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Abstract

Governments monitor air quality for regulatory purposes and, more recently, to provide information so individuals can act to lower their exposure to air pollution. Recent developments in low-cost technologies have also led to private adoption of air-quality monitors that produce publicly accessible air-quality readings. We study the adoption of these private air-quality monitors. We find that shocks to air pollution from wildfire result in substantial adoption. We also find that additional private monitors are concentrated in white, wealthy, and politically liberal neighborhoods. In contrast, there is no evidence that pollution shocks lead to higher adoption in neighborhoods with lower pre-existing access to monitors, higher long-run pollution, or those with more vulnerable populations. The resulting stark differences in the availability of localized air-quality information suggest that private provision may worsen not ameliorate inequalities in the impacts of poor air quality.

JEL-Codes: Q530, Q520.

Keywords: air quality, air quality monitoring, wildfire, information.

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1 Introduction

Air pollution is responsible for 4.2 million premature deaths a year worldwide (World Health Organization, 2021). The global distribution of pollution is profoundly unequal with large populations in low and middle-income countries exposed to extremely low levels of air quality (Murray et al. 2020). In the United States, exposure to air pollution is disproportionately higher in African-American communities (Currie et al. 2023). Public access to pollution information can have substantial benefits by facilitating behavior change that lowers the impacts of pollution, and can also lead to lower pollution (Barwick et al. 2019, Jha and La Nauze 2022); however, access to air-quality monitoring is uneven and gaps in air-quality monitoring networks are exploited by polluters and local governments (Grainger et al. 2019, Zou 2021, Axbard and Deng 2023, Ito and Zhang 2020).

Public information on air quality has traditionally been provided by governments via a public monitoring network. However, dramatic reductions in the cost of home monitors have led to the development of a substantial non-government monitoring network that now exceeds the size of the public network. Because information from these private monitors is readily and publicly available, this network of monitors is a public good, and its distribution is important for governments determining their own investments in air-quality information.[1] Despite the growing body of evidence for the benefits of air-quality monitoring, little is known about the demand, provision, and use of it by households.

In this paper, we explore how shocks to air quality affect the demand for privately owned air-quality monitors and how this demand affects the distribution of information. Specifically, we leverage plausibly exogenous variation in air quality arising from the component of PM2.5 that is caused by wildfire smoke in California. Using a difference-in-differences approach, we show that wildfire smoke events increase adoption of self-installed, low-cost outdoor air-quality monitors manufactured by Purple Air. We find that exposure to wildfire smoke results

1. Privately owned air-quality monitors that provide public information can be classified as impure public goods in the sense of Cornes & Sandler (1984).

in a sharp temporary spike in air-quality monitor adoption. We find the size of the monitor adoption response is not correlated with yearly average PM2.5 exposure or the percentage of the population that is elderly or very young, who from a health perspective are most at risk from negative air-quality shocks; if anything, neighborhoods with high concentrations of young children are less likely to install monitors. We also show the adoption response is uncorrelated with distance from an existing government monitor but more likely to occur in neighborhoods with existing private monitors. The increase in adoption is strongest in neighborhoods with high shares of white, highly educated, wealthy, and Democratic-leaning residents.

The results speak directly to an ongoing policy debate on the public provision of air-quality monitoring. For example, in 2021, the United States provided $50 million in community air monitoring grants administered through the Environmental Protection Agency (EPA) to *enhance air-quality monitoring in communities across the United States with environmental and health outcome disparities stemming from pollution and the COVID-19 pandemic*. Similarly, the state of California designated $35 million in Community Air Grants which includes money to improve air-quality monitoring in areas of California most severely impacted by air pollution.[2] An explicit goal of both of these programs is to advance environmental justice by improving air-quality monitoring in areas with historically high pollution burdens. Our findings show that the goal of improving air-quality monitoring in disadvantaged neighborhoods is unlikely to be reached through low-cost private monitoring alone, as private adoption is strongly concentrated in neighborhoods with relatively wealthy and educated residents and is uncorrelated with neighborhood pollution levels.

