The Price of Staying In: Estimating Pollution Avoidance Costs Using Comprehensive US Consumer Data

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Abstract

Wildfire smoke is reversing decades of air quality improvements in the United States. We use variation in plausibly random wildfire smoke exposure combined with nationwide mobility and consumer spending data to estimate the welfare costs of smoke avoidance behavior. We find that severe smoke episodes reduce foot traffic to local businesses by 3-17% and decrease deferrable consumer spending by $38,000-$236,000 per county per day. This translates to welfare effects of $459-$17,756 per county per day, equivalent to 10% of the daily health costs on the most severe day. We find that avoidance increases with smoke intensity and that people do not simply reallocate spending across space and time. Public health guidance encourages avoidance behavior as a way to reduce smoke exposure. As climate-driven wildfire activity and associated smoke exposure continue to increase, our results highlight the economic consequences of people's protective decisions.

1 Introduction

Wildfire smoke is reversing decades of air quality improvements in the United States (Burke, Childs, et al., 2023; Currie et al., 2023). Research has documented substantial health and labor-market costs of smoke exposure (Ma et al., 2024;

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Heft-Neal et al., 2023; Borgschulte et al., 2022), as well as protective behavioral responses (Burke, Heft-Neal, et al., 2022; Berestycki and M. K. Chen, 2026). These responses are central to public health guidance, which encourages people to stay indoors and limit exposure during smoke events (Stone et al., 2019). However, we know relatively little about how these adjustments impact everyday consumption activities, how they differ across places, and the welfare costs they impose. This paper addresses these questions by estimating how wildfire smoke changes visits to businesses and recreation sites across the continental United States and translating those changes into estimates of forgone consumption and welfare.

We combine daily measures of wildfire smoke-attributable fine particulate matter ($PM_{2.5}$), mobile-device measured visits and consumer spending to points of interest in a county-level panel spanning 2019-2023. We focus on deferrable sectors that can plausibly be either postponed or forgone. The daily mobility data allow us to estimate a nonlinear exposure-response relationship and to identify heterogeneous effects around smoke events. We then use the estimated relationship between weekly visits and spending to value the associated reductions in consumption.

We make three primary contributions. First, we measure smoke avoidance across a broad set of consumption activities. Visits decline at high smoke concentrations, with statistically detectable reductions concentrated on days when smoke-attributable $PM_{2.5}$ exceeds 60 $\mu g/m^3$. Visits fall by approximately 2.7%, 4.1%, and 16.6% when smoke $PM_{2.5}$ is between 60-100, 100-200, and 200-500 $\mu g/m^3$, respectively. At lower concentrations, the pooled estimates are not statistically distinguishable from zero. Previous research documents avoidance through staying home (Burke, Heft-Neal, et al., 2022; Berestycki and M. K. Chen, 2026), changes in visits to particular destinations such as zoos and movie theaters (Graff Zivin and Neidell, 2009; He et al., 2022), and purchases of protective goods (Zhang and Mu, 2018; Sheldon and Zhan, 2026). We extend this evidence by estimating how smoke changes the volume of consumption-related visits across local economies throughout the United States. Measuring visits also captures adjustments that stay-at-home indicators can miss. For example, people may cancel a restaurant reservation or recreational outing while continuing to leave home for other purposes.[1]

Second, we characterize how avoidance varies across places and activities. Previous research shows greater avoidance in higher-income and more-educated places (Burke, Heft-Neal, et al., 2022; Berestycki and M. K. Chen, 2026). We add to this evidence by showing that reductions in visits are larger in the most urban counties with high baseline pollution. A related literature documents the effects of wildfire and smoke on outdoor recreation (Gellman et al., 2025; Gellman et al., 2022; Lee et al., 2023; Hesseln, Loomis, and González-Cabán, 2004; Hesseln, Loomis, González-Cabán, and Alexander, 2003) and on economic activity in recreation destinations (M.-K. Kim and Jakus, 2019; Lee et al., 2023). We build on these studies by examining how smoke responses vary with local dependence on natural amenity tourism across the continental United States. We construct a measure that combines the presence of parks and forests with the local employment and earnings shares of tourism-related sectors. We find larger reductions in visits in counties with more natural amenities. We also find larger declines in sectors where activities typically occur outdoors.

The timing of the response provides further evidence about the adjustment process. In an event-study analysis, we show that declines in visits emerge most clearly on the second day of a smoke spell (defined as consecutive days of smoke),

[1] The broader pollution literature documents effects on output and labor productivity (Dechezleprêtre et al., 2019; Graff Zivin and Neidell, 2012; Hill et al., 2023; Chang et al., 2019; Fu et al., 2021).

with larger reductions during longer spells. This differs from the immediate response to ozone alerts documented by Graff Zivin and Neidell (2009). A delayed response could reflect smoke arriving after plans have been made, delays in recognizing exposure, or constraints on changing activities. Our estimates do not distinguish among these explanations, but they suggest that the ability to respond within a smoke episode matters alongside the average response to pollution. This distinction connects our findings to research on risk perceptions and the costs and constraints of disaster adaptation (Anderson et al., 2023; Wagner, 2022a; Baylis and Boomhower, 2026; Wagner, 2022b).

Our third contribution is to estimate the consumption and welfare costs associated with these behavioral responses. A central challenge is that fewer visits during a smoke event may reflect activity shifted to another day or location. We therefore test for both intertemporal and spatial substitution before interpreting the declines as forgone activity. We find no detectable compensating increase in visits before smoke spells or during the two weeks after onset, and no evidence that smoke in neighboring counties increases local visits. These results support interpreting the observed declines as activity forgone within the locations and time horizons we study. While we cannot rule out substitution toward online purchases or consumption outside the observed sectors and horizons in our study, we note that these effects are consistent with Sheldon and Zhan (2026), which tracks both online and in-person spending, and with H. Chen et al. (2026), which tracks spending across all product categories.

We estimate the economic cost of the consumption activities that people forgo to avoid smoke. H. Chen et al. (2026), Barwick et al. (2024), S. Kim and Trusov (2025), Liu et al. (2025), and Brandt et al. (2024) show that pollution and information about pollution affect consumer spending in China and Spain. We extend this evidence by linking smoke-induced reductions in visits to spending across the United States. Using the estimated relationship between visits and spending, we calculate that smoke-attributable $PM_{2.5}$ above 60 $\mu g/m^3$ reduces spending by approximately \$38,000–\$236,000 per affected county per day, depending on smoke intensity. However, these expenditure reductions do not translate dollar-for-dollar into welfare losses because households retain the money they would have spent. We therefore use demand elasticities from the literature to approximate the value of the forgone consumption net of expenditure savings, yielding welfare losses of approximately \$500-\$18,000 per county-day.[2] Applying these estimates to smoke exposure from the 2023 Canadian wildfires in June–July 2023 implies approximately \$85.4 million in forgone spending and \$1.4 million in consumption-related welfare losses.[3] Although the welfare losses are smaller than estimated mortality costs (Ma et al., 2024), they capture an additional burden from the valued activities people give up to reduce exposure.

2 Data

This section describes our process for constructing, cleaning, and combining each data source and presents relevant summary statistics. Our final dataset is a county-day panel of mobile-device measured foot-traffic, consumer spending, smoke

[2] Previous research estimates the effect of pollution on spending directly and is therefore limited to settings where spending data exist. We instead estimate the relationship between visits and spending once and then infer spending changes from changes in foot traffic alone. Our estimates of both welfare and spending effects are nonetheless broadly consistent with the existing literature, suggesting this approach could be used to monetize changes in deferrable sector foot traffic in settings where spending data are unavailable.

[3] During the months of June and July in 2023, the South, Northeast, and Midwestern United States experienced 1,218 county-days of (60, 100] $\mu g/m^3$ of smoke, 645 of (100, 200] $\mu g/m^3$ and 6 of (200, 500] $\mu g/m^3$ from Canadian wildfires.

exposure, and weather conditions, matched to non-time-varying county characteristics, for the continental United States from 2019-2023.

2.1 Smoke Exposure and Weather

Our first dataset is a continuous measure of smoke exposure produced by O'Dell et al. (2019), who provide daily, gridded estimates of surface-level smoke $PM_{2.5}$ from 2006 to 2024 covering the continental United States. The dataset contains a kriged interpolation of $PM_{2.5}$ from EPA monitors, a measure of background $PM_{2.5}$, and an indicator for whether a smoke plume is detected overhead in satellite imagery. Following the recommendations of the dataset creators, we calculate smoke-attributable $PM_{2.5}$ in grid cells with a detected plume by subtracting background $PM_{2.5}$ from interpolated $PM_{2.5}$ for that day. If no plume is overhead, smoke-attributable $PM_{2.5}$ is zero. The dataset includes two measures of background $PM_{2.5}$: the mean and the median of non-smoky $PM_{2.5}$ per grid cell. We use the median series in our main analysis, though our results are robust to using the mean (see Appendix A).

We aggregate background and smoke-attributable $PM_{2.5}$ to the county level using an area-weighted average of the grid cells that overlap each county.[4] Because smoke products attribute any increase in $PM_{2.5}$ under a plume to smoke, they can misattribute ordinary fluctuations in background pollution. This matters for our setting because economic activity raises both background $PM_{2.5}$ and foot traffic, which would bias our estimates. We therefore set smoke-attributable $PM_{2.5}$ to zero on any county-day where it does not exceed one standard deviation of background $PM_{2.5}$.[5]

We estimate the background standard deviation from all smoke-free $PM_{2.5}$ observations in each county-quarter. Some county-quarters have too few smoke-free days for a reliable estimate. When a quarter has fewer than 20, we extend the window backward from the start of the quarter until we reach 20 smoke-free days or 90 days before the quarter begins, whichever comes first. No estimate in our sample uses fewer than 10 smoke-free observations. Across the 1.2 million county-days with any HMS plume coverage, the median, mean, and 95th percentile of lookback days are 0, 13.2, and 78. After filtering, 746,095 county-days (12% of the sample, and 62% of those with plume coverage) have elevated $PM_{2.5}$ from smoke. Appendix A shows that our main results are robust to stricter thresholds and to filters that remove other AQI increases likely to be misattributed to smoke.

Figure 1 summarizes the smoke exposure data. Panel (a) shows the average annual number of county-days in each bin of smoke-attributable $PM_{2.5}$. We plot the y-axis on a log-scale because the counts span more than three orders of magnitude, from about 135,000 county-days per year in the lowest bin to 60 in the highest. Nearly all smoke exposure is at low levels, and about 98% of smoke days have less than 30 $\mu g/m^3$. Only 0.24% of all county-days (1.8% of smoke days) have more than 35 $\mu g/m^3$ of smoke $PM_{2.5}$, the EPA's 24-hour standard and the level above which air quality is classified as unhealthy for sensitive groups. Because background $PM_{2.5}$ averages 10 $\mu g/m^3$, however, even moderate smoke can push total $PM_{2.5}$ above this threshold.

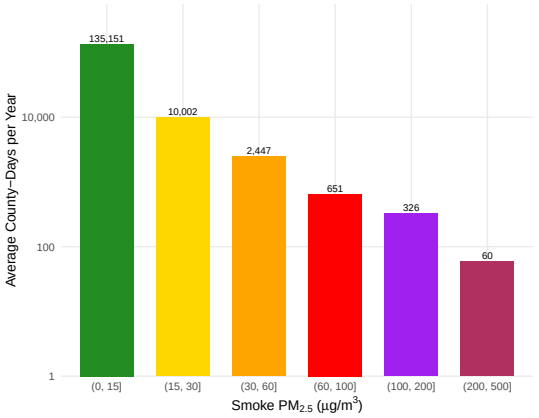
[4] Aggregating to the county level reduces sampling noise in our outcome variable, visits, and the large spatial extent of smoke plumes limits intra-county variation in exposure. Appendix J.1 details the justification for county aggregation and presents an empirical test of whether intra-county smoke variation is associated with foot traffic.

[5] Other sources of poor air quality may also reduce visits and be misattributed to smoke. Appendix Figure ?? plots overall AQI against AQI inferred from smoke and shows high-AQI events at low smoke levels that this filter is meant to remove.

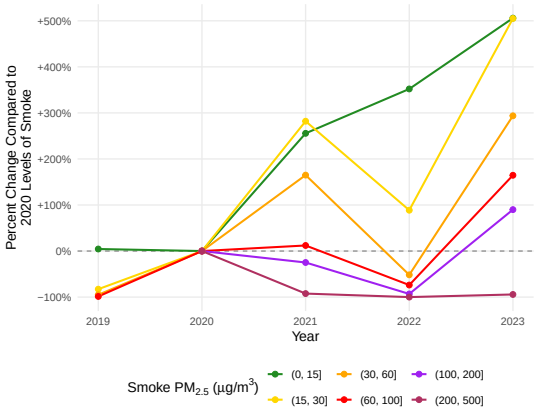
Panel (b) shows the change in smoke days in each bin relative to 2020. We use 2020 as the base year because there were no county-days above 100 $\mu g/m^3$ in 2019. By 2023, smoke days had increased in every bin except the highest. Days in the two lowest bins rose roughly sixfold, and days between 100 and 200 $\mu g/m^3$ nearly doubled. The increase is not steady from year to year. Most bins fell below their 2020 levels in 2022, and days above 200 $\mu g/m^3$ stayed below their 2020 level in every year, likely reflecting the unusually severe 2020 fire season.

Figure 1: Days of smoke $PM_{2.5}$ exposure in sample

(a) Average annual number of smoke days



(b) Percent change of smoke days over time



Notes: Panel (a) displays the average annual number of smoky county-days with different levels of smoke attributable $PM_{2.5}$ from 2019-2023, and plots the y-axis on a log-scale. The figure does not display days where there was no smoke-attributable $PM_{2.5}$. Most of the days with some smoke-attributable $PM_{2.5}$ are categorized in the (0, 15] smoke $PM_{2.5}$ bin. Almost 30% of county-days in our sample were smoke impacted in the Summer, but the majority of very unhealthy and hazardous smoke days occurred during the Fall. Panel (b) plots the year-over-year percent change in smoke days in our sample. There were no days of smoke exposure for the highest bins (100, 200] and (200, 500] in 2019, so we plot the percent change relative to 2020.

Our primary specifications explore the effects of smoke-attributable $PM_{2.5}$. However, moderate smoke can push total $PM_{2.5}$ above unhealthy levels in places with high background pollution, so avoidance responses may depend on baseline air quality as well as on smoke itself. To examine this, we construct a measure of each county's typical background $PM_{2.5}$ using its average non-smoke $PM_{2.5}$ in 2018, the year before our sample begins. We use this measure to test for heterogeneous responses in Section 4, and Appendix Figure I.1 maps it across counties.

Our weather data come from gridMET, which provides measures of daily precipitation, maximum humidity, and minimum and maximum temperature on a 4km grid (Abatzoglou, 2013). We aggregate each variable to the county by averaging over grid cells whose centroids fall within the county boundary. We then bin each variable into quantiles over the full sample. For precipitation, we construct the quantiles over the county-days with positive precipitation values and treat the zero precipitation days as the reference category. These bins allow weather to affect our outcomes nonlinearly.

2.2 Consumer Behavior Data

2.2.1 Foot Traffic: Advan Patterns

We use foot traffic data from Advan Patterns (Advan Research, 2022). Advan constructs visits to points of interest (POIs) from a panel of anonymized mobile device users across the United States. POIs include stores, restaurants, parks, and

many other destinations. A user enters the panel by opting into location tracking in any of the thousands of apps that partner with Advan and leaves it by opting out or deleting those apps. Because smoke could in principle change who is in the panel, Appendix C tests for a relationship between month-over-month changes in unique panel members and wildfire smoke events and finds none.

Our main outcome is total daily visits to POIs in deferrable sectors in a county. Two visits on a county-day can originate from one person visiting two places in a county or two people visiting either the same place or different places. This outcome therefore captures both margins of avoidance behavior, the extensive margin of staying home entirely and the intensive margin of visiting fewer places.

Each POI in the data is assigned a six digit North American Industry Classification System (NAICS) code. We follow Barwick et al. (2024) and define arts, entertainment, food, and accommodation sectors as "deferrable". We identify these sectors using the two-digit NAICS codes 71 and 72, which are commonly used to identify tourism, sometimes along with transportation (Webster, 2014; Peiró-Signes et al., 2015; Leidner, 2004).[6]

We focus on these sectors for two reasons. First, businesses in these sectors are less insulated from demand shocks than those in other sectors. Unlike goods producers, service providers cannot build inventory to buffer against fluctuations in demand. For example, an empty restaurant table or hotel room is a lost sale that cannot be recovered. This makes the service industry's cash flow sensitive to sudden shifts in demand. Second, spending in these sectors constitutes a significant share of consumption in the United States. In 2024, deferrable sector spending was $1.7 trillion, or almost 6% of GDP. This translates to $5,000 per capita, and accounts for 9.7% of disposable income.[7]

Because deferrable spending is economically significant, susceptible to demand shocks, and shown to be impacted by pollution avoidance behavior in other contexts, our analysis focuses on outcomes in these sectors. This effectively limits our analysis to foot traffic (and spending) in arts, entertainment, accommodation, and food services, which excludes grocery and convenience stores (which fall under retail trade, NAICS 44). In additional analyses, we disaggregate these industries by six-digit NAICS codes and classify each as likely indoor or likely outdoor. [8]

2.2.2 Spending: SafeGraph Spend

To support back-of-the-envelope calculations about the economic value of foot traffic, we complement the foot traffic data with a proprietary consumer spending dataset from SafeGraph (SafeGraph, 2022). SafeGraph obtains a panel of credit and debit card transactions across the United States from one of the largest transaction data providers, then aggregates and anonymizes them to spending in millions of stores, restaurants, and other locations. These locations are POIs defined as in the Advan patterns foot traffic data. Most variables are aggregated to the POI-month level (e.g., online spending,

[6] We exclude transportation since our analysis focuses on instantaneous avoidance behavior and transportation is often purchased in advance and outside the destination community.

[7] Author's own calculations from the Bureau of Economic Analysis National Income and Product Accounts. We estimate annual disposable income from Table 2.1. Personal Income and Its Disposition, which we convert to 2017 dollars using the Consumer Price Index from the Federal Reserve (accessed via the `fredr` package in R). We estimate deferrable spending and other personal consumption expenditures from Table 2.3.6. Real Personal Consumption Expenditures by Major Type of Product, Chained Dollars. We define deferrable spending with the expenditures categories "Recreation services" and "Food services and accommodations".

[8] Some county-days are missing visits data non-randomly in our overall panel. November 30th of each year records no visits for any counties, as does January 1, 2023 and all visits to counties in northern Vermont in 2022. We remove these counties and days from our analysis to maintain a balanced panel.

number of customers in each income range, and customer home city). Raw spending is available daily, but without these other variables. We sum daily raw spending across all POIs in deferrable sectors in a county and add it to our county-day panel.

We do not analyze spending data on its own for three primary reasons. First, spending data cover considerably fewer POIs than the foot traffic data, so a direct analysis would rely on a smaller and possibly less representative set of establishments.[9] Second, many card transactions are not processed on weekends, which artificially inflates recorded spending on Mondays and deflates it on Saturdays and Sundays. Third, spending per visit varies greatly by the income of the spender, so changes in spending can reflect changes in who shops as well as how much. The income data that could control for this are only available weekly, where delayed weekend processing shifts Saturday and Sunday spending into the following week, or monthly, where they are too aggregated to disentangle pollution effects from sales, holidays, and other place-specific shocks. We therefore use the spending data only to estimate the overall relationship between foot traffic and consumer spending, not to estimate the effect of pollution on spending directly.

2.3 Measuring County Heterogeneity and Spillover

We construct three variables to explore heterogeneity across counties: natural amenity tourism dependence, county rurality, and household income. We use each of these variables in the heterogeneity analysis in Section 4 and summarize their values in Figure I.1.

We proxy for natural amenities using data on the geographic extents of all National and State parks and forests, along with County, Regional and Local parks within the United States, produced by ESRI in partnership with TomTom (ESRI, 2014). We then compute the share of each county's area that is a park or forest. While this measures the prevalence of natural amenities in a county, it does not specifically capture whether the county's tourism depends on them.

