Wildfire and Visitation in U.S. National Parks

Preliminary Draft

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Abstract

The recent rapid increase in wildfire activity due to climate change poses unprecedented challenges to park managers working to mitigate fire risk using limited resources. This paper estimates the effect of wildfires on visitation to 32 national parks across the western U.S. Using a comprehensive dataset on wildfire and smoke, I provide the first large-scale evidence of the negative relationship between wildfire activity and park visitation. I find that, on average, wildfires reduce national park visits by about 700,000 per year and this reduction is disproportionately larger for popular parks with a high level of fire activities. These effects can be explained by a lack of access due to emergency closures throughout the season. I also investigate the global externalities associated with wildfire smoke and find that travelers are not responsive to the smoke from distant sources to a significant degree. These results demonstrate the importance of local adaptation efforts in mitigating economic loss in tourism arising from wildfire avoidance.

1 Introduction

Millions of people flock to the vast public lands in the United States, with outdoor recreation contributing $459.8 billion in economic activity in 2019 (Bureau of Economic Analysis, 2020). In recent years, U.S. national parks attract an increasing number of visitors, already setting new visitation records three times since 2014 (Ziesler, 2020). Most parks' peak visitation seasons coincide with fire season, which is projected to last longer with more severe fires in the foreseeable future (Abatzoglou and Williams, 2016). Catastrophic fires can cause large-scale disturbance to the ecosystem and result in emergency closures and smoke-related health risks, which may trigger shifts in visitation patterns (Englin et al., 1996; Starbuck et al., 2006; Sánchez et al., 2016). Intensifying fire events pose unprecedented challenges both to park managers working to mitigate fire risk using limited resources during extended peak seasons (Rothman, 2005; Fisichelli et al., 2015), and to gateway communities that have grown heavily reliant on the tourism industry stemming from park recreation (White et al., 2016). While the effects of wildfires on outdoor recreation demand are well studied (e.g., see Bawa, 2017), the previous literature has predominantly focused on a specific region (Loomis et al., 2001; Duffield et al., 2013; Kim and Jakus, 2019) and/or a specific type of recreation use, such as camping or biking (Hesseln et al., 2002, 2004; Gellman et al., 2021), leaving the aggregated impact of wildfires largely unmeasured. However, recreation demand in different regions may not respond to wildfires similarly and most park visitors are often day users, who would rather spend a day driving along scenic roads and hiking short trails than venturing into the backcountry (Vaske and Lyon, 2014). As a result, policy implications drawn from existing studies based upon narrow and likely unrepresentative samples cannot be broadly applied to the entire population of the park visitors.

This paper provides a large-scale evaluation of the effect of wildland fires on recreational visits to 32 national parks in the western United States. My starting point is an extensive data set on wildfire and smoke that integrates ground-based historical fire records obtained from several wildland fire management agencies, fire closure history recovered from social media, and satellite-based remote sensing data. I then link the wildfire data to monthly visits to each national park over the last 25-year timespan. Visitation data from the National Park Service (NPS) reports observed visitation at both the entire park and the entrance station levels. I can then estimate the causal effect of wildfires on visitation that are representative of all national park visitors by exploiting large variations in the time, size, and location of wildfires and smoke at a granular level.

This study addresses three primary research questions. First, I exploit month-over-month variation in fire events within or proximate to each park to provide a large-scale estimate of the impact of wildfires on outdoor recreation. In this way, my empirical strategy is most similar to that of Keiser et al. (2018), who use year-to-year variation in in-park ozone concentration to estimate the impact of air pollution on park visitation. Second, I explore potential channels for the negative relationship between fire and visitation, including emergency fire closures and an increase in wildfire risk. Third, I disentangle the effect of wildfire-induced smoke from the direct effect of local fires so that the smoke effects can be directly compared to the overall fire effects.

My analysis yields three important findings. First, using a representative sample of all park visitors, I consistently find a statistically negative relationship between wildfire activities and monthly park visitation. Following the previous literature (Duffield et al., 2013; Kim and Jakus, 2019), I measure wildfires near national parks as the total acres of fires burned within a certain radius of the park's geographic centroid. A primary identification concern that may arise when estimating fire-visitation effects is that because wildfires may not be randomly assigned, existing estimates may be biased by reverse causality whereby increasing visitation could increase human-started fires or omitted variables introduced by spatial spillovers from fire activities at nearby recreation areas. My analysis overcomes this challenge by exploiting the plausibly random variation in lightning-caused fires and accounting for potentially confounding spillover effects. I show that on average, the monthly visitation loss is 0.064 percent per thousand acres burned, and the impact carries over to the following month, after flexibly controlling for differences in local weather, seasonality, and time-varying unobservable factors in each park. This translates to an annual visitation loss of about 700,000 visits for 32 western parks in my sample, suggesting a smaller impact than Gellman et al. (2021)'s study using campsite reservation data.

Second, I present evidence regarding the main channel to explain the fire-induced visitation impacts described above: lack of access due to emergency closures, which account for a large share of the decrease in visitation and have impacts nearly 10 times larger than fires not causing any closures. Because parks are rarely closed entirely, I obtain daily measures of emergency fire closures for every park entrance station from the park's social media posts and State Departments of Transportation. I show that monthly visitation falls by 2.27 percent for an additional park closure day in a month, suggesting that prolonged site closure may generate a relatively large loss in tourism. Furthermore, I confirm park visitors do respond to fires that are either too small in scale or burning too remotely to cause any closure event, despite in a much smaller magnitude. The channel for these smaller fires is possibly due to health concerns or reduced visibility, which has been well documented in the literature (Schultze et al., 1983; Thapa et al., 2013). To distinguish the effects of such small fires from fire closure, I exclude dates with closure events and measure wildfire risk by the number of days with fires but no closure in place. I find a significantly negative but much smaller effect for wildfire risk: a 0.24 percent decrease in visitation for an additional "small fire day".

Third, I find that wildfire smoke alone has a weak and statistically insignificant impact compared to the local fire effect. My estimation approach includes both measures of monthly fire days and smoke days constructed from the National Oceanic and Atmospheric Administration (NOAA) Hazard Mapping System Fire and Smoke Product (HMS). The coefficient for the fire variable is insensitive to the exclusion of the smoke variable from the model, and these two variables are highly correlated. These findings raise the concern that simply including both fire and smoke variables in the model will exacerbate the attenuation bias due to potential measurement error in the smoke variable and collinearity between smoke and fire measures (Wooldridge, 2010). To address this concern, I follow a procedure adapted from Brey and Fischer (2016) and restrict smoke days to dates with above-average ozone concentrations and no active local fires. After adjusting the smoke variable to account for smoke days in the absence of local fire, I find that the estimated impact of smoke days on visitation remains small and statistically insignificant, while the coefficient on the fire variable is always significant and fairly stable.

This study makes three primary contributions to the literature. First, my analysis moves beyond the previous studies by providing the first large-scale estimation of fire-visitation effects that are representative of the affected population of national park visitors. Existing quantification of the wildfire impact on recreation demand has generally relied on a limited sample from a specific type of recreation users or a specific region. The external validity of their estimates is therefore worth questioning. Should we expect the campers and backcountry users' response to fire risk similar to those day users who would likely take a day trip along parks' scenic roads? By contrast, my analysis leverages the only available comprehensive visitation data from NPS and takes advantage of the high granularity of my data to overcome the identification challenges in the two prior time-series studies on contemporaneous fire effects (Duffield et al., 2013; Kim and Jakus, 2019). My research finds a robust estimate of the wildfire-visitation relationship that better represents the park recreation in the American West. Compared to Gellman et al. (2021) a high-profile recent study using campground use data, I estimate a much smaller fire-induced visitor loss, which suggests that the impact evaluation based on a limited sample could be misleading.

Moreover, this study is the first attempt to identify the relative importance of two channels in which wildfire activities may cause tourism loss: fire-induced closures and risk perception of wildfire. I find restricted access due to emergency fire closures throughout the season is the main channel, but visitors may also respond to risk perception of relatively small or remote fires although with a much smaller magnitude. Such evidence may be useful for park managers facing the challenge of allocating limited resources to mitigate wildfire risk when the park is overrun with visitors. Given that record-breaking wildfires are expected to become more frequent, the results suggest a greater need for timely adjustment for incident planning and park management. For example, the impact of fire closures can be mitigated by providing timely information on fire restrictions and encouraging visitors to explore other unrestricted regions of the park that meet desired safety and health conditions.

Finally, my findings speak directly to the ongoing and contentious debate on the use of both prescribed fires and those naturally ignited fires that will be allowed to burn as a proactive fire prevention policy. Despite the efficacy of prescribed burning in reducing the size of wildfires (Cochrane et al., 2012), the primary concern surrounding its use centers on its potential risks arising from externalities, i.e., smoke from prescribed burning may have a substantial impact on downwind air quality. My findings suggest that the effects of large catastrophic fires burning in the heavily visited areas drive a large fraction of visitor loss, while smoke from distant sources creates somewhat small spatial externalities in local tourism flows. This shed light on the potential benefits from the use of off-season prescribed burning, especially for those tourism-dependent gateway communities that are vulnerable to shifting visitation patterns in the face of a natural disaster (Smith et al., 2016; Kim and Jakus, 2019). Carefully planning prescribed burning in the heavily visited areas at the right timing can, to a large extent, mitigate the risk of a catastrophic fire which causes most of the visitation loss and thus tourism loss, meanwhile, reduce the externality to nearby communities as much as foreseeably possible. Among them, many are indigenous communities that heavily rely on the tourism industry, for example, the Blackfeet Nation near the Glacier National Park.

The remainder of this paper is organized as follows. The next section describes the data sources and construction of the dataset. Section 3 explains my empirical strategy in detail. Section 4 presents main results on visitation impacts of wildfires, investigates the potential channels, estimates the effect of wildfire smoke, provides back-of-the-envelope calculation and compare it with existing estimates in the literature. Section 5 discusses the findings and concludes.

2 Data

This section summarizes data sources and defines key variables used in the analysis. A comprehensive description of data construction can be found in Appendix A.1. I link historical records on wildfire occurrences to month visits to 32 national parks (Figure 1) to produce a dataset that spans from 1993 to 2019 in the western US (Arizona, California, Colorado, Idaho, Montana, New Mexico, Nevada, Oregon, Texas, Washington and Wyoming). I restrict my analysis to parks in these states for two reasons: first, lightning-started fires are relatively more common in the western US than in the eastern US, which is dominated by human-started wildfires (Nagy et al., 2018), so this focus alleviates the concern about reverse causality as discussed in section 3. Second, the visitation to western national parks collectively accounts for about 70% of the annual national park attendance.

2.1 Wildfire and Smoke data

To create a metric on fire activity, I start with ground-based wildfire records. I use fire records from the U.S. Forest Service (USFS) Fire Program Analysis's fire occurrence database (Short, 2017), the most comprehensive database available for wildfire records acquired from local, state and federal agencies. This dataset reports geo-location coordinates of wildfire ignitions and includes attributes such as final burned area, discovery and end date of the fire. For each fire event, I calculate the distance from the fire origin to the centroids of national parks. I define a park's wildfire size as the total acres burned by all fires within a certain radius of that park centroid in a given month. I chose a radius of 50 miles, following Duffield et al. (2013) and Kim and Jakus (2019). An example case of Yellowstone National Park in September 2003 is provided in Figure 2. Wildfire size serves as the primary measure of monthly fire activity in the analysis.

In order to measure the difference in park accessibility under wildfire emergency conditions, I utilize two archival sources to identify the consequential park closure days. The first source for closure records is the official news releases from the park service and historical social media posts from Twitter and Facebook. When the park closes to traffic due to any reason such as fire, weather or road construction, each park's social media team will announce the closure to visitors by putting out a press release on the park website and on the Twitter and Facebook home pages. Since many park closures are not related to wildfires, I filter the text from news and social media that contain keywords such as "wildfire" or "fire". While this method provides me with accurate information on affected areas and closed segments of the park roads, the search generates only a limited subset of records on traffic conditions on state highways used to enter the park. I therefore also obtain historical state route closures related to wildfires from state departments of transportation. Together, these two sources allow me to construct a proxy measure for fire closure days that is available from 2011. I define "fire closure days" as the number of days within a month when a park entrance is closed due to wildfires.

