal, conversion to residential and commercial uses from fire-prone land cover near federal lands should be slower when the predominant federal policy is one of fire management, allowing some natural fires to burn and also using prescribed burning, rather than total fire suppression. In both models, the estimated coefficient for this variable is negative, but not statistically significant.

Given the aspects of the dependent variable discussed in Section 5, we estimate several non-linear models. Table 4 presents the results of a Tobit random-effects model with state fixed effects (col. 1) and a parcel fixed-effects censored regression model (col. 2), both of which use two censoring limits: a lower limit of zero, and an upper limit of the total parcel size (27,889 pixels).[21] Table 4 also presents a Poisson parcel fixed-effects model (col. 3), which drops any parcels with zero conversion through the whole time series, shrinking the sample from 877 to 314 parcels. Columns 4 and 5 together report the results of a hurdle model, first a linear probability FE model in which the independent variables are regressed on the probability of any development conversion in a parcel-period, and second, a linear FE model estimated only on the positive counts. The panel FE model in column 5 drops all parcel-periods with zero conversion (thus the number of parcels, 314, is the same as in column 3, but any zero conversion periods for

[21] For the fixed-effects model, we use pantob, a Stata module developed by Bo Honoré at Princeton University for estimation of panel data censored regression models (see: http://www.princeton.edu/~honore/stata/index.html).

these parcels are also dropped, further reducing the number of observations). Standard errors are robust and clustered by parcel in columns 3, 4, and 5.[22]

The Table 4 results provide some initial support for our main hypothesis. The main coefficient of interest, *nearfed*firemgt*, is negative in all models, though only statistically significant in the Poisson model.[23] The estimated marginal effects from the Table 4 models are reported in the first row of Table 6. The marginal effect of *nearfed*firemgt* on development conversion from the Tobit model in column 1 is -6.49 pixels per parcel, about a 17 percent reduction in mean conversion on western parcels between 1970 and 2000, for the change in the unconditional expected value of development conversion. Conditional on D_{it} being uncensored ($0 < D_{it} < 27{,}889$), the marginal effect of *nearfed*firemgt* from column 1 is -8.41 pixels, a 22 percent reduction from the mean. The computation of marginal effects from the censored regression FE model in column 2 of Table 4 would depend on the unobserved parcel fixed effects, which the pantob estimator strips away (Honoré 2008). Although it is computationally possible to recover marginal effects from this model, they would not be comparable to those from the Tobit RE model, or the others we estimate, and they would not be statistically significant (given the size of the standard errors estimated for this model), so we do not compute them.

The weakly significant Poisson FE results suggest a larger effect than the Tobit RE model – parcels located near the federal fire suppression "umbrella" experienced about 32 percent less development conversion when fire management was the predominant policy (from 1973-1986,

[22] For the Poisson fixed effect model in column (3), we use xtpqml, a Stata module to estimate fixed-effects Poisson (quasi-ML) regression with robust standard errors, developed by Tim Simcoe at Boston College (see: http://ideas.repec.org/c/boc/bocode/s456821.html).

[23] The negative binomial fixed effects model, relaxing the Poisson's assumption that the conditional mean is equal to the conditional variance, is another alternative. This model does not converge for our sample. In any case, by estimating the Poisson model with parcel fixed effects, we have removed the most significant source of overdispersion — underlying heterogeneity.

and 1992-2000), relative to the Yellowstone period (1986-1992) with its stronger emphasis on fire suppression. The two-part model suggests no impact of the shift in fire suppression policy on the probability of development near federal land, and a statistically insignificant conditional marginal effect: a decrease of 73 pixels per acre, or about 44 percent of mean conversion conditional on some development (165 pixels).

It may be that our classification of each period as either a purely fire management period or a total suppression period has introduced some bias, since two of the four periods in the data include, at least to a small degree, a mix of the two policies. Table 5 reports results from estimating the more flexible model in equation (2) for the western parcels, where we allow each period to enter the equation independently, interacted with *nearfed*, rather than using our *firemgt* variable. The omitted period is 1986-1992, the Yellowstone period, characterized by a very significant shift back to total suppression. The periods 1980-1986 and 1992-2000 are periods in which fire management is the predominant federal policy. During 1973-1980, fire management is in ascendancy, but may not be the predominant policy in the very earliest years of the period. If our hypothesis is correct, and we have classified these periods correctly in our earlier models, then the coefficients on all of the included interactions between time periods and proximity to federal lands (σ_t from Eq. 2) should be negative.

Like Table 4, Table 5 reports results from panel censored regression and Poisson models, as well as a two-part hurdle model, and standard errors are robust and clustered by parcel in columns 3-5. Results in Table 5 are strongly consistent with our hypothesis. First, the coefficients on all three interactions between our observed time periods and proximity to federal lands are negative, in all of the models reported in Table 5. In the Tobit RE and Poisson models, the effects of the fire policy shift are significant for the latter two periods. The fact that the period 1973-1980 was one in which the total fire management policy was introduced could explain the observed weaker effect of this interaction, in comparison to the other periods. The last period is also negative and significant in both parts of the hurdle model.

Estimated marginal effects for the Table 5 models are presented in rows 2-4 of Table 6 (again, with the exception of the censored regression FE model). When we estimate marginal effects of the *nearfed* variables from the Tobit model coefficients in column 1, the effects are: -8.46 pixels for 1973-80 (a 23 percent reduction from the average conversion in that period); -6.76 pixels in 1980-86 (24 percent), and -14.56 pixels in 1992-2000 (30 percent), for the change in the unconditional expected value of development conversion. Conditional on D_{it} being uncensored ($0 < D_{it} < 27,889$), the marginal effects from the Tobit model in column 1 are: -11.58 in 1973-80 (31 percent of mean conversion); -9.14 pixels in 1980-86 (33 percent); and -20.91 in 1992-2000 (43 percent).

The Poisson model results suggest parcels experienced 40 percent less development in1980-1986, and 39 percent less in 1992-2000, relative to the total fire suppression period. In the linear probability model (column 4) and the conditional linear model (col. 5), we find significant reductions in the probability of development and the amount of conversion, respectively, in 1992-2000, relative to the Yellowstone period. The estimated marginal effect in the conditional linear model in column 5 is quite large. Mean conversion in the column 5 sample (the 825 parcel-periods with some conversion) is 165 pixels. Thus, the coefficient estimate on *nearfed*1992-2000* in this final model suggests that, conditional on any conversion, land near the federal suppression umbrella experienced an 86 percent reduction in conversion under fire management, relative to total suppression.