We measure tourism dependence using the Quarterly Workforce Indicators (QWI) dataset from Census Bureau. The QWI draws on administrative unemployment insurance records to provide county-level employment and earnings by sector across the United States. Following our focus on deferrable sectors, we treat arts, entertainment, and recreation and accommodation and food services (NAICS 71 and 72) as the sectors most associated with tourism. We calculate the share of county-wide earnings and the share of employment in these sectors and define tourism dependence as the larger of the two.

To create a final measure of natural amenity tourism dependence, we winsorize both the fraction of the county that is parkland and the tourism dependency measure at their 99th percentiles to reduce the influence of extreme values. We then normalize each measure to have a maximum value of one by dividing by its maximum value, and compute the final measure as the fourth root of their product, which generates a more normal distribution.

To measure county rurality, we use the Community Assets and Relative Rurality (CARR) Index (K. S. Nelson and Nguyen, 2023). The index applies factor analysis to hundreds of variables measuring the availability and accessibility

[9] 98% of POIs with spending also have visits data but only 38.7% of all 1.5 million POIs with recorded visits also have recorded spending data. Per county, 33% of county POIs have spending out of the average 494 (median 111) POIs with visits. Aggregated to county-weeks, 83% of counties have spending and visits data every week in the sample. 5% of counties have no spending data over half of the sample period.

of services and amenities, plus four variables measuring relative rurality, and reduces them to a single aggregate measure. Inputs include standard rurality measures such as population density and the share of developed land, as well as proximity to (and the number of) essential services like public schools, grocery stores, and hospitals. The advantage of CARR over the USDA ERS Rural Urban Continuum Codes (RUCC) is that it captures the sense in which sparsely populated, undeveloped places with easy access to essential services are in important ways more urban than isolated rural counties. RUCC distinguishes non-metro counties only by adjacency to a metro area, whereas CARR uses travel time to specific essential services to measure accessibility more precisely. The index is constructed from data last updated in 2019 at the census block group level and aggregated to the county level using an area-weighted average. We downloaded the county-aggregated data directly from the project website (K. Nelson, 2024).

We explore heterogeneity across county household income using median county income from the 2014-2018 American Community Survey. There is one county, Rio Arriba County, NM for which there is no data for median household income in the 2018 vintage. To maintain a complete sample, we average the 2017 and 2019 vintages for this county.

Finally, we analyze the possibility of spatial spillovers by exploring effects across neighboring counties. To identify a reasonable set of neighbors for each county, we construct a travel-time measure between counties. First, we identify each county's centroid and find the nearest point on each neighboring county's border. We then use the Open Source Routing Machine (OSRM) package in R with OpenStreetMap data to calculate driving time from the source county centroid to these border points.[10]

3 Empirical Strategy

Our primary empirical strategy relies on high-dimensional fixed effects and the pseudo-random timing and location of wildfire smoke. In our baseline specification, we regress the inverse hyperbolic sine of total visits in county c to POIs in deferrable sectors on day d on binned smoke intensity.[11]

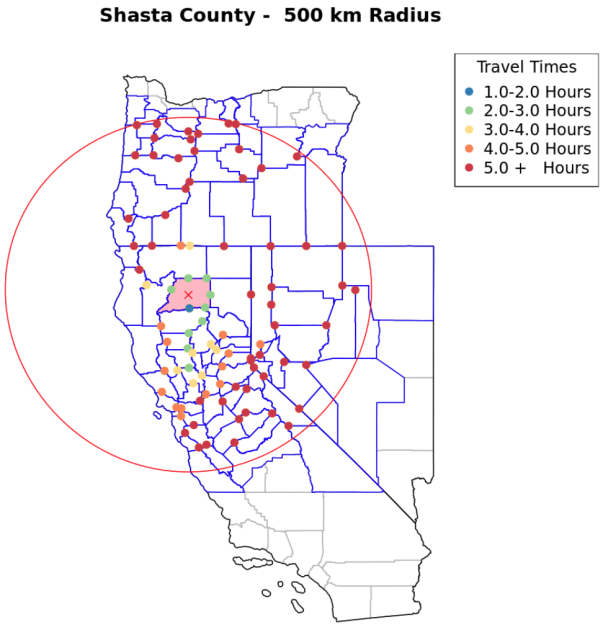
$$\text{asinh}(\text{visits}_{c,d}) = \left(\sum_b \gamma_b \mathbb{1}\left\{PM^{smoke}_{c,d} \in \text{bin}_b\right\}\right) + \beta T_{c,d} + day_d + \theta_{c,dow,z} + \phi_{s,m} + \varepsilon_{c,d} \tag{1}$$

where $\text{bin}_b = \{(0, 15], (15, 30], (30, 60], (60, 100], (100, 200], (200, 500]\}$, $T_{c,d}$ is a vector of flexible weather controls for minimum and maximum temperature, precipitation, and humidity in county c on day d. The day-of-sample fixed effect day_d absorbs shocks common to all counties on a given day, such as holidays and national events, and the state-by-month fixed effect $\phi_{s,m}$ absorbs state-specific shocks that vary over time. The term $\theta_{c,dow,z}$ is a county-by-season-by-day-of-week

[10] If we are unable to compute the inverse drive time between a county centroid and the closest neighboring county point, then we fallback to the Euclidean distance between the two points which we translate into hours driven by assuming a driving speed of 40 kilometers per hour (≈25 miles per hour). We are only unable to compute drive time when estimating routes to the Channel Islands of California and Nantucket in Massachusetts which are are inaccessible by car. OSRM selects the closest road segment to the county centroid as the starting point. This generally does not change the results, as the median distance between the county centroid and the closest road segment is 0.156 km, and the 95th percentile is 1.17 km. There are 80 source centroid/neighbor boundary pairs where given each end point snaps to a road segment such that the drive distance is less than the Euclidean distance. In these cases, we take the Euclidean distance driven at 40 km/hr as the lower bound on drive time. This approach has two limitations. First, county centroids may not represent population centers, particularly in large or irregularly shaped counties. Second, the nearest border point may not reflect actual destinations. We interpret our calculated times as averages of typical travel times between counties. Figure 2 plots an example of how we calculate driving time from Shasta county in California to neighboring counties within a 500 km radius.

[11] The number of visits in a county is left-skewed, and the inverse hyperbolic sine (or asinh) transformation ensures that the error term is approximately normally distributed while maintaining the entire sample. Only 0.4% of county-days have no recorded visits to deferrable sectors.

Figure 2: Spatial spillover example



Notes: This is an example figure showing the neighbors and drive time buckets computed for each neighbor of Shasta county in California, shaded in pink. First we draw our candidate neighbors from the set of all neighbors that are within 500 km of the county centroid (red x within the pink Shasta county). We then find the closest point in the neighbor county to the centroid of the source county (the closes point of each neighbor is depicted with a dot in this figure). We use open street map routing to compute the drive times between the county centroid (red x) and the the closest point to the neighboring county. We then bucket the dots in units of 1 hour and plot.

fixed effect, where $z \in \{\text{Spring, Summer, Fall, Winter}\}$. This fixed effect allows each county to have its own weekly visit pattern in each season. For example, if a county sees many more visits on summer weekends, its baseline for a summer Saturday should be higher than its baseline for a winter Tuesday (see Appendix B for model details).

We also examine heterogeneity in the effects of smoke by interacting the smoke $PM_{2.5}$ bins with indicators for the quartiles of each county-level characteristic introduced in Section 2.3. We estimate similar interactions with bins of background $PM_{2.5}$ and with an indicator for whether the smoke occurs on a weekend.[12] We examine each in separate regressions and plot the effect of smoke within each group.

3.1 Intertemporal Substitution

Contemporaneous reductions in foot traffic may misstate the welfare costs of smoke if people shift the timing of their activities. People may move visits earlier in anticipation of a smoke event or postpone them until it clears. In either case, if lost visits are displaced rather than forgone, contemporaneous estimates overstate net losses. We test for intertemporal substitution by estimating how visits evolve in the days before and after the onset of a smoke spell.

We estimate a local projections difference-in-differences (LP-DiD) model following Dube et al. (2025). This approach combines the local projections (LP) framework of Jordà (2005) with a "clean control" sample restriction that avoids the negative weighting bias of conventional two-way fixed-effects (TWFE) event-study specifications when treatment timing is staggered and effects are dynamic and heterogeneous (Sun and Abraham, 2021; Goodman-Bacon, 2021; Callaway and

[12] Appendix Figure I.2 shows that every smoke bin contains both weekday and weekend observations.

Sant'Anna, 2021; Borusyak et al., 2024). LP-DiD also accommodates treatments that switch on and off repeatedly, as smoke does, and has been used to study the effects of natural disasters on county economic outcomes and of fires on local labor markets (Roth Tran and Wilson, 2025; Coulombe and Rao, 2025).

We define the binary smoke day indicator $D_{c,d} = \mathbb{1}\{PM^{smoke}_{c,d} \geq 60\ \mu g/m^3\}$. Smoke can stay above 60 $\mu g/m^3$ for many days, and we call a run of consecutive smoke days a smoke spell. We define the first day of a smoke spell or smoke spell onset as $\Delta D_{c,d} = D_{c,d} - D_{c,d-1}$. Because wildfire smoke is non-absorbing, meaning counties can enter and exit smoke spells repeatedly, we follow the non-absorbing treatment extension in Dube et al. (2025) and define a smoke spell onset as a county-day satisfying

$$\Delta D_{c,d} = 1 \quad \text{and} \quad D_{c,d-j} = 0 \ \text{ for all } j = 1, \ldots, H, \tag{2}$$

where H is a washout period ensuring that smoke in treated counties remained strictly below 60 $\mu g/m^3$ for the H days before onset. We set $H = 14$ in all specifications.[13]

We estimate effects from seven days before to fourteen days after onset, using the day before onset as the reference period. For each horizon $h \in \{-7, \ldots, 14\} \setminus \{-1\}$, we estimate

$$\text{asinh}(\text{visits}_{c,d+h}) - \text{asinh}(\text{visits}_{c,d-1}) = \gamma^h \Delta D_{c,d} + \beta^h T_{c,d} + day^h_d + \phi^h_{s,m} + \theta^h_{c,dow,z} + \varepsilon^h_{c,d}, \tag{3}$$

where the outcome is the long difference in the inverse hyperbolic sine of daily deferrable visits between the day before onset and day $d+h$. The coefficient γ^h is the change in asinh visits at horizon h relative to the day before onset, and $\exp \gamma^h - 1$ approximates the percent change. $\Delta D_{c,d}$ equals one for counties newly entering a smoke spell (i.e., those satisfying Equation (2)), and zero for clean controls, described below. We use the same fixed effects and flexible weather controls as in our main specification (Equation 1). Because each horizon is a separate regression, all coefficients and fixed effects are indexed by h. Standard errors are clustered at the county level.

For each horizon h, the estimation sample includes treated county-days satisfying Equation (2) and clean control county-days satisfying:

$$D_{c,d+j} = 0 \ \text{ for all } j \in \{-H, \ldots, \max(h, 0)\}. \tag{4}$$

Clean controls thus have no smoke days above 60 $\mu g/m^3$ from $H = 14$ days before d through day $d + \max(h, 0)$, that is, through day $d+h$ for post-onset horizons and through day d for pre-onset horizons. This condition excludes counties still experiencing the effects of a previous smoke spell and counties that enter a spell between d and $d + h$. Dube et al. (2025) show that this restriction removes the negative weighting bias in conventional TWFE estimators. Under parallel trends and no anticipation, LP-DiD identifies a variance-weighted average treatment effect on the treated with non-negative weights. Our results are similar when we use a stricter threshold of 30 $\mu g/m^3$ to define the washout conditions, so lighter smoke before onset does not drive our results.[14] Restricting the clean control condition to have zero$\mu g/m^3$ also leaves the

[13] November 30 in each year and January 1, 2023 have no visits data and are omitted before constructing the LP-DiD panel. Lags, leads, and washout windows therefore count retained daily observations: December 1 follows November 29. A window crossing these gaps spans one additional calendar day. The same observation sequence is used for treated and control counties.

[14] Appendix D reports these results. Let $D'_{c,d} = \mathbb{1}\{PM^{\text{smoke}}_{c,d} \geq 30\ \square g/m^3\}$. We require $D'_{c,d-j} = 0$ for $j = 1, \ldots, H$ for treated county-days and

results unchanged.[15]

After onset, positive estimates would indicate that visits lost to smoke are postponed rather than forgone, while persistently negative estimates would indicate that they are not recovered. Our welfare estimates of forgone consumption therefore depend on the sign of the post-onset estimates.

Because smoke spells vary in length, we also estimate Equation (3) separately by spell duration. We classify each onset satisfying Equation (2) as a one-day spell if $D_{c,d+1} = 0$, a two-day spell if $D_{c,d+1} = 1$ and $D_{c,d+2} = 0$, and a spell of three or more days if $D_{c,d+1} = D_{c,d+2} = 1$. Longer spells are rare, with only 174 lasting four or more days, so three or more days is our longest category. For each category, we use the same specification and fixed effects, restrict the treated sample to onsets of that spell type, and hold the clean control group fixed.

3.2 Spatial Substitution

As with intertemporal substitution, avoidance may cause smaller welfare losses than our primary estimates imply if visits and their associated consumption are displaced to other places rather than forgone. If visits shift from smoky counties to nearby counties with less smoke, those nearby counties gain visits, and comparing them with smoky counties would overstate the reduction caused by smoke. We test for such spatial spillovers by estimating a Spatial Durbin Error Model (SDEM), which allows visits in each county to depend on smoke in neighboring counties. The primary estimating equation is as follows:

$$\text{asinh}(\text{visits}_{c,d}) = \sum_b \gamma_b \mathbb{1}\left\{PM^{smoke}_{c,d} \in \text{bin}_b\right\} + \sum_b \delta_b \sum_{c'} w_{c,c'} \mathbb{1}\left\{PM^{smoke}_{c',d} \in \text{bin}_b\right\} + \beta T_{c,d} + day_d + \theta_{c,dow,z} + \phi_{s,m} + u_{c,d} \quad (5)$$

$$u_{c,d} = \lambda \sum_{c'} w_{c,c'} u_{c',d} + \varepsilon_{c,d} \quad (6)$$

where the weather controls and fixed effects match Equation (1) and λ captures spatial correlation in unobserved shocks. The weights $w_{c,c'}$ are inverse drive times between counties c and c', calculated using OSRM (described in Section 2.3). A county's weight on itself is zero ($w_{c,c} = 0$), and each county's row is normalized to sum to one. We set $w_{c,c'} = 0$ when no part of the destination county (c') falls within a four-hour drive radius of the centroid of the origin county (c). Figure 2 shows the neighbors of an example county. The four hour radius covers average commute times (U.S. Census Bureau, 2024) as well as most single-day recreation trips.[16]

The outcomes of interest are the direct, indirect, and overall effects of smoke on asinh(visits). The direct effect (γ_b in Equation 5) is interpreted as the change in a county's visits when it has smoke in bin b. The indirect effect (δ_b) is the change in a county's visits when all of its neighbors have smoke in bin b. A positive δ_b would indicate spatial substitution, with visitors shifting toward a county when its neighbors have smoke. The total effect is the sum of the direct and indirect

$D'_{c,d+j} = 0$ for $j \in \{-H, \ldots, \max(b, 0)\}$ for clean controls.

[15] We do not apply a zero threshold to the washout condition because low smoke often precedes high smoke events, and would discard 99.6% of smoke spells.

[16] According to a U.S. Forest Service survey of how far people traveled to reach national forests, 80% of visitors to national forests and national wilderness areas traveled less than 200 miles (U.S. Forest Service, 2023), or roughly three and a half hours by highway.

effects ($\gamma_b + \delta_b$) and measures the visit response when a county and all of its neighbors have smoke in the same bin. In practice, however, neighboring counties rarely all have smoke in the same bin at the same time, so we estimate a typical episode effect for own-county smoke bin k,

$$\gamma_k + \sum_b \bar{w}_{k,b}\,\delta_b, \qquad \bar{w}_{k,b} = E\Big[\sum_{c'} w_{c,c'}\,\mathbb{1}\{PM^{smoke}_{c',d} \in \text{bin}_b\}\,\Big|\,PM^{smoke}_{c,d} \in \text{bin}_k\Big], \tag{7}$$

where $\bar{w}_{k,b}$ is the average share of county c's neighbors with smoke in bin b on days when county c itself has smoke in bin k, computed from the sample using the weighting matrix W. Because $\bar{w}_{k,b}$ is fixed at its sample value, the estimated effect is a linear combination of γ_k and the δ_b, and we compute its standard error from their joint covariance. As in our other specifications, we report all effects as percent changes, by exponentiating the coefficients (and the endpoints of their confidence intervals) and subtracting one. Appendix Table G.1 reports all 36 weights in a 6×6 table.

3.3 Forgone Spending

We translate changes in visitation into welfare effects by first estimating the relationship between visits and consumer spending using the SafeGraph Spend data, and then relating forgone spending to welfare. We use this two-step approach rather than estimating the effect of smoke on spending directly because of the measurement problems in the spending data described in Section 2.2.2. Because weekend transactions are often processed on Mondays (Section 2.2.2), we estimate the relationship between spending and visits at the county-week level,

$$\text{asinh}(\text{spend}_{c,w}) = \beta\,\text{asinh}(\text{visits}_{c,w}) + \psi T_{c,w} + week_w + \theta_{c,z,k} + \phi_{s,m} + \varepsilon_{c,w} \tag{8}$$

where $\text{spend}_{c,w}$ is consumer spending in deferrable sectors in county c in week w, in 2017 dollars, and $\text{visits}_{c,w}$ is the number of visits to places of interest in the same sectors. We define the week from Tuesday through Monday to accommodate weekend transactions reported on Monday.[17] $T_{c,w}$ is a vector of the counts of days in week w falling in each of the weather quantile bins described in 2.1. The fixed effects include week-of-sample ($week_w$), state-year-month ($\phi_{s,m}$), and county (c) by season (z) by week-of-month (k; $\theta_{c,z,k}$). Standard errors are clustered by county.

Because visits are measured from a sample of devices, sampling noise attenuates β in Equation 8. To correct for this, we use obviously related instrumental variables (ORIV; Gillen et al. (2019)). We randomly split POIs into two fixed halves, indexed by $h \in \{1, 2\}$, and let $-h$ denote the other half, so that $-h = 2$ when $h = 1$ and vice versa. We estimate a two-stage least squares regression, where the first stage regresses visits in half h on visits in half $-h$ and the second stage regresses spending in half h on instrumented visits in half h. In both stages, we stack the two halves into a single regression,

$$\text{asinh}(\text{visits}^h_{c,w}) = \pi\,\text{asinh}(\text{visits}^{-h}_{c,w}) + \sum_v \sum_b \lambda_{v,b}\, n^{v,b}_{c,w} + week^h_w + \theta^h_{c,z,k} + \phi^h_{s,m} + u^h_{c,w} \tag{9}$$

$$\text{asinh}(\text{spend}^h_{c,w}) = \beta\,\widehat{\text{asinh}(\text{visits}^h_{c,w})} + \sum_v \sum_b \delta_{v,b}\, n^{v,b}_{c,w} + week^h_w + \theta^h_{c,z,k} + \phi^h_{s,m} + \varepsilon^h_{c,w} \tag{10}$$

[17] We drop November 30 of every year and January 1, 2023, because the foot-traffic data records no visits on those days.

All fixed effects are interacted with the half indicator, so levels and trends can differ between the halves, while π, β, and the weather coefficients are pooled. ORIV assumes that sampling noise in visits is independent across the two halves. Because POIs are assigned to halves at random, this holds for noise arising from which devices happen to visit which POIs, making visits in one half a valid instrument for visits in the other. Fluctuations in the overall size of the device panel, however, affect both halves. Any such shared noise would leave the ORIV estimate partly attenuated, which would make our spending estimates conservative. We use spending and visits from the same half so that β reflects spending per visit at the same set of POIs.

4 Results

We estimate equation 1 and plot the transformed coefficients, $\exp(\gamma_b) - 1$, and associated 95% confidence intervals for each bin of smoke $PM_{2.5}$ in Figure 3. Each coefficient corresponds to the percent change in foot traffic on a day with smoke in that bin, relative to days with no smoke. The effects are strongly nonlinear and increasing with smoke exposure. Foot traffic falls by about 3% on days with smoke $PM_{2.5}$ between 60 and 100 $\mu g/m^3$, 4% between 100 and 200 $\mu g/m^3$, and 17% above 200 $\mu g/m^3$. This pattern is consistent with evidence that the health effects of smoke are also nonlinear (Ma et al., 2024; Qiu et al., 2025).