Our findings on the evolution of the private monitoring network complement previous work exploring the origins and effects of the public monitoring network. Pollution reductions in Clean Air Act nonattainment counties are steepest in neighborhoods close to public monitors (Bento et al. 2015, Auffhammer et al. 2009). However, the locations of public

2. See: California AB 617, ww2.arb.ca.gov/capp; American Rescue Plan Enhanced Community Air Monitoring, www.epa.gov/arp/enhanced-air-quality-monitoring-funding-under-arp.

monitors are themselves endogenously determined, as local regulators seek to avoid placing monitors in highly polluted areas where the monitor may risk incurring regulatory action (Grainger et al. 2019), and are also likelier to place monitors in areas with high income and white residents (Grainger and Shreiber 2019).

That both public and private monitors are less likely to be placed in low-income areas is concerning, as Hausman and Stolper (2021) theoretically show that low-income households suffer greater deadweight loss from lack of air-quality information even when limited information is uniformly distributed across households. This finding arises from unmonitored pollution affecting low-income areas in the well-established context of residential sorting on environmental quality (Banzhaf and Walsh 2008, Gamper-Rabindran and Timmins, 2011).

We find that shocks to air quality increase private monitoring in wealthy, white, politically liberal neighborhoods. Although the descriptive correlation between private monitoring and these demographic variables has been shown before (deSouza and Kinney 2021), we causally identify the effect of air-quality shocks on private adoption. We show that such shocks exacerbate, rather than reduce, inequality in information.

2 Background and Data

We measure private provision of air-quality monitoring using the count of new installations of Purple Air's home air-quality monitors. Customers purchase a Purple Air monitor which can be placed indoors or outdoors. Upon registering a new device as a public monitor, the monitor is added to an online interactive map that reports real-time air-quality data from all users with public Purple Air monitors. We gather data on monitors from Purple Air's API. Data contains the installation date, geographic coordinates, and whether the monitor is indoor or outdoor. We gather all 2,825 monitor installations providing public data on the Purple Air Map in California from 2016 (when the first installation appears) through April 2019.[3] 82% of these monitors are installed outdoors, measuring ambient air quality.

3. Monitors that are installed but set to private are not observed.

Purple Air monitors are high quality, with a root mean squared error of 8 μg/m^3 under typical PM2.5 conditions (Barkjohn et al. 2021), although Purple Air monitors tend to overpredict PM2.5 concentrations during extreme smoke events (Barkjohn et al. 2022). The EPA provides a correction equation that is integrated into the publicly available real-time map on Purple Air's website.[4]

Although questions remain about the measurement error from low-cost monitors, in aggregate, the private contribution of air-quality information from Purple Air monitors has the potential to be more economically meaningful than the contribution of information from government monitors. California's EPA system consists of 700 monitors at 250 sites; although the government monitor network is *One of the most extensive in the world* (California Air Resources Board Annual Network Plan 2017), the number of private monitors installed during our study period greatly exceeds the number of government monitors.

To isolate an exogenous source of variation in air quality, we use smoke from wildfires in California. Data comes from Stanford University's Environmental Change and Human Outcomes (ECHO) Lab (Childs et al. 2022). The Echo Lab uses ground and satellite data and a machine learning method to estimate the component of PM2.5 concentration that is the result of wildfire smoke. Echo Lab provides this estimate at the census tract level daily from 2006 to 2020. We define a major smoke event as one in which the wildfire smoke component of PM2.5 exceeds 100 μg/m^3. PM2.5 concentrations of 100 μg/m^3 are quite severe; this measure is in the "Unhealthy" range of 151-200 on the Air Quality Index (AQI) scale. Note however, that because the ECHO Lab measure includes only the smoke component, overall air-quality levels are at least 100 μg/m^3.[5] We combine smoke data with information on the locations of the EPA's existing monitors, demographics from the American Community

4. see: www.epa.gov/sciencematters/epa-research-improves-air-quality-information-public-airnow-fire-and-smoke-map and www.map.purpleair.com.

5. We show robustness in Appendix Figures A3-A6 to use of alternative thresholds for defining a smoke event. Treatment effects are detectable to around 35 μg/m^3 (Figure A3). We run event studies that 1) define treatment equal to 1 if smoke exceeds 35 μg/m^3 in a tract-month (Figures A4 and A5) and 2) define treatment equal to 1 if smoke exceeds 100 μg/m^3 while controlling for a set of indicators for smaller 35 μg/m^3 events (Figure A6). Defining a major smoke event at this lower threshold produces smaller but statistically significant and economically meaningful results.

Survey (ACS), and voting returns from California's Statewide Database.