The sign of estimated coefficients on the variables of interest is consistently negative in the models reported in Tables 4 and 5, and these coefficients are statistically significant in many models. The model best-suited to our data is the Poisson, since it best accommodates the distribution of the dependent variable, it can include a parcel fixed effect, and because we are able to estimate robust standard errors. The censored regression model with parcel FEs would be an appealing alternative, but for the facts that: (1) marginal effects, while they can be estimated, cannot be compared to the other models, significantly hindering interpretation; and (2) standard errors are not robust. Nonetheless, the results would be bolstered by statistically significant estimates from this model, which we do not obtain. However, this estimator generates very large standard errors for all coefficients in these models – not just those that test our main hypothesis.

The remaining coefficient estimates in Tables 4 and 5 are largely in line with our expectations. The longer the period of observation, the greater the amount of development conversion that takes place. The significance of *base development* varies. Once we control for the existing level of development and the other covariates, the impact on development of being near federal land, all else equal, is potentially still positive, although not significant in Table 4, and we can only identify this coefficient in the Tobit RE model. Finally, the Poisson estimates in Table 4 indicate a weakly significant independent effect of the shift in fire policy (*firemgt*) on development conversion, suggesting that the policy shift may have had some residual impact outside of the 2.4-kilometer distance from federal lands that we have defined as the "suppression umbrella." As a result, we implemented a robustness check of our definition of $nearfed_i$, testing distances of both 1.2 km and 4.2 km. Results in Table 4 and 5 are robust to all three definitions of being "near" federal land. Results using a definition of 1.2 km tend to be slightly larger in magnitude and for some specifications, more highly significant, suggesting stronger development impacts nearer to federal land. All estimates that are statistically significant in Tables 4 and 5 remain so in both robustness checks.

6.2 Full-sample models testing for regional difference in policy impact

We next turn to a further test of our hypothesis by including the southeastern pine forest parcels in our sample, increasing the number of parcel-periods from 3508 to 4212. Beginning with the models that define each period as one in which either fire management or total suppression is the dominant policy, Table 7 presents results of specifications similar to those reported in Table 4, only now we are interested in both $nearfed_i*firemgt_t$, and the three-way interaction between $firemgt_t$, $nearfed_i$, and SE_i. As was true when we estimated Eq. (1) for the western parcels only, there are a mix of statistically significant and insignificant estimates for $nearfed_i*firemgt_t$ in Table 7, though perhaps because the sample size has increased significantly, three of the estimates are significant, rather than one as in Table 4. In contrast, the triple interaction is statistically insignificant in all models.

Marginal effects for the Table 7 models are presented in the first two rows of Table 9. For all of the models for which marginal effects can be estimated, except the linear probability model, we estimate a statistically significant marginal effect of the policy shift on development conversion in the West. Like the coefficient estimates, the marginal effects for the three-way interaction are statistically insignificant in all models. Unlike in the West, as discussed in Section 3, fire suppression policy was relatively consistent between 1970 and 2000 in the Southeast. Our results clearly suggest that the policy shifts we use to identify the impacts of suppression on development in the West had no impact on development conversion near federal land characterized by southern pine forest.

In Table 8 we estimate the more flexible model in Eq. (2), using the full western and southeastern sample of 4212 parcels. Marginal effects are reported in rows 3-8 of Table 9, and the results provide additional strong support for our hypothesis. The estimates for $nearfed_i$ interacted with each of the non-Yellowstone periods are negative and statistically significant in the Tobit RE model, and estimates are negative and significant for the last two periods in the Poisson model (col. 3) and the conditional panel FE model (col. 5). Again, we might expect a somewhat weaker contrast between 1973-1980 and the Yellowstone period, since federal agencies were shifting away from total suppression toward fire management during this initial period, so it is not surprising that the estimates for this first period are negative but statistically insignificant in some models. As in the Table 6 models for the western sample alone, the federal fire suppression policy shift may even have had an impact on the probability of conversion (col. 4) in the most recent period, 1992-2000. In contrast, the three way interaction terms between each of the non-Yellowstone land cover observation periods, $nearfed_i$, and SE_i, are almost never statistically significant. The single exception is a *positive* and weakly significant coefficient for the period 1992-2000 in the Poisson model (col. 3, last row).

The magnitudes of the fire suppression policy impacts reported in Table 9 are consistent with those reported in the discussion of Table 6 in Section 6.1. Though the magnitudes of the marginal effect estimates, themselves, are somewhat larger (with the exception of the estimate for the conditional-on-conversion model in col. 5, for 1992-2000), the average amount of conversion overall and within each period is larger as well, when we include the southeastern parcels in the sample. For example, the Poisson marginal effect in row 1 of Table 6 (-12.45) suggests that the shift in federal fire suppression policy from the Yellowstone fires caused a 32 percent decrease relative to mean conversion in the West (38.91 pixels) between 1970 and 2000.

The Poisson marginal effect in row 1 of Table 9 (-16.25) suggests that the policy shift caused a 35 percent decrease relative to mean conversion in the full sample (46.43 pixels).

7. Potential remaining concerns

The results reported in Section 6 support our hypothesis that federal fire suppression between 1970 and 2000 induced some development in areas where fire is a significant risk. However, it is useful to consider some other potential explanations for our results. First, recall that our statistical results hinge on the temporary federal fire suppression policy shift resulting from the 1988 Yellowstone fires. Landowners in the West (and the rest of the United States) would have been well aware of these fires, given the significant media coverage – likely even more aware of the fires than of any fire suppression policy shift that may have resulted from their occurrence, although the temporary switch back to suppression and the outrage about fire management was documented in major newspapers. It is possible, however, that the Yellowstone fires, themselves, could have heightened landowner awareness of fire risk, and shifted development patterns.

This potential confounder does not jeopardize our results, however, since it would tend to attenuate our coefficient estimates, particularly for 1973-1980 and 1980-1986. Heightened awareness of fire risk would raise the expected cost of residential and commercial development in fire-prone regions, slowing conversion during the Yellowstone period, relative to the surrounding periods. If there is a "Yellowstone" effect of fire awareness in the data that persists after 1992, this could partially explain the negative coefficients we estimate for 1992-2000 in Table 5. The Table 8 results for the full sample, suggest, however, that any higher awareness of fire risk that might have reduced development in the West was not significant enough in the Southeast to decrease development near federal lands in that region.