Below 60 $\mu g/m^3$, we find no statistically significant change in foot traffic. This is notable because smoke above 30 $\mu g/m^3$, combined with average background $PM_{2.5}$ of 10 $\mu g/m^3$, pushes total $PM_{2.5}$ above the EPA's 24-hour standard of 35 $\mu g/m^3$. The point estimate for the 30 to 60 $\mu g/m^3$ bin is negative but small and statistically indistinguishable from zero. One plausible explanation for this is that sensitive groups, whom the standard is designed to protect, reduce their activity at these levels, but their response is too small to detect in aggregate visits. Alternatively, people may not respond until smoke is visibly severe, consistent with evidence that avoidance begins well above the levels at which smoke becomes harmful (Berestycki and M. K. Chen, 2026).

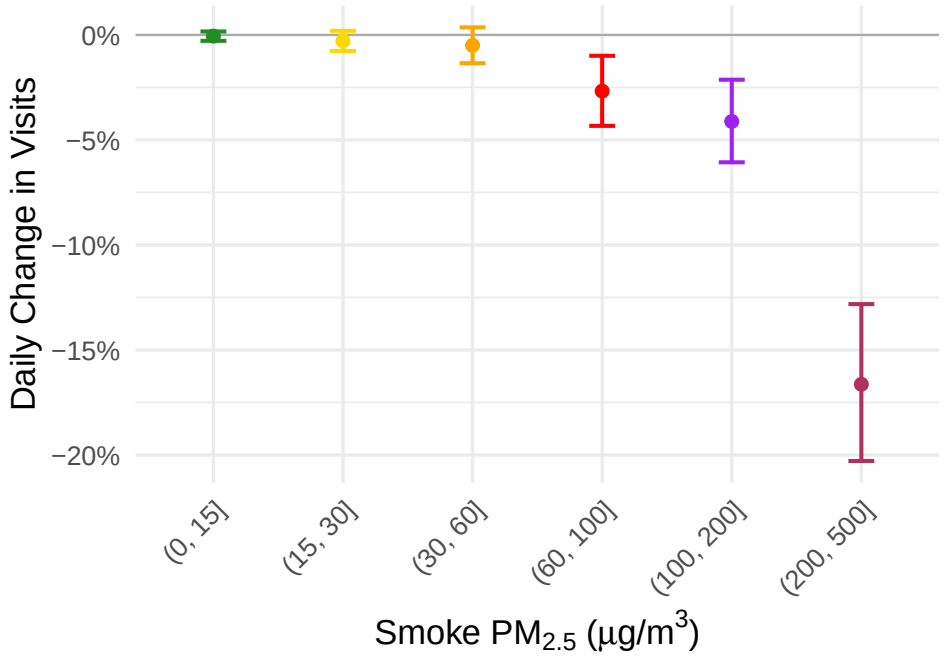
The estimated effect of smoke above 60 $\mu g/m^3$ are nearly identical across fixed-effect specifications once weather is controlled for (see Table B.2 in the appendix). Including county-by-season day-of-week fixed effects removes the spurious effects of low-level pollution, improves the precision of the estimates, and absorbs weekly visit patterns that vary across counties and seasons.

We next examine how responses vary across counties and exposure types by interacting each smoke bin in Equation (1) with quartiles of the county characteristics and separately with a weekend indicator. Figure 4 shows these results, with each panel plotting transformed coefficient estimates from separate regressions. Overall, these analyses show that the pooled estimates mask large differences. A day of smoke between 60 and 100 $\mu g/m^3$ reduces visits by about 3% overall, but by up to roughly 16% in the most urban counties with no statistically significant effect in others.

We find that avoidance is concentrated in higher-income and, in our main specification, more urban counties. The top income quartile shows the largest reductions at high smoke levels, consistent with evidence that higher-SES residents respond more strongly to smoke (Burke, Heft-Neal, et al., 2022; Berestycki and M. K. Chen, 2026).[18] Counties that

[18] These estimates also suggest a 6% increase in visits in the second income quartile at 60 to 100 $\mu g/m^3$. This estimate could reflect visit patterns specific

Figure 3: Day-of smoke exposure effect on foot traffic



Notes: This figure plots estimates from a regression of the inverse hyperbolic sine of daily deferrable visits on indicators for bins of smoke-attributable $PM_{2.5}$. Each point shows the percent change in visits on a day with smoke in each bin relative to days with no smoke, calculated as $\exp(\gamma_b) - 1$, with 95% confidence intervals. The regression includes flexible weather controls, and day, state-year-month, and county-season-day-of-week fixed effects. Standard errors are clustered by county.

depend on natural amenity tourism also respond more at high smoke levels. At the highest levels of $PM_{2.5}$, however, the least tourism-dependent quartile has the largest point estimate, with a wide confidence interval. Because this measure is only weakly correlated with the other characteristics, the pattern is unlikely to reflect income or rurality.[19]

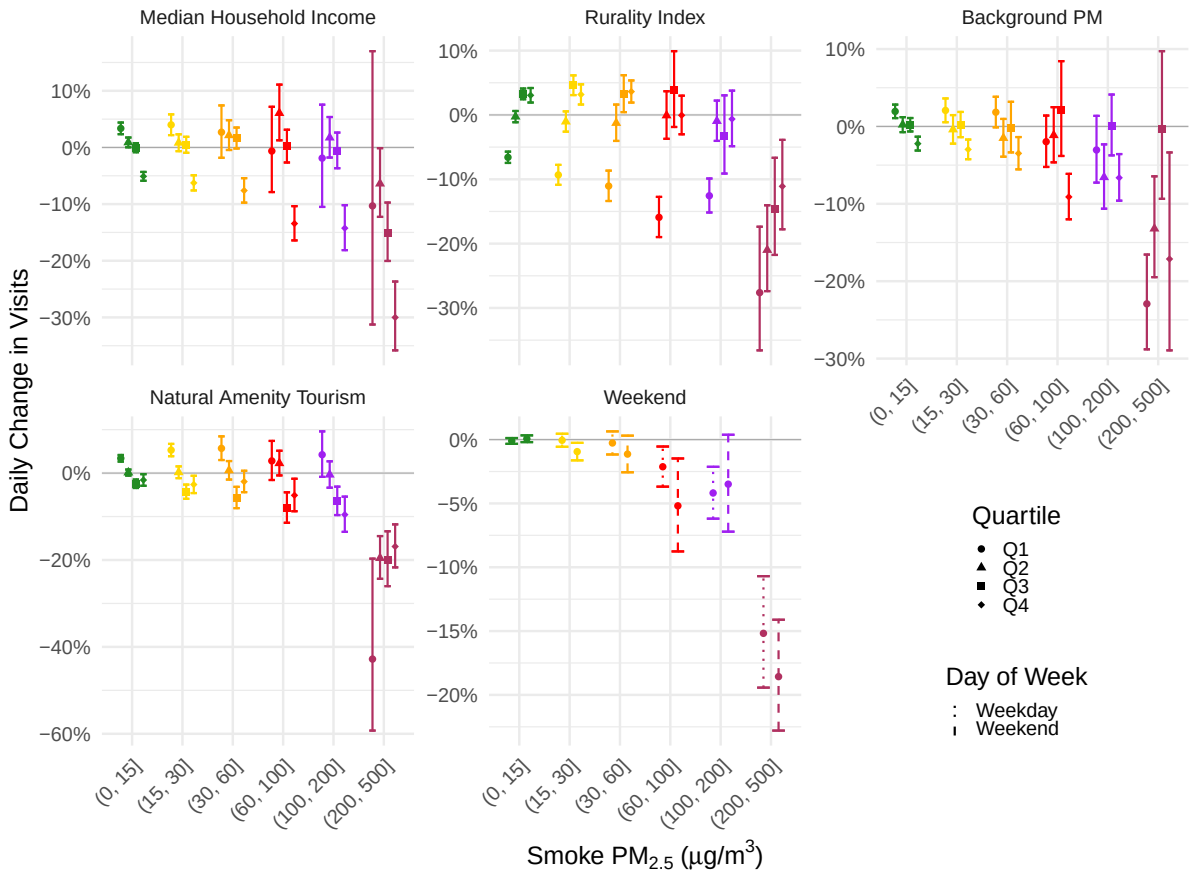
Background pollution may moderate avoidance differently at different smoke levels. Below 100 $\mu g/m^3$, high-background $PM_{2.5}$ counties reduce visits more, consistent with moderate smoke pushing their total $PM_{2.5}$ above an AQI threshold. In the highest bin, however, the lowest-background $PM_{2.5}$ counties show the largest response, though confidence intervals overlap. Weekend and weekday responses are similar, and the differences are not statistically significant in any bin. We find no clear evidence that weekday obligations such as work moderate the response of deferrable visits to smoke.

Three broad patterns emerge from this analysis. First, the pooled estimates understate how concentrated avoidance is, since some counties reduce visits several times more than the average while others show no detectable response. Second, the most robust difference is by income. High-income counties reduce visits most at the highest smoke levels in both specifications, which suggests that the capacity to avoid smoke, and the business losses that follow, are unevenly distributed. Third, differences at moderate smoke levels are fragile, so the heterogeneity that matters most for the aggregate cost of smoke arises during the most severe events. Together, these results suggest that the economic cost of smoke avoidance depends not only on how much smoke a region receives but on the characteristics of the communities it reaches.

to this group that happen to coincide with smoke. Appendix Figure I.4 allows for such group-specific daily shocks by interacting quartiles with day fixed effects. In that specification, this and other implausible estimates at low smoke levels largely disappear, while the larger responses of high-income counties at the highest smoke levels persist.

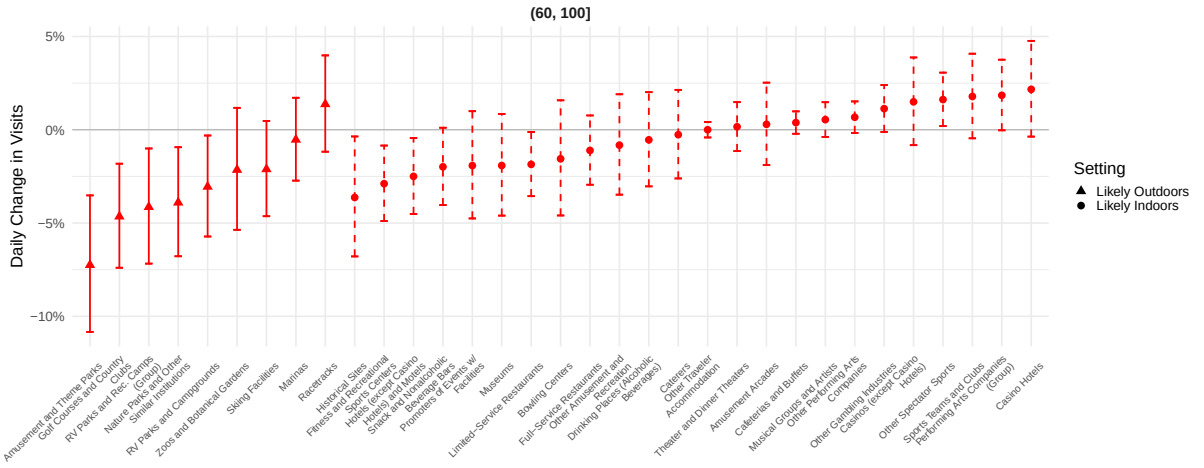
[19] The largest correlation is −0.15, between natural amenity tourism and background $PM_{2.5}$. Appendix Figure I.3 reports all Pearson and Spearman correlations.

Figure 4: Day of Smoke Exposure with Heterogeneities (All Bins)



Notes: Each panel in the figure plots the coefficients $\Gamma^{j}_{b,\{q,d\})}$ described in I.o.1 for different measures of heterogeneity. For panels a-d, $\Gamma^{j}_{b,\{q,d\})} = \Gamma^{j}_{b,q\in\{1,2,3,4\})}$ and is the change in visits due to smoke for counties across each quartile of the heterogeneity measure depicted. In panel e, $\Gamma^{j}_{b,\{q,d\})} = \Gamma^{j}_{b,d\in\{\text{Sat., Sun.}\})}$, and plots the change in visits from weekend smoke and weekday smoke. The coefficients for each panel are calculated in a different regression. All standard errors are clustered by county.

Figure 5: Indoor/Outdoor Results



Notes: Figure plots the transformed coefficients on the 60 to 100 μg/m^3 bin, which indicate the percent change in visits on days with smoke in that range, relative to days with no smoke. Equations are estimated separately for each NAICS code (six-digit, plus the four-digit groups 7111 and 7212 for places without six-digit codes) within the deferrable sectors (NAICS 71 and 72). Each regression replaces total deferrable visits in Equation 1 with visits to POIs in that industry. Triangles with solid lines are likely outdoor industries, and circles with dashed lines are likely indoor industries. Bars show 95% confidence intervals, and standard errors are clustered by county. Appendix Figure E.1 shows all smoke bins.

As a final heterogeneity analysis, we examine whether visits respond differently across industries. Because existing studies find that smoke reduces outdoor recreation in particular (e.g., Gellman et al. (2022)), we test for differential effects across likely indoor and likely outdoor POIs. We classify each POI as likely indoor or likely outdoor based on its six-digit NAICS code, aggregate visits to the county level within each industry, and estimate Equation 1 separately for each.[20]

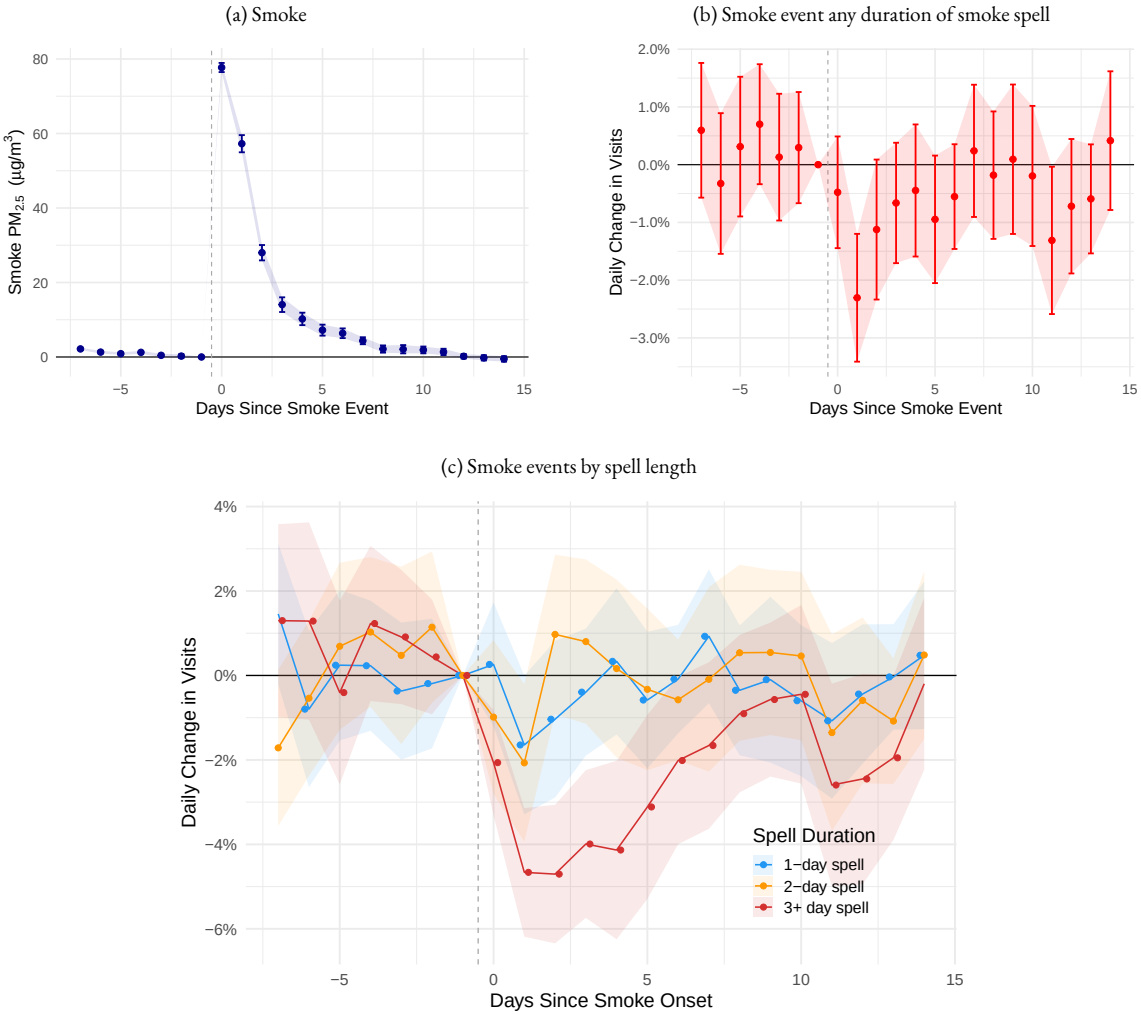
At smoke levels between 60 and 100 μg/m^3 (Figure 5), foot traffic falls more in outdoor sectors than in indoor sectors. Five of the nine likely outdoor sectors show significant reductions, compared with only four of the twenty-four likely indoor sectors. In addition, eight of the nine outdoor sectors have negative point estimates, while only twelve of the twenty-four indoor sectors do, and most indoor estimates are not statistically distinguishable from zero. Appendix Figure E.1 shows results for all smoke bins and reveals that this gap is similar at 100 to 200 μg/m^3. Above 200 μg/m^3, however, indoor sectors such as restaurants and bars also decline sharply, suggesting that at hazardous levels people forgo outings altogether rather than shifting toward indoor activities.

4.1 Intertemporal Substitution

Figure 6 plots results of the intertemporal substitution analysis described in 3.1. Panel (a) validates the empirical strategy by replacing the outcome in the LP-DiD model (Equation 3) with smoke-attributable $PM_{2.5}$, so it shows how $PM_{2.5}$ evolves after the onset of a smoke spell (the first day with smoke above 60 μg/m^3). As expected, smoke rises sharply, stays elevated for a few days and then reduces to zero around twelve days later. Panel (b) uses the same specification with deferrable visits as the outcome, showing how foot traffic evolves in the days before and after the onset of a smoke spell. We find no evidence of pre-trends. Visits fall by 2.3% on the day after onset, with smaller and mostly insignificant reductions over the

[20] Appendix E lists the full classification. It uses six-digit codes, except for two four-digit industry groups (7111 and 7212) that some places are coded to only at that level.

Figure 6: Intertemporal Substitution



Notes: Each panel plots LP-DiD estimates relative to the day before the onset of a smoke spell (Section 3.1, Equation 3). Onset is the first day with smoke-attributable PM above 60 μg/m^3 after at least 14 days without one. Treated counties may experience additional smoke after onset, and clean controls have no smoke above 60 μg/m^3 from 14 days before through each horizon.Panel (a) uses smoke-attributable $PM_{2.5}$ as the outcome. Panels (b) and (c) use the inverse hyperbolic sine of deferrable visits and plot the percentage change, calculated as $\exp(\gamma^h) - 1$. Panel (c) estimates effects separately by the total number of consecutive days smoke stays at or above 60 μg/m^3. Bars and shaded areas show 95% confidence intervals, and standard errors are clustered by county.

following days. Visits never rise significantly above their pre-onset level, so smoke-related foot traffic losses do not appear to be recovered through intertemporal substitution. Panel (c) splits the treated group by final spell length, as described in Section 3.1. Spells lasting three or more days reduce visits by about 4.7% on the second and third days, more than twice as large as the reductions for one-day or two-day spells, and the effect persists for about a week. Longer spells also involve more total smoke, so this difference reflects differences in both duration and exposure.

The response consistently peaks on the day after onset rather than the day of onset, which suggests that people need time to recognize and respond to pollution. On the first day, people may already be out when the smoke arrives and unable to change their plans, or they may not recognize the haze as smoke until later. Awareness may then spread through word of mouth, air quality apps, or simply noticing the pollution. Smoke can also be difficult to anticipate. Smoke forecasts rely on satellite detections of fires already burning so they lag for newly ignited fires (Chow et al., 2022). Where smoke

is forecastable, however, timelier information could shift avoidance toward the first day of smoke, potentially further reducing exposure.