Our study period notably encompasses the severe California wildfire seasons of 2017 and 2018. The 2018 season, a record at the time (although since surpassed by the 2020 and 2021 fire seasons) saw nearly 2 million acres of land burned, over 24,000 structures destroyed, and 100 lives lost.[6] The major smoke-producing event of our study period is November 2018's Camp Fire, which delivered 100 $\mu g/\text{m}^3$ of smoke to 1,829 census tracts. While areas that ever directly experience a wildfire are more likely to be small, sparsely populated, and at the edge of the wildland-urban interface, plausibly exogenous variation in air-quality arises from the timing of fire occurrence and atmospheric conditions which affect how wildfire smoke is transported to downwind locations.

Table 1 presents summary statistics on the demographics and pollution characteristics of census tracts that ever have a Purple Air monitor installed. Tracts with Purple Air monitor installations are more likely to be wealthy, white, and urban, although slightly further from existing EPA monitors.

3 Empirical Strategy

To estimate the effect of air-quality shocks on the private provision of air-quality monitoring, we use a difference-in-differences approach conducted on a panel of 8,057 census tracts in California. Because monitor installations are relatively infrequent events, we aggregate our daily installation and smoke data to the monthly level. Each observation in our panel represents a tract-month. To explore the dynamic response to smoke events, we conduct an event study specified as:

$$Y_{st} = \alpha + \delta_s + \gamma_t + \sum_{i \in PRE} \beta_i 1\{t - t_s^* = i\} + \sum_{i \in POST} \pi_i 1\{t - t_s^* = i\} + \epsilon_{st} \tag{1}$$

Where Y_{st} is the count of new Purple Air monitor installations in census tract s in month

6. source: CAL FIRE at www.fire.ca.gov/incidents/2018

t, δ_s is a tract fixed effect, and γ_t is a month-by-year fixed effect. The coefficients β_i capture the pre-period trend in monitor installation in tract s in advance of an event occurring at time t_s^*. An event is defined as a month in which the wildfire smoke contribution to PM2.5 exceeded 100 $\mu g/\text{m}^3$ in tract s. The post-period coefficients π_i capture the effect of smoke events on monitor installations over time. Our baseline specification uses a six month window on either side of a smoke event, so $i \in PRE$ ranges from -6 to -1 and $i \in POST$ ranging from 0 to 6.

For the heterogeneity analysis, we estimate the following equation:

$$Y_{st} = \alpha + \beta_1 Smoke\_Event_{st} + \delta_s + \gamma_t + \epsilon_{st} \tag{2}$$

$Smoke\_Event_{st}$ is an indicator equal to 1 if at month t the tract has experienced a day in which the wildfire smoke contribution to PM2.5 exceeded 100 $\mu g/\text{m}^3$, and 0 otherwise. The coefficient of interest is β_1 measuring information demand response to smoke events. The other variables are the same as Equation 1. Robust standard errors are clustered at the tract level.

The use of a two-way fixed effects model with variation in treatment timing introduces concerns about the weighting of treatment effects arising from an implicit assumption of treatment homogeneity as highlighted by a number of recent papers.[7] This concern is applicable in our setting, as the existence of multiple treated units with variations in timing creates the potential for erroneous comparisons between treated and not-yet-treated units. In the appendix, we therefore show event study results using the efficient D-i-D-imputation method of Borusyak et al. (2021). Note this estimator assumes that treatment is an absorbing state; that is, $Smoke\_Event_{st} = 1$ in the period the event occurs and for all periods thereafter. This is a different treatment definition than our baseline estimates in which $Smoke\_Event_{st} = 1$ in the tract-month of the event's occurrence and zero otherwise, which

7. de Chaisemartin and D'Haultfœuille 2020b, Borusyak and Jaravel 2021, Goodman-Bacon 2021, Sun and Abraham 2020, Callaway and Sant'Anna 2021

we believe better captures the short-lived nature of smoke events. Results from the D-i-D-imputation are nearly identical to those of the classic event study.

4 Results

Figure 1 depicts the event study showing the response of monitor installations within 6 months of a major smoke event. We observe a large and significant increase in monitor adoptions following a smoke event. This adoption response quickly decays, so that monitor adoptions four months after the event are indistinguishable from zero. The effect size is economically important- an increase of 0.074 monitor installs in the month following a smoke event represents an increase of 21% relative to the overall mean of 0.35 monitors per tract.