Data from satellite-based estimates of wildfire smoke plumes and of fire activity were downloaded from The National Oceanic and Atmospheric Administration (NOAA) Hazard Mapping System (HMS). Both datasets are developed by HMS specialists who monitor and analyze sub-daily visible imagery from seven NOAA and National Aeronautics and Space Administration (NASA) satellites. The HMS spatial data files include the daily polygons of the smoke plumes and spatial points of active fires detected during daylight hours since August 2005. I define a smoke-impacted day at a park as any day when a smoke plume intersects the park boundary. To construct smoke exposure measure, I aggregate the number of smoke-impacted days to the month level to obtain the measure "smoke days" in a given month. Section 4.4 provides a detailed discussion of the stability of the estimated effect of smoke days. To compare the impacts of smoke to the impacts of fires, I also create the metric "fire days" by following the approach employed by Burke et al. (2021). I begin by grouping all nearby fire points on a given day by placing a 375-meter buffer around each fire point and merging overlapping buffers into hotspots. I then estimate the daily fire size as the total acres of merged buffers. I filter out days with fire size smaller than 300 acres and count the total number of the remaining days within a month as the "fire days". Further details on the construction and alternative definitions of smoke days and fires days are given in Appendix A.1.

2.2 Park Visitation Data

Data on park visitation come from NPS Visitor Use Statistics. For each NPS-managed unit (e.g., national park and national monument), NPS maintains historical data on monthly visitation rates for different categories of user groups, including recreation visits, non-recreation visits and overnight stays. A "visit" is generally defined as the entry of a visitor into a park site (Manning, 2011). The total number of visits is primarily collected via automated vehicle counters set up at entrance roads and estimated by multiplying the number of vehicles by a person-per-vehicle multiplier (Ziesler and Pettebone, 2018). I focus on recreation visits because these data are the most used and broadly applicable statistics to reflect visitors' demand for national park visits (Bergstrom et al., 2020). Large, iconic parks report vehicle counts for each individual entrance station. Such vehicle entrance counts are also obtained and used as weights to build weighted-average fire closure days at the park level.

2.3 Weather and Travel Costs

The analysis includes a flexible set of control variables including temperature, precipitation, vapor pressure deficit (VPD) (a measure of atmosphere dryness), and real gasoline price (a proxy for travel cost). Specifically, I extract gridded data on monthly maximum temperature, minimum temperature, total precipitation, minimum VPD and maximum VPD from PRISM Climate Group (PRISM Climate Group, 2004), which provides gridded data at a 4 km spatial resolution. To construct weather conditions at the park level, I average the monthly measures over all grid cells within park boundaries. Furthermore, I obtain monthly gasoline prices for all western states from the U.S. Energy Information Administration (EIA, 2020). Following Duffield et al. (2013) and Kim and Jakus (2019),I divide the gasoline price by real per capita personal income to proxy state-wide travel costs at all distances.

3 Empirical Framework

The first step in my empirical analysis is to establish the causal relationship between wildfires and national park visitation. To identify the effect of fire activities on visitation, I estimate a Poisson fixed effects panel model using the equation:

$$\mathbb{E}\left[visitation_{iym}\right] = \exp\left(\beta \cdot fireSize_{iym} + \boldsymbol{X}'_{iym}\boldsymbol{\gamma} + \alpha_{iy} + \delta_{im} + \theta_{ys} + \epsilon_{iym}\right) \tag{1}$$

where the dependent variable $fireSize_{iym}$ denotes the monthly recreation visits to park i in a year y and month m. The variable of interest $fireSize_{iym}$ is constructed as the total acres burned by all fires within a 50-mile radius of the park in the preferred specification. To address unobservable factors that are correlated with both wildfire and visitation, I include a key set of controls $\boldsymbol{X}_{iym}$. This set includes a vector of weather variables – ten bins of monthly average temperature, total precipitation and average VPD – to account for the nonlinear effect of weather on visitation found in prior studies (Fisichelli et al., 2015; Keiser et al., 2018). My estimates are robust to less flexible functional forms of weather. I also control for income-adjusted gasoline price to capture the variation in travel costs and income determinants of demand for national park visits, as previous studies provide evidence that visitation is positively correlated with personal income and negatively associated with fuel travel costs (Bergstrom et al., 2020).

Equation 1 also includes a rich set of fixed effects, including park-by-year (α_{iy}), park-by-month (δ_{im}) and month-of-sample (θ_{ys}). Specifically, park-by-year fixed effects capture within-year variations in park-specific factors that determine recreation visits but are not captured by covariates, such as park entrances fees, free time for leisure and demographic characteristics. Park-by-month fixed effects control for seasonal unobservables across parks, such as changes in regional fire suppression efforts and differences in peak/off-season visitation. Lastly, month-of-sample fixed effects pick up the time-varying shocks that are common in each month, such as economic recessions and the rise of social media. In sum, these fixed effects allow me to compare a park to itself at the same month of the year as well as across years with different levels of monthly wildfire activity.

Because $visitation_{iym}$ is a count variable with non-negative values and positively skewed distributions, all panel models in my analysis are estimated using Poisson Pseudo Maximum Likelihood (PPML) rather than a log-linearized approach. As a robust alternative to log-linear regressions, PPML can handle zero visitation when the park is completely shut down and gives consistent estimates of slope parameters without any distributional assumptions of the data (Silva and Tenreyro, 2006; Wooldridge, 2010; Silva and Tenreyro, 2011). The coefficient of interest in the Poisson model β, can be interpreted as the percentage change in visits to park i resulting from an additional acre burned in proximity to the park. Because of the long time-series nature of my panel data, the error term, ϵ_{iym}, may exhibit month-over-month serial correlation within parks. To correct for autocorrelation in monthly visitation and wildfire size, all standard errors are clustered at the park-by-year level.

My identification strategy crucially relies on the assumption that unobserved determinants of visitation, ϵ_{iym}, are independent of variation in wildfire size, conditional on covariates and fixed effects included in equation 1. However, such an assumption can be violated due to reverse causality induced by human-started fires, thereby biasing the estimates of β. There is strong evidence that the vast majority of fires in the US are caused by humans (Balch et al., 2017). If large wildfire events discourage visits while crowds of visitors increase the frequency of human-caused fires at the same time, a potential identification concern arises. I address this concern by restricting the fire sample to lightning fires, as variation of natural fires are plausibly as good as random. I therefore ensure that preferred estimates are free from reverse causality.

Another primary challenge in consistent estimation is omitted-variable bias. It's well established in the recreation demand literature that the availability of substitute and complementary recreational opportunities is an important determinant of demand for visits (Englin et al., 2008). Since the fire activities in park i and at nearby alternative sites can be highly correlated and both could impact visitation, failing to control for such spatial spillovers from neighboring recreation areas might introduce omitted-variable bias. My fixed-effects estimates are thus expected to pick up the potential spillover effects and under- or over-estimate the true effect depending on whether visitors perceive nearby parks as substitutes or complements on average. To probe this possibility, in my preferred specification, I have included an additional control variable that is defined as the total fire size burned in all other nearby NPS units (not limited to national parks) within 80 miles of park i. I provide several robustness checks and show that my results are not confounded by the spatial spillover from nearby destinations.

4 Results

4.1 Visitation and Burned Acres

I find a strong and negative relationship between wildfire size and observed aggregate visitation. Table 1 presents PPML estimates of different versions of equation 1 with with alternative sets of three-way interacted fixed effects and control variables. All models include park-by-year fixed effects. The model in column (1) includes park-by-fire season[1] and fire season-by-year fixed effects. Column (2) uses park-by-month and month-of-sample fixed effects to more flexibly account for common or individual shocks to the demand for western national parks. If the wildfire activities are confounded by time varying unobservable factors, failure to control for them could lead to biased estimates. The inclusion of more granular fixed effects than those in column (1) should adequately mitigate such bias by fully absorbing variations in visitation caused by these confounders. Indeed, I find a slight drop in the magnitude of coefficient estimates on burned acres from column (1) to column (2), but the estimates stay negative and significant. However, the results are less precisely estimated with the most exhaustive fixed effect, suggesting that such attenuated estimates may suffer from reverse causality and omitted variable bias that still persist even after conditioning on controls and fully interacted fixed effects.

I further test whether the effect estimated in equation 1 is threatened by reverse causation. If considerable variation in wildfire events is directly correlated with excessive human use in the most visited areas, ignoring the potential endogeneity could lead

[1] Fire season is defined as June to September, when wildfires are most likely to occur, spread and threaten recreational resources in the western US.

to an underestimate of the damages caused by wildfires. Column (3) investigates this possibility by restricting wildfire size to the acres burned by lightning-caused fires. The coefficient estimate is substantially larger (1.5 times) than that in column (2), estimating that a thousand-acre increase in total lightning-caused fire size is associated with 0.063 percent monthly visitor loss. The larger point estimate could be a result of leveraging the presumably random variation in lightning fires, which burned three times larger and longer, on average, than human-caused fires. Most notably, this finding demonstrates the downward bias by reverse causality and implies that wildfire cause, which has been largely overlooked by previous studies, should be considered when looking at its effect on outdoor recreation.

The final column of 1 presents the results from my preferred specification, which also accounts for the spatial spillovers from neighboring parks managed by NPS. The coefficient on lightning-caused fire size remains relatively stable after the inclusion of burned acres on all other NPS-managed units within 100 miles ("nearby parks" variable). The estimate reported in column (4) is not sensitive to alternative definitions of "nearby parks", such as one using an alternative radius or including protected areas managed by USFS and BLM (Appendix Figure A.1). Besides, my results suggest there is lack of evidence of spillover effects in the nearby protected areas; the point estimates of neighboring parks' burned acres are very close to zero and statistically insignificant across all specifications. Thus, the decrease in visitation is not driven simply by substitution or complementary effects – travelers diverted away from the local park because of the changes in fire activities in neighboring parks.

I perform several additional robustness checks on my preferred specification—i.e., column (4) of Table 1. The detailed results are reported in Appendix Table A1. First, columns (1)-(2) explore the impact of different wildfire zone definitions used to count the total acres burned and find that using an alternative definition of total wildfire size still yields significant coefficient estimates. Note that the coefficient on wildfire size using both smaller wildfire zones is larger in magnitude than the 50-mile buffer zone in Table 1 for a smaller average wildfire size, indicating that nonlinearities may exist in the wildfire-visitation relationship. Second, columns (3)-(4) indicate these findings are also robust to alternative functional forms of weather controls. The estimate of greatest interest is also insensitive to alternative model specifications (e.g., log-linear, different clustering choices and fire season only subsample), suggesting that these results are not driven by confounding variation between wildfire activities and visitation. Taken together, I conclude that on average monthly visitation loss is 0.064% per thousand acres burned.

4.2 Temporal Spillovers and Nonlinear Impacts

This subsection expands on the main results in two ways: conducting a dynamic version of equation 1 with leads and lags of wildfire size and examining the nonlinear relationship between visitation and burned area from wildfires. Beyond the contemporaneous effect of wildfire activities on visitation reflected in Table 1, one might expect impacts to persist over time. I estimate a dynamic version of equation 1 that includes three leads and lags of the acres burned by lightning-caused fires. This also serves as a placebo test of my identification strategy, as current month visitation shouldn't be affected by fire events that will happen in the future. The coefficient plot of leads and lags in Figure 3 depicts insignificant coefficients of all leads, which support the identification assumption. On the other hand, I find a negative and significant effect of fires in the prior month that is similar in magnitude to the estimated contemporaneous effect. Visitors appears to delay their visits to the third month, but such a result could also be caused by serial correlation in wildfire size, as I find a null effect of the second lag. These findings are consistent with those in Duffield et al. (2013) and Kim and Jakus (2019), that the impact of wildfires carries over to the following month.

Next, I examine the nonlinearities in the visitation impacts of wildfire activity. Panel (a) of Figure 4 plots the results by regressing a binned version of equation 1 rather than restricting the response to be linear. The blue dots denote coefficients of indicators for 40 bins of total fire size, and the shaded blue area corresponds to the 95% confidence interval. It should be noted that the reference bin is zero burned acres, represented by the first 24 bins that are combined into a single bin. I also report a histogram of all bins at the bottom. The distribution of lightning-caused fire size within the 50-mile buffer around each park is highly skewed because of frequent low levels of monthly fire activity during my study period. Only 11.5% of the park-month observations experience greater than 300 aggregate burned acres. The effect of wildfires fluctuates around zero and is imprecisely estimated through the lower 90th percentile (36th bin). Above the 90th percentile, however, the effect drops sharply. The visitation loss during the month with wildfire size in the 97.5th percentile (40th bin) is 2.39%, which is about 10 times as much as the month in the 90th percentile. These findings do provide evidence of a nonlinear effect for wildfire size in the highest percentiles where wildfires burned at least 1000 acres.