A second concern is that our results could be confounded by general trends in the housing market, if those trends are not sufficiently controlled for in the estimation. All of the models include parcel and/or state fixed effects, a time trend, and interactions between state fixed effects and the time trend. Nonetheless, our inability to identify the coefficients of interest and also include parcel by period fixed effects leaves our results open to this possibility. In order for general housing trends to confound our results, it must be true that: (1) 1986-1992 (the Yellowstone period) was a housing "boom" relative to other periods *and* (2) this boom was specific to areas within 2.4 kilometers of federal lands in the West.

We consider the first point in Figure 5, which graphs housing starts between 1970 and 2000 for the whole United States (top series), western United States (middle series) and western United States, single-unit structures only (bottom series). In fact, for both the whole U.S. and western U.S., 1986 (the start of the Yellowstone period) seems to be the peak of a short boom starting around 1982, and housing starts then fell between 1986 and 1992. For single-unit structures in the western U.S., housing starts appear to be approximately constant during the Yellowstone period. Figure 5 suggests there was no general housing boom in our suppression period, but unfortunately cannot tell us if there could have been a localized boom very close to federal lands in the west during this period. We find that implausible – recall that our identification strategy hones in on parcels within less than a mile of federal lands, focusing on fire contagion. Additionally, to be consistent with the full-sample results in Section 6.2, such a story would require that landowners differentially preferred to develop properties very near federal lands in the West during this period, but not the Southeast. However, we cannot firmly rule out this alternative explanation for our results.

8. Discussion and policy implications

Are our estimates of the impact of federal fire suppression policy on land conversion economically significant? Using the Table 6 results, the marginal effects of the *nearfed* interaction variables with time periods from the Poisson model in column 3 suggest that, had the policy of total fire suppression been in place continuously from 1973-2000, rather than only during the Yellowstone period, an additional 14,227 pixels, or about 12,662 acres, would have been converted to developed uses within the land area covered by the 314 parcels in the Poisson sample (about 40 acres per parcel).[24]

The unconditional Tobit results from the RE model in column 1 of Table 6 imply that a policy of total suppression, if sustained during the whole 30-year period, could have encouraged the development of an additional 26,117 pixels, or 23,244 acres on the 877 parcels in the Tobit sample (about 27 acres per parcel). The conditional Tobit marginal effects imply a result more comparable to the Poisson results – total suppression, if maintained over the whole period, would have induced an additional 37 acres of development per parcel.

Recall that the size of a single parcel is almost 28,000 acres. Thus, while these results imply a large percentage change in development conversion in the western parcels due to fire suppression, because the amount of development on these parcels is small, the absolute change in acreage developed due to the policy shift is quite small. Even by 2000, on average, only 3.3

[24] Using the estimated Poisson marginal effects from the full sample in column 3 of Table 9, we obtain almost identical results. Keeping the total suppression policy in place from 1973-2000, rather than just during the Yellowstone period, would have induced an additional 20,104 pixels (17,893 acres) of development on the 438 parcels in the Poisson sample in Table 9, about 41 additional developed acres per parcel.

percent of the land area within a parcel is developed. The amount of development *conversion* is even smaller, of course – the average per parcel-period is 38.91 pixels (34.6 acres) for the west and 84.90 pixels (75.6 acres) for the southeast. Of course, if the effects we estimate of fire suppression policy changes on development could be extrapolated to all land near USFS, BLM, and NPS land in the West, and not just the parcels in our western sample from USGS, the effects of the policy shift, in absolute acreage, would be much larger.

Finally, our estimates of federal policy impacts are identified from a brief return to total suppression due to the Yellowstone fires, a change only a few years in length between two longer periods of the more nuanced fire management policy, which continues to the present day. The fact that we see strong evidence of an impact on development patterns, even as a result of this temporary change, suggests that earlier fire suppression efforts in the decades between 1910 and 1970 may have provided even more significant inducement for residential and commercial development in high fire-hazard regions. A policy of fire management, however, also results in substantial amounts of suppression. We cannot identify the impact this has compared to a situation of no federal fire suppression activity at all.

9. Conclusions

We use panel data on land-use change in the western United States between 1970 and 2000 to test the hypothesis that federal fire suppression policy has influenced land development in areas at risk for damage from wildland fires. The econometric analysis identifies the effects of federal fire suppression efforts on development by exploiting a natural experiment – a major but temporary policy shift toward increased suppression that took place as a result of the significant Yellowstone fires during summer and fall 1988 – as well as variation in the benefits of fire suppression based on proximity to federal lands. We also undertake another set of tests based on the fact that the Southeast did not experience this strong return to suppression after the Yellowstone fires. Models control for underlying trends in development over time and across states, and, in most models, within parcels.

Our results suggest that this temporary shift in federal fire suppression policy may, in fact, have induced some land development on private land near federal land affected by fire suppression efforts. Poisson models suggest that the conversion of forest, grassland and shrubland to developed uses was 32-44 percent lower (depending on the time period) on parcels near the federal fire suppression “umbrella” when the more liberal fire management policy was in place, relative to a total suppression policy. Depending on the time period, Tobit models suggest an impact in the range of 23-36 percent in the unconditional expected value of conversion, or 28-43 percent, conditional on development being uncensored at zero and the maximum parcel size.

Though we find significant impacts of fire suppression policy on land development in the West, we cannot draw any conclusions from this analysis about the costs and benefits of fire suppression as federal policy. To fully evaluate any fire suppression policy, one would need estimates of the benefits of suppression, including the benefits to private landowners (both that had been residing in fire-prone regions and those that move in as a result of suppression activities) from their location choice, as well as the costs of firefighting activities, and the cost of damages when fires do occur and are not fully or immediately contained. However, we offer the first empirical evidence that federal fire suppression activity can induce development, and that this effect may be quite large. The paper demonstrates that economic analysis of fire suppression investments must include both the benefits resulting from that induced development, and the costs, since the value of assets at risk when a fire does occur is larger than in the absence of federal suppression efforts.

Economists have drawn attention to the ability of public policies to induce land development in risky locations through subsidies of various kinds (including subsidized insurance, the construction of infrastructure meant to reduce risk, direct payment for damages in the aftermath of catastrophic events, and other mechanisms), but the literature offers scant empirical evidence to support these claims. Our empirical test is one of only a handful in the economics literature that quantifies the impact of such policies on development, and the first to focus on the increasingly severe and expensive problem of wildland fires spreading into and through residential areas in the suburbs and exurbs of the American West. The costs of induced development in this case include human morbidity and mortality (both firefighters and civilians), as well as the destruction of homes and other assets.