4.2 Spatial Substitution

We find little evidence of spatial substitution using the spatial Durbin error model. The direct effects in Panel (a) of Figure 7 closely match the main regression results, indicating that accounting for neighboring counties' smoke does not change the estimated effect of a county's own smoke. The only notable difference is that the coefficient on the highest smoke bin, (200, 500] $\mu g/m^3$, implies a smaller reduction in visits than in the main results (-12% in the direct effect estimate compared to -17% in the main results in Figure 3).

Panel (b) plots the indirect effect, the change in source-county visits when all neighboring counties are smoky, holding source-county smoke constant. Spatial substitution would bias our estimates if smoke in a source county pushed visitors into neighboring counties. The indirect coefficient tests the mirror image of this mechanism, so a positive coefficient would mean that visits to the source county rise when its neighbors are smoky. We find no meaningfully positive coefficients. The indirect effects are near zero in every bin except (200, 500] $\mu g/m^3$, which is large and negative.

The negative indirect effect in the highest bin likely reflects how closely severe smoke in a county coincides with severe smoke in its neighbors. When a county has (200, 500] $\mu g/m^3$ of smoke, on average 54% of its drive-time weighted neighbors also exceed 200 $\mu g/m^3$.[21] No county-day with all neighbors above 200 $\mu g/m^3$ is itself below 200, and even when half of neighbors exceed 200, the county is never below 100. As a result, few county-days have heavily smoky neighbors and a less smoky source county, and these are the observations that identify the indirect effect from the direct effect.

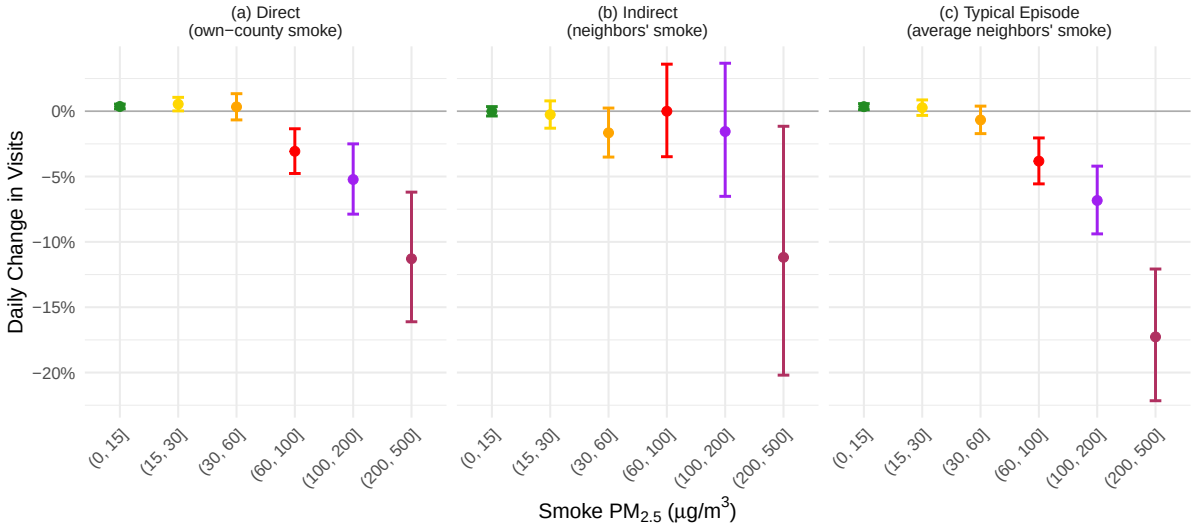
Panel (c) of Figure 7 reports the typical effect of smoke after accounting for local spatial spillovers. A typical episode is a day when a county has smoke in a given bin and its neighbors have the mix of smoke levels observed on average such days, as in Equation 7. Relative to the main results, it shows slightly larger reductions in visits for the (60, 100] and (100, 200] $\mu g/m^3$ bins, reflecting both somewhat larger effects and the negative indirect coefficient on neighbors in the highest bin. For the (200, 500] $\mu g/m^3$ bin, the typical episode effect (about -18%) is close to the main estimate. Because the direct and indirect effects of the most severe smoke are hard to separate, we interpret the highest-bin spillover results cautiously, but we find no evidence of positive spatial substitution. If the negative spillover we estimate for the most severe episodes is not an artifact of the small sample, it implies that smoke also reduces foot traffic outside the affected county, which would make our welfare estimates more conservative.

4.3 Identification Threats

We investigate three possible threats to identifying the effect of smoke on visitation. First, when smoke comes from nearby fires, it is difficult to distinguish avoidance of smoke from avoidance of the fire itself. Second, the COVID-19 pandemic caused large decreases in foot traffic and coincided with some large smoke events, so imperfect controls may misattribute pandemic-related behavior change to smoke. Third, poor indoor air quality may lead people to seek out places with better

[21] The 54% statistic comes from the neighbor smoke bin column (200, 500] and own-county smoke bin row (200, 500] in appendix Table G.1.

Figure 7: Spatial Substitution



Notes: Figure plots estimates from the spatial Durbin error model (Equation 5), reported as percent changes in visits. Panel (a) shows the direct effect of own-county smoke. Panel (b) shows the indirect effect when all of a county's neighbors, weighted by inverse drive time, have smoke in each bin. Panel (c) shows the typical episode effect, that is, the change in visits on a day when a county has smoke in each bin and its neighbors have the average smoke levels observed on such days (Equation 7). Bars show 95% confidence intervals, and standard errors account for cross-county dependence in the error through the spatial autoregressive error process as defined in Equation 6.

air filtration, which would increase foot traffic on smoke days.

We estimate our preferred specification, controlling for the number of satellite-detected fire points in the county. Appendix Table H.1 shows that the coefficients on the three highest smoke day bins fall by 0.003, 0.003, and 0.01 (approximately 0.3, 0.3, and 1 percentage points), or 6 to 11% of the baseline estimates. Dropping county-days with fires, rather than controlling for them, shrinks the magnitudes of the (60, 100] and (100, 200] $\mu g/m^3$ effects by up to 0.01 relative to the baseline, while the (200, 500] effect is essentially unchanged, ranging from 0.002 larger to 0.008 smaller in magnitude. The literature estimating smoke avoidance from mobility data does not separate these two channels (Burke, Heft-Neal, et al., 2022; Berestycki and M. K. Chen, 2026). Long-range smoke transport provides plausibly random exposure to pollution, but some of the total avoidance may still be a response to the fire itself. Avoidance driven by heightened awareness of the fire and smoke is difficult to disentangle from direct avoidance of the fire or from evacuations. Satellite fire counts are also an imperfect measure of fire risk, since, in large western counties, a fire in the same county may not raise perceived or realized risk or trigger an evacuation. We therefore interpret our welfare estimates as the combined effect of smoke and co-occurring fire on avoidance behavior.

Our identification relies on daily, location-specific variation in smoke exposure. Because the timing and location of wildfires, and the direction and distance of smoke transport, are plausibly exogenous, this variation is unlikely to be correlated with pandemic-related changes in behavior or regulation. To test this more directly, we follow Burke, Heft-Neal, et al. (2022) and tighten our fixed effects from state-month to county-month and from day to state-day (Appendix Table K.1). With these fixed effects, the exposure-response pattern is unchanged, with larger quantities of smoke leading to larger reductions in foot traffic. The additional controls preserve the broad pattern, with attenuation concentrated in

the highest bin. Because state-day fixed effects absorb statewide shocks such as pandemic restrictions on each date, these results suggest that COVID-19 is not driving our main findings.

Finally, people with poor indoor air quality at home, for example due to lack of air filters or high smoke infiltration (Burke, Heft-Neal, et al., 2022), may seek out places with better air filtration, which would appear as increased foot traffic in response to smoke. If these trips are to establishments in deferrable sectors, they would attenuate our estimated effect of smoke on foot traffic. Official "clean air shelters" (Wheeler et al., 2021; Wright et al., 2023) are typically large public buildings such as libraries and schools, which are not in our foot traffic data and so cannot impact our estimates. To examine this more directly, we identify the three indoor deferrable sectors that could plausibly serve as informal clean air shelters, namely fitness and recreational sports centers, hotels, and museums.[22] Figure 5 shows that on a (60, 100] $\mu g/m^3$ smoke day, these three sectors are among the indoor sectors with the largest reductions in visits, falling in the lowest quartile of estimated changes. While we cannot observe why people visit these locations, seeking shelter does not seem to be driving our results.

4.4 Welfare Implications

To translate the change in foot traffic into changes in welfare, we proceed in three steps. We first estimate the change in consumer spending associated with changes in foot traffic (Section 3.3). We then calculate the deadweight loss from this forgone consumption. Finally, we compare these losses with other estimates of pollution on spending patterns as well as the implied mortality costs of smoke exposure in our setting, drawing on estimates from Ma et al. (2024).

Our preferred split-sample IV (ORIV) estimate (Section 3.3) implies that a 1% increase in visits to deferrable POIs is associated with a statistically significant 0.904% increase in deferrable spending (standard error 0.166). The first-stage F-statistic (cluster-robust Wald) is 12.31. This elasticity is close to one and we cannot reject that spending rises proportionally with visits. An elasticity slightly below one is plausible, since some visits, such as trips to parks, brief stops, or browsing, involve no spending, and others involve cash or prepaid purchases that card data for that week do not capture. Applying this elasticity to smoke-induced changes in visits assumes that visits forgone because of smoke are associated with the same spending as other marginal changes in visits.

We view the resulting losses as conservative for several reasons. If our data undercount spending associated with visits, or if smoke deters mostly discretionary, higher-spending trips, such as dining out, our estimates understate spending losses. In addition, smoke may deter valued trips that involve little day-of spending, such as visits to parks, or to amusement parks and fitness centers covered by season passes or memberships, which are among the sectors with the largest declines in visits (Figure 5). Because spending does not capture the value of these trips, welfare losses based on spending would understate the full welfare cost of avoidance.

Appendix Figure J.1 compares this estimate with alternative specifications. Our preferred estimate restricts the sample to POIs that appear in both the foot traffic and spending panels, so that visits and spending are measured at the same places. In the full sample, visits to POIs without spending data add variation in visits with no corresponding variation in recorded

[22] Because casinos and casino hotels often permit indoor smoking, we do not assume they offer better air quality than individual homes.

spending, which biases the elasticity toward zero. The ORIV estimate is about 50% larger than the OLS estimate on this same stacked sample (0.61) and more than twice the OLS estimate on county totals (0.44). Including all POIs lowers all three estimates, to 0.15 and 0.17 for the two OLS specifications and 0.28 for ORIV. Because matched POIs may also differ systematically from other POIs, for example by being in more populated or higher-income places, we treat the full-sample ORIV estimate as a lower bound.[23]

4.4.1 Deadweight Loss from Forgone Consumption

SafeGraph Spend represents approximately 0.9% of annual deferrable consumer spending reported by the Bureau of Economic Analysis National Income and Product Accounts.[24] Accordingly, we divide the county-level spending data by 0.0092 to translate our estimates of the effects of visits on spending into county-level consumption effects, which comes to an average of C = \$1,568,806 in 2017 dollars (\$14.79 per capita).[25] Together, our results imply a 1% change in visits translates to a \$14,182 change in average daily county spending, calculated as \$1,568,806 × 0.904/100.

We then apply this conversion factor to the marginal changes in visits from smoke exposure in each pollution bin (from Panel B of Figure 3). For each additional day a county is exposed to smoke-derived pollution in bins (60, 100], (100, 200], and (200, 500] $\mu g/m^3$, deferrable spending changes by -\$37,984, -\$58,468, and -\$236,036, respectively. We plot the estimated change in spending in Panel (a) in Figure 8.

Following Barwick et al. (2024), we approximate welfare losses due to lost consumption by calculating the deadweight loss associated with the wildfire smoke induced reduction in demand. One can imagine that smoke creates an additional cost of visiting a site and purchasing items, which acts like a tax that generates no revenue. Formally, we calculate $\frac{1}{2} \times \%\Delta c \times \Delta c/|\varepsilon|$, where $\%\Delta c$ is the percent change in consumption, Δc is the dollar value of the change in consumption, and ε is the price elasticity for consumer goods. We consider price elasticities ranging from 1 to 1.5 (Deaton and Muellbauer, 1980; Blundell et al., 1993; Banks et al., 1997). Using 1 as our estimate for ε, pulling Δc from our change in spending, and estimating $\%\Delta c$ as $\Delta c/C$ where C is the average county-day deferrable spending defined above, we estimate welfare effects of -\$459, -\$1,089, and -\$17,756 per county-day in (60, 100], (100, 200], and (200, 500] smoke $PM_{2.5}$ bins, respectively. We plot the estimated change in welfare in Panel (b) in Figure 8.

4.4.2 Contextualizing the Welfare Estimates

We offer context for our welfare estimates in two ways. First, we compare our implied cost estimates to the costs associated with mortality. Second, we benchmark our estimates against those in the literature that estimate changes in spending from pollution in other countries.

We estimate mortality costs using the relationship between binned annual average smoke $PM_{2.5}$ and smoke induced

[23] As a check on whether matched POIs respond differently to smoke, we re-estimate equation (1) using visits to matched POIs only. The point estimates are similar to our main results, though less precise given the smaller sample.

[24] We estimate deferrable spending and other personal consumption expenditures from Table 2.3.6. Real Personal Consumption Expenditures by Major Type of Product, Chained Dollars. We define deferrable spending with the expenditures categories "Recreation services" and "Food services and accommodations".

[25] The average daily county spend is weighted by the percent of visits that occur in POIs that have any recorded spend in the same POIs within the same month. This ensures that the average daily county spend is representative of the visits and spending that we observe. All monetary values are converted to real 2017 dollars using the Consumer Price Index from the Federal Reserve (Boysel and Vaughan, 2023).

mortality estimates from Ma et al. (2024). This approach captures delayed-onset effects of smoke exposure, providing a fuller accounting of mortality costs than studies examining only contemporaneous mortality. Additionally, Ma et al. (2024) uses binned annual averages, which allow for non-linear effects of wildfire smoke pollution on mortality. Using these estimates, we find an increase in daily mortality per 100,000 people of 0.005, 0.013, 0.012, and 0.021 for the bins (30, 60] (60, 100], (100, 200], and (200, 500], respectively.[26] We monetize these costs using the EPA's recommended value of a statistical life, $7.4 million in 2006 dollars, or about $9.0 million in 2017 dollars. We plot the results in panel (c) of Figure 8.[27][28]

Most previous studies that estimate reductions in consumption due to pollution use credit card transactions rather than mobility data and spending data. An important question, then, is whether our approach, which translates changes in foot traffic into changes in spending, produces estimates comparable to these direct measures. Four papers that study how pollution avoidance behavior affects spending in different contexts show varying results, three of which are consistent with ours. In Spain, ozone pollution decreases consumer spending in brick and mortar stores by 3.9% while $PM_{2.5}$ shows no effect (Brandt et al., 2024). However, the highest $PM_{2.5}$ level during their study period was around 30 $\mu g/m^3$. Since we only observe spending reductions above 60 $\mu g/m^3$, a null effect of $PM_{2.5}$ at these concentrations is consistent with our results.

In China, students reduce dining hall spending by 0.016% per 1 $\mu g/m^3$ and shift to delivery during pollution episodes (Liu et al., 2025). The average nonzero $PM_{2.5}$ in our (60, 100] and (200, 500] bins is 75.9 and 282.4 $\mu g/m^3$, implying reductions in spending of 1.21% and 4.5%. These are smaller than our estimates of 2.4% and 15.1%, but dining halls are an explicitly indoor setting. Our sector-specific estimates (Figure 5) show that at 60 to 100 $\mu g/m^3$, visits to limited service restaurants, the sector most comparable to a dining hall, fall by about 1.8%, close to the reduction their estimates imply. Visits to outdoor sectors fall by up to four times this level. Differences in setting may therefore explain much of this gap.

Relative to a baseline of 0-50 $\mu g/m^3$, H. Chen et al. (2026) find that exposure to 51-100 $\mu g/m^3$ reduces overall spending by 1.79%, 200-300 $\mu g/m^3$ by 3.8%, and above 300 $\mu g/m^3$ by 7.4%. They do not estimate nonlinear effects for deferrable spending, but they do estimate linear effects for both overall and deferrable spending, using thermal inversions as an instrument for $PM_{2.5}$. They find that a 1 $\mu g/m^3$ increase reduces deferrable spending by 0.093% and overall spending by 0.14%. Applied to the average $PM_{2.5}$ in our (60, 100] and (200, 500] bins, their linear IV estimate implies reductions in deferrable spending of 7% and 26%, respectively. We estimate that deferrable spending falls by 2.4% in the (60, 100] bin and 15.1% in the (200, 500] bin. Both estimates are larger than their nonlinear estimates for overall spending, as expected for deferrable sectors, but smaller than their linear IV estimates for deferrable spending. Further analysis would be needed

[26] See Appendix section F for more information about how we translate the results in the paper into daily mortality effects per smoke-bin. The average county population in our sample is 106,075, which is approximately 100,000, so we interpret these as the average daily mortality effect per county from an additional day of smoke $PM_{2.5}$ in each bin.

[27] In Appendix Section F.1, we describe alternative methods for calculating mortality costs and discuss why we prefer the estimates derived from Ma et al. (2024).

[28] Because Ma et al. (2024) estimate mortality as a function of annual average smoke $PM_{2.5}$, we convert each smoke day into its contribution to the annual average, dividing the day's smoke $PM_{2.5}$ by 365. We apply their estimates to that annual-equivalent increase to obtain the mortality attributable to a single day of smoke. This method may underestimate the mortality effect of smoke. Because smoke exposure is often autocorrelated, a county with one day of heavy smoke typically has others, placing it at higher annual exposure. At higher annual exposure, each additional smoke day likely has a larger mortality effect than our calculation implies. Additionally, a single day in the lower bins (below 30 $\mu g/m^3$) is too small to register as smoky in the annual analysis, even though such smoke, added to typical background pollution of around 10 $\mu g/m^3$, can push total $PM_{2.5}$ above the level the EPA classifies as unhealthy for sensitive groups.

to determine whether the larger spending responses in H. Chen et al. (2026) reflect aggregation, functional form, or other factors.

In contrast, S. Kim and Trusov (2025) find that South Korean consumers *increase* spending during poor air quality, which the authors attribute to self-soothing behavior. Like H. Chen et al. (2026), they use individual bank card data with individual fixed effects, yet the two studies find effects in opposite directions. We therefore treat S. Kim and Trusov (2025) as an exception.

We can also compare our results to the welfare analysis in Barwick et al. (2024), who estimate the welfare effects of pollution information using the staggered rollout of a pollution information campaign in China. Part of their estimated effect operates through avoidance behavior that reduces spending in plausibly deferrable sectors. To make the results comparable, we convert aggregate estimates of forgone spending and welfare losses into per capita terms. Barwick et al. (2024) find that a doubling of pollution reduces trips by 1.4%, reduces consumption by $377k per city-day, and generates welfare losses of $1.8k to $2.6k. Per person, this translates to $0.10 in reduced consumption and $0.0005-$0.0007 in lost welfare.[29] These figures are in dollars but are not adjusted for differences in income between China and the United States. The ratio of US to Chinese real GDP per capita for our sample periods is 5.2,[30] which scales their per capita estimates to $0.52 in reduced consumption and $0.0026-$0.0036 in lost welfare. For comparison, we find that a day in the (60, 100] bin reduces visits by 2.7%, consumption by $38k, and welfare by $459.[31] Per capita, this translates to $0.36 in reduced consumption and $0.0043 in lost welfare.[32] Our estimates are therefore close to those in Barwick et al. (2024), with somewhat smaller consumption losses, but somewhat larger welfare losses (compared with their $0.0036 at the same price elasticity of 1). This difference may reflect the greater salience or distinct health effects of wildfire smoke relative to ambient pollution, or the fact that Barwick et al. (2024) capture only the additional avoidance induced by the information campaign (though they find no evidence of avoidance behavior before the rollout).

5 Discussion and Conclusion

Using daily county-level data on wildfire smoke, mobile-device measured visits, and consumer spending across the continental United States from 2019 to 2023, we find that severe smoke reduces visits in deferrable sectors by 3 to 17%, depending on smoke intensity. These declines translate to reductions in deferrable spending of $38,000 to $236,000 and in welfare of $459 to $17,756 per county per smoke day. The losses appear to be forgone rather than displaced, since we find no compensating increase in visits before a smoke spell or in the two weeks after onset, and no increase in visits when neighboring counties are smoky. To our knowledge, these are the first national estimates of the welfare costs of smoke avoidance operating through reduced consumer spending.