Another way to demonstrate the damaging impact of large fires is to compare the visitation loss arising from a typical fire across different fire size categories. I first group wildfires by size class defined by National Wildfire Coordinating Group (NWCG), and simultaneously estimate the effects of total burned area by each class. Multiplied by the average wildfire areas in each category, the results in Panel B of Figure 4 are depicted side by side in a similar fashion to Panel A. The visitation loss associated with fires over 5000 acres (NWCG class-G) are more pronounced than fires in all other categories. A mean sized wildfire of class G will reduce monthly visitation by 2.43%. Although class G fires are rare, they are very destructive, having burned up to 83% of all wildfire acreage in the sample. Therefore, the reported estimates of average response of visitation to wildfire size in Table 1 is most likely driven by large fires. This finding is consistent with the previous literature that finds that fire-induced visitation impacts depend on the intensity of fire events (Bawa, 2017). The profound effect of severe wildfires on visitation is likely explained by the fact that larger fires are more likely to affect the heavily visited area in a park regardless of its origin. And the most disastrous fires are also most likely to trigger park closures.

The results so far show that the effect of wildfires depends on their size. I now consider how this effect might vary with its distance from the park centroid. Figure 5 shows the results from simultaneously estimating the effect of total fire size within each 5-mile distance bin out to a distance of 50 miles. For example, the first blue bar represents the impact of total burned area in the 5-mile buffer zone. Despite the clear evidence of distance decay beyond 15 miles, the impact of acres burned within 5 – 15 miles of the park center is about two times higher than that of the total area of fires burning less than 5 miles from the park center. One possible explanation for this result is that the 5-mile buffers around park centroids encompass relatively remote areas within parks that tend to be far away from most heavily visited, developed areas, since visitor centers, main roads and campgrounds are often located nearer the park boundaries.

4.3 Channels Behind the Findings

As noted above, fires burning 5000 acres or larger and those burning within 5 – 15 miles of a park's geographical center have the largest effects on monthly visitation. One possible explanation for these findings is that severe wildfires, even those originating from remote locations, are often accompanied by high winds and are more likely to quickly spread to heavily traveled areas, thus prompting evacuation and park closures. Additionally, fires starting within 5 – 15 miles of the park centroid have high chances of burning near park facilities like visitor centers and headquarters and forcing the closure of the main road used to enter the park. Figure A.2 plots the distribution of the distances from visitor centers to park centroids. On average, a visitor center in my sample is 12.27 miles away from the park centroid, which happens to fall inside the 5 – 15 miles bin. The presence of a fire close enough to visitor centers or to the main access road to the facilities would likely close access to the surrounding area and travel networks. That is, the previous results could be explained if the fire-caused closure is one of the main channels of visitation loss. As such, I now examine the relative importance for the channels behind the fire-induced visitation loss, especially the degree to which visitors respond to emergency fire closures, even when the park is only partially closed.

It is hypothesized that the closure channel should account for the majority of lost visitors in comparison to a fire scenario in which full access to the park is granted but underlying fire risks are present. I test this hypothesis by estimating a version of equation

1 where the variable of interest is replaced by a proxy for "fire closure days"[2], as described in section 2.1. I also control for "fire days," which account for the number of days when wildfires are burning either too small in scale or too far away from the populated areas to cause any closure event. The estimating equation is:

$$\begin{aligned}\mathbb{E}\left[visitation_{iym}\right] = \exp\big(&\beta_1 \cdot closureDays_{iym} + \beta_2 \cdot fireDays_{iym} \\ &+\boldsymbol{X}'_{iym}\boldsymbol{\gamma} + \alpha_{iy} + \delta_{im} + \theta_{ys} + \epsilon_{iym}\big) \qquad (2)\end{aligned}$$

where the coefficient of interest β_1 now describes the effect of an additional day of fire closure on monthly visits. Note that $closureDays_{iym}$ and $fireDays_{iym}$ are both measured in days so that β_1 and β_2 can be directly comparable. All other covariates and fixed effects in the preferred specification from equation 1 are included.

The results are shown in Table 2. A comparison of the first row and second row in column (1) suggests that fire closures clearly result in relatively sizable reductions in visitation. The coefficients on $closureDays_{iym}$ and $fireDays_{iym}$ both have the expected sign (negative) and are statistically significant. In terms of the magnitude of the estimated effect, the coefficient of fire closure days is at least a magnitude larger than that of fire days. The difference is even greater if I adjust the calculation of fire days to exclude dates with closure events (column (2)) and if I use an alternative definition of closure days that includes only the closures caused by lightning fires (column (3)). Focusing on the column (3), an additional day of fire closures in a month would reduce visitation by 2.27 percent, while the effect is 10 times smaller – 0.24 percent – for each fire day without any consequential closure in place[3]. Taken literally, these estimates highlight that lack of access due to emergency closures is indeed the main channel to explain fire-induced visitation impacts. As more than half of the fires resulting in closures burned more than 5000 acres, the substantial impacts of fire closure further confirm my previous finding

[2]Currently, the closure data are only available for western national parks receiving more than one million annual visits (excluding parks in California). These parks include Arches, Bryce, Capital Reef, Canyonlands, Crater Lake, Glacier, Grand Canyon, Grand Teton, Mount Rainier, Olympic, Rocky Mountain, Yellowstone and Zion National Parks.

[3]I also conduct a placebo test by adding leads and lags of fire closure days and find that the effect of fire closures does not persist over time (Appendix Figure A.4).

of the nonlinear effects of wildfire size. Interestingly, the significant effect of fire days indicate that visitors also respond to risk exposure of relatively small or remote wildfires, even if they did not cause any fire closure.

4.4 Smoke Effects

Increasing wildfire activities are often accompanied by a number of days of smoke and haze in the air. Travelers could still change their travel plans due to smoke-related health concerns, even when there is no fire closure imposed in the vacation region. Many of the previous studies on air pollution in scenic areas have shown that the public places great value on preserving visibility (Schultze et al., 1983; Chestnut and Rowe, 1990; Smith et al., 2005) and is more concerned about the health impact of increased haze and smoke from wildfires, particularly for less healthy individuals (Thapa et al., 2013; Haider et al., 2019). As a result, this concern may be intermingled with the influence of other wildfire outcomes including closures and high fire risk in the destination park, making it difficult to distinguish the independent smoke effects. As recent research utilizing satellite-based smoke data indicates that wildfire smoke can travel long distances from the source fires (Rolph et al., 2009; Miller et al., 2017; Borgschulte et al., 2018), considerable global externalities could be associated with wildfires—namely, the externalities that arise if visitors respond to smoke and haze coming from fires thousands of miles away. If distant wildfire smoke creates substantial global externalities compared to the effects of local fires, little could be done for the local park managers to mitigate the damaging impact of smoke without partnering with other agencies. This section considers this specific externality and estimates how it affects visitation, given the presence of local fires. To the best of my knowledge, this is the first study attempting to isolate the smoke effects in evaluating the impact of wildfires on outdoor recreation.

To explicitly separate smoke effects from the influence of local fire activity, the estimation equation was specified in a manner similar to equation 2 by replacing $closureDays_{iym}$

with the smoke measure:

$$\mathbb{E}\left[visitation_{iym}\right] = \exp\big(\beta_1 \cdot fireDays_{iym} + \beta_2 \cdot smokeDays_{iym} \\ + \boldsymbol{X}'_{iym}\boldsymbol{\gamma} + \alpha_{iy} + \delta_{im} + \theta_{ys} + \epsilon_{iym}\big) \quad (3)$$

where $smokeDays_{iym}$, my measure of smoke exposure, is the number of days in which the park is covered by smoke, as described in section 2.1. The results are displayed in Table 3. Columns (1) and (2) estimate the effect of fire days and smoke days separately, while column (3) estimates them simultaneously. The estimated effect of fire days is always significant and is not sensitive to the inclusion of smoke days in the model. However, the coefficient of smoke days is imprecisely estimated in column (3), suggesting that visitors, on average, are not responsive to wildfire smoke.

However, two major concerns might arise regarding the smoke measurement. The first concern is that the smoke effect estimated by simply adding the smoke variable in the regression might be confounded with the direct influence of local wildfire burn. This might be less of a concern if the measures of fire and smoke are not correlated. Yet my data reveal that the number of fire days and smoke days are highly correlated (correlation($fireDays_{iym}, smokeDays_{iym}$) = 0.549). To provide a sense of the correlation between fire and smoke measures, Figure 6 further explores a few of their key features. Panel (a) compares the time series of the monthly number of fire days and of smoke days for the entire period, 2006 – 2019. As can be seen, two series move up and down together, and the number of smoke days greatly exceeds the number of fire days for most of the months. Because the fire variable is designed to only capture the local wildfire activities, such a difference echoes findings from previous studies that suggest a significant share of smoke plumes comes from distant sources. Smoky days usually come earlier in the spring and peak in mid-summer, and they may stay in the region until the end of the year. In panel (b), I break the smoke days into two components: non-fire smoke days and smoke days with active local fires. On average, non-fire smoke days (blue area) contribute to more than 80% of the total. In contrast, when I break down the fire days into two components in a similar manner in Panel (c), I find that the majority of fire days also experience smoke (red area). Overall, these findings imply that fire days are likely a subset of smoke days, and it is important to focus on the effect of non-fire smoke days to avoid attributing the direct effect of wildfire burn to the effect of distant smoke.

To address the issue of the high correlation between fire and smoke measures, I re-estimate equation 3 by restricting the calculation of smoke days to dates with no active local fires. Thus, the coefficient estimates of β_2 can be interpreted as the effect of global externalities generated by wildfire smoke. The results are shown in column (4) of Table 3. Compared to column (3), the coefficient of smoke days is still small and statistically insignificant, while the estimated effect of fire days is fairly stable. These results indicate that the null effect of smoke days is not driven by the collinearity between fire and smoke measures.

Another potential concern is the measurement error in the satellite-based smoke data, as the extent of smoke plumes only approximate to the areas with heavy smoke. The detection of any plume is based on the elevated smoke concentration in the atmospheric column well above the surface. The presence of a smoke plume covering the park does not necessarily indicate a strong increase in the surface air quality (Rolph et al., 2009; Ford et al., 2017; Brey et al., 2018; Burkhardt et al., 2019). Consequently, simply overlaying the smoke polygons with the park boundary to determine if the park is exposed to smoke may overcount the number of smoke-impacted days. To address this measurement error, I employ a procedure adapted from Brey and Fischer (2016), who refine the smoke measure using ground-based pollution readings. Specifically, I first obtain data on ozone concentration (O3)[4] from the Environmental Protection Agency (EPA)'s Air Quality System (AQS) database and calculate the park-specific seasonal mean and standard deviation of ozone for days with no overlapping HMS smoke plume. Next, I define a park as smoke-impacted on a given day if (i) any part of it intersects smoke plumes

[4]I use ozone as the primary measure of surface-level air pollution because it is the most widely monitored pollutant in national parks and has daily measurements available for most of the parks (>22 parks) in my sample. These data preserve my sample to the largest extent possible, whereas PM2.5 measures are available for only 9 parks and focus on urban locations, and visibility measures from the Interagency Monitoring of Protected Visual Environments (IMPROVE) program only report data every third day or every sixth day.

on that day, and (ii) the ozone concentration for that day is more than one standard deviation above the park-specific seasonal mean. By restricting the definition of smoke days, I am able to reduce the possibility of misclassifying a day with little surface smoke concentration as smoke-impacted, thereby reducing the measurement error.

Using this refined measure of smoke days, I replicate Table 3 and report the result in Table 4. Column (4) of Table 4 corresponds to my preferred estimates, as it addresses two aforementioned concerns and reports the coefficient estimates of non-fire smoke days, which is adjusted by O3. Comparing the preferred estimates with the initial estimates (column (3) of Table 3), while the estimated effect of fire days slightly decreases, I find that it remains highly significant. On the other hand, the magnitude of the smoke effect drastically drops. The results after addressing two concerns lend further credence to my initial findings that park visitors, on average, do not respond to smoke from distant sources to a statistically significant degree. I found similar results using different methods to adjust the smoke day (see Appendix Table A.3). Therefore, the fire-induced visitation loss is primarily attributed to local fires.