The corresponding event study using the imputation method of Borusyak et al. (2021) is shown in Figure A1. While there is no pretrend in monitor installations until two months before a smoke event, there is a small and significant increase in monitor adoptions in the two months preceding a smoke event. We investigate the origins of this pretrend in the Appendix, where we show the small increase in adoptions observed during the pre-period is likely a true adoption response to smoke events below the 100 $\mu g/\mathrm{m}^3$ threshold.

Previous studies have documented behavioral responses to wildfire exposure that are typically short-lived, such as a temporary decline in house prices (McCoy and Walsh 2018) and increases in pro-environment voting (Hazlett and Mildenberger 2020, Coury 2023). We find a similar pattern: a spike in private monitor adoptions following a major smoke event that quickly returns to baseline, as in the insurance uptake response to floods (Gallagher 2014) and earthquakes (Lin 2020). Following Gallagher (2014), in the Appendix we provide evidence that monitor adoptions are driven by direct smoke exposure rather than news coverage via TV media markets.

Having documented a large increase in monitor adoption following air-quality shocks, we next look to see where new monitors are being installed. To begin with, we assess whether the same pollution shock leads to greater private monitor adoption in locations that are further from the existing air-quality monitoring stations. To do so, we run the specification Equation 2 on quintile bins of census tracts defined by distance to existing EPA monitors. The first regression limits the sample to the 20% of tracts nearest to a government monitor. Subsequent regressions limit the sample to all tracts in ensuing quintiles. Panel (a) of Figure 2 plots the coefficients on smoke events from these regressions. The effect of air-quality shocks is positive, but there is no relationship between distance to existing monitors and adoption. This finding suggests that private monitors are not substitutes for government monitors.

We next investigate whether the pollution shock has a greater impact on monitor adoption in areas with worse long-run pollution exposure, and that contain more vulnerable populations. Following a similar procedure to that outlined above, Panel (b) of Figure 2 shows that there is no relationship between pre-existing levels of PM2.5 (measured by public monitors) and monitor adoption in response to smoke events.[8] Panels (c) and (d) show that these new installations are also not disproportionately benefiting communities with a higher share of children under 10 (panel c), or a higher share of adults over 80 (panel d). If anything, there is a negative relationship between the share of children in a community and monitor installation following a smoke event.

We next investigate whether there is significant clustering in monitor adoption; i.e., whether new private monitors are more likely to be installed near existing private monitors. This type of clustering may exist because of a correlation in preferences of residents, or due to spillovers between residents. Table 2 shows the effect of smoke events on adoption where we split the sample by the presence of existing Purple Air monitors. The effect of air-quality shocks on monitor adoptions is especially strong in tracts that already have Purple Air monitors; each smoke event produces an increase of 0.4 monitors per month in tracts

8. The point estimate for the upper quantile of Figure 2 panel (b) is large and poorly estimated. In the Appendix, we discuss this finding at length and test the interaction between *Smoke_Event* and initial PM2.5 levels. There is no statistically significant trend in the adoption response by initial pollution levels.

with more than two monitors. Recall also that Purple Air adoptions are more likely to be urban and in tracts that have a small geographic area. This means that new adoptions are measuring air pollution in areas where coverage is already dense, and the new monitors may consequently provide little additional information.

We next investigate the socio-economic characteristics of the neighborhoods that benefit from the new monitors. Figure 3 shows that neighborhoods with the highest share of white, highly educated[9], Democratic voting and highest-income residents receive a disproportionate share of monitors induced by smoke exposure. For neighborhoods in the lowest quintile share of white, highly educated, and income, we cannot reject that the smoke event had no impact on monitor installation. Smoke events in neighborhoods with the lowest quintile share of registered Democrats produce a significantly positive effect on monitor adoption, though the response is much smaller than in the neighborhoods with the highest Democratic share.

To formalize these results, in Appendix Table A2, we test interaction terms to show that the monitor adoption response is increasing in the share of white, highly educated, Democratic voting and high-income residents but is invariant to the share of elderly residents (over 80), distance to public monitor, and yearly average pollution, and decreasing in the share of the population under 10. Private monitor adoptions are therefore likely to worsen, rather than reduce, inequalities in the impacts of poor air quality.

5 Conclusion

Air-quality monitoring can reduce the impacts of poor air quality by facilitating mitigation and avoidance behavior. This monitoring has traditionally been provided by governments but new low-cost sensors are resulting in substantial private provision of air-quality information. We use plausibly exogenous variation in wildfire smoke to show that air-quality shocks cause substantial spikes in private air-quality monitor installations.