Research in the natural sciences suggests that climate change is increasing the incidence of large wildland fires in the U.S. West. We provide evidence that federal policy may inadvertently have increased the population exposed to the risk of such fires. Our results are directly relevant to ongoing debates over preparedness for and response to hurricanes, droughts, floods, and other events that become natural disasters when they occur in highly populated areas, and that may be expected to occur with increasing frequency due to the changing global climate.

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Table 1. Summary statistics

Variable	Definition	N	Mean	Std dev	Min	Max
D_{it}	number of pixels developed since t-1					
West		3508	38.91	176.63	0	5198
Southeast		696	84.90	273.09	0	3115
Combined		4212	46.43	196.43	0	5198
Pr(D_{it}>0)	Pr(pixels developed since t-1 >0)					
West		3508	0.24	0.42	0	1
Southeast		696	0.50	0.50	0	1
Combined		4212	0.28	0.45	0	1
years in period$_t$	number of years in period t	4212	6.75	0.83	6	8
base development$_t$	number of pixels developed at beginning of					
West	period t (10,000s)	3508	0.07	0.23	0	2.68
Southeast		696	0.16	0.42	0	2.65
Combined		4212	0.08	0.27	0	2.68
nearfed$_i$	=1 if within 2.4 km of federal land, else 0					1
West		3508	0.64	0.48	0	1
Southeast		696	0.23	0.42	0	1
Combined		4212	0.57	0.50	0	
nearUSFS	=1 if within 2.4 km of USFS land, else 0					1
West		3508	0.30	0.46	0	1
Southeast		696	0.19	0.39	0	1
Combined		4212	0.28	0.45	0	
nearNPS	=1 if within 2.4 km of NPS land, else 0					1
West		3508	0.04	0.20	0	1
Southeast		696	0.02	0.15	0	1
Combined		4212	0.04	0.19	0	
nearBLM	=1 if within 2.4 km of BLM land, else 0					1
West		3508	0.49	0.50	0	1
Southeast		696	0	0	0	1
Combined		4212	0.41	0.49	0	
firemgt$_t$	=1 if federal fire management policy, else 0	4212	0.75	0.43	0	1
all_in_fed$_i$	=1 if completely within federal land					
West		4472	0.22	0.41	0	1
Southeast		768	0.09	0.29	0	1
Combined		5248	0.20	0.40	0	1

Table 2. Distribution of states in the samples: western, southeastern, and combined

State	Number	Percent of Region Sample	Percent of Total
Western sample states			
Arizona	56	6.39	5.32
California	169	19.27	16.05
Colorado	50	5.70	4.75
Idaho	60	6.84	5.70
Montana	52	5.93	4.94
Nebraska	28	3.19	2.66
Nevada	21	2.39	1.99
New Mexico	60	6.84	5.70
North Dakota	18	2.05	1.71
Oregon	117	13.34	11.11
Texas	59	6.73	5.60
Utah	45	5.13	4.27
Washington	98	11.17	9.31
Wyoming	44	5.02	4.18
Region sample total	877	100.00	83.29
Southeastern sample states			
Alabama	17	9.77	1.61
Arkansas	39	22.41	3.70
Florida	27	15.52	2.56
Georgia	5	2.87	0.47
Kentucky	4	2.30	0.38
Louisiana	11	6.32	1.04
Mississippi	20	11.49	1.90
Oklahoma	18	10.34	1.71
Tennessee	16	9.20	1.52
Texas	17	9.77	1.61
Region sample total	174	100.00	16.52
OK parcels west of 100th meridian	2	0.23	0.19
Total combined sample parcels	1053		100.00

Table 3. Effects of federal fire policy changes on conversion to developed land in the western sample using "fire management" dummy (linear models)

Variable	Panel FE (1)	Panel RE (2)
years in period	9.76***	10.12***
	(2.89)	(3.15)
base development	-186.28	663.44***
	(693.94)	(96.33)
base development^2	-136.11	-237.64***
	(259.46)	(46.56)
nearfed		17.49
		(13.51)
firemgt	-4.60	-3.80
	(6.76)	(7.05)
nearfed*firemgt	-14.36	-16.20
	(9.89)	(10.45)
constant	yes	yes
parcel FEs	yes	no
state FEs	no	yes
state FEs*time trend	yes	yes
R^2 within	0.03	0.01
between	0.08	0.41
overall	0.06	0.28
Observations (N)	3508	3508
Parcels (I)	877	877
States (S)	14	14

Notes: Dependent variable is number of 60m x 60m pixels within a parcel converted to developed uses since the last period from forest, grassland, shrubland, and mechanically and nonmechanically disturbed land. Each parcel (*i*) has approximately 27,889 pixels, with an average of 588 pixels developed in 1973. Standard errors reported in parentheses are robust and clustered by parcel. ***indicates significance at 0.01, ** at 0.05, and * at 0.10.

Table 4. Effects of federal fire policy changes on conversion to developed land in the western sample using "fire management" dummy, (accounting for zeros and skewed distribution of dependent variable)

Variable	Tobit RE (1)	Tobit FE (2)	Poisson FE (3)	Linear probability FE (4)	Panel FE for D>0 (5)
years in period	31.79**	93.90***	0.25***	0.00	49.94***
	(13.28)	(25.89)	(0.08)	(0.01)	(14.56)
base development	1512.30***	-460.44	-1.06	-0.50**	-394.50
	(102.51)	(1385.01)	(0.99)	(0.21)	(758.18)
base development^2	-569.93***	-425.53	-0.76**	0.30*	-248.96
	(55.33)	(489.18)	(0.31)	(0.16)	(262.14)
nearfed	37.96				
	(38.47)				
firemgt	-38.21	-57.93	-0.23*	-0.03	-28.08
	(29.56)	(64.12)	(0.13)	(0.02)	(30.16)
nearfed*firemgt	-43.10	-165.54	-0.38*	0.00	-73.24
	(31.05)	(233.67)	(0.22)	(0.02)	(48.02)
constant	yes	no	no	yes	yes
parcel FEs	no	yes	yes	yes	yes
state FEs	yes	no	no	no	no
state FEs*time trend	yes	yes	yes	yes	yes
R^2 within				0.02	0.10
between				0.00	0.07
overall				0.00	0.05
Observations (N)	3508	3508	1256	3508	825
Parcels (I)	877	877	314	877	314
States (S)	14	14	14	14	14

Notes: Dependent variable in columns 1, 2, 3 and 5 is the number of 60m x 60m pixels within a parcel converted to developed uses since the last period from forest, grassland, shrubland, and mechanically and nonmechanically disturbed land. Column 4 dependent variable is the probability of any development within a parcel since the last period from this same list of land covers. Each parcel (*i*) has approximately 27,889 pixels, with an average of 588 pixels developed in 1973. Standard errors reported in parentheses are robust and clustered by parcel in columns 3, 4, and 5. ***indicates significance at 0.01, ** at 0.05, and * at 0.10. Poisson FE model in column 3 drops parcels with no development conversion in the whole time series. Panel FE model in column 5 drops all parcel-periods with zero development conversion.