4.5 Local economic impacts of wildfires

I report the relationship between local wildfire activity and visitation in sections 4.1 – 4.2 and provide evidence that visitation is not responsive to smoke from a remote source in section 4.4. Based on those findings, this section provides back-of-the-envelope estimates for the implied change in visitation and economic loss due to wildfires for the local economy[5]. To account for the within-year substitution pattern, the estimated concurrent effect (-0.045% per thousand acres) and lagged effect (-0.046% per thousand acres) are both used to calculate the annual visitation loss. The mean annual visitation loss is computed as the product of the visitation reduction effect of wildfires, park-level annual mean wildfire size, and the baseline annual visitation of each park.

I find that the visitation loss for an average fire year is 717,013 visits or 1.35% of the annual average attendance to 32 parks in my sample. Figure 7 shows the heterogeneity in

[5]Local gateway regions are defined as all counties within or intersecting a 60-mile radius around each park boundary.

the loss of visitation across parks. It's noteworthy that these estimates assume the same elasticity of visitation with respect to wildfire size across different parks, so the variation in the park-specific visitation losses stems from the difference in the mean wildfire size and the baseline visitation of each park. Correspondingly, the most affected parks happen to be the most heavily visited parks with a high level of wildfire activities. In particular, Yosemite national park (NP) has the largest visitation loss, which amounts to 3.51% of annual average Yosemite visitation.

I compare my estimates of visitation loss to the numbers reported by three previous studies. Duffield et al. (2013) estimate that for Yellowstone NP, the visitation loss in an average fire year for the period 1986 – 2011 is 1.3%, which aligns closely with my estimate for Yellowstone NP (1.59%). A comparison with Kim and Jakus (2019) reveals that my estimates for Arches, Canyonlands, Capitol Reef National Park are about 2 times bigger than their estimates, but I obtain a slightly smaller visitation loss for Bryce Canyon NP. Such discrepancy might be due to their decisions to trim the data to only include summer months and rely on park-level times series analysis instead of fixed-effect strategies employed in this study.

A much more meaningful comparison should be to a recent study, Gellman et al. (2021), using an empirical strategy similar to mine. Instead of using NPS's official visitation count data, Gellman et al. (2021) utilize a rather new daily data on campground use from Recreation.gov from 2008 to 2017. They estimate the visitation loss due to wildfire and smoke average is about 1.39 million visits per year, which is approximately one time larger than my estimate (717,013 visits per year). There are at least two reasons why Gellman et al. (2021) may overestimate the wildfire impacts on the affected visitor population: First, campers, as a specific sub-population of all national park visitors, might have a substantially larger elasticity of demand with respect to burned acres, which has been documented by Duffield et al. (2013). I reestimated my preferred model specification in 4.1 by restricting the NPS total visitation data to the "backcountry users and campers" category as the dependent variable, which is a more comparable subsample to campers in Gellman et al. (2021). Using the restricted sample, I found greater responsiveness of backcountry users and campers to the change in wildfire size: one thousand acres increase in wildfire size decrease backcountry visits by 0.104%, compared to my original estimate of 0.064% for total visitation. If I use the marginal effect estimated based on the restricted sample in the back-to-envelope calculation, an impact of 1.17 million visitation loss would be estimated due to wildfire. Furthermore, compared to day users, overnight campers may also be more sensitive to bad air quality due to high smoke exposure, given their many hours spent in the backcountry. And I demonstrate that national park visitors, comprised of both day users and campers, are surprisingly resilient to smoky days. Combining these two factors into consideration leads me to believe that running analysis on campers only might overstate the visitation loss attributed to wildfire activities. My approach based on the entire population of all national park visitors, on the other hand, provides more representative estimates of visitation loss across all types of recreation users and regions.

Is a roughly 700,000 annual visitation loss economically meaningful to justify a change in mitigation planning? The answer to this question is location-specific. To demonstrate this point, I convert my estimates of park-specific visitation loss to the park's monetary contribution to the local economy (including visitor spending, labor income, and total economic output) based on the multipliers estimated by NPS. My estimates imply a \$66.71 million reduction in visitor spending, \$29.61 million loss in labor income, and a total loss of \$86.22 million in local economic output per year. Among all local communities around the national parks, the most vulnerable local economies appear to be the ones that heavily rely on tourism, for example, the gateway communities around Redwood, Crater Lake, and Glacier NP. In those communities, the direct loss in labor income and economic output due to the wildfire-caused visitor reduction could account for up to 5%, which translates to an annual loss of about \$2200 to local workers. Furthermore, it is worth noting that such an estimate could likely represent a lower bound of the total economic loss to local communities because it does not account for region-wise and industry-wise spillover effects. Last but not least, an complete cost-benefit assessment should incorporate the wildfire damage to property, ecosystems, and the estimated costs of firefighting. However, such an estimation is beyond the scope of the current study.

5 Conclusion

This paper has presented a large-scale analysis of the impacts of wildfires on visitor use in national parks across the western US. I have compiled a comprehensive dataset on wildfire occurrences, smoke and visitation from several sources at the monthly-park level spanning from 1993 to 2019. Building on prior research evidence, I perform a series of tests to address the concern of reverse causality and ensure my results are not confounded by spatial spillovers from neighboring recreation areas. I consistently find a statistically significant and negative effect of burned area on aggregate monthly visitation. My preferred estimates show that an additional thousand acres burned is associated with a 0.064% decrease in the current month visitation. I also find evidence of persistent impacts on the subsequent month's visitation and prominent impacts for large fires and/or fires burning in the heavily visited areas.

I then highlight the importance of the main channel behind the above findings: emergency fire closures, which account for the majority of visitation loss and have impacts nearly 10 times larger than fires not causing any closures. I show that an additional ten days of park closure due to wildfires will lead to a total visitation loss of 22.73% per month. My results support and extend previous literature on assessing the non-market value of access to recreation sites in a regional context.

I also present estimates of global externalities associated with wildfire smoke that, to my knowledge, is the first paper to disentangle the non-local smoke effects from the local fire effects. I find that without the presence of local fires, the distinct impact of smoke from remote sources on visitation is surprisingly small[6]. This is likely due to the fact that, after the past five years of severe fires with popular parks getting overcrowded, visitors would rather continue their plan even under the smoky conditions than forego a

[6] Keiser et al. (2018) and Gellman et al. (2021) provide some empirical evidence that recreation use declines in response to poor air quality. However, a direct comparison of these results to my results can be problematic due to sampling differences. For example, Gellman et al. (2021) study is based on campsites on all public lands, not just national parks, while Keiser et al. (2018) study selected 33 national parks across the continental US.

visit and cancel their bookings made a year ago. Unless threatened by active local fires and disrupted from accessing the park, visitors are willing to travel despite the heavy smoke from distant sources.

Overall, my results underscore the importance of local efforts in combating the damaging impacts of wildfires on national park gateway communities, as the climate warms. To mitigate the visitation loss due to mandatory evacuations and park closures, it's crucial to factor the potential economic impacts of reduced visitors on local tourism into the wildfire management plan. For example, park managers can promote the less-visited areas and prioritize wildfire prevention and suppression efforts on "visitor hubs" early in the season. In addition, the small effect of smoky days indicates there may be considerable benefits from the use of off-season prescribed burning.

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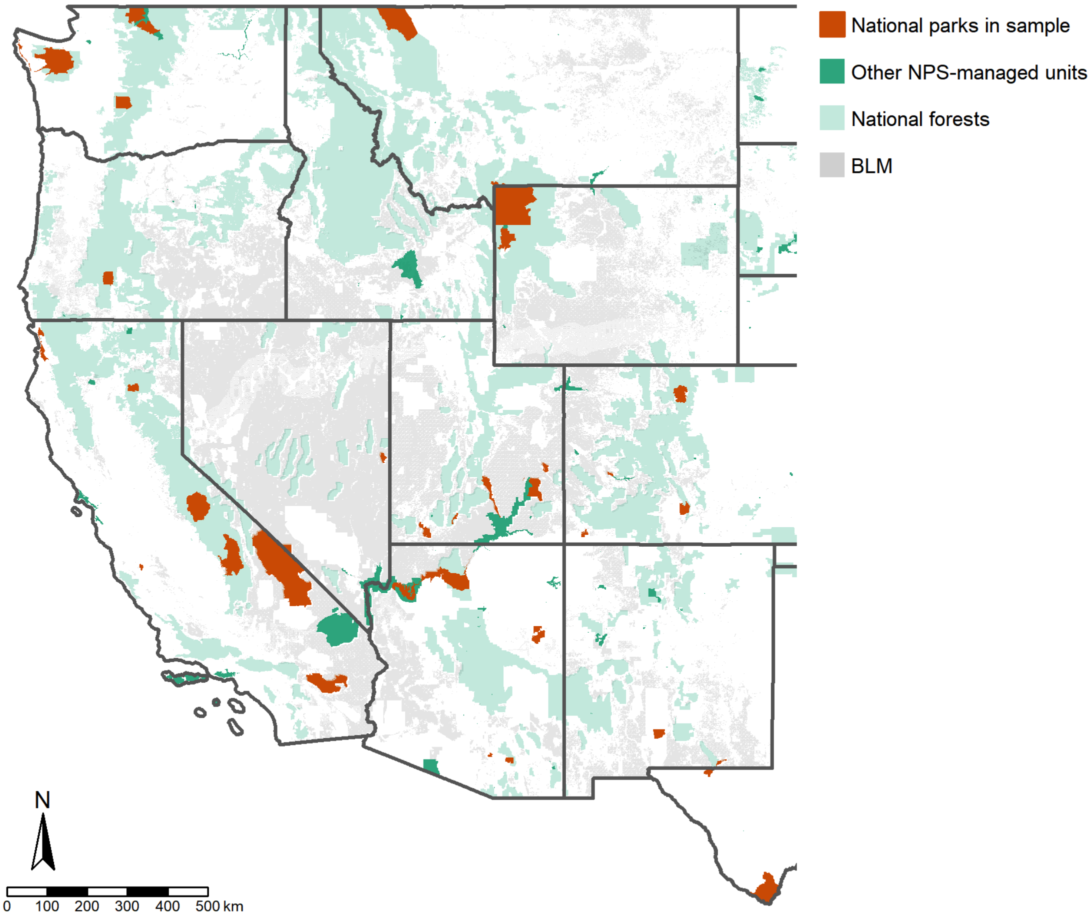
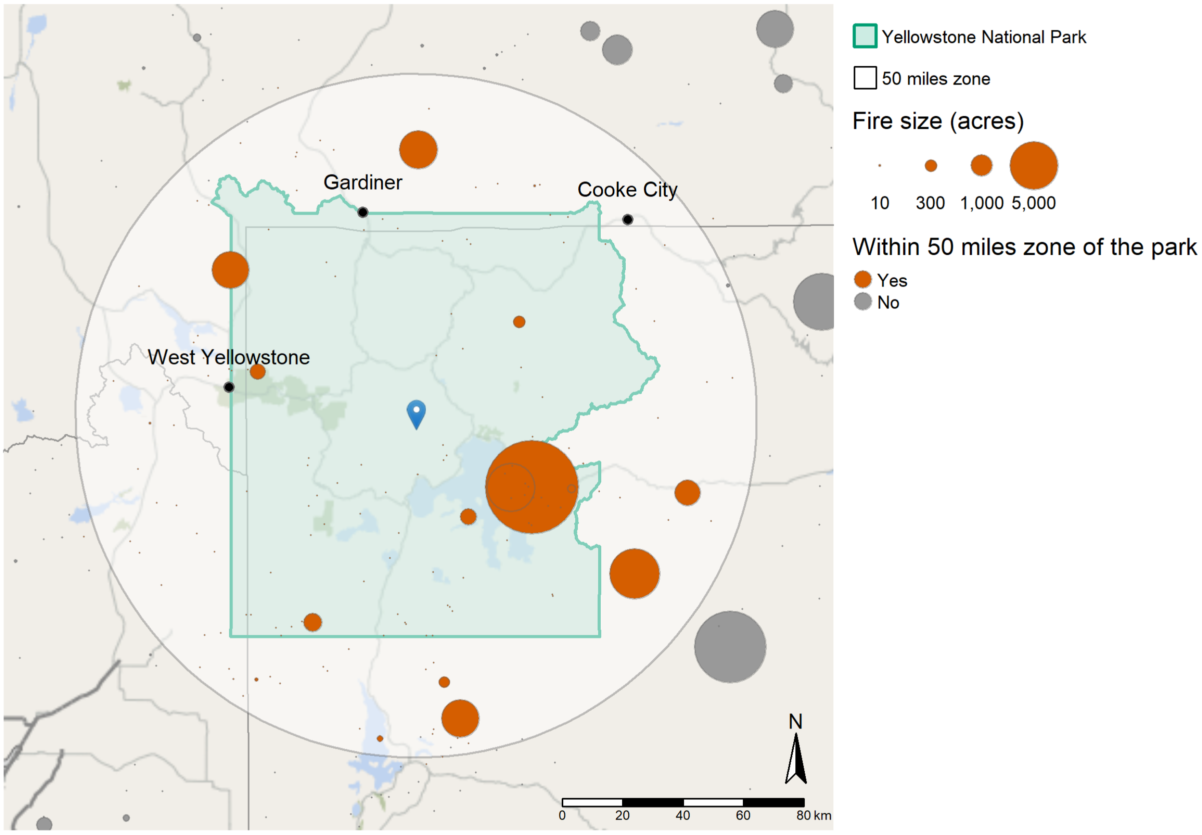
Figures