9. We define highly educated as the share of residents holding a PhD or a professional degree. We use the definition of professional degree from the American Community Survey, which includes MD, DDS, DVM, LLB, and JD, among others. It does not include bachelor's or master's degrees

We also study the distribution of private low-cost air monitors that results from these shocks. The provision of air-quality information as a way to empower local communities is a key component of many environmental justice initiatives aimed at pollution mitigation. These initiatives highlight the fact that not all communities have granular air-quality information from the existing network of government monitors.

We find that the adoption of new monitors is mostly uncorrelated with measures of pollution exposure or vulnerability to air pollution. Rather, new private monitor adoptions cluster in areas that are primarily wealthy, white, highly educated, and that already have private monitors nearby. The large expansion of the private monitoring network therefore is unlikely to substitute for public policies aiming to decrease inequality in air-quality information coverage and the impacts of air pollution.

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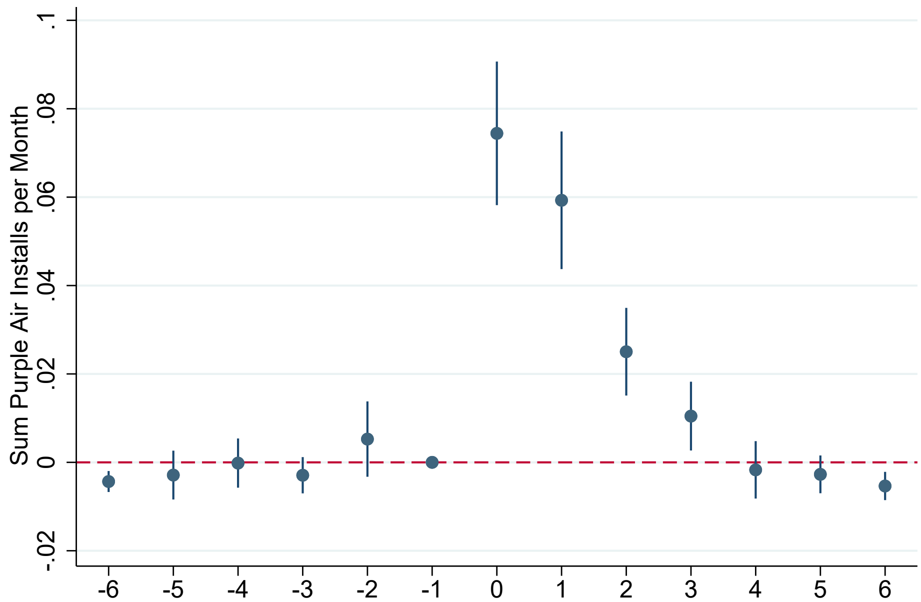
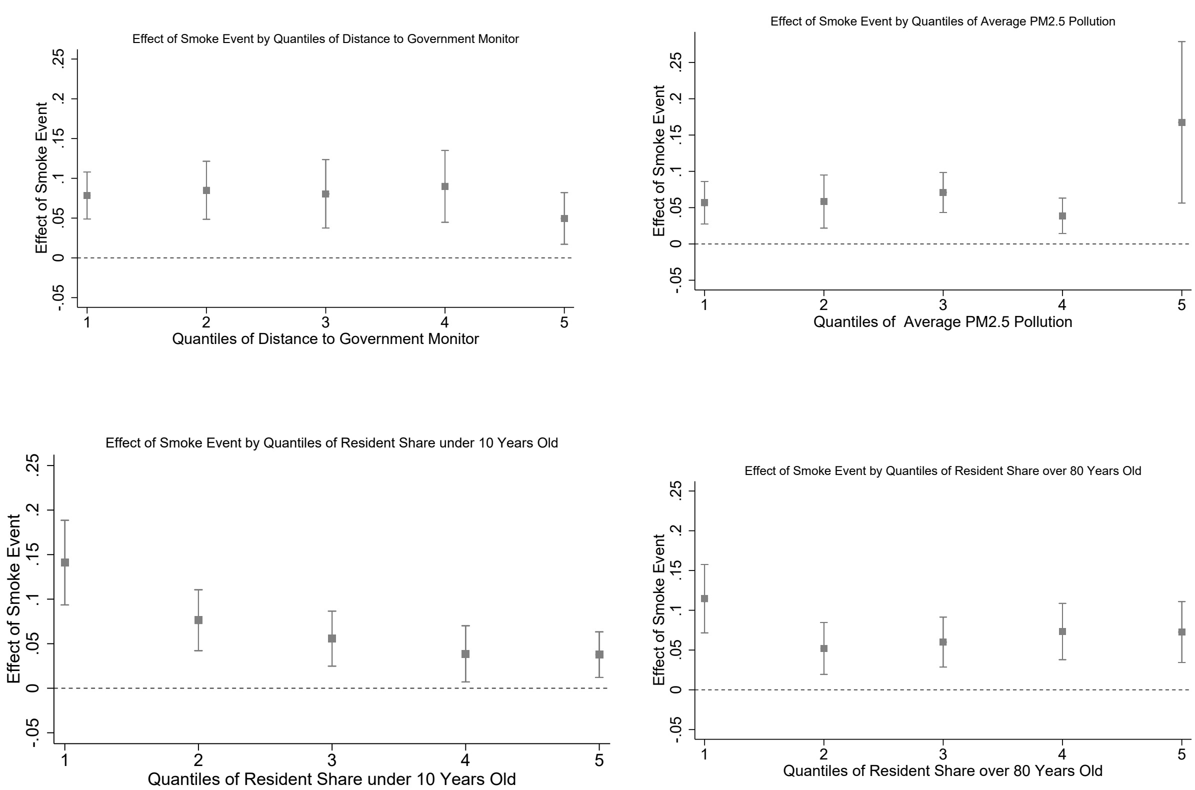
Figures