Several patterns suggest that our results reflect avoidance behavior, rather than other changes that coincide with

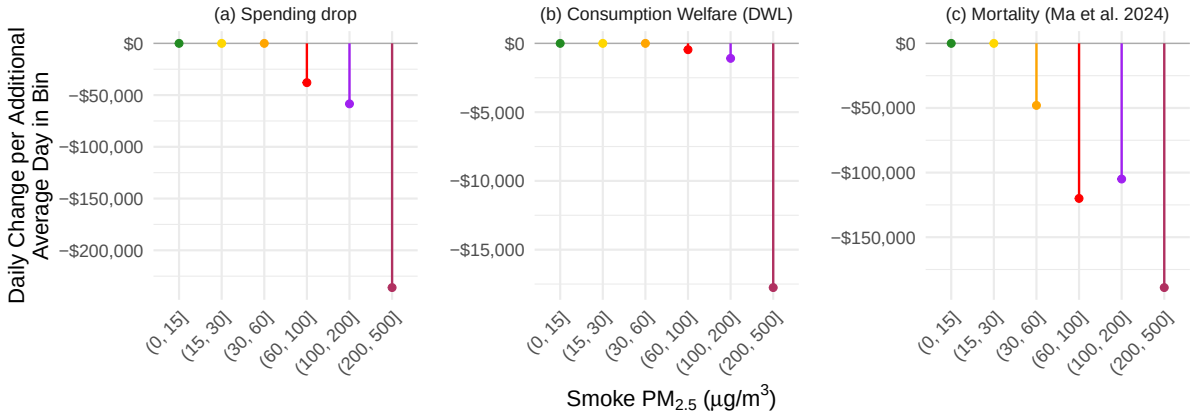
[29] Chinese cities have 3.7 million residents on average.

[30] From the Penn World Table, expenditure-side real GDP per capita at chained PPPs (in 2021 US$) is $13,118.88 for China in 2013 (the start of the pollution information program rollout) and $68,682.79 for the United States in 2017 (the year to which we deflate all of our spending data) (Feenstra et al., 2015).

[31] Barwick et al. (2024) use unitless AOD rather than $PM_{2.5}$ and report effects for a doubling of AOD. They note that daily average $PM_{2.5}$ concentrations in the 2000s exceeded 50 $\mu g/m^3$, so we treat the (60, 100] bin as the most comparable.

[32] The average county in our sample has 106,075 residents.

Figure 8: Comparing health costs and spending costs



Notes: This figure compares estimated daily costs per county from avoidance behavior and mortality across smoke $PM_{2.5}$ bins. Panel (a) shows the change in deferrable spending, calculated from the estimated change in visits and the elasticity of spending with respect to visits. Panel (b) shows the deadweight loss from foregone consumption, assuming a price elasticity of 1. Panel (c) shows mortality costs, calculated by converting each smoke day into its contribution to annual average smoke $PM_{2.5}$, applying the estimates in Ma et al. (2024), and valuing deaths using the EPA value of a statistical life. In bins above 60 $\mu g/m^3$, mortality costs exceed the welfare loss from forgone consumption by roughly 10 to 250 times.

smoke. If people are avoiding exposure, declines in visits should be larger when exposure is more costly and among those better able to avoid it. We find evidence for both. Visits decline more as smoke-attributable $PM_{2.5}$ rises, and at 60 to 200 $\mu g/m^3$, visits to outdoor sectors fall more than visits to indoor sectors. At hazardous levels, visits to indoor sectors such as restaurants and bars also fall sharply, suggesting that people forgo outings altogether rather than shifting toward indoor activities. Prior work finds that higher-income residents respond more strongly to smoke, generally attributing this to a greater capacity for avoidance (Burke, Heft-Neal, et al., 2022). Consistent with this, income is the most robust dimension of heterogeneity we find, with high-income counties reducing visits most during the most severe smoke. Differences across counties by rurality and background pollution at moderate smoke levels, by contrast, do not survive allowing for group-specific daily shocks. Weekends, when work obligations ease, offer another test of the capacity to avoid, but we find no statistically significant difference between weekend and weekday responses. This likely reflects our focus on deferrable sectors, since visits to restaurants, entertainment, and recreation are largely discretionary on weekdays as well as weekends.

The timing of the avoidance response suggests that people face frictions in learning about and adjusting to smoke. Visits fall most on the day after onset rather than on the day smoke arrives, and the decline is largest and most persistent for spells lasting three or more days. This differs from the immediate response to ozone alerts documented by Graff Zivin and Neidell (2009). The delayed response could reflect adjustment frictions, such as smoke arriving after plans are made, or information frictions, such as people mistaking smoke for ordinary haze. More accurate smoke forecasts and wider dissemination could provide timelier information, shifting avoidance toward the first day of exposure, when it is currently weakest.

We translate these declines in visits into forgone consumption using the estimated relationship between visits and spending. The implied spending and welfare effects fall within the range of direct estimates from other settings, close to Barwick et al. (2024) and smaller than H. Chen et al. (2026). These welfare estimates capture only one component of the

costs of smoke. They measure the value of consumption people give up to avoid exposure and are additive to health costs (Ma et al., 2024), defensive expenditures (Sheldon and Zhan, 2026), labor market effects (Borgschulte et al., 2022), lost outdoor recreation (Gellman et al., 2025), and diminished well-being (Jones, 2018).

Our estimated costs of avoidance are small relative to the mortality costs of smoke. Using estimates from Ma et al. (2024), a day of smoke above 60 $\mu g/m^3$ implies mortality costs of roughly \$108,000 to \$189,000 per county, compared with our estimated welfare losses from forgone consumption of \$459 to \$17,800. Avoidance costs are therefore 1% or less of mortality costs on most smoke days and about 10% on the most severe days, above 200 $\mu g/m^3$. We also find no detectable avoidance between 30 and 60 $\mu g/m^3$, even though smoke at these levels, combined with typical background pollution, pushes total $PM_{2.5}$ above the EPA's 24-hour standard. Together, these patterns are consistent with evidence that risk misperception limits personal adaptation to disasters (Anderson et al., 2023; Wagner, 2022a; Baylis and Boomhower, 2026). They may also reflect the limited protection that forgoing these activities provides, since smoke also infiltrates indoor spaces.

Several caveats apply to our estimates. First, controlling for satellite-detected fires in the same county reduces our estimates above 60 $\mu g/m^3$ by 6 to 11%, so part of the response may reflect the fires themselves rather than their smoke. We therefore interpret our estimates as the combined effect of smoke and co-occurring fire, as do most studies of wildfire smoke, which typically do not control for fire (Burke, Heft-Neal, et al., 2022; Berestycki and M. K. Chen, 2026; Borgschulte et al., 2022). Second, we test for substitution only within two weeks and across counties within a four-hour drive. We cannot rule out substitution over longer periods, to distant destinations, or toward online purchases, although studies that track online spending (Sheldon and Zhan, 2026) and spending across all product categories (H. Chen et al., 2026) suggest such substitution is limited. Third, our spending and welfare estimates rely on the elasticity of spending with respect to visits among POIs observed in both panels. Finally, because we cannot test whether the composition of the spending panel changes with smoke, we do not estimate the effect of smoke on spending directly.

Wildfire activity is increasing (Abatzoglou and Williams, 2016), and smoke is reversing decades of improvement in US air quality (Burke, Childs, et al., 2023). Decisions about how much to invest in fire prevention and suppression should account for the full costs of wildfire, including the consumption people forgo to protect themselves. These costs can be substantial even far from the fires. Applying our estimates to smoke from the 2023 Canadian wildfires implies approximately \$85.4 million in forgone spending and \$1.4 million in welfare losses across the South, Northeast, and Midwest in June and July 2023 alone. At the same time, avoidance costs are borne unequally, and we do not estimate how effective avoidance is at reducing exposure. Encouraging more avoidance may lower health costs, but only by raising avoidance costs. Reducing smoke itself, through fire prevention and management, can lower health costs without adding to avoidance costs. As fire seasons lengthen, this approach is likely to become central to smoke policy.

AI Use Declaration

During the preparation of this manuscript, the authors used OpenAI's ChatGPT to assist with language editing, organization, and clarity. The authors reviewed and revised all AI-assisted text and take full responsibility for the content of the manuscript.

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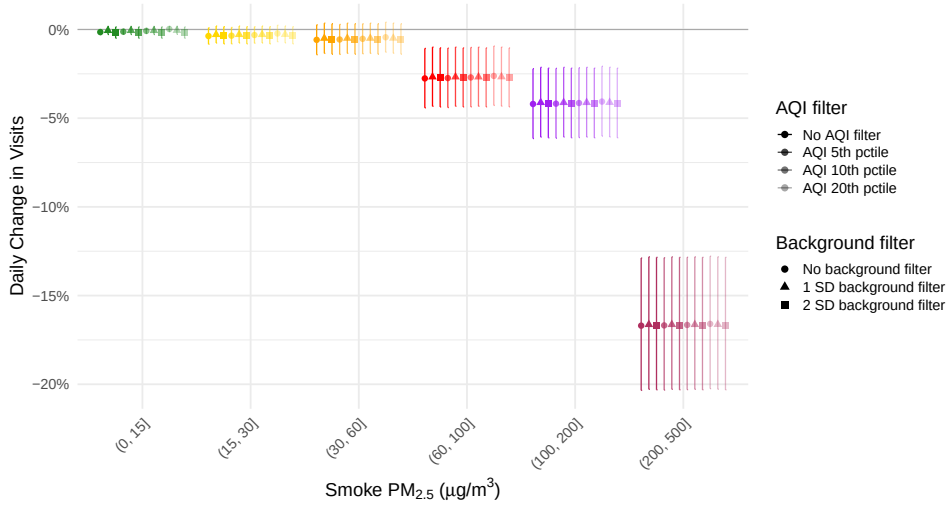
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Figure A.1: Additional smoke filter effect on main results



Notes: Our main results are invariant to AQI and background $PM_{2.5}$ based filters. We recreate the same analysis with the mean series as well as the median and our results do not change.

A Robustness to Alternative Smoke Measurements

One challenge with smoke data from O'Dell et al. (2019) is that other pollution events may also elevate the smoke attributable $PM_{2.5}$ in an area. In Appendix Figure **??** we plot the EPA's daily air quality index against what the air quality index would be if the air quality was only affected by smoke. There are many days where the degraded outside air pollution may be attributed to smoke, even when it is likely only a tiny part of this. To account for this, wherever the smoke pollution is only 5%, 10% and 20% of the overall AQI we set smoke pollution to 0 and re-run the main analysis in 1. This prevents other large polluting events that may also have significant avoidance behavior from being associated unduly with low amounts of smoke pollution. This filter is only possible for a subset of county-days for which the AQI is defined. We follow the Environmental Protection Agency's guidelines to transform smoke $PM_{2.5}$ into the corresponding AQI, and provide details in Section A.1. The AQI filters do not change our results.

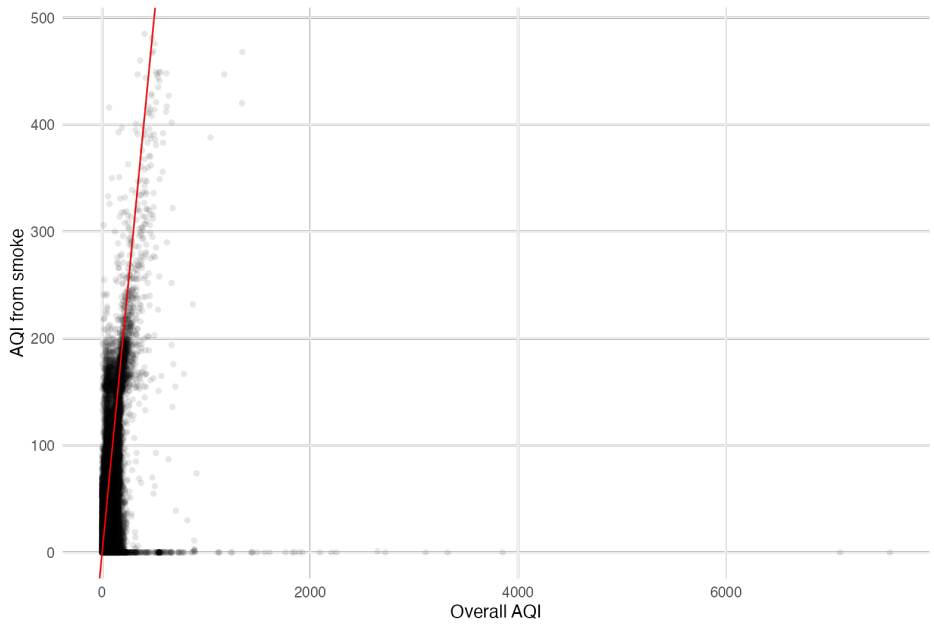
As discussed in the main text Section 2.1, another possible source of bias is potentially endogenous background pollution. Wherever the smoke pollution is less than one or less than two standard deviations, we set smoke pollution to 0 and re-run the main analysis in 1. In Figure A.1, we plot the results of every combination of AQI filter and background smoke filter, including the raw results with no background filter. We find that almost all of the results are identical except for the lowest bin, which the 2SD background filter pushes the estimate to be more negative than the rest of the results, though the overall coefficients are still quite small. Overall, the results are robust to any of these smoke measurements.

A.1 Relating Smoke $PM_{2.5}$ To Corresponding Air Quality Index

The EPA defines $PM_{2.5}$ concentration ranges that correspond to Air Quality Index (AQI) categories. To convert a $PM_{2.5}$ measurement to an AQI value, we linearly interpolate between the concentration boundaries and their corresponding AQI values. The conversion formula is:

$$\text{AQI}(x) = \begin{cases} 0 & \text{if } c = 0 \\ \text{lerp}(0, 50, 0.0, 9.0, c) & \text{if } 0 \leq c < 9.1 \\ \text{lerp}(51, 100, 9.1, 35.4, c) & \text{if } 9.1 \leq c < 35.5 \\ \text{lerp}(101, 150, 35.5, 55.4, c) & \text{if } 35.5 \leq c < 55.5 \\ \text{lerp}(151, 200, 55.5, 150.4, c) & \text{if } 55.5 \leq c < 150.5 \\ \text{lerp}(201, 300, 150.5, 250.4, c) & \text{if } 150.5 \leq c < 250.5 \\ \text{lerp}(301, 400, 250.5, 350.4, c) & \text{if } 250.5 \leq c < 350.5 \\ \text{lerp}(401, 500, 350.5, 500.4, c) & \text{if } 350.5 \leq c < 500.5 \\ 500 & \text{if } c \geq 500.5 \end{cases} \tag{11}$$

where $c = \lfloor 10x \rfloor / 10$ is the $PM_{2.5}$ concentration (in $\mu g/m^3$) truncated to one decimal place, and lerp is the linear interpo-



Notes: Decreases in air quality (higher Air Quality Indices) can have many causes, including but not limited to particulate matter from smoke. Occasionally, large decreases in the overall air quality in a county are associated with small amounts of surface level smoke $PM_{2.5}$. Since this only seems to disproportionately affect small amounts of smoke we test if setting smoke pollution equal to zero when the overall AQI is larger than the AQI from smoke affects our results in Figure A.1. This avoids attributing behavioral responses to other bad pollution episodes to relatively low levels of smoke pollution.

lation function:

$$\text{lerp}(s_a, f_a, s_p, f_p, c) = \frac{c - s_p}{f_p - s_p}(f_a - s_a) + s_a$$

Here, s_a and f_a are the starting and final AQI values for the category, s_p and f_p are the starting and final $PM_{2.5}$ concentrations for the category, and c is the observed concentration. The final AQI value is rounded to the nearest integer.

B Functional Form

We find that the Poisson model leads to almost identical results to a visits-weighted asinh OLS regression, which indicates that the small amount of county-days with zero visits are not significantly impacting our results. We prefer the unweighted asinh OLS for three reasons. First, we are interested in how avoidance behavior varies across counties. Weighting by visits would instead estimate the effect of smoke on foot traffic in the most populated counties, which also tend to be the richest and most educated that may have different avoidance behaviors. Second, we compare the results of the Poisson to the unweighted OLS and estimate that the coefficients that change serve to offset each other, resulting in similar estimated aggregate declines in foot traffic due to smoke between the models. Third, we use LP-DiD and spatial Durbin error models to assess spatial and temporal substitution, whose extensions to count models are not well established. We thus prefer the unweighted OLS asinh model, and are particularly interested in the γ_b coefficients on each bin. J. Chen and Roth (2024) show that asinh coefficients depend on the units of the outcome when there are zeros or the counts are small. However, with only 0.4% zeros and large visits counts (daily deferrable visits have a mean of 12,932 and a median of 950), $asinh(x) \approx ln(2x)$ and γ_b are approximate log units. To estimate the percent change in visits due to a day with smoke in bin b, we translate γ_b into $\exp(\gamma_b) - 1$. For small coefficients, $\exp(\gamma_b) - 1 \approx \gamma_b$, but the approximation breaks down as γ_b grows. Some of our coefficients are large, so we report $\exp(\gamma_b) - 1$ wherever a figure or the text describes the percent change due to smoke bin b, and γ_b wherever a figure or the text describes a coefficient on smoke bin b.

Because the inverse hyperbolic sine (asinh) approximates the natural log for sufficiently large values, we interpret our coefficients as approximate percent changes in visits, calculated as $100 \times (e^{\beta} - 1)$. We use this transformation throughout the paper wherever we describe a coefficient as a percent change. Figure B.1 plots histograms of asinh-transformed daily deferrable visits. Panel (a) shows the distribution used in the main analysis. Panel (b) shows the same distribution after removing the 11 counties with no recorded deferrable visits on more than 50% of days in the sample. When we drop these counties and re-estimate our main specification, the coefficients and county-clustered standard errors (in parentheses) are nearly identical to those in column 1 of Table B.1: -0.0005 (0.001), -0.003 (0.002), -0.005 (0.004), -0.027** (0.009), -0.040*** (0.010), and -0.181*** (0.023) for an additional smoke day in the (0, 15], (15, 30], (30, 60], (60, 100], (100, 200],

Table B.1: Functional Form Analysis

Dependent Variable:	asinh(Deferrable Daily Visits) asinh OLS (main) (1)		asinh(Deferrable Daily Visits) asinh OLS (visit-weighted) (2)		Deferrable Daily Visits Poisson (PPML) (3)	
	Coeff.	% Change†	Coeff.	% Change†	Coeff.	% Change
Variables						
Smoke Day (0, 15]	-0.0006	-0.06%	-0.0073**	-0.73%**	-0.0076***	-0.75%***
	(0.0011)	(0.11)	(0.0030)	(0.30)	(0.0029)	(0.28)
Smoke Day (15, 30]	-0.0029	-0.29%	-0.0070	-0.70%	-0.0064	-0.63%
	(0.0024)	(0.24)	(0.0047)	(0.47)	(0.0047)	(0.47)
Smoke Day (30, 60]	-0.0050	-0.50%	-0.0030	-0.30%	-0.0049	-0.49%
	(0.0044)	(0.44)	(0.0081)	(0.81)	(0.0072)	(0.72)
Smoke Day (60, 100]	-0.0271***	-2.68%***	-0.0408**	-4.00%**	-0.0432**	-4.23%**
	(0.0087)	(0.85)	(0.0166)	(1.59)	(0.0172)	(1.64)
Smoke Day (100, 200]	-0.0421***	-4.12%***	-0.0432***	-4.23%***	-0.0497***	-4.85%***
	(0.0105)	(1.00)	(0.0161)	(1.54)	(0.0153)	(1.45)
Smoke Day (200, 500]	-0.1819***	-16.63%***	-0.1050***	-9.96%***	-0.1097***	-10.39%***
	(0.0228)	(1.90)	(0.0283)	(2.55)	(0.0276)	(2.47)
Fit statistics						
Observations	5,638,360		5,638,360		5,635,945	
Squared Correlation	0.98109		0.96521		0.98276	

† *% Change is approximate for the asinh specifications; exact for the log-link Poisson model*
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Clustered (County) standard-errors in parentheses. All regressions include flexible weather controls, state-year-month, day of week-county-season, and day fixed effects. The % change column in the asinh specifications are estimated as $100 \times (e^{\beta} - 1)$ with delta-method s.e. in parentheses. In Column (2), we weight the regression by 1+ daily deferrable visits. We do this to ensure that the differences in results between Column (1) and Column (2) are not due to different sample sizes, since a pure visits weighted OLS model would drops any county-days with zero visits. However, the coefficients and standard errors are identical to four decimals between weighting with visits + 1 and just visits.

and (200, 500] $\mu g/m^3$ bins, respectively. This suggests our main results are not driven by the 0.4% of county-days with zero deferrable visits.