Table 5. Effects of federal fire policy changes on conversion to developed land in the western sample using period dummies

Variable	Tobit RE (1)	Tobit FE (2)	Poisson FE (3)	Linear probability FE (4)	Panel FE For D>0 (5)
years in period	32.61**	90.71***	0.17***	0.00	52.94***
	(14.48)	(25.10)	(0.03)	(0.01)	(14.67)
base_devel	1514.66***	-383.95	-1.10	-0.50**	-385.33
	(102.49)	(1652.54)	(0.90)	(0.21)	(760.17)
base_devel2	-572.39***	-455.09	-0.73***	0.29*	-270.84
	(55.36)	(496.00)	(0.27)	(0.16)	(262.97)
nearfed	63.11*				
	(36.65)				
nearfed*1973-1980	-61.29	-181.29	-0.52	0.00	-77.29
	(40.13)	(375.47)	(0.33)	(0.03)	(63.51)
nearfed*1980-1986	-48.06*	-152.80	-0.52**	-0.01	-65.04
	(26.61)	(323.21)	(0.21)	(0.02)	(41.44)
nearfed*1992-2000	-113.84***	-252.17	-0.50***	-0.04*	-142.59***
	(35.39)	(192.20)	(0.19)	(0.02)	(52.87)
constant	yes	no	no	yes	yes
parcel FEs	no	yes	yes	yes	yes
state FEs	yes	no	no	no	no
state FEs*time trend	yes	yes	yes	yes	yes
R^2 within				0.02	0.10
between				0.00	0.06
overall				0.00	0.04
Observations (N)	3508	3508	1256	3508	825
Parcels (I)	877	877	314	877	314
States (S)	14	14	14	14	14

Notes: Dependent variable in columns 1, 2, 3 and 5 is the number of 60m x 60m pixels within a parcel converted to developed uses since the last period from forest, grassland, shrubland, and mechanically and nonmechanically disturbed land. Column 4 dependent variable is the probability of any development within a parcel since the last period from this same list of land covers. Each parcel (*i*) has approximately 27,889 pixels, with an average of 588 pixels developed in 1973. Standard errors reported in parentheses are robust and clustered by parcel in columns 3, 4, and 5. ***indicates significance at 0.01, ** at 0.05, and * at 0.10. Poisson FE model in column 3 drops parcels with no development conversion in the whole time series. Panel FE model in column 5 drops all parcel-periods with zero development conversion.

Table 6. Marginal effects of fire policy shifts in western sample, from Table 4 and 5 estimates

Variable	Tobit RE (1)	Tobit FE (2)	Poisson FE (3)	Linear probability FE (4)	Panel FE For D>0 (5)
nearfed*firemgt	-6.49 (uncond.) -8.41 (cond.)	N/A	-12.45*	0.00	-73.24
nearfed*1973-1980	-8.46* (uncond.) -11.58 (cond.)		-15.13	0.00	-77.29
nearfed*1980-1986	-6.76* (uncond.) -9.14* (cond.)		-11.15**	-0.01	-65.04
nearfed*1992-2000	-14.56*** (uncond.) -20.91*** (cond.)		-19.03***	-0.04*	-142.59***

Notes: Marginal effects are expressed in terms of the number of pixels converted to developed uses in columns 1, 3, and 5, and in terms of the change in the probability of any conversion in column 4. All marginal effects are estimated using parameter estimates in Tables 4 and 5. Poisson marginal effects in column 3 are calculated using estimated incident rate ratio (IRR); we multiply (1-IRR) by mean D_{it} for the western sample (38.91 pixels for the whole time series, 37.08 for 1973-80, 27.61 for 1980-86, and 48.69 for 1992-2000). ***indicates significance at 0.01, ** at 0.05, and * at 0.10.

Table 7. Models of effects of federal fire policy changes on conversion to developed land in the combined sample using "fire management" dummy

Variable	Tobit RE (1)	Tobit FE (2)	Poisson FE (3)	Linear probability FE (4)	Panel FE for D>0 (5)
years in period	28.80***	70.55***	0.21***	0.01	42.76***
	(10.97)	(26.18)	(0.06)	(0.01)	(12.76)
base development	1515.66***	-383.73	-0.93	-0.43**	270.17
	(83.26)	(1024.06)	(0.83)	(0.19)	(637.76)
base development^2	-558.81***	-38.13	-0.20	0.15	-156.93
	(41.06)	(341.05)	(0.35)	(0.12)	(187.89)
nearfed	50.50				
	(32.64)				
firemgt	-19.68	-31.82	-0.15	-0.01	-19.80
	(22.55)	(42.78)	(0.11)	(0.02)	(24.45)
nearfed*firemgt	-57.14**	-151.48	-0.43*	-0.02	-75.79*
	(25.55)	(171.88)	(0.23)	(0.02)	(46.49)
nearfed*firemgt*SE	47.31	158.25	0.56	-0.01	72.60
	(50.56)	(169.83)	(0.40)	(0.07)	(55.93)
constant	yes	no	no	yes	yes
parcel FEs	no	yes	yes	yes	yes
state FEs	yes	no	no	no	no
state FEs*time trend	yes	yes	yes	yes	yes
R^2 within				0.03	0.08
between				0.03	0.02
overall				0.03	0.02
Observations (N)	4212	4212	1752	4212	1172
Parcels (I)	1053	1053	438	1053	438
States (S)	23	23	23	23	23

Notes: Dependent variable in columns 1, 2, 3 and 5 is the number of 60m x 60m pixels within a parcel converted to developed uses since the last period from forest, grassland, shrubland, and mechanically and nonmechanically disturbed land. Column 4 dependent variable is the probability of any development within a parcel since the last period from this same list of land covers. Each parcel (*i*) has approximately 27,889 pixels, with an average of 716 pixels developed in 1973. Standard errors reported in parentheses are robust and clustered by parcel in columns 3, 4, and 5. ***indicates significance at 0.01, ** at 0.05, and * at 0.10. Poisson FE model in column 3 drops parcels with no development conversion in the whole time series. Panel FE model in column 5 drops all parcel-periods with zero development conversion.