Figure 1: Event Study for the Effect of Smoke Events on Monitor Adoptions



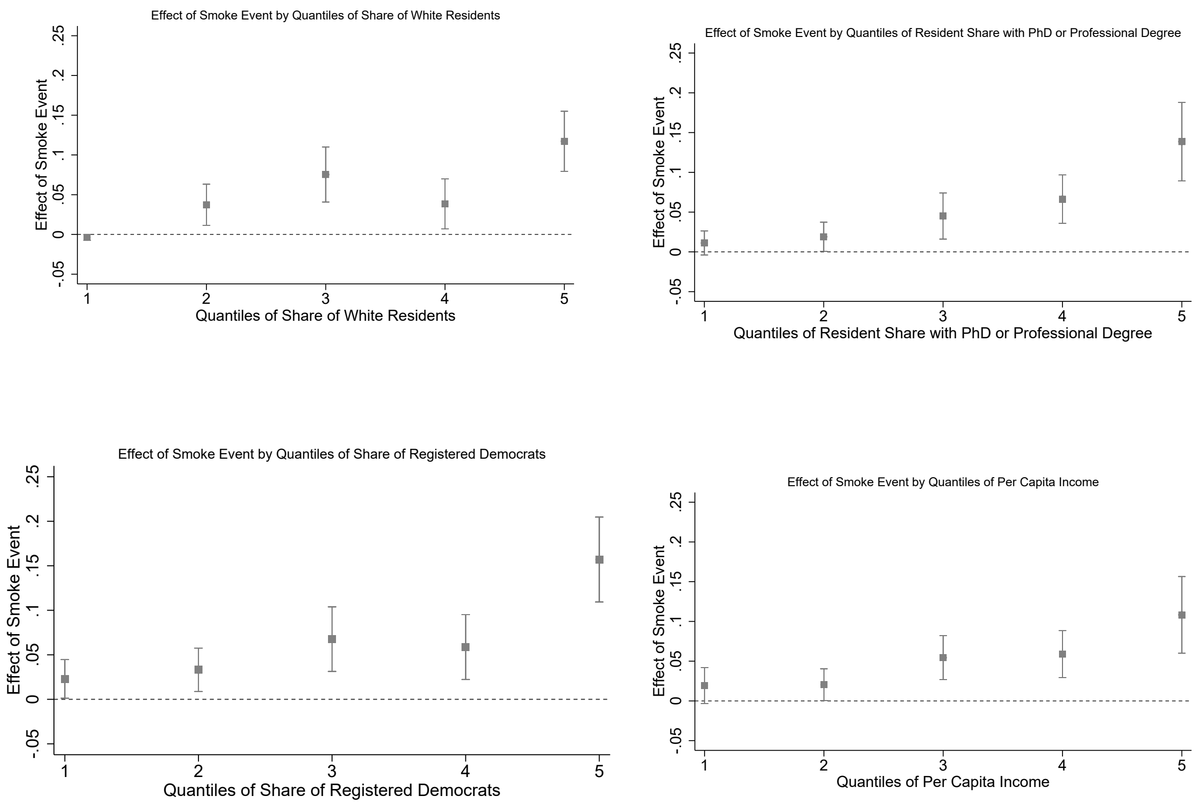
This figure plots the event study indicators for 100 $\mu g/\mathrm{m}^3$. smoke events. The outcome is the monthly count of monitors installed in a tract. Event window spans 6 months on either side of a negative air-quality shock in which the wildfire component of PM2.5 exceeds 100 $\mu g/\mathrm{m}^3$. 95% confidence intervals are shown using robust standard errors clustered at the tract level.