Table B.1 also compares our main specification (column 1) to a Poisson model (column 3), which is designed for count outcomes. With fixed effects, Poisson drops observations in fixed-effect groups where the outcome is always zero, since those observations are perfectly predicted. Relative to asinh OLS, Poisson estimates a larger effect for the (60, 100] $\mu g/m^3$ bin (-0.043 vs. -0.027) and a smaller effect for the (200, 500] $\mu g/m^3$ bin (-0.110 vs. -0.182). These differences come from how the two models weight observations. Poisson places more weight on counties with larger visit counts: it estimates the effect as a percent of mean visits, which is a visit-weighted average of county-level proportional effects (J. Chen and Roth, 2024). To confirm this, we estimate an asinh OLS model weighted by deferrable visits and report the results in column 2. We weight by visits plus one so that county-days with zero visits are not dropped for having zero weight. The Poisson and weighted asinh OLS estimates are nearly identical, and the remaining differences come from Poisson dropping fixed-effect groups with entirely zero visits.

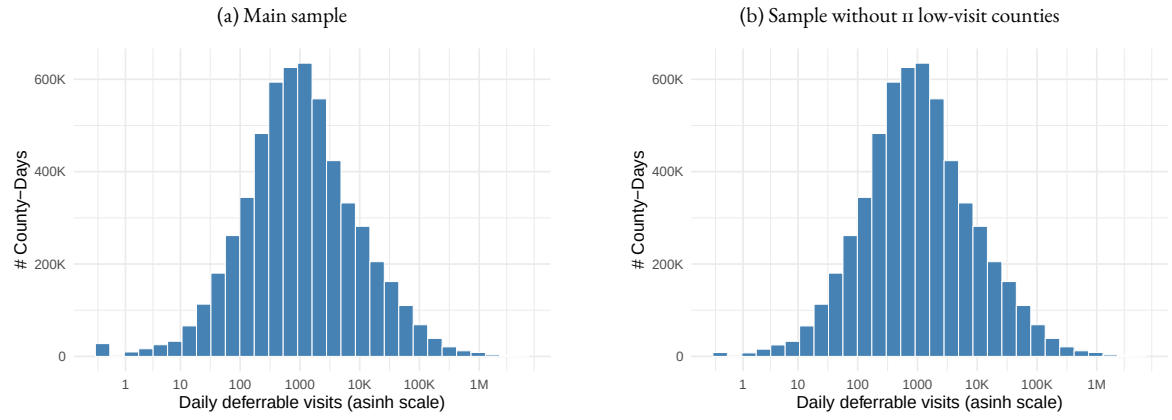
Table B.2 shows how our main estimates change across fixed effects and weather controls. Columns 1 and 2 include county, day, and state-year-month fixed effects. Columns 3 and 4 replace county fixed effects with county-by-day-of-week fixed effects, and Columns 5 and 6 allow these to vary by season. Even-numbered columns include flexible weather controls. Column 6 is our preferred specification, and Figure 3 plots its transformed coefficients. Without weather controls, smoke in the the (0, 15] and (15, 30] bins is associated with small and precisely estimated increases in visits, potentially because light smoke coincides with weather that draws people outdoors. Weather controls largely remove these effects. The (30, 60] coefficient is insignificant in every column, and it turns negative once we control for weather. Above 60 $\mu g/m^3$, estimates are nearly identical across fixed-effect specifications once weather is controlled for, and county-by-season day-of-week fixed effects improve precision slightly. Column 6 is our preferred specification because it removes the spurious low-level effects and absorbs weekly visit patterns that vary across counties and seasons.

Table B.2: Fixed effects comparison

Dependent Variable:	asinh(Deferrable Daily Visits)					
	(1)	(2)	(3)	(4)	(5)	(6)
Smoke Day (0, 15]	0.0064***	0.0033***	0.0055***	0.0022*	0.0049***	-0.0006
	(0.0011)	(0.0013)	(0.0011)	(0.0012)	(0.0011)	(0.0011)
Smoke Day (15, 30]	0.0071***	0.0001	0.0059**	-0.0013	0.0057**	-0.0029
	(0.0025)	(0.0026)	(0.0025)	(0.0026)	(0.0024)	(0.0024)
Smoke Day (30, 60]	0.0054	-0.0032	0.0071	-0.0015	0.0036	-0.0050
	(0.0048)	(0.0048)	(0.0049)	(0.0049)	(0.0044)	(0.0044)
Smoke Day (60, 100]	-0.0212**	-0.0303***	-0.0181*	-0.0272***	-0.0192**	-0.0271***
	(0.0099)	(0.0099)	(0.0099)	(0.0098)	(0.0088)	(0.0087)
Smoke Day (100, 200]	-0.0334***	-0.0432***	-0.0334***	-0.0432***	-0.0351***	-0.0421***
	(0.0116)	(0.0117)	(0.0114)	(0.0115)	(0.0104)	(0.0105)
Smoke Day (200, 500]	-0.1693***	-0.1827***	-0.1685***	-0.1823***	-0.1718***	-0.1819***
	(0.0247)	(0.0248)	(0.0235)	(0.0237)	(0.0228)	(0.0228)
Weather Controls		Yes		Yes		Yes
County	Yes	Yes				
Day	Yes	Yes	Yes	Yes	Yes	Yes
State-Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
DoW-County			Yes	Yes		
DoW-County-Season					Yes	Yes
Fit statistics						
Observations	5,638,360	5,638,360	5,638,360	5,638,360	5,638,360	5,638,360
R^2	0.97794	0.97802	0.97907	0.97915	0.98103	0.98109
Within R^2	0.00004	0.00341	0.00004	0.00375	0.00004	0.00315

Notes: Table reports estimates from regressions of the inverse hyperbolic sine of daily deferrable sector visits on indicators for bins of smoke-attributable $PM_{2.5}$. The omitted category is days with no smoke. Coefficients are untransformed, and $\exp(\hat{y}) - 1$ gives the approximate percent change in visits. Weather controls include quantile bins of precipitation, maximum relative humidity, and minimum and maximum temperature. Column 6 is our preferred specification and is plotted in Figure 3. The sample includes county-days from 2019 to 2023. Standard errors, clustered by county, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure B.1: Distribution of Deferrable Visits



Notes: Panel (a) of this figure plots the distribution of daily deferrable visits. The x-axis is the number of visits on an asinh scale, and the y-axis the county-days with that number of daily deferrable visits. Panel (b) is the same as Panel (a), except removing the 11 counties that have no recorded daily visits for more than 50% of the days in the sample.

C Changes in Visits Panel Composition

To account for the panel heterogeneity that may change over time, we leverage two empirical strategies. First, to account for trends in changes in panel size, we control for changing monthly levels of foot traffic over time via a state-year-month fixed effect. Our second empirical strategy rests on the fact that wildfire smoke can travel thousands of miles (Langmann et al., 2009), and that the psuedo-random timing and location of wildfire smoke is likely unrelated to intra-month panel composition changes. While we don't have data on the intra-month panel composition, we can test if monthly smoke exposure is related to the monthly panel composition. For example, if people leave the panel in the same months affected

Table C.1: Monthly Panel Composition and Smoke

Dependent Variable:	Residents per Sampled Device
Model:	(1)
Variables	
Smoke Days (0, 15]	-2.11×10^{-6}
	(2.73×10^{-6})
Smoke Days (15, 30]	1.23×10^{-5}
	(1.39×10^{-5})
Smoke Days (30, 60]	-2.19×10^{-5}
	(5.13×10^{-5})
Smoke Days (60, 100]	-5.2×10^{-6}
	(0.0002)
Smoke Days (100, 200]	9.45×10^{-5}
	(0.0002)
Smoke Days (200, 500]	-5.82×10^{-5}
	(0.0001)
Fixed-effects	
State	Yes
Year - Month	Yes
Fit statistics	
Dependent variable mean	15.379
Observations	2,940
R^2	0.99970
Within R^2	0.00032

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Each observation is a state-month in the balanced panel, 2019-2023. The dependent variable is Advan's scaling multiple (normalized visits by state scaling divided by raw visit counts), the number of residents each sampled device represents, averaged over the state's county-days. Smoke bins count the county-days in the state-month whose smoke-attributable $PM_{2.5}$ falls in each bin, in $\mu g/m^3$.

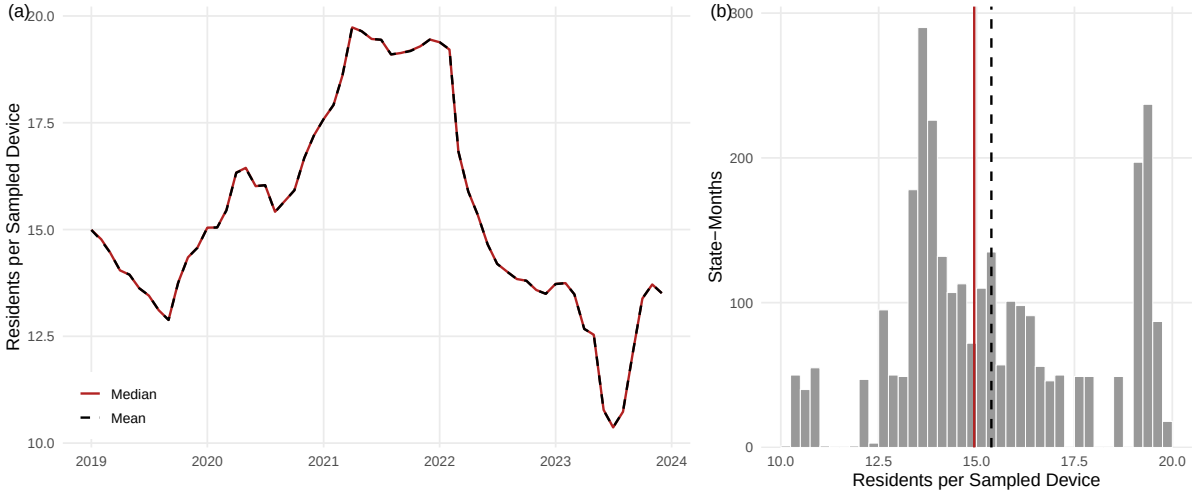
by wildfire smoke, then while we control for monthly panel composition changes by month, we cannot reject that that reduction in visits and panel leaving happened at the same time during the month. We test this second strategy explicitly by regressing the number of smoky days per state-month-year on the state-month normalization weights provided to us by Advan patterns. These normalization weights are calculated by Advan by dividing the number of unique people in the panel with visits each state by the population of the state. Figure C.1 plots the distribution of normalization weights and the mean and median across counties over time. We find no relationship between the normalization weights and the number of smoke days per state-month, so we do not find that panel heterogeneity would be influencing out results through relationship with smoke days. We show the results of this regression in table C.1.

D LP-DID Event Study With Additional Controls

One concern with the LP-DID analysis is that the days with more than 60 $\mu g/m^3$ of smoke $PM_{2.5}$ may have systematically more smoke exposure in the pre-period, which may be driving some of the changes in visits. In Panel (a) of the main text Figure 6, treated counties have slightly elevated smoke above control counties, though this is negligible (1-5 $\mu g/m^3$) compared to the exposure during the smoke event itself.

To confirm that this slightly elevated smoke in the pre-period is not biasing our estimate of the effect of smoke on visits, we conduct a robustness check where we expand our restriction for treated counties to be excluded from treatment. In the original analysis, the county-smoke spells considered treated if the county is exposed to more than 60 $\mu g/m^3$ of smoke. If an additional smoke event of over 60 $\mu g/m^3$ of smoke occurs after the first one, then both smoke events are excluded. This is how Dube et al. (2025) recommends to create a clean control group and to isolate the treatment effect. In this robustness check, we tighten our requirement for inclusion in the treatment group to be that in the 14 days before a 60 $\mu g/m^3$ smoke spell, there cannot have been any day with more than 30 $\mu g/m^3$ of smoke. This reduces our number of

Figure C.1: Monthly residents per sampled device



Notes: This graph describes the monthly residents per sampled device that Advan patterns provides for every state-month. Panel (a) shows the residents per sampled device over time, aggregated using the average and median over counties. The two measures coincide for our sample period. Panel (b) plots a histogram of the residents per sampled device, where the y-axis is the number of state-months that have that normalization rate.

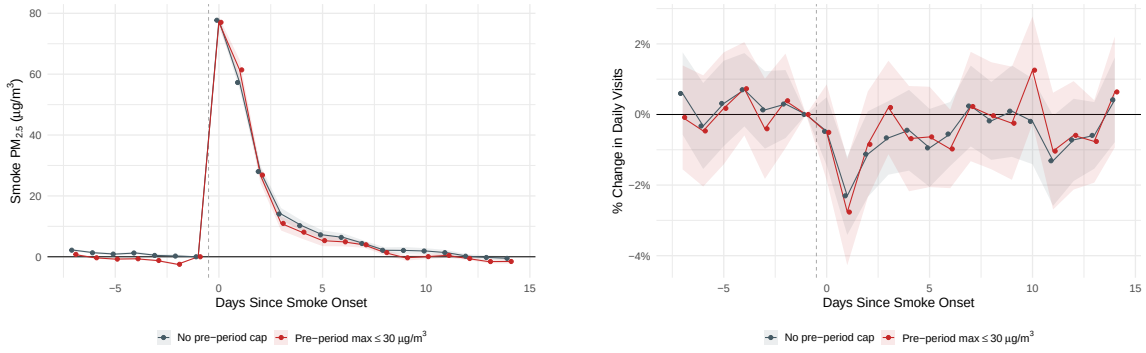
spells from 2,308 to 1,252 (a 45.8% reduction), but improves the comparability between the control and treated counties, as the counties are not exposed to significantly higher amounts of pollution in the pre-period that may impact avoidance behavior either through increased salience of wildfire smoke or other channels.

We find that our results do not change. We do not test the effect of reducing the smoke exposure to 0 or even 15, because the number of smoke spells that enter into our analysis reduces from 2,308 events overall to 1,252 events with 30 $\mu g/m^3$ to only 362 events and 10 events with 15 $\mu g/m^3$ and 0 $\mu g/m^3$, respectively. Therefore a complete restriction of pre-period smoke makes the analysis extremely noisy, and our preferred specification is to filter at most to 30 $\mu g/m^3$. Figure D.1 reports the resulting event-study estimates, which are similar to our main results. Additionally restricting the control county-days to only those with less than 30 $\mu g/m^3$ or even zero $\mu g/m^3$ does not change the results.

Figure D.1: Event Studies with Clean Controls

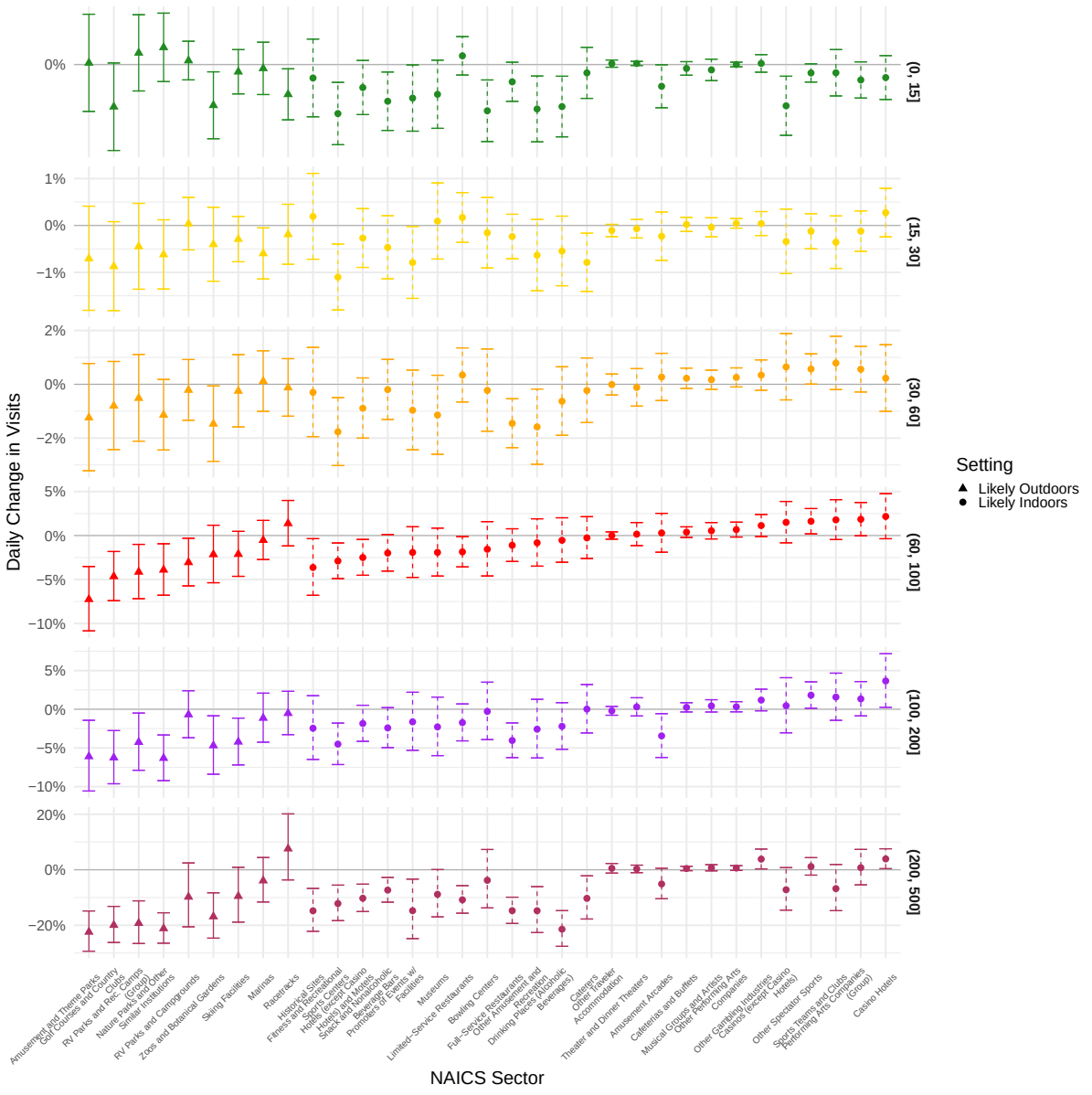
(a) Smoke event effect on smoke with additional pre-period smoke filter

(b) Smoke event effect on visits with additional pre-period smoke filter



Notes: This figure shows the original treated-only robustness check. An smoke spell onset has smoke at least 60 $\mu g/m^3$ after 14 retained observations strictly below 60. The additional restriction requires smoke no greater than 30 in the treated pre-period; controls retain the below-60 rule through each horizon.

Figure E.1: Likely Indoor/Outdoor Visits Analysis for all Smoke Types



Notes: Figure plots the transformed coefficients on each $\mu g/m^3$ bin, which indicate the percent change in visits on days with smoke in that range, relative to days with no smoke. Equations are estimated separately for each NAICS code (six-digit, plus the four-digit groups 7111 and 7212) within the deferrable sectors (NAICS 71 and 72). Each regression replaces total deferrable visits in Equation 1 with visits to POIs in that industry. Triangles with solid lines are likely outdoor industries, and circles with dashed lines are likely indoor industries. Bars show 95% confidence intervals, and standard errors are clustered by county.

Table E.1: Classifications of likely indoor/outdoor NAICS codes.