Table 8. Effects of federal fire policy changes on conversion to developed land in the combined sample using year dummies

Variable	Tobit RE (1)	Tobit FE (2)	Poisson FE (3)	Linear probability FE (4)	Panel FE For D>0 (5)
years in period	32.51***	70.41***	0.16***	0.01	42.26***
	(10.56)	(21.72)	(0.04)	(0.01)	(9.86)
base_devel	1517.57***	-374.71	-0.96	-0.44**	274.22
	(83.27)	(1020.10)	(0.83)	(0.19)	(636.77)
base_devel2	-560.41***	-40.52	-0.19	0.15	-162.37
	(41.09)	(274.54)	(0.35)	(0.12)	(186.79)
nearfed	64.99**				
	(31.46)				
nearfed*1973-1980	-60.98**	-182.38	-0.57	-0.02	-85.21
	(25.18)	(352.33)	(0.35)	(0.03)	(60.64)
nearfed*1980-1986	-47.74*	-149.76	-0.55**	-0.01	-75.35*
	(25.18)	(281.16)	(0.22)	(0.02)	(40.63)
nearfed*1992-2000	-112.69***	-194.26	-0.46**	-0.06***	-111.70**
	(30.90)	(162.45)	(0.22)	(0.02)	(51.23)
nearfed*1973-1980*SE	8.97	33.48	-0.02	-0.01	37.16
	(75.48)	(357.28)	(0.47)	(0.08)	(79.98)
nearfed*1980-1986*SE	88.16	209.35	0.87	0.03	121.99
	(66.98)	(334.75)	(0.55)	(0.10)	(79.49)
nearfed*1992-2000*SE	31.46	134.82	0.62*	-0.06	67.98
	(69.92)	(157.00)	(0.37)	(0.07)	(61.44)
constant	yes	no	no	yes	yes
parcel FEs	no	yes	yes	yes	yes
state FEs	yes	no	no	no	no
state FEs*time trend	yes	yes	yes	yes	yes
R^2 within				0.04	0.08
between				0.03	0.02
overall				0.02	0.02
Observations (N)	4212	4212	1752	4212	1172
Parcels (I)	1053	1053	438	1053	438
States (S)	23	23	23	23	23

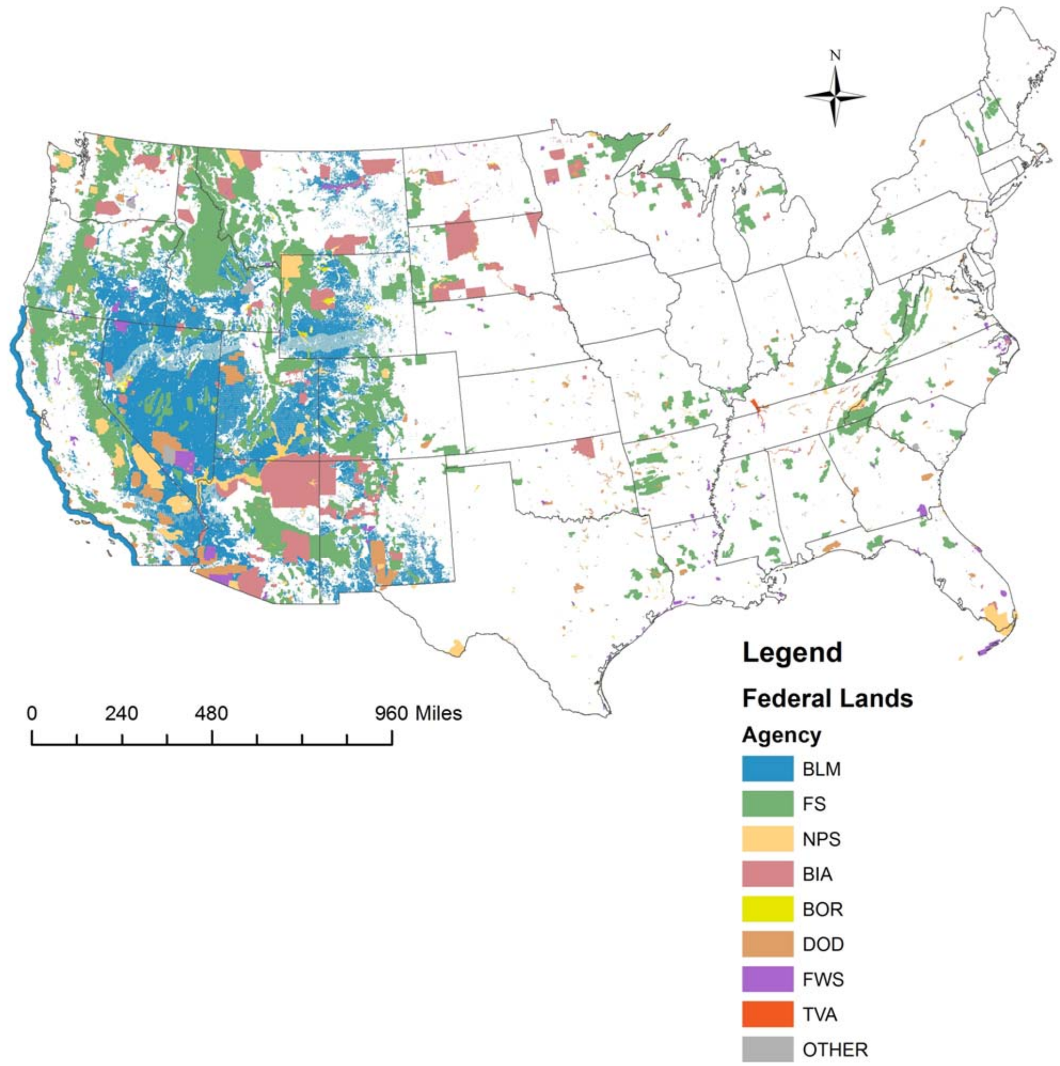
Notes: Dependent variable in columns 1, 2, 3 and 5 is the number of 60m x 60m pixels within a parcel converted to developed uses since the last period from forest, grassland, shrubland, and mechanically and nonmechanically disturbed land. Column 4 dependent variable is the probability of any development within a parcel since the last period from this same list of land covers. Each parcel (*i*) has approximately 27,889 pixels, with an average of 716 pixels developed in 1973. Standard errors reported in parentheses are robust and clustered by parcel in columns 3, 4, and 5. ***indicates significance at 0.01, ** at 0.05, and * at 0.10. Poisson FE model in column 3 drops parcels with no development conversion in the whole time series. Panel FE model in column 5 drops all parcel-periods with zero development conversion.