Figure 2: Effect of Smoke Event on Monitor Adoption by Vulnerability



Each panel of Figure 2 plots the coefficients β_1 on the treatment indicator $Smoke\_Event_{st}$ from five iterations of the fixed effects specification of Equation 2, where the sample is restricted to tracts within each quintile of the variable denoted on the horizontal axis. Top Row: Panel A defines quintiles by the tract centroid's distance from the nearest government monitor; Panel B defines quintiles by the average yearly PM2.5 reading at the nearest government monitor. Bottom Row: Panel C defines quintiles by the share of residents under 10 years of age; Panel D defines quintiles by the share of residents over 80 years of age. 95% confidence intervals are shown using robust standard errors clustered at the tract level.

Figure 3: Effect of Smoke Event on Monitor Adoption by Demographic Characteristics



Each panel of Figure 3 plots the coefficients β_1 on the treatment indicator $Smoke\_Event_{st}$ from five iterations of the fixed effects specification of Equation 2, where the sample is restricted to tracts within each quintile of the variable denoted on the horizontal axis. Top Row: Panel A defines quintiles by the share of white residents in the tract; Panel B defines quintiles by the share of residents with PhD or professional degrees. Bottom Row: Panel C defines quintiles by the share of registered Democrats; Panel D defines quintiles by per capita income. 95% confidence intervals are shown using robust standard errors clustered at the tract level.

Tables

Table 1: Summary Statistics for Tracts with and without Purple Air Monitors

	Ever Installed=0	Ever Installed =1	T-test
Share under 10 years old	12.39	10.74	13.34
	(4.51)	(4.10)	
Share over 80 years old	3.61	4.29	-7.92
	(3.04)	(3.32)	
Share Doctorate and Professional	2.42	5.06	-27.32
	(3.16)	(4.46)	
Distance Centroid to Monitor	10.92	13.49	-8.86
	(9.32)	(13.90)	
Share Democrats	0.62	0.63	-2.32
	(0.17)	(0.18)	
Mean Yearly PM2.5	11.26	10.24	13.34
	(2.75)	(2.69)	
Share Non-Hispanic White	35.21	52.55	-24.81
	(25.10)	(24.68)	
Per Capita Income	34371.18	51947.95	-29.59
	(19022.)	(28504)	
Housing Units	1690.98	2035.41	-15.11
	(780.20)	(949.84)	
Number of Installs	0	1.77	
	(0)	(1.56)	
Number of Tracts	6455	1602	

Summary statistics for tracts that ever installed Purple Air during the study period and those without Purple Air monitors. Tracts with monitor adoptions are on average slightly farther from a government monitor, have a higher Democratic vote share, are wealthier, whiter, and more urbanized than tracts without Purple Air.

Table 2: Effect of Smoke Events on Monitor Adoptions by Existing Monitors

VARIABLES	(1) Existing Installs = 0	(2) Existing Installs >0	(3) Existing Installs >2
100 ug Smoke Event = 1	0.0642***	0.131***	0.402***
	(0.00255)	(0.0170)	(0.127)
Observations	289,719	24,471	2,409
R-squared	0.062	0.142	0.218