Code	Title	Code	Title
Panel A: Likely outdoors (9 codes)			
711212	Racetracks	713920	Skiing Facilities
712130	Zoos and Botanical Gardens	713930	Marinas
712190	Nature Parks and Other Similar Institutions	7212†	RV (Recreational Vehicle) Parks and Recreational Camps
713110	Amusement and Theme Parks	721211	RV (Recreational Vehicle) Parks and Campgrounds
713910	Golf Courses and Country Clubs		
Panel B: Likely indoors (33 codes)			
7111†	Performing Arts Companies	713950	Bowling Centers
711110	Theater Companies and Dinner Theaters	713990	All Other Amusement and Recreation Industries
711120	Dance Companies	721110	Hotels (except Casino Hotels) and Motels
711130	Musical Groups and Artists	721120	Casino Hotels
711190	Other Performing Arts Companies	721191	Bed-and-Breakfast Inns
711211	Sports Teams and Clubs	721199	All Other Traveler Accommodation
711219	Other Spectator Sports	721214	Recreational and Vacation Camps (except Campgrounds)
711310	Promoters of Performing Arts, Sports, and Similar Events with Facilities	721310	Rooming and Boarding Houses, Dormitories, and Workers' Camps
711320	Promoters of Performing Arts, Sports, and Similar Events without Facilities	722310	Food Service Contractors
711410	Agents and Managers for Artists, Athletes, Entertainers, and Other Public Figures	722320	Caterers
711510	Independent Artists, Writers, and Performers	722330	Mobile Food Services
712110	Museums	722410	Drinking Places (Alcoholic Beverages)
712120	Historical Sites	722511	Full-Service Restaurants
713120	Amusement Arcades	722513	Limited-Service Restaurants
713210	Casinos (except Casino Hotels)	722514	Cafeterias, Grill Buffets, and Buffets
713290	Other Gambling Industries	722515	Snack and Nonalcoholic Beverage Bars
713940	Fitness and Recreational Sports Centers		

E Indoor Outdoor Visits Classification

Table E.1 lists each NAICS code, its description, and its indoor or outdoor classification. All codes are six-digit industries except two four-digit industry groups, 7111 (Performing Arts Companies) and 7212 (RV Parks and Recreational Camps). Some places with these four-digit industry groups in the foot-traffic data are coded only to the four-digit level, so we classify these two groups directly.

F Mortality welfare estimates

The health estimates discussed in the paper Ma et al. (2024) use binned annual averages of (0.1, 0.2], (0.2, 0.3], (0.3, 0.4], (0.4, 0.5], (0.5, 0.7], (0.7, 1], (1, 5], and 5+ $\mu g/m^3$ to allow for non-linear effects of smoke pollution on mortality. To connect our daily pollution bins to Ma et al. (2024)'s annual bins, we divide the average $PM_{2.5}$ in each daily bin by 365, representing the additional annual pollution from a single day of smoke:

- (0, 15] and (15, 30] bins: Average values of 3.4 and 19.7 $\mu g/m^3$ yield annualized values below 0.1 when divided by 365, falling below the threshold for measurable mortality effects.
- (30, 60] bin: Average $PM_{2.5}$ of 40.2 $\mu g/m^3$ corresponds to 0.11 annualized, mapping to the (0.1, 0.2] annual bin with an associated 0.16 increase in monthly mortality per 100,000 people.
- (60, 100] bin: Average $PM_{2.5}$ of 75.8 $\mu g/m^3$ corresponds to 0.21 annualized, mapping to the (0.2, 0.3] bin with an associated 0.4 increase in monthly mortality per 100,000 people.
- (100, 200] bin: Average $PM_{2.5}$ of 130.9 $\mu g/m^3$ corresponds to 0.36 annualized, mapping to the (0.3, 0.4] bin with an associated 0.35 increase in monthly mortality per 100,000 people.
- (200, 500] bin: Average $PM_{2.5}$ of 283 $\mu g/m^3$ corresponds to 0.78 annualized, mapping to the (0.7, 1] bin with an associated 0.63 increase in monthly mortality per 100,000 people.

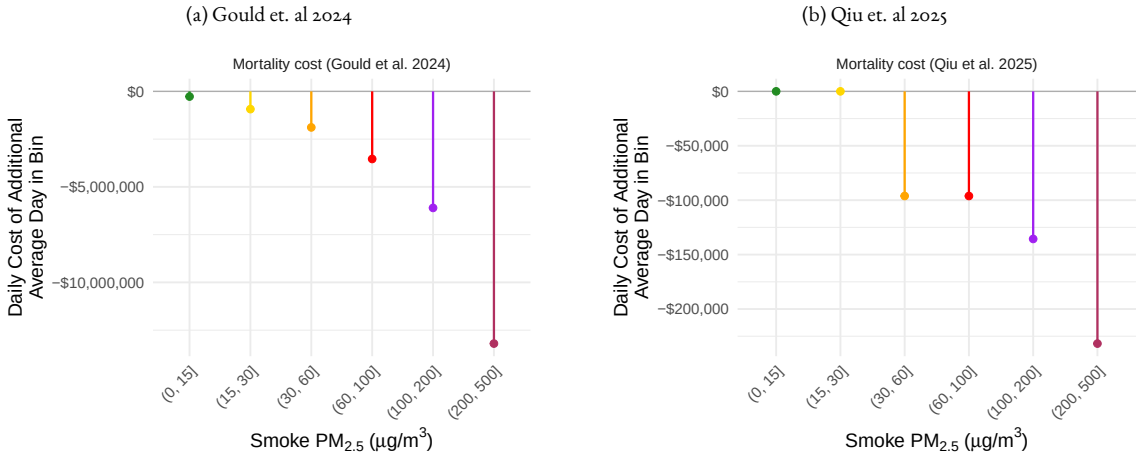
These mortality estimates are expressed per 100,000 people (approximately the average county population in our sample: 106,075) and per month. We divide by 30 to convert to daily mortality, interpreting these as the average daily mortality effect per county from an additional day of smoke $PM_{2.5}$ in each bin. To make mortality costs comparable with consumer spending losses, we multiply mortality estimates by the EPA's recommended value of statistical life: $7.4 million in 2006 dollars, equivalent to $899,936 per 10 % reduction in mortality risk in 2017 dollars.

F.1 Alternative mortality welfare estimates

In Panel (a) of Figure F.1, we use the result of an empirical meta-analysis published in the (Gould et al., 2024) which estimated that an additional $\mu g/m^3$ of $PM_{2.5}$ increases same day all-cause mortality by 0.15%. While the authors analyze emergency room visits later in the paper and find that the effect of wildfire smoke is non-linear - their meta-analysis does not account for non-linearity. In Panel (b) of Figure F.1, we use the findings of Qiu et al. (2025) to estimate the non-linear effect of wildfire smoke on mortality. The methodology of Qiu et al. (2025) is very similar to that of Ma et al. (2024) but with different annual pollution bins, so we use the same strategy as described in F to transform the annual smoke exposure on mortality into daily smoke exposure on mortality.

We prefer Ma et al. (2024) because their bins uniquely correspond to ours, unlike Qiu et al. (2025) which when translated to our bins leads to the same mortality estimate for (30, 60] and (60, 100]. We also prefer Ma et al. (2024) over Gould et al. (2024) because estimating the cost of a day of smoke with only $\mu g/m^3$ does not account for nonlinear effects of smoke on mortality. The non-linear estimates are likely more conservative than the meta-analysis result which implies two orders of magnitude more deaths for the largest smoke day bin.

Figure F.1: Alternative health cost estimates



Notes: These graphs show alternative estimates of the mortality costs of a day in each smoke bin. The left panel uses estimates from Gould et al. (2024), and the right panel uses estimates from Qiu et al. (2025).

G Spatial Substitution

H Other explanations for reductions in activity

H.1 Controlling for fire activity

Figure H.1 summarizes the prevalence of satellite-detected fires in the sample, which motivates the alternative fire controls reported in Table H.1.

I Heterogeneity Analysis

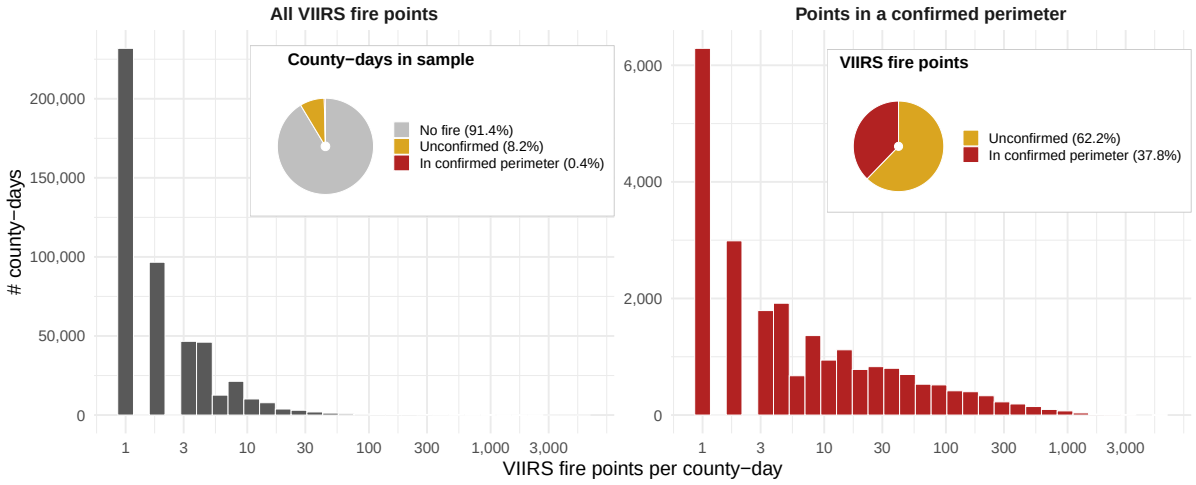
The equation we use to estimate heterogeneities by county characteristics is as follows:

Table G.1: Smoke co-occurance across neighboring counties

Own-county	Weighted share of neighbors in smoke bin						
smoke bin	(0, 15]	(15, 30]	(30, 60]	(60, 100]	(100, 200]	(200, 500]	No smoke
(0, 15]	0.787	0.022	0.001	0.000	0.000	0.000	0.189
(15, 30]	0.299	0.604	0.064	0.005	0.001	0.000	0.027
(30, 60]	0.076	0.269	0.543	0.082	0.012	0.001	0.017
(60, 100]	0.033	0.073	0.296	0.452	0.126	0.006	0.015
(100, 200]	0.014	0.023	0.088	0.253	0.554	0.058	0.009
(200, 500]	0.006	0.013	0.027	0.069	0.340	0.539	0.006

Notes: This table contains the empirical average co-occurance of smoke exposure for each smoke bin or $\bar{w}_{k,b}$ defined in equation 7. As an example, the second column in the first row (Smoke bin k= (0,15]) implies that in our sample if a county is exposed to smoke between (0, 15] $\mu g/m^3$, then on average 2.2% of the county's neighbors were exposed to between (15, 30] $\mu g/m^3$ of smoke. The fraction of neighboring counties that are smoke free declines significantly as the source county k smoke exposure increases, with less than 1% of neighboring counties smoke free for a source county exposed to over 100 $\mu g/m^3$ of smoke.

Figure H.1: Fire Prevalence



Notes: Distribution of satellite-detected fire points per county-day, by source. Both panels use a log-scaled x-axis. The left panel shows the number of county-days by count of VIIRS fire points, using all detections. Most county-days have only a few fire points, and 62% of fire points are unconfirmed and may be false positives. The inset pie chart shows the share of all county-days in the sample with no fire, unconfirmed fire only, or confirmed fire. The right panel restricts to confirmed fires, which shifts the distribution toward higher fire-point counts, since confirmed fires tend to be larger. The inset pie chart shows the share of all VIIRS fire points that are associated with a confirmed fire or an unconfirmed fire.

Table H.1: Controlling for Fire Co-occurrence with Smoke

Dependent Variable:	asinh(Deferrable Daily Visits)					
Sample:	Full sample		Any fire dropped		Wildfire dropped	
	Main	Log VIIRS ctrl.	>25	>100	>25	>100
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Smoke Day (0, 15]	-0.0006	-0.0005	-0.0005	-0.0005	-0.0005	-0.0006
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Smoke Day (15, 30]	-0.0029	-0.0025	-0.0026	-0.0026	-0.0024	-0.0025
	(0.0024)	(0.0025)	(0.0025)	(0.0024)	(0.0025)	(0.0024)
Smoke Day (30, 60]	-0.0050	-0.0035	-0.0028	-0.0025	-0.0025	-0.0025
	(0.0044)	(0.0044)	(0.0044)	(0.0043)	(0.0044)	(0.0043)
Smoke Day (60, 100]	-0.0271***	-0.0241***	-0.0190**	-0.0202**	-0.0182**	-0.0198**
	(0.0087)	(0.0086)	(0.0091)	(0.0090)	(0.0090)	(0.0089)
Smoke Day (100, 200]	-0.0421***	-0.0387***	-0.0316***	-0.0347***	-0.0315***	-0.0343***
	(0.0105)	(0.0103)	(0.0106)	(0.0107)	(0.0105)	(0.0106)
Smoke Day (200, 500]	-0.1819***	-0.1704***	-0.1839***	-0.1765***	-0.1793***	-0.1739***
	(0.0228)	(0.0231)	(0.0287)	(0.0264)	(0.0274)	(0.0260)
log(1 + VIIRS)		0.0079				
		(0.0058)				
log(1 + VIIRS)^2		-0.0028**				
		(0.0014)				
Fit statistics						
Observations	5,638,360	5,638,360	5,629,917	5,636,087	5,633,411	5,636,395
R^2	0.98109	0.98109	0.98108	0.98109	0.98108	0.98109
Within R^2	0.00315	0.00316	0.00314	0.00315	0.00315	0.00315

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Smoke bins are indicators for the day's smoke-attributable $PM_{2.5}$, in $\mu g/m^3$; the omitted category is a day with no smoke $PM_{2.5}$. Every column uses flexible weather controls and Day, State-Year-Month, and Day of Week-County-Season fixed effects. Column (1) is the main day-of specification. Column (2) adds the log of one plus the VIIRS fire detections inside a fire perimeter and its square as controls. Columns (3)-(6) instead drop every county-day whose VIIRS detection count exceeds the stated threshold: total detections under *Any fire dropped*, detections inside a fire perimeter under *Wildfire dropped*.

$$
\begin{aligned}
asinh(\text{visits}_{c,d}) = \sum_b \Big(& \gamma^j_b \mathbb{1}\{PM^{smoke}_{c,d} \in \text{bin}_b\} \\
& + \sum_{q \in 2,3,4} \lambda^j_{b,q} \mathbb{1}\{PM^{smoke}_{c,d} \in \text{bin}_b\} \times \mathbb{1}\{(q-1)/4 < ECDF(H^j_c) \leq q/4\} \Big) \\
& + \beta^j T_{c,d} + day^j_d + \theta^j_{c,dow,z} + \phi^j_{s,m} + \varepsilon^j_{c,d}
\end{aligned} \tag{12}
$$

where the only difference between this equation and Equation 1 is the addition of the interaction term $\sum_{q \in 2,3,4} \lambda^j_{b,q} \mathbb{1}\{PM^{smoke}_{c,d} \in \text{bin}_b\} \times \mathbb{1}\{(q-1)/4 < ECDF(H^j_c) \leq q\}$. H^j_c is the dimension of heterogeneity j that we are interested in for county c. H^j_c is a county-specific variable that changes slowly over time (e.g. median annual county income, seasonal background $PM_{2.5}$). We estimate the empirical quartiles of each H^j_c from the earliest year each heterogeneity characteristic is available before the rest of the analysis starts. $\lambda^j_{b,q}$ identifies the difference in within-county PM response between quartiles 2, 3, and 4, relative to the first quartile.

$$
\begin{aligned}
asinh(\text{visits}_{c,d}) = \sum_b \Big(& \gamma^j_b \mathbb{1}\{PM^{smoke}_{c,d} \in \text{bin}_b\} \\
& + \lambda^j_b \mathbb{1}\{PM^{smoke}_{c,d} \in \text{bin}_b\} \times \mathbb{1}\{d \in \{\text{Sat., Sun.}\}\} \Big) \\
& + \beta^j T_{c,d} + day^j_d + \theta^j_{c,dow,z} + \phi^j_{s,m} + \varepsilon^j_{c,d}
\end{aligned} \tag{13}
$$

Rather than reporting $\lambda^j_{b,q}$ directly, which is interpreted as the effect relative to the omitted bin, we translate the estimates into absolute terms so that each quartile has its own level response. Define the level coefficient for bin b and quartile q as:

$$
\Gamma^j_{b,q} = \gamma^j_b + \lambda^j_{b,q} \qquad q \in \{2,\ 3,\ 4\}, \qquad \Gamma^j_{b,1} = \gamma^j_b, \tag{14}
$$

or, written out by quartile,

$$
\Gamma^j_{b,q} = \begin{cases} \gamma^j_b & \text{Q1: } 0 < ECDF(H^j_c) \leq 0.25 \\ \gamma^j_b + \lambda^j_{b,2} & \text{Q2: } 0.25 < ECDF(H^j_c) \leq 0.5 \\ \gamma^j_b + \lambda^j_{b,3} & \text{Q3: } 0.5 < ECDF(H^j_c) \leq 0.75 \\ \gamma^j_b + \lambda^j_{b,4} & \text{Q4: } 0.75 < ECDF(H^j_c) \leq 1. \end{cases} \tag{15}
$$

The analogous object for the smoke timing specification in Equation 13 is:

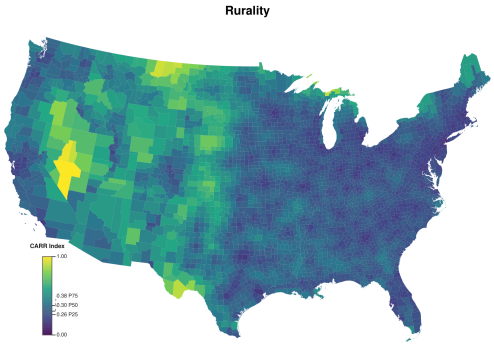
$$
\Gamma^j_{b,d} = \begin{cases} \gamma^j_b & d \in \{\text{Mon., } \ldots, \text{Fri.}\} \\ \gamma^j_b + \lambda^j_b & d \in \{\text{Sat., Sun.}\}. \end{cases} \tag{16}
$$

I.o.1 Levels Plot Coefficients

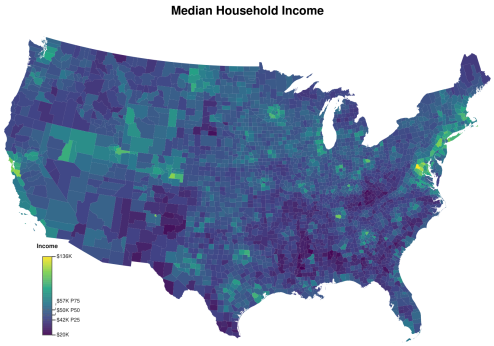
Let $\Gamma^j_{b,\{q,d\})}$ represent the superset of $\Gamma^j_{b,q}$ defined in 15 for county characteristics heterogeneities calculated over the four quartiles, and $\Gamma^j_{b,d}$ defined in 16 for smoke timing heterogeneities. $\Gamma^j_{b,\{q,d\}}$ is the level response of the percent change in deferrable foot traffic to smoke $PM_{2.5}$ in bin b for counties in quartile p of characteristic H^j_c, where the first quartile is the omitted category and its level is the uninteracted coefficient γ^j_b. Standard errors on $\Gamma^j_{b,\{q,d\})}$ account for the covariance between the additional terms. For quartiles 2-4 this leads to the following expression for the variance: $Var(\Gamma^j_{b,q \in \{2,3,4\}}) = Var(\gamma^j_b) + Var(\lambda^j_{b,q}) + 2\,Cov(\gamma^j_b, \lambda^j_{b,q})$, with an analogous expression for weekend smoke $Var(\Gamma^j_{b,d \in \{\text{Sat., Sun.}\}})$.