Figure 1: Locations of 32 western national parks in my sample



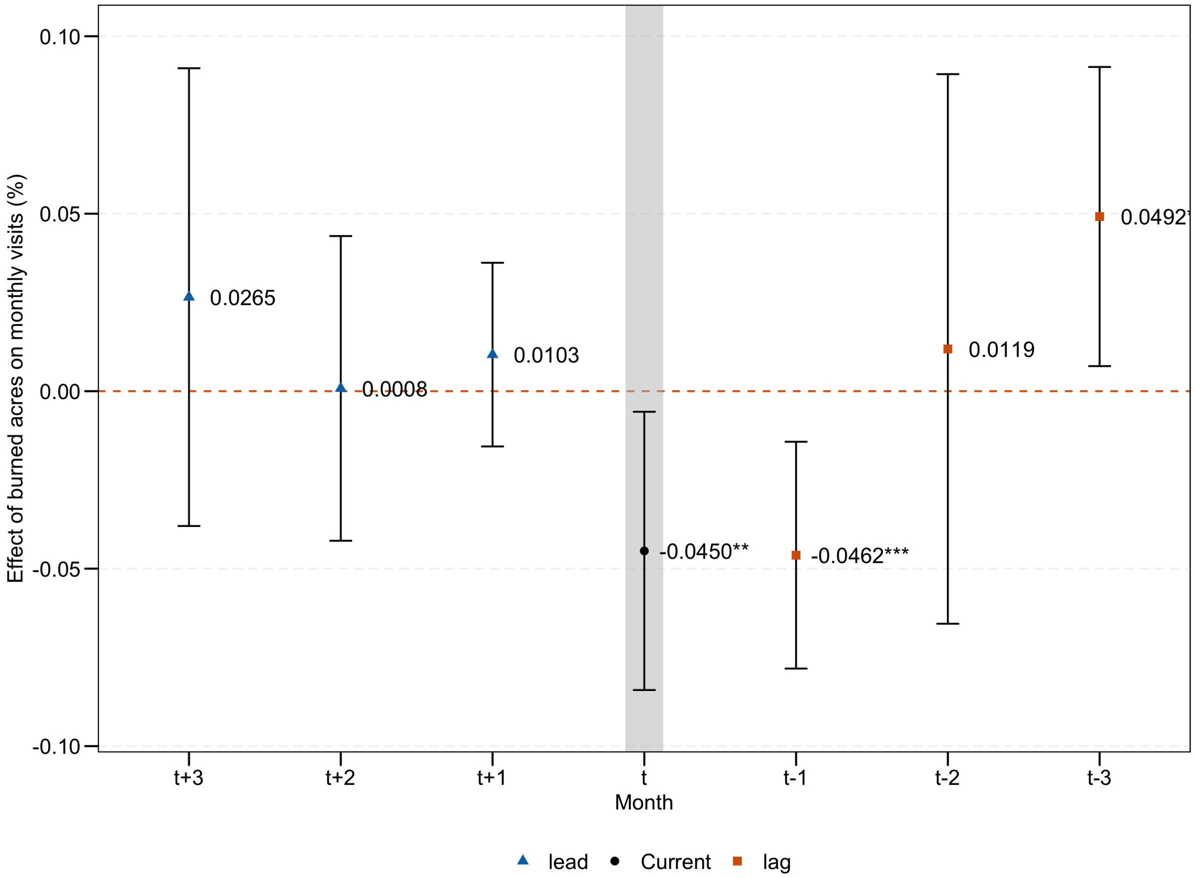
Notes: This figure plots the locations of the 32 western national parks considered in this study.

Figure 2: Illustration describing the construction of the total fire size



Notes: This figure illustrates all wildfire events within a 50-mile radius around the centroid of Yellowstone National Park in September 2003. Each red bubble represents a wildfire event. The location of the bubble shows the ignition point, and the size of the bubble denotes the size of the wildfire event in terms of the total burned area within the final perimeter.

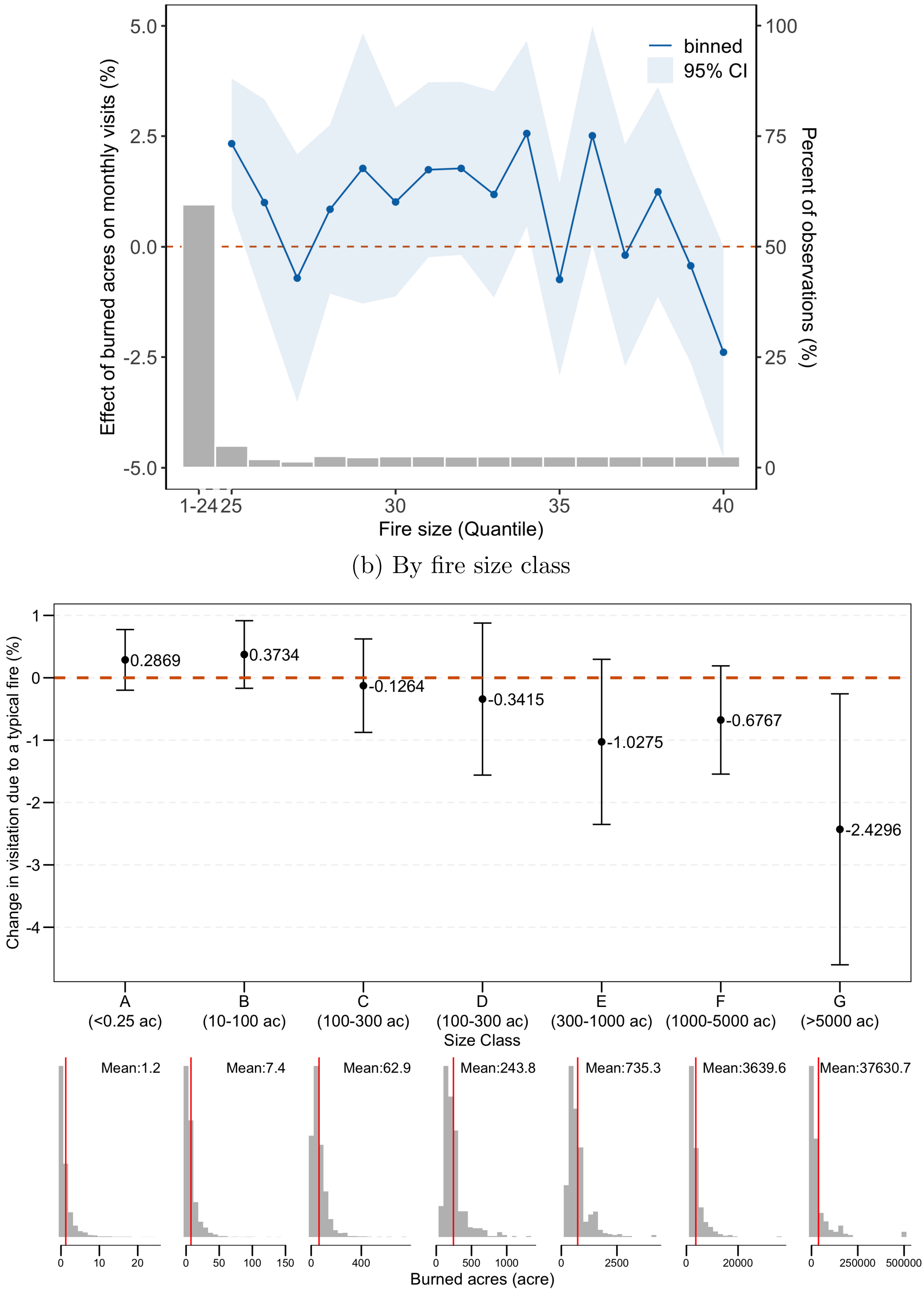
Figure 3: Estimated effects of leads and lags



Notes: This figure plots estimated coefficients from a lead-and-lag test where the independent variables are the wildfire sizes in month $t+k$ ($k = 3, 2, 1, 0, -1, -2, -3$). The whiskers represent the 95% confidence intervals based on standard errors clustered at the park-by-year level. ***p<0.01, **p<0.05, *p<0.1. The insignificant coefficients of all leads suggest the estimation strategy passes the placebo test. The significant coefficient for the first lag term means the impact of wildfire on visitation may persist in the following month.

Figure 4: Non-linear effects of burned acres on visitation

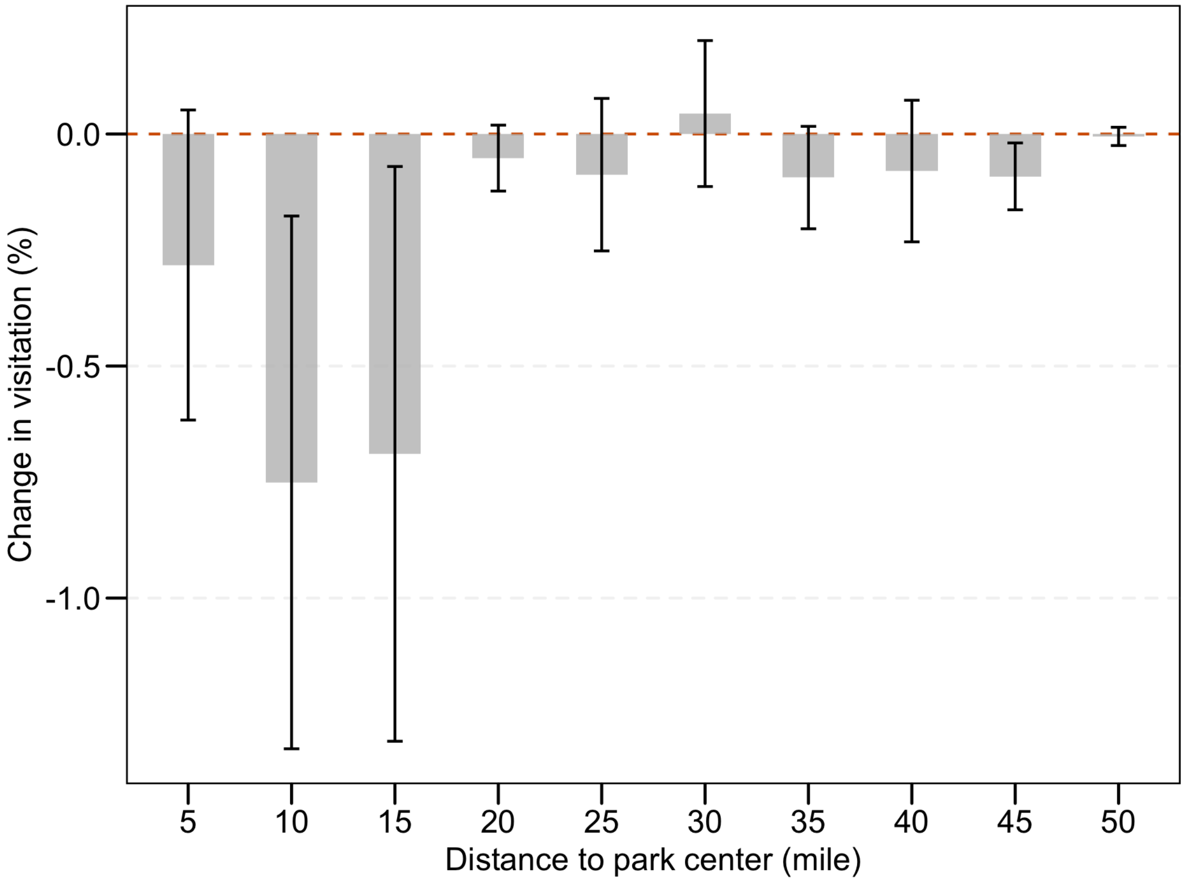
(a) Binned specification



(b) By fire size class

Notes: Panel (a) shows the estimated coefficients of indicators for 16 bins of total fire size. The shaded blue area is the 95% confidence interval, where the standard errors are clustered at the park-by-year level. The grey histogram shows the proportion of observations in each bin. Panel (b) shows the point estimates and 95% confidence interval of response of visitation to a mean wildfire in size class A – G. The distributions of burned acres and average wildfire size in each size class are shown at the bottom.