Table 9. Marginal effects of fire policy shifts in combined sample

Variable	Tobit RE (1)	Tobit FE (2)	Poisson FE (3)	Linear probability FE (4)	Panel FE For D>0 (5)
nearfed*firemgt	-10.21** (uncond.) -11.92** (cond.)	N/A	-16.25*	-0.02	-75.79*
nearfed*firemgt*SE	9.46 (uncond.) 10.34 (cond.)		34.82	-0.01	72.60
nearfed*1973-1980	-10.08* (uncond.) -12.33* (cond.)		-16.13	-0.02	-85.21
nearfed*1980-1986	-8.06** (uncond.) -9.74** (cond.)		-11.73**	-0.01	-75.35*
nearfed*1992-2000	-17.24*** (uncond.) -22.10*** (cond.)		-18.04**	-0.06***	-111.70**
nearfed*1973-1980*SE	1.66 (uncond.) 1.90 (cond.)		-0.87	-0.01	37.16
nearfed*1980-1986*SE	19.21 (uncond.) 20.00 (cond.)		38.42	0.03	121.99
nearfed*1992-2000*SE	6.10 (uncond.) 6.79 (cond.)		42.08*	-0.06	67.98

Notes: Marginal effects are expressed in terms of the number of pixels converted to developed uses in columns 1, 3, and 5, and in terms of the change in the probability of any conversion in column 4. All marginal effects are estimated using parameter estimates in Tables 7 and 8. Poisson marginal effects in column 3 are calculated using estimated incident rate ratio (IRR); we multiply (1-IRR) by mean D_{it} for the combined sample (46.43 pixels for the whole time series, 42.49 for 1973-80, 34.36 for 1980-86, and 60.58 for 1992-2000). ***indicates significance at 0.01, ** at 0.05, and * at 0.10.

Figure 1. Map of U.S. Federal Lands, 2011



Source: Created using data from the U.S. National Atlas, http://www.nationalatlas.gov/