Figure I.1: Maps of Heterogeneity Variables

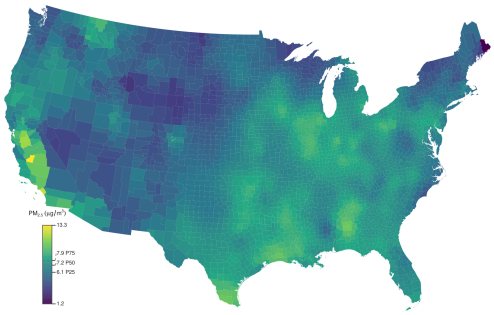
(a) Rurality Index



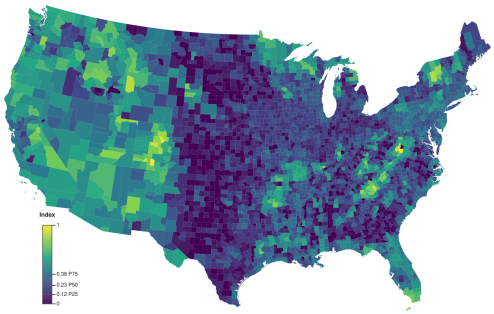
(b) Median Household Income



(c) Background $PM_{2.5}$



(d) Natural Amenity Tourism



Notes: This Figure displays the spatial distributions of the different available heterogeneity variables used in our heterogeneity analyses. Panel (a) visualizes the spatial distribution of the Community Assets and Relative Rurality (CARR) index measure of rurality. Panel (b) visualizes the spatial distribution of median household income in 2018. Panel (c) visualizes the map of background $PM_{2.5}$ described in Section 2.1. Panel (d) visualizes the spatial distribution of our natural amenity tourism dependency measure. This measure combines dependence on tourism related sectors with park and forest area by county. See section 2.3 for details on the heterogeneities.

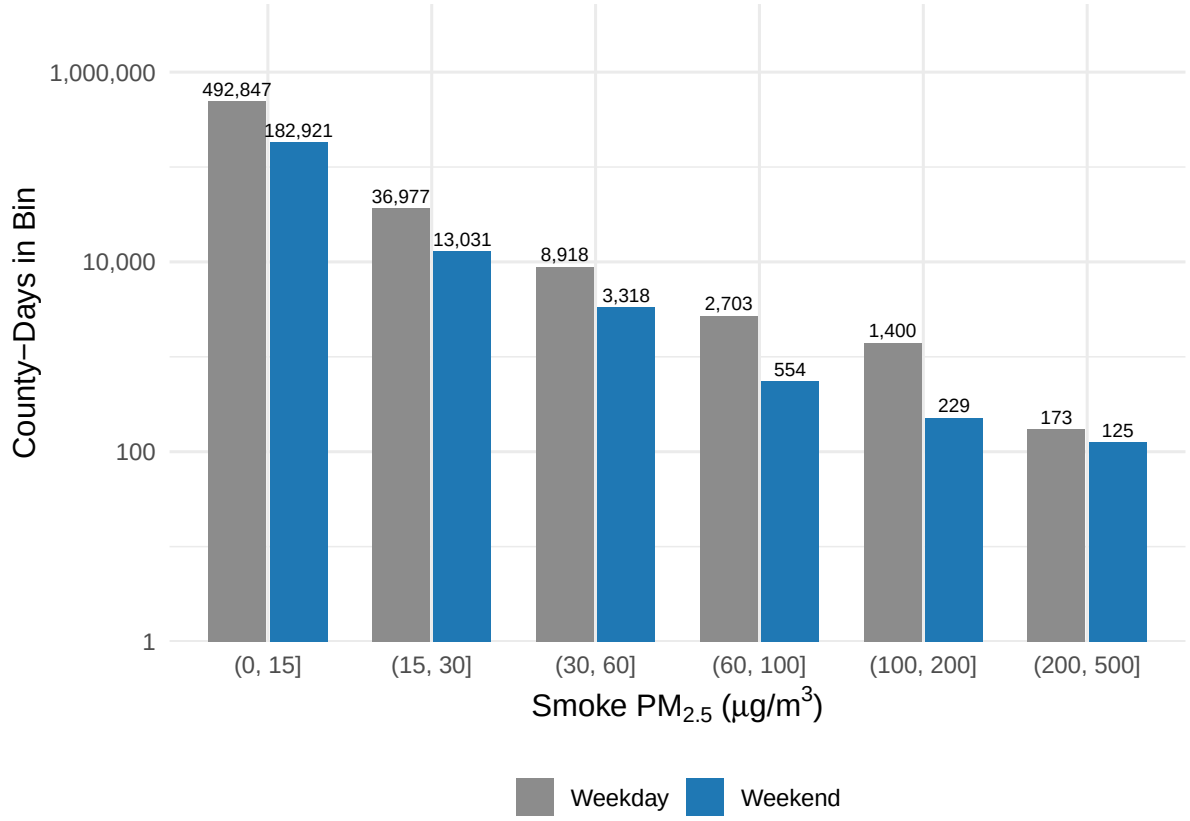
I.1 Heterogeneity Variable Descriptions

I.2 Adding Day FE to the heterogeneities plot

$$
\begin{aligned}
asinh(\text{visits}_{c,d}) = \sum_b \Big(& \gamma_b^j \mathbb{1}\{PM_{c,d}^{smoke} \in \text{bin}_b\} \\
& + \sum_{q \in 2,3,4} \lambda_{b,q}^j \mathbb{1}\{PM_{c,d}^{smoke} \in \text{bin}_b\} \times \mathbb{1}\{(q-1)/4 < ECDF(H_c^j) \leq q/4\} \Big) \\
& + \beta^j T_{c,d} + day_d^j \times \mathbb{1}\{(q-1)/4 < ECDF(H_c^j) \leq q/4\} + \theta_{c,dow,z}^j + \phi_{s,m}^j + \varepsilon_{c,d}^j
\end{aligned}
\tag{17}
$$

where the only difference between this equation and Equation 12 is the replacement of the day fixed effect day_d^j with an interaction term $day_d^j \times \mathbb{1}\{(q-1)/4 < ECDF(H_c^j) \leq q/4\}$. H_c^j is the dimension of heterogeneity j that we are interested in for county c. H_c^j is a county-specific variable that changes slowly over time (e.g. median annual county income, seasonal background $PM_{2.5}$). We estimate the empirical quartiles of each H_c^j from the earliest year each heterogeneity characteristic is available before the rest of the analysis starts. This update adds an additional control for the differences in levels of visits on a given day per heterogeneity characteristic to avoid conflating shared responses to other heterogeneity group specific shocks with the group's response to smoke. However, this fixed effect absorbs much of the identifying variation.

Figure I.2: Weekday/Weekend Smoke Day Counts



Notes: Number of weekday and weekend county-days in each smoke bin.

J Visits Spend Relationship

J.1 County Aggregation

We aggregate both smoke exposure and foot traffic data to the county level for three principal reasons. First, our research question concerns how smoke exposure affects regional economic activity. Smoke plumes affect large geographic areas, and individuals move throughout their county for work, shopping, and recreation, making the county a natural unit for analyzing behavioral responses to air quality shocks. Second, county-level analysis aligns with the scale at which public health advisories and air quality information are typically disseminated, making our findings more policy-relevant. Third, aggregating to the county level is likely to improve the signal in mobility data. POI-level data is very noisy since many POIs have zero or near-zero traffic on any given day. Li et al. (2024) analyzes the same mobility dataset that we use, and finds that aggregating foot traffic data to counties improves the correlation between population and visits compared to aggregating to census tract or census block group. This indicates that aggregating to the county improves the quality of our measure of outcome of interest, and may increase the external validity of our findings to individuals outside of the panel.

Aggregating to the county level is likely to reduce measurement error in our outcome variable through reducing sampling bias (Li et al., 2024). However, a valid concern is that aggregating our treatment variable (smoke) that is measured at a grid cell level may introduce enough measurement error from aggregation to offset the improved measurement from reducing sampling bias. For smoke exposure, we document that within-county variance in smoke-attributable PM is significantly smaller than between-county variance (see Appendix Figure J.2). This spatial pattern indicates that smoke exposure is relatively homogeneous within counties: when a county experiences a smoke event, most of the county experiences similar exposure levels. One limitation of spatial aggregation is that we cannot observe whether foot traffic responses to smoke vary across different areas within a county. In Appendix Table J.1, we test whether or not the intra-county variation of smoke is related to foot traffic, and we find no relationship. This supports our conclusion that the measurement error we may introduce by aggregating the smoke data to the county is limited. Given that smoke exposure itself exhibits limited within-county variation, we argue that county-level aggregation provides the most reliable approach

Figure I.3: Correlation matrix of continuous heterogeneity variables

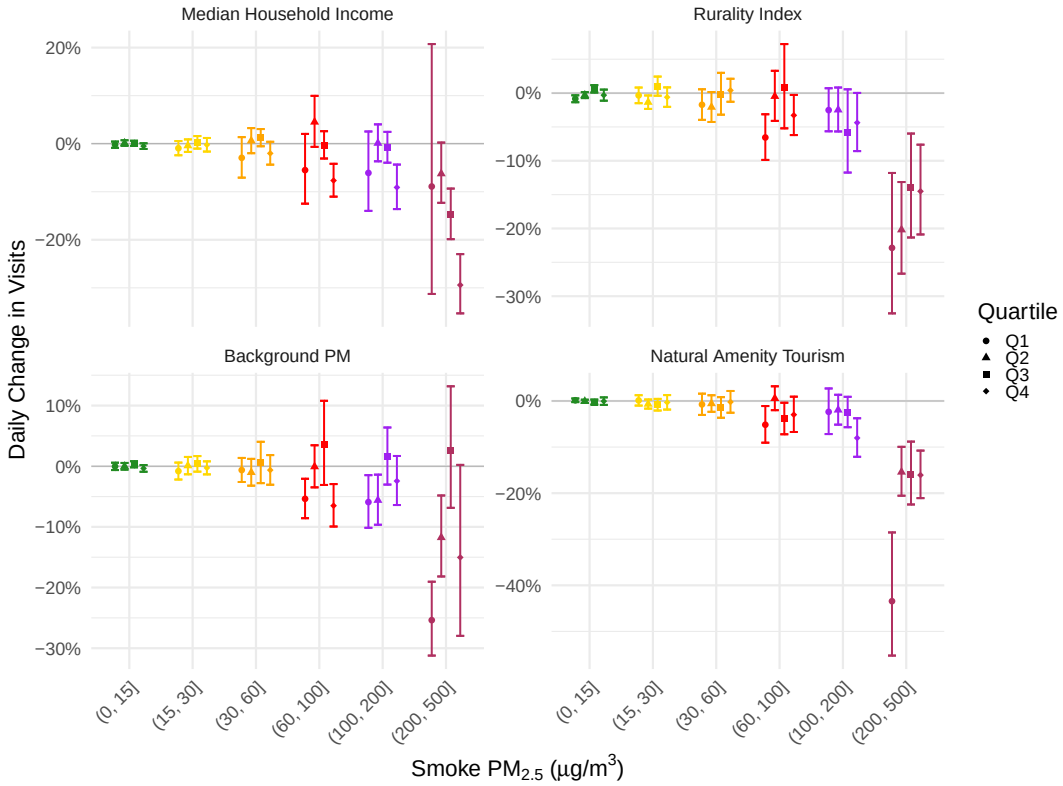


Notes: This figure is a correlation matrix of each of the continuous heterogeneity variables that we use in our analysis.

for identifying how smoke exposure affects economic activity.

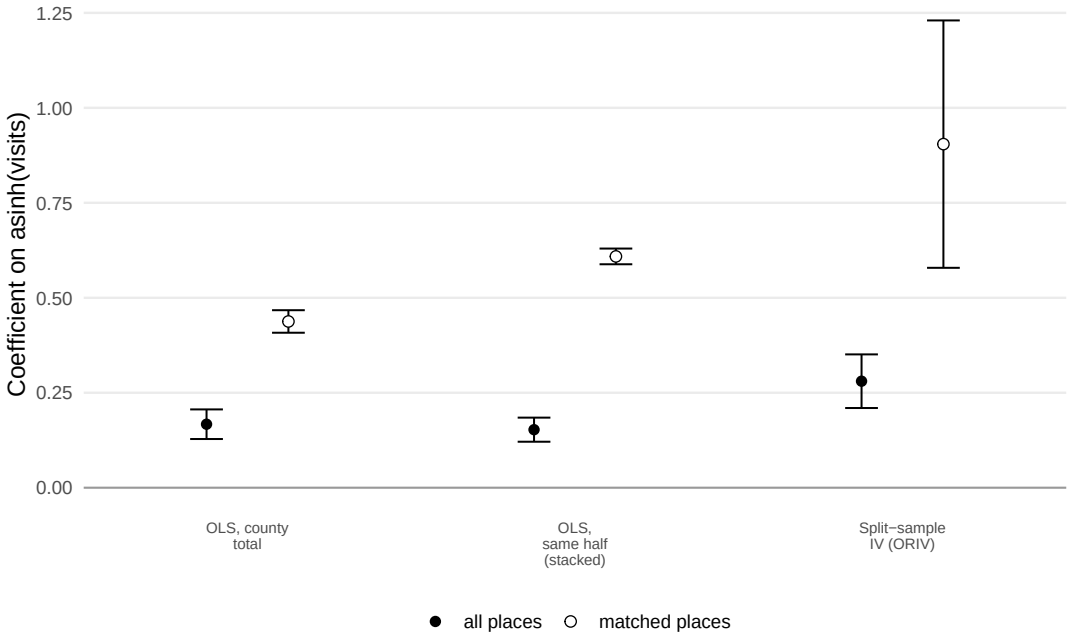
K COVID-19 Pandemic

Figure I.4: Heterogeneity in Smoke Responses with Date-by-Quartile Fixed Effects



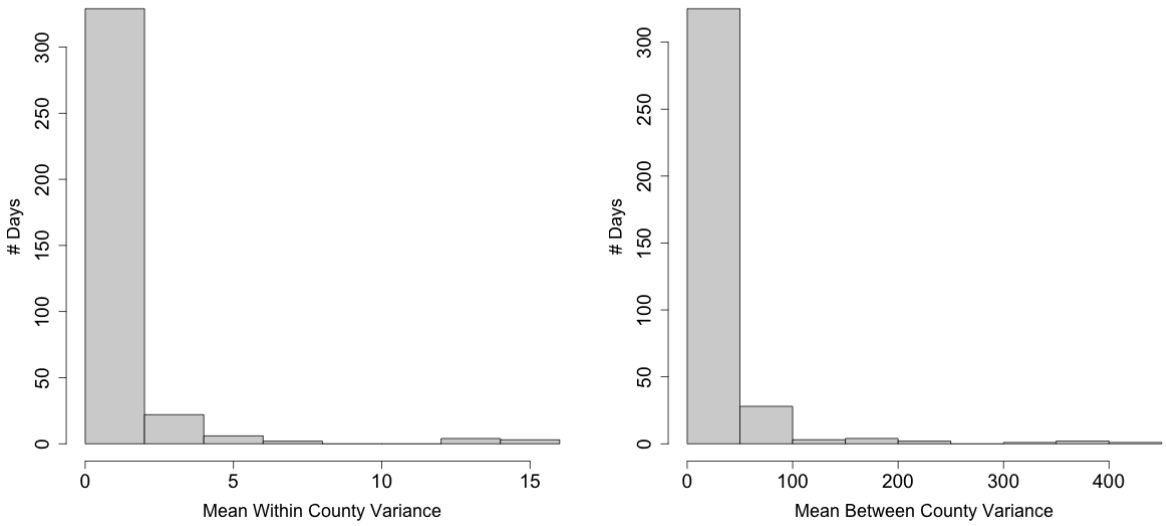
Notes: Each panel shows the estimated change in visits due to smoke within each quartile of the indicated county characteristic, as described in Section I.o.1. Separate regressions replace the common date fixed effects with date-by-quartile fixed effects, allowing daily shocks to differ across quartiles. Points show $\exp(\Gamma^{j}_{b,q}) - 1$ with 95% confidence intervals; standard errors are clustered by county. The weekday/weekend panel is omitted because weekend status is determined entirely by calendar date. Interacting date fixed effects with weekend status therefore adds no controls, so this specification cannot provide the analogous robustness check for weekday/weekend differences.

Figure J.1: Comparison between relationship of visits and spend with different models



Notes: This plot shows estimates of the visits-spend relationship from six specifications: OLS, OLS estimated on the same randomly assigned half of the sample, and the ORIV split-sample IV estimator (see Section 3.3). Each is estimated first on the full sample and then on the subset of POIs with at least one day of both spend and visits in the same month (matched places)

Figure J.2: Distributions of within and between county variance



Notes: The left panel shows the within-county variation. For each day, we calculate the variance of the smoke $PM_{2.5}$ for all grid cells within a given county. We then average these over all counties for the day. The distribution of resulting daily average of within-county variation is plotted in the leftmost panel. The right panel shows between county variation. For each day, we calculate the variance between mean county smoke $PM_{2.5}$ for all counties (county level smoke is averaged across grid cells). The distribution of the between-county variance is plotted on the rightmost panel. The between-county variance is significantly larger than the within-county variance.

Table J.1: County Smoke Variance

Dependent Variable:	asinh(Deferrable Daily Visits)		
	Main Specification	Within SD (Median)	Within SD (Average)
Model:	(1)	(2)	(3)
Variables			
Smoke Day (0, 15] μg $PM_{2.5}$	-0.0006	-8.72×10^{-6}	-1.51×10^{-5}
	(0.0011)	(0.0012)	(0.0012)
Smoke Day (15, 30] μg $PM_{2.5}$	-0.0029	-0.0024	-0.0024
	(0.0024)	(0.0024)	(0.0024)
Smoke Day (30, 60] μg $PM_{2.5}$	-0.0050	-0.0044	-0.0044
	(0.0044)	(0.0044)	(0.0044)
Smoke Day (60, 100] μg $PM_{2.5}$	-0.0271***	-0.0265***	-0.0265***
	(0.0087)	(0.0087)	(0.0087)
Smoke Day (100, 200] μg $PM_{2.5}$	-0.0421***	-0.0422***	-0.0422***
	(0.0105)	(0.0104)	(0.0104)
Smoke Day (200, 500] μg $PM_{2.5}$	-0.1819***	-0.1815***	-0.1815***
	(0.0228)	(0.0229)	(0.0229)
Within-County log SD of Smoke $PM_{2.5}$		-9.92×10^{-5}**	-9.83×10^{-5}**
		(4.77×10^{-5})	(4.81×10^{-5})
SD at 5% (μg $PM_{2.5}$)	–	0.099	0.086
SD at 95% (μg $PM_{2.5}$)	–	1.909	1.840
5% → 95% change (log SD units)	–	2.959	3.068
Effect on visits of 5% → 95%	–	-0.029%	-0.030%
Fit statistics			
Observations	5,638,360	5,614,700	5,614,700
R^2	0.98109	0.98113	0.98113
Within R^2	0.00315	0.00317	0.00317

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The within-county SD of smoke $PM_{2.5}$ enters as log(SD) shifted by 15, which keeps every county-day with smoke variation strictly positive; smoke-free county-days (zero variance) are set to zero and so sit below all of them. Percentiles are taken over the estimation sample, among county-days with non-zero smoke variance. The effect on visits is the coefficient times the change in log SD units, in asinh (approximately log) points. All regressions include flexible weather controls and Day, State-Year-Month, and Day of Week-County-Season fixed effects. Columns 2 and 3 have fewer observations than column 1 because the within-county SD is undefined for counties that fall entirely within a single smoke $PM_{2.5}$ grid cell; these 13 counties, all small Virginia independent cities, account for the 23,660 dropped county-days.

Table K.1: Additional Fixed Effects

Dependent Variable:	asinh(Deferrable Daily Visits)	
Model:	(1)	(2)
Variables		
Smoke Day (0, 15]	-0.0006	-0.0009**
	(0.0011)	(0.0004)
Smoke Day (15, 30]	-0.0029	-0.0036***
	(0.0024)	(0.0011)
Smoke Day (30, 60]	-0.0050	-0.0097***
	(0.0044)	(0.0030)
Smoke Day (60, 100]	-0.0271***	-0.0236***
	(0.0087)	(0.0047)
Smoke Day (100, 200]	-0.0421***	-0.0426***
	(0.0105)	(0.0092)
Smoke Day (200, 500]	-0.1819***	-0.1086***
	(0.0228)	(0.0178)
Flexible Weather Controls	Yes	Yes
Fixed-effects		
Day	Yes	Yes
State-Year-Month	Yes	Yes
Day of Week-County-Season	Yes	Yes
County-Month of Sample		Yes
State-Day		Yes
Fit statistics		
Observations	5,638,360	5,638,360
R^2	0.98109	0.99518
Within R^2	0.00315	0.00170

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Smoke bins are indicators for the day's smoke-attributable $PM_{2.5}$, in $\mu g/m^3$; the omitted category is a day with no smoke $PM_{2.5}$. Column 1 is the main specification.