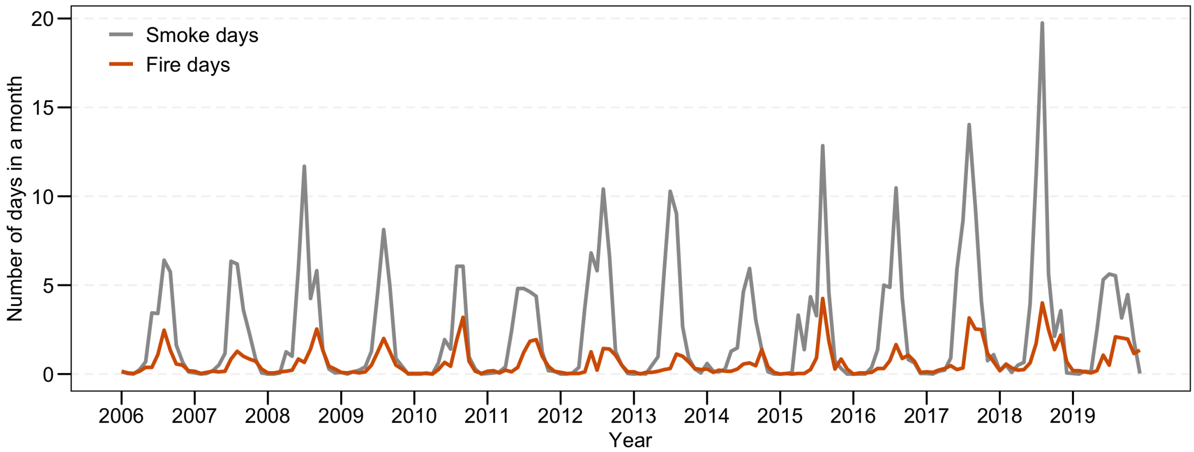
Figure 5: The effect of burned acres by distance to the park center



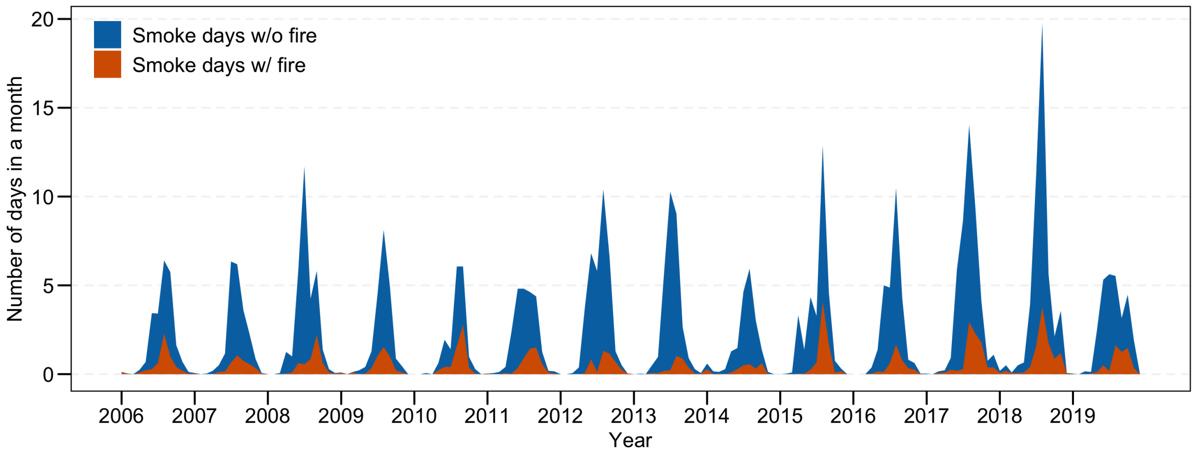
Notes: This figure plots the point estimates and 95% confidence intervals of the marginal effects of total fire size within certain distance bins to the park's geographic centroid. These marginal effects are estimated by regressing visitation on the total fire size within each 5-mile distance bin. Distance is shown on the x-axis, such that the point estimates located at x = 10 can be interpreted as the impact of total burned area within 5 – 10 miles from the park center.

Figure 6: Monthly fire days and smoke days with their components

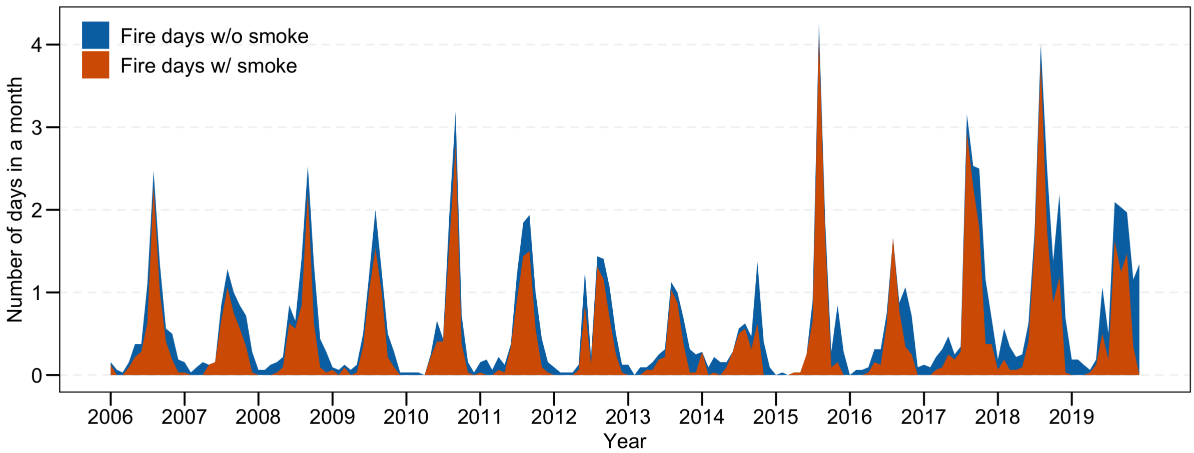
(a) Trends



(b) Contribution to smoke days

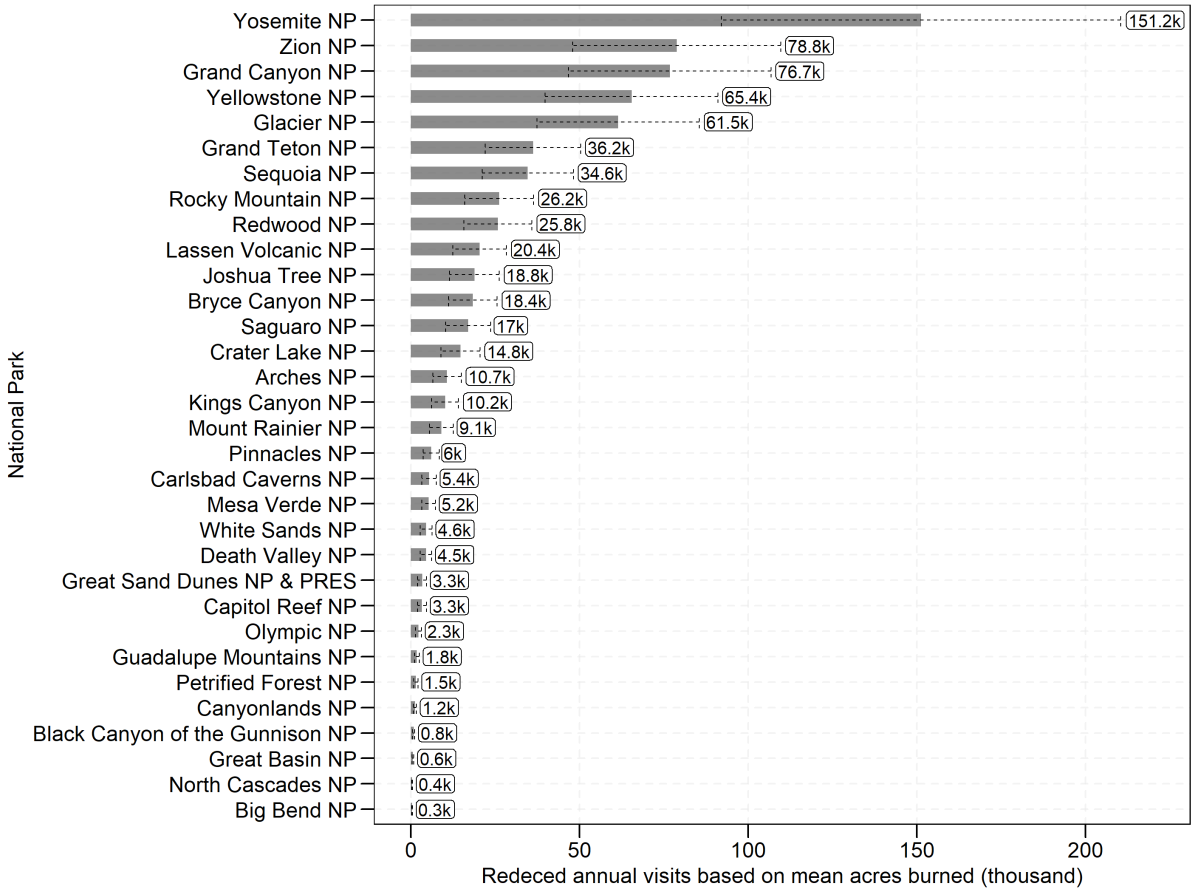


(c) Contribution to fire days



Notes: Panel (a) plots the trends in the number of fire days in each month and the number of smoke days in each month from 2006 to 2019. Panel (b) decomposes the number of smoke days into two components: the number of non-fire smoke days (blue area) and the number of smoke days with active local fires (red area). Panel (c) decomposes the number of fire days into the number of fire days without smoke (blue area) and the number of smoke days with smoke (red area).

Figure 7: Visitation Loss due to wildfires



Notes: This figure plots the estimates and 95% confidence interval from a back-of-the-envelope calculation of implied reductions in annual visitation due to wildfires

Tables

Table 1: Effect of burned acres on monthly recreation visits

	(1)	(2)	(3)	(4)
Fire size within 50 miles (thousand ac)	-0.0527**	-0.0413*	-0.0634**	-0.0636**
	(0.0222)	(0.0232)	(0.0303)	(0.0304)
Observations	8,736	8,736	8,736	8,736
Number of Parks	32	32	32	32
Mean fire size (thousand ac)	2.312	2.312	1.687	1.687
Controls	Yes	Yes	Yes	Yes
Park $\times$ Year FE	Yes	Yes	Yes	Yes
Park $\times$ Fire Season FE	Yes			
Fire Season $\times$ Year FE	Yes			
Park $\times$ Month FE		Yes	Yes	Yes
Month-of-Sample FE		Yes	Yes	Yes
Lightning fires only			Yes	Yes
Nearby Parks				Yes

Notes: This table reports the effect of total burned acres (in thousand acres) on monthly recreation visits from Poisson pseudo-maximum-likelihood estimates. All coefficient estimates are multiplied by 100 to demonstrate the effect in percentage points. All regressions include 10 bins of monthly mean temperature, monthly precipitation and monthly mean vapor pressure deficit (VPD) as well as income-adjusted gasoline price. Fixed-effects strategies and additional controls are listed at the bottom of this table. Column (4) is the preferred specification. Standard errors are clustered at the park-by-year level. ***p<0.01, **p<0.05, *p<0.1.

Table 2: Effect of fire closures on monthly recreation visits

	(1)	(2)	(3)
Fire closure days	-1.856***	-2.007***	-2.273***
	(0.358)	(0.330)	(0.300)
Fire days	-0.251***	-0.247***	-0.241***
	(0.0942)	(0.0949)	(0.0923)
Observations	1,560	1,560	1,560
Number of Parks	13	13	13
Mean fire closure days	0.0677	0.0677	0.0477
Mean fire days	0.828	0.787	0.793
Controls	Yes	Yes	Yes
Park × Year FE	Yes	Yes	Yes
Park × Month FE	Yes	Yes	Yes
Month-of-Sample FE	Yes	Yes	Yes
Lightning fires only			Yes

Notes: This table reports the effect of monthly fire closure days and fire days on monthly recreation visits from Poisson pseudo-maximum-likelihood estimates of eq. (2). All coefficient estimates are multiplied by 100 to demonstrate the effect in percentage points. All regression includes 10 bins of monthly mean temperature, monthly precipitation and monthly mean vapor pressure deficit (VPD) as well as income-adjusted gasoline price. Fixed-effects strategies and additional controls are listed at the bottom of this table. Column (2) instead uses the adjusted fire days, which exclude dates with closure events in the calculation. ***p<0.01, **p<0.05, *p<0.1.