Figure 2. All USGS parcels and selected federal lands

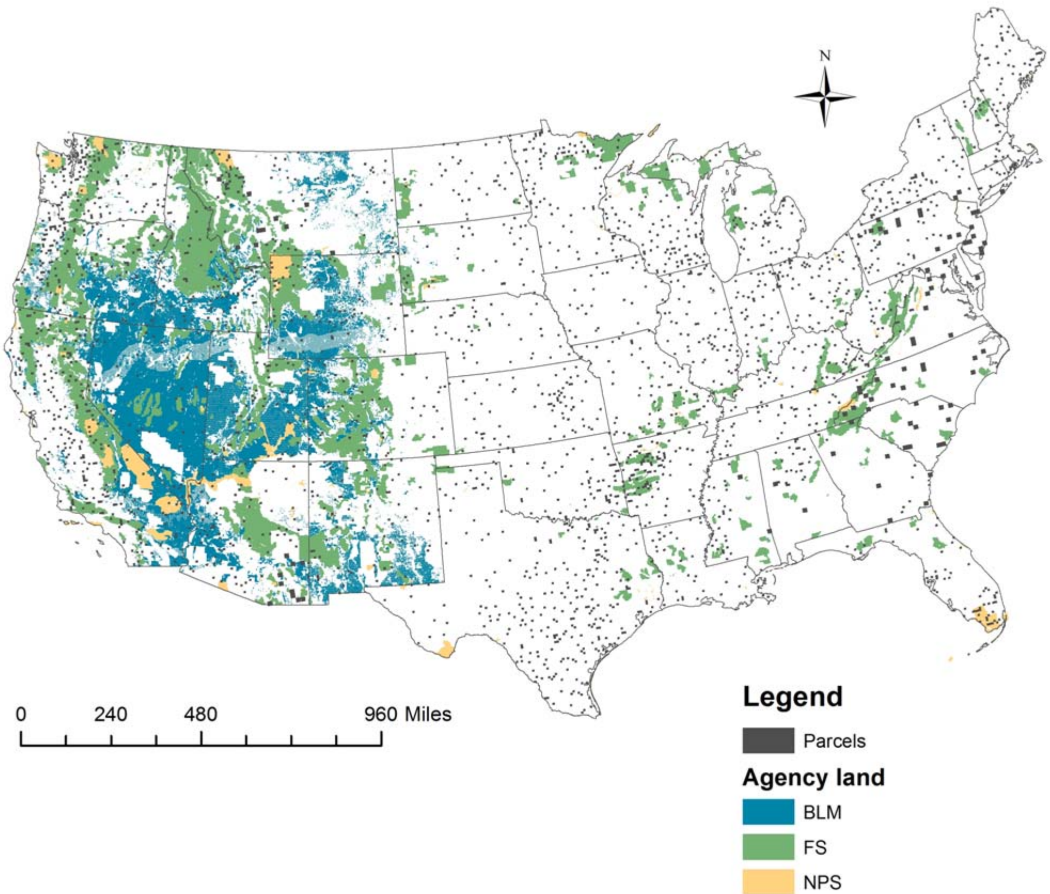


Figure 3. Sample parcels

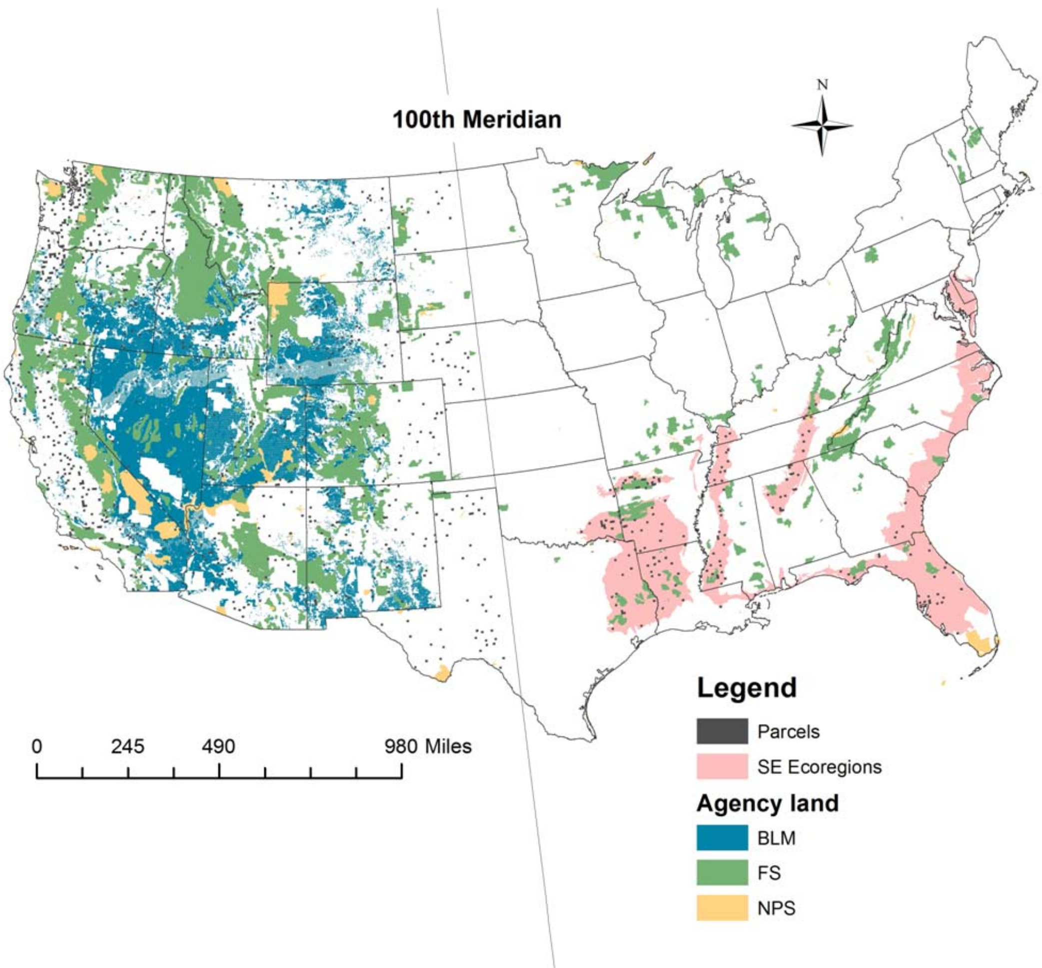


Figure 4. Timeline of land-cover observations and federal fire suppression policy shifts

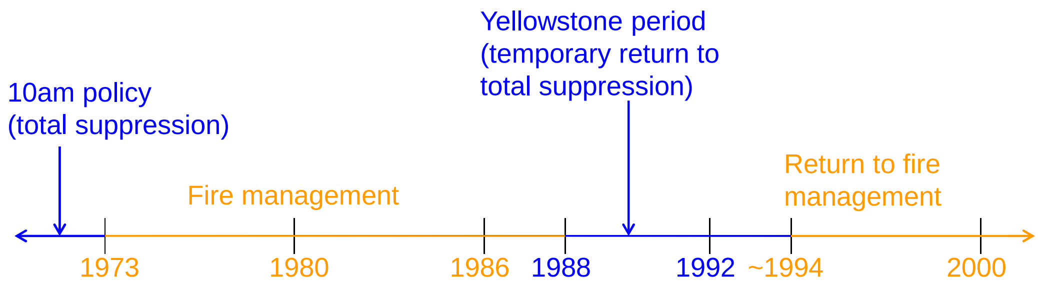
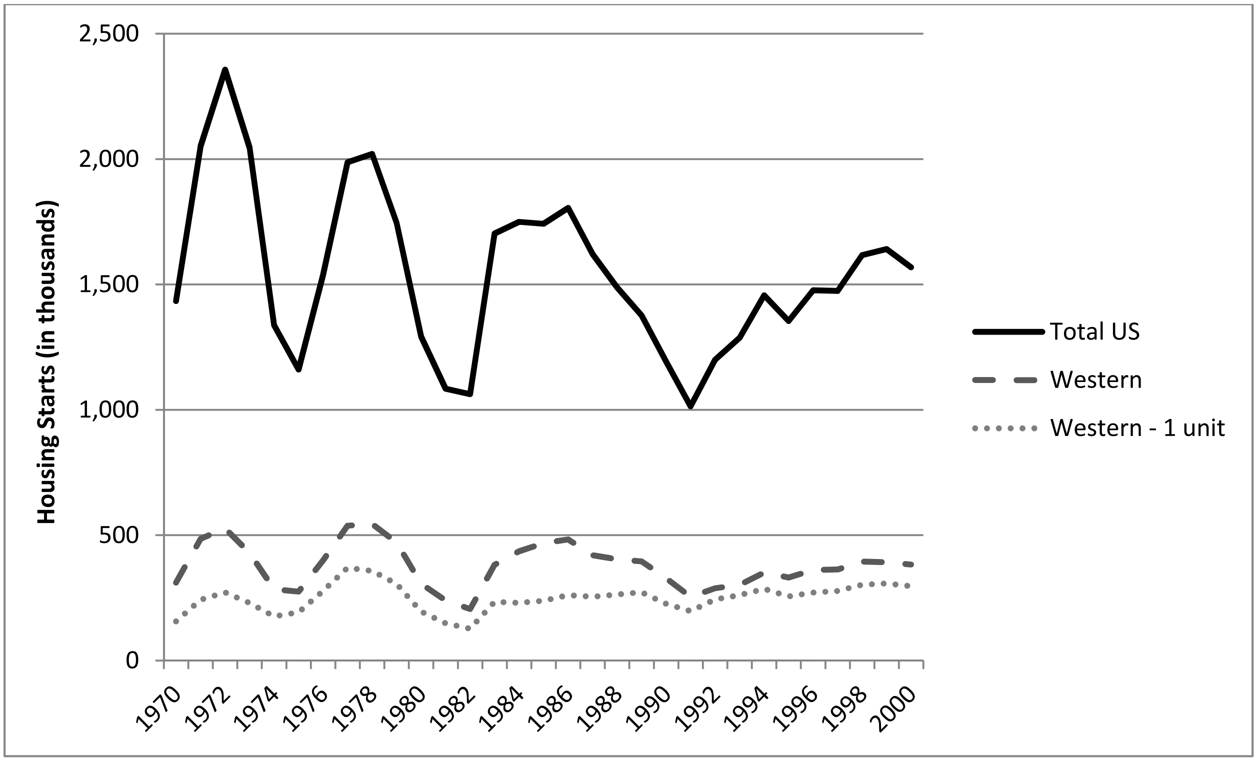


Figure 5. Housing starts, U.S. and western U.S., 1970-2000



Source: U.S. Census Bureau (see: http://www.census.gov/const/startsan.pdf).