Table 3: Effect of wildfire smoke on monthly recreation visits

	(1)	(2)	(3)	(4)
Fire days	-0.370***		-0.433***	-0.338***
	(0.0876)		(0.104)	(0.108)
Smoke days		-0.0920	0.109	0.111
		(0.0615)	(0.0734)	(0.0796)
Observations	5,376	5,376	5,376	5,376
Number of Parks	32	32	32	32
Mean fire days	0.656		0.656	0.656
Mean smoke days		2.400	2.400	1.956
Controls	Yes	Yes	Yes	Yes
Park × Year FE	Yes	Yes	Yes	Yes
Park × Month FE	Yes	Yes	Yes	Yes
Month-of-Sample FE	Yes	Yes	Yes	Yes
Non-fire smoke days only				Yes

Notes: This table reports the effect of monthly fire days and smoke days on monthly recreation visits from Poisson pseudo-maximum-likelihood estimates of eq. (3). All coefficient estimates are multiplied by 100 to demonstrate the effect in percentage points. The "smoke days" is calculated as the number of days in which the park intersects any smoke plume. Column (4) instead use the non-fire smoke days which exclude dates with active local fire in calculation. All regression includes 10 bins of monthly mean temperature, monthly precipitation, and monthly mean vapor pressure deficit (VPD) as well as income-adjusted gasoline price. Fixed-effects strategies and additional controls are listed at the bottom of this table. ***p<0.01, **p<0.05, *p<0.1.

Table 4: Effect of wildfire smoke on monthly recreation visits (Adjusted by ozone)

	(1)	(2)	(3)	(4)
Fire days	-0.370***		-0.367**	-0.368***
	(0.0876)		(0.159)	(0.134)
Adjusted smoke days		-0.204	-0.0201	0.0399
		(0.149)	(0.170)	(0.238)
Observations	5,376	3,381	3,381	3,381
Number of Parks	23	23	23	23
Mean fire days	0.656		0.656	0.656
Mean smoke days		0.833	0.833	0.651
Controls	Yes	Yes	Yes	Yes
Park × Year FE	Yes	Yes	Yes	Yes
Park × Month FE	Yes	Yes	Yes	Yes
Month-of-Sample	Yes	Yes	Yes	Yes
Non-fire smoke days only				Yes

Notes: This table reports the effect of monthly fire days and smoke days on monthly recreation visits from Poisson pseudo-maximum-likelihood estimates of eq. (3). All coefficient estimates are multiplied by 100 to demonstrate the effect in percentage points. The "smoke days" is calculated as the number of days in which (i) the park intersects any smoke plume, and (ii) the ozone concentration for that day is more than one standard deviation above the park-specific seasonal mean. Column (4) instead use the non-fire smoke days which exclude dates with active local fire in calculation. All regression includes 10 bins of monthly mean temperature, monthly precipitation, and monthly mean vapor pressure deficit (VPD) as well as income-adjusted gasoline price. Fixed-effects strategies and additional controls are listed at the bottom of this table. ***p<0.01, **p<0.05, *p<0.1.

A Appendix

A.1 Data Appendix

Table A.1: Summary statistics, 1992 - 2019

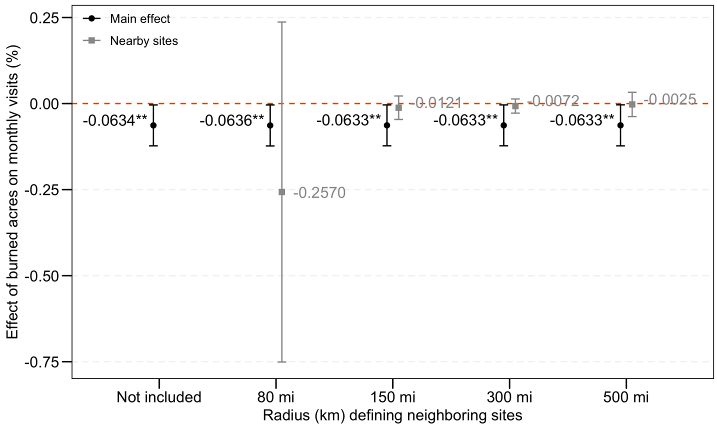
	Mean	Std. Dev.	Min.	Max.	N
Recreation Use					
Recreation Visitation (times)	99,977	147,722	0	980,702	9,216
Backcountry visitation (nights)	21,074	49,531	0	416,510	9,216
Wildfire and smoke data					
Wildfire size within 50 miles (thousand acres)	22.14	169.20	0.00	5,038.33	9,216
Wildfire size of lightning fires within 50 miles (thousand acres)	16.02	152.05	0.00	5,028.33	9,216
Wildfire size of class A fires within 50 miles (thousand acres)	0.00	0.01	0.00	0.24	9,216
Wildfire size of class B fires within 50 miles (thousand acres)	0.02	0.06	0.00	1.40	9,216
Wildfire size of class C fires within 50 miles (thousand acres)	0.08	0.31	0.00	7.38	9,216
Wildfire size of class D fires within 50 miles (thousand acres)	0.15	0.69	0.00	13.13	9,216
Wildfire size of class E fires within 50 miles (thousand acres)	0.41	2.05	0.00	41.61	9,216
Wildfire size of class F fires within 50 miles (thousand acres)	2.05	11.29	0.00	358.52	9,216
Wildfire size of class G fires within 50 miles (thousand acres)	13.31	149.89	0.00	4,999.45	9,216
Wildfire size within 0-5 miles (thousand acres)	0.32	6.91	0.00	280.80	9,216
Wildfire size within 5-10 miles (thousand acres)	0.62	7.82	0.00	232.21	9,216
Wildfire size within 10-15 miles (thousand acres)	0.59	6.57	0.00	188.47	9,216
Wildfire size within 15-20 miles (thousand acres)	1.71	36.85	0.00	1,516.24	9,216
Wildfire size within 20-25 miles (thousand acres)	0.83	13.35	0.00	872.75	9,216
Wildfire size within 25-30 miles (thousand acres)	1.25	22.57	0.00	816.36	9,216
Wildfire size within 30-35 miles (thousand acres)	3.23	50.87	0.00	1,628.18	9,216
Wildfire size within 35-30 miles (thousand acres)	2.09	34.69	0.00	1,248.98	9,216
Wildfire size within 40-45 miles (thousand acres)	1.48	25.20	0.00	1,153.04	9,216
Wildfire size within 45-50 miles (thousand acres)	3.91	117.33	0.00	4,999.47	9,216
Days of fire closures	0.07	0.70	0.00	12.64	1,560
Days of fire closures caused by lightning fires	0.05	0.60	0.00	12.64	1,560
Days of local fires	0.66	2.48	0.00	29.00	5,376
Days of smoke	2.40	4.79	0.00	31.00	5,376
Days of smoke without local fires	1.96	3.94	0.00	31.00	5,376
Days of smoke adjusted by ozone	0.83	2.19	0.00	23.00	3,389
Controls					
Mean temperature (°C)	9.66	9.38	-12.70	32.25	9,216
Total Precipitation (mm)	68.88	103.93	0.00	1,093.07	9,216
Mean vapor pressure deficit (hPa)	10.25	8.12	0.20	45.78	9,216
Income-adjusted gasoline price index (price/thousand $)	0.06	0.02	0.03	0.12	8,736
Wildfire size on nearby NPS units within 80 miles (thousand acres)	0.48	6.62	0.00	305.26	9,216

Notes: This table reports the summary statistics at the park-by-month level for all 32 western national parks in my sample. All wildfire size variables are for the period 1992 - 2015. The fire closure variables are available from 2010 to 2019. The smoke analysis uses data available from 2006 to 2019.

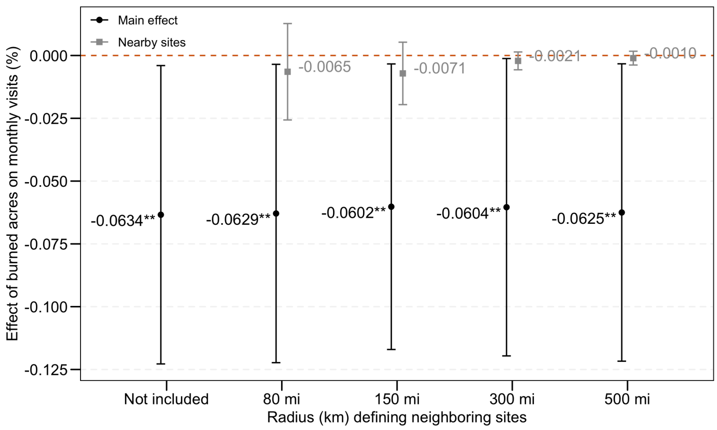
A.2 Supplementary Figures

Figure A.1: Comparison of main effects with the spillover effects

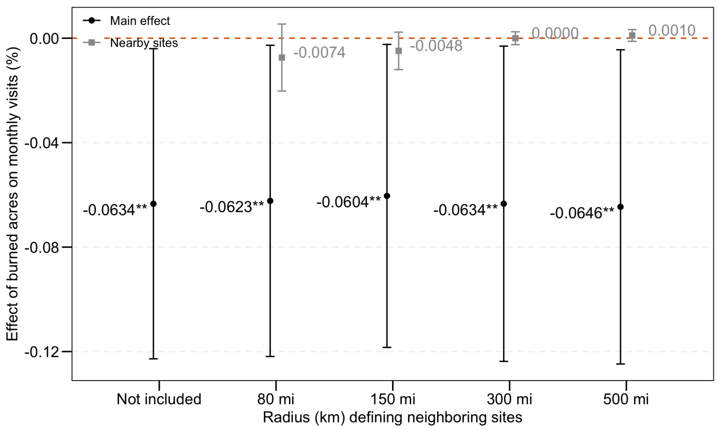
(a) Lands managed by NPS



(b) Lands managed by NPS and USFS

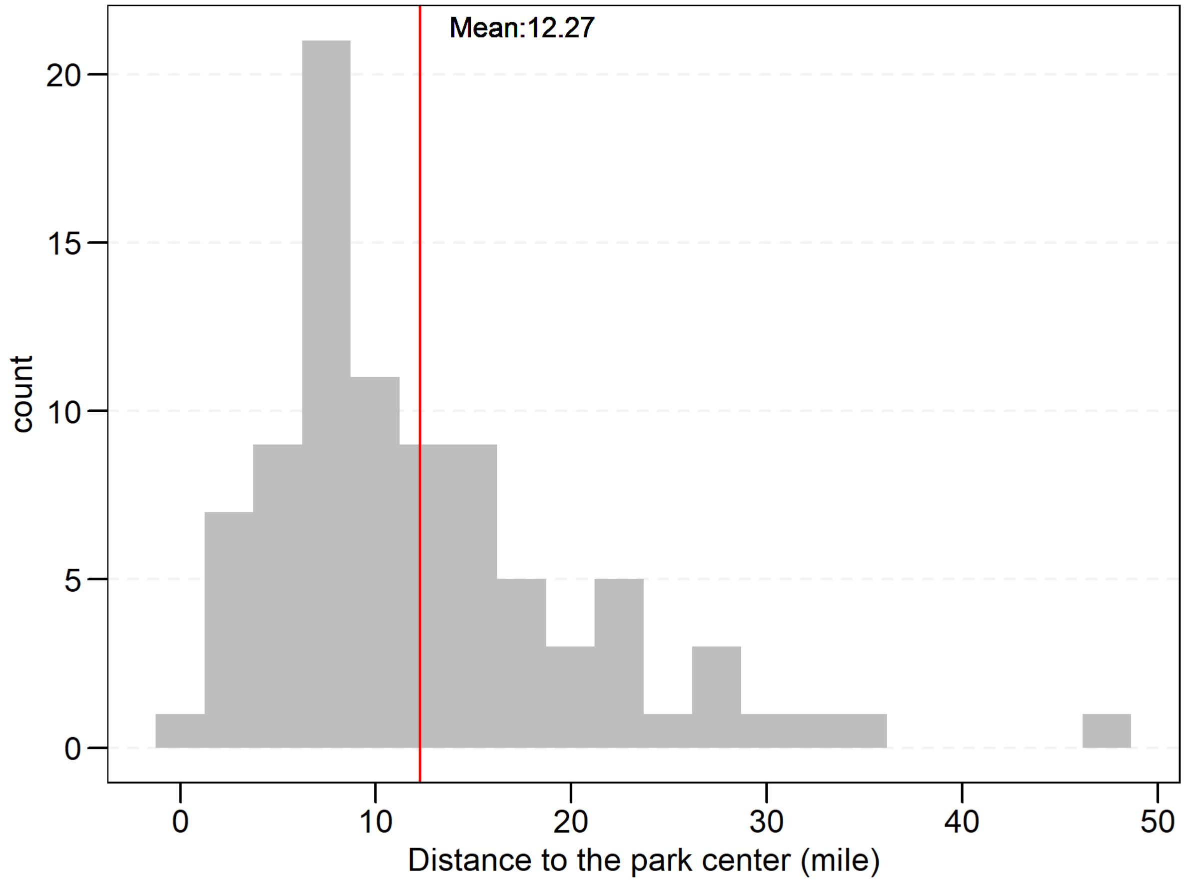


(c) Lands managed by NPS, USFS and BLM



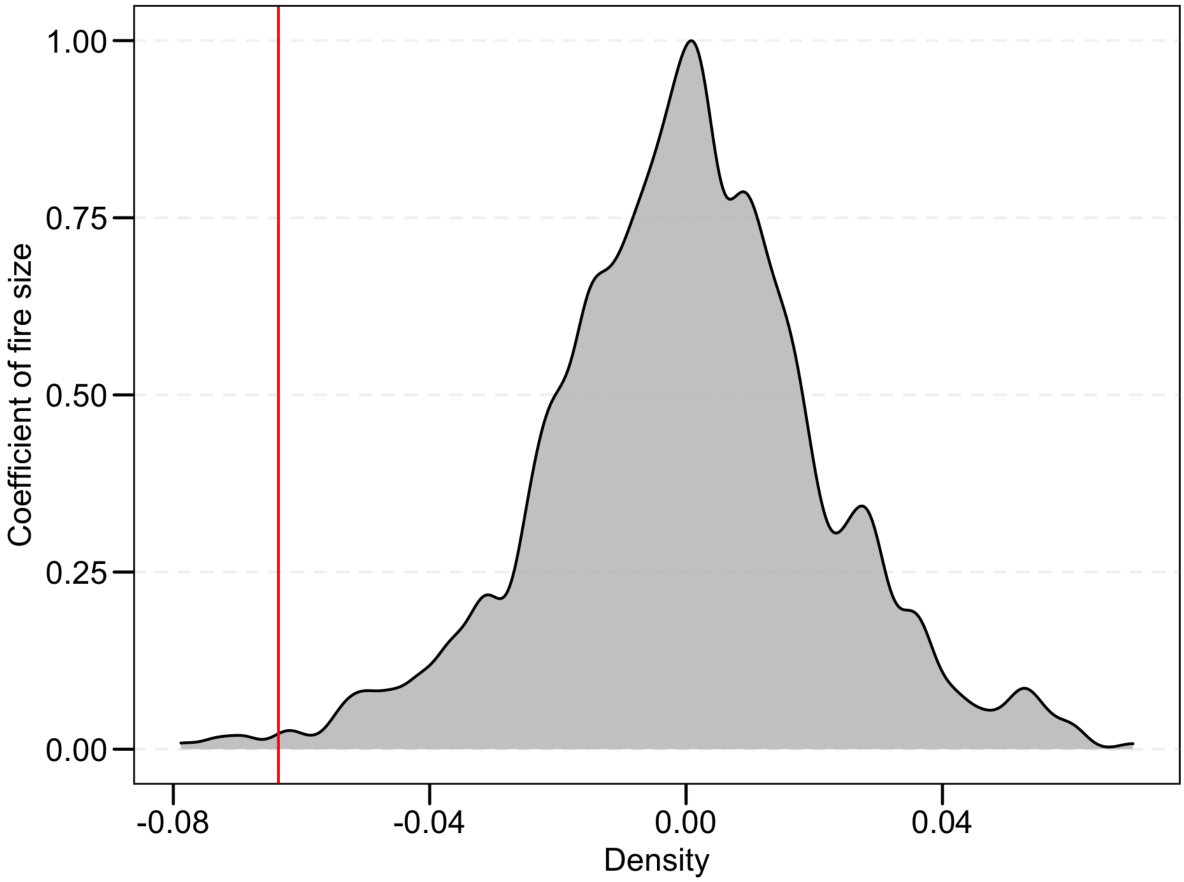
Notes: Each panel plots the estimated effect of wildfire size within a 50-mile radius around the national park of interest (black bars) together with the estimated effect of aggregate wildfire size at all nearby recreational areas within a specified radius (grey bars). The radius cutoff used to define "nearby" is shown on the x-axis The panel title reports the type of the recreation units that are considered as "nearby parks" in proximity to the national park of interest.

Figure A.2: The effect of burned acres by distance to the park center



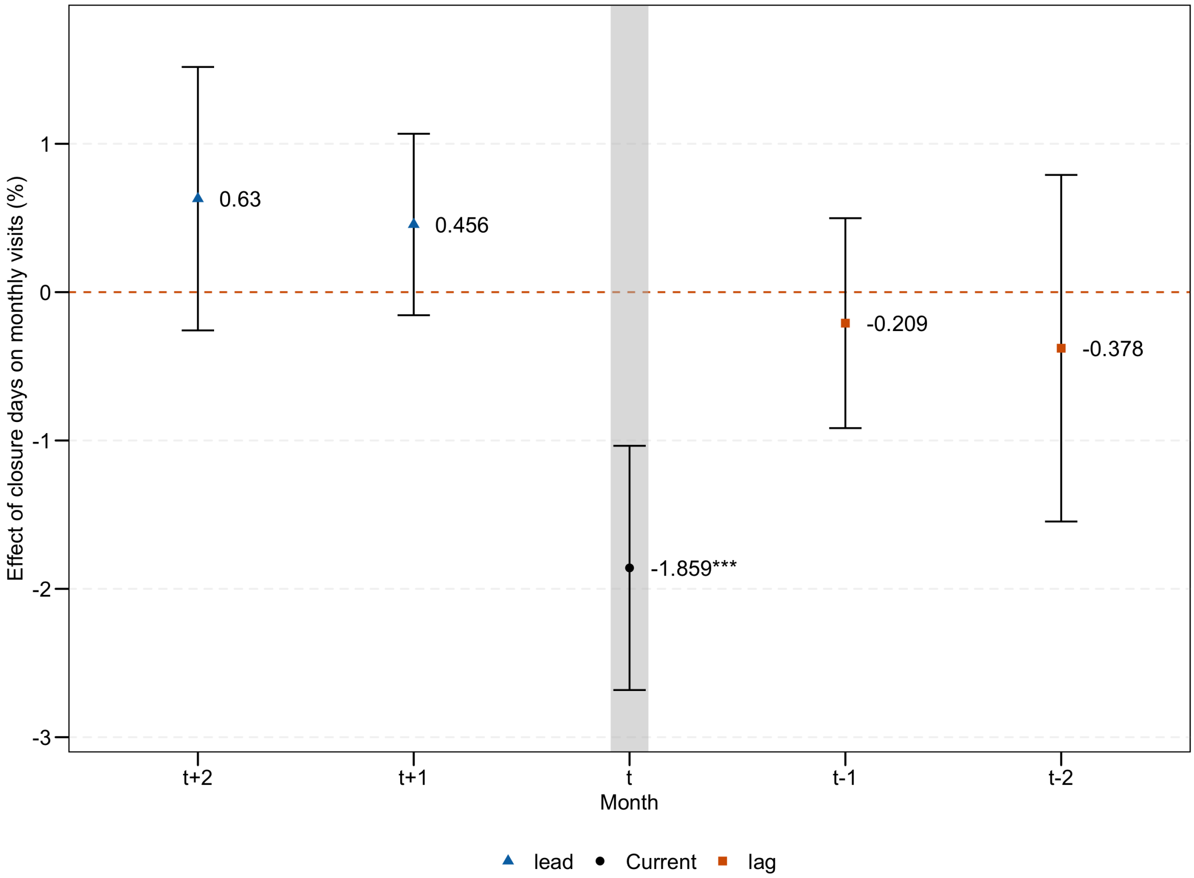
Notes: This figure shows the distribution of the distances from visitor centers (n = 88) to their associated parks' geographic centroids.

Figure A.3: Exact distribution for test of the null effect of wildfire size



Notes: This figure shows the distribution for the permutation test of the null effect of wildfire size (β). I permute the dependent variables $fireSize_{iym}$ 1000 times and then plot the distribution of its coefficient estimates. The red line shows where the actual estimate reported in column (4) of Table 1 lies. The one-sided p-value from Wald t-statistics is 0.00, i.e., rejecting the null $\beta = 0$.

Figure A.4: Effects of leads and lags of fire closure days



Notes: This figure plots estimated coefficients from a lead-and-lag test of eq. (2), where the independent variables are instead the fire closure days in month $t+k$ ($k = 3, 2, 1, 0, -1, -2, -3$). The whiskers represent the 95% confidence intervals based on standard errors clustered at the park-by-year level. ***p<0.01, **p<0.05, *p<0.1.

A.3 Supplementary Tables

Table A.2: Robustness check of the main results

	(1) Burned acres inside park (only)	(2) Burned acres within 5-mile buffer around park	(3) Piecewise linear	(4) Quadratic	(5) Clustered by park	(6) Two-way clustered	(7) Log-linear model	(8) Subsample of fire season only
Fire size (thousand ac)	-0.491*	-0.205*	-0.0634**	-0.0652**	-0.0636**	-0.0636*	-0.0500*	-0.0701**
	(0.264)	(0.123)	(0.0307)	(0.0320)	(0.0292)	(0.0330)	(0.0262)	(0.0357)
Observations	8,736	8,736	8,736	8,736	8,736	8,736	8,721	2,944
Number of Parks	32	32	32	32	32	32	32	32
Mean fire size	0.179	0.380	1.687	1.687	1.687	1.687	1.687	1.687
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Park × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Park × Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-of-Sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lightning fires only	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nearby Parks	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column presents the coefficient estimate from re-estimating eq. (1) with an alternative definition of wildfire size, functional forms for weather controls, clustering choices, specification or using the subsample of the data. All regressions include park-by-year, park-by-month and month-of-sample fixed effects, as well as controls for weather and income-adjusted gasoline price. ***p<0.01, **p<0.05, *p<0.1.

Table A.3: Effect of wildfire smoke: other adjusted smoke measures

	(1)	(2)	(3)	(4)
Panel A: Fully covered by smoke				
Fire days	-0.370***		-0.366***	-0.354***
	(0.0876)		(0.0876)	(0.0845)
Smoke days		-0.135	-0.0722	0.112
		(0.114)	(0.114)	(0.126)
Observations	5,376	5,376	5,376	5,376
Number of Parks	32	32	32	32
Mean fire days	0.656		0.656	0.656
Mean smoke days		1	1	0.914
Panel B: Medium density smoke				
Fire days	-0.370***		-0.342***	-0.369***
	(0.0876)		(0.111)	(0.0942)
Smoke days		-0.306***	-0.0588	0.0104
		(0.0872)	(0.0950)	(0.120)
Observations	5,376	3,381	3,381	3,381
Number of Parks	32	32	32	32
Mean fire days	0.656		0.656	0.656
Mean smoke days		0.955	0.955	0.653
Panel C: Overlap any developed area				
Fire days	-0.370***		-0.430***	-0.338***
	(0.0876)		(0.104)	(0.0844)
Smoke days		-0.0916	0.106	0.115
		(0.0625)	(0.0751)	(0.0821)
Observations	5,376	3,381	3,381	3,381
Number of Parks	32	32	32	32
Mean fire days	0.656		0.656	0.656
Mean smoke days		2.283	2.283	1.868
Controls	Yes	Yes	Yes	Yes
Park × Year FE	Yes	Yes	Yes	Yes
Park × Month FE	Yes	Yes	Yes	Yes
Month-of-Sample FE	Yes	Yes	Yes	Yes
Non-fire smoke days only				Yes

Notes: This table presents the robustness checks for regression results of eq. (3) in the main text. Three panels reflect three different specifications with different measurements of smoke days. The layout of each regression table is the same as Table 3 in the main text. ***p<0.01, **p<0.05, *p<0.1.