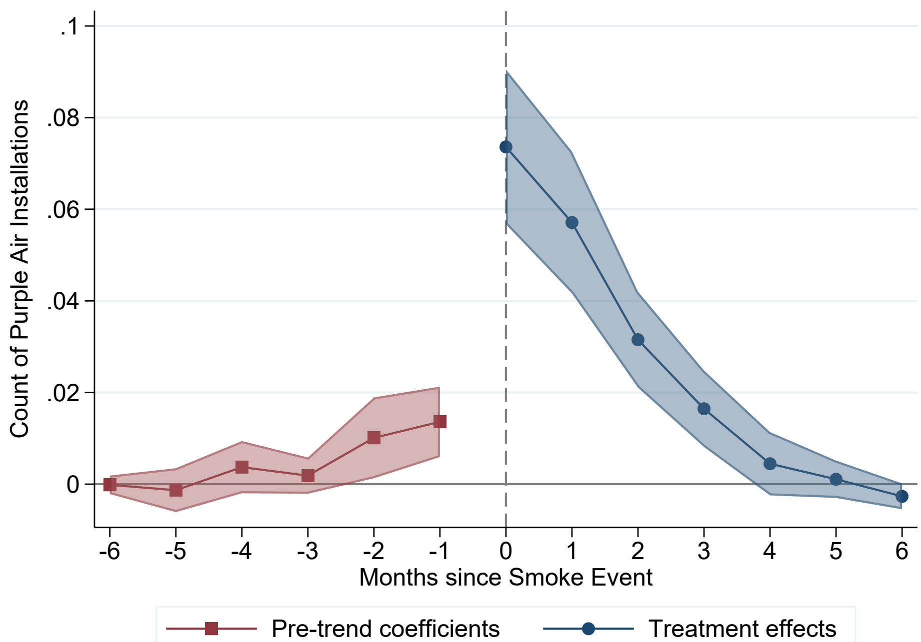
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Effect of smoke events on monthly monitor adoptions. Column 1 includes tracts with no existing Purple Air monitors. Column 2 includes tracts with at least one existing monitor. Column 3 includes tracts with more than two monitors. Specification includes tract and month-by-year fixed effects. Robust standard errors are clustered at the tract level.

Appendix

In this Appendix, we expand on the event study results of Figure 1 in the main paper, and also on the heterogeneity results presented in Figures 2 and 3. We first present the event study of Figure 1 using the imputation estimator of Borusyak et al. (2021), which relaxes the assumption of homogeneous treatment effects. The results are shown in Figure A1:

Figure A1: Event Study for the Effect of Smoke Events on Monitor Adoptions

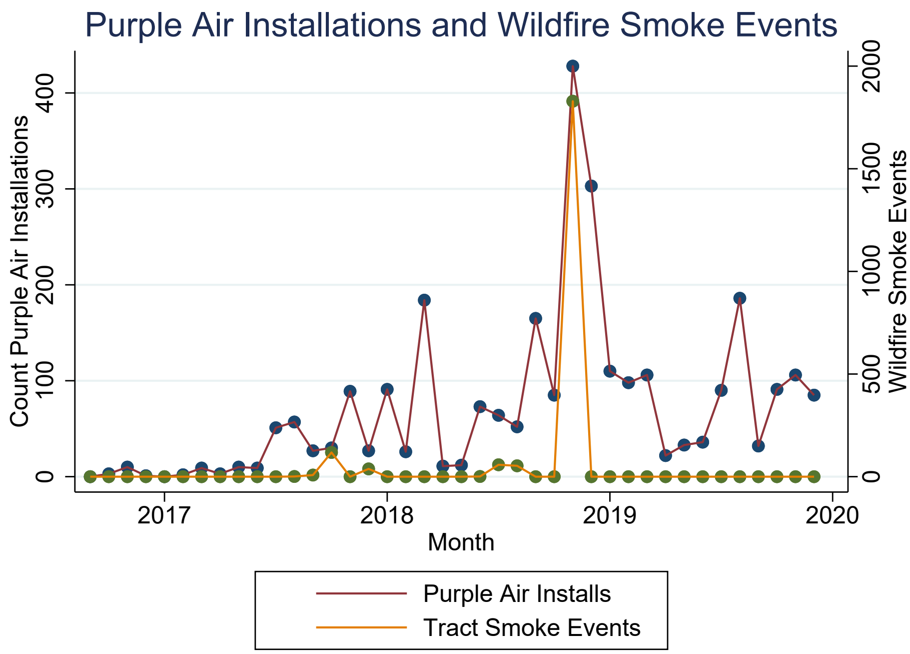


Event Study using the D-i-D imputation estimator of Borusyak et al. 2021. The outcome is the monthly count of monitors installed in a tract. The event window spans 6 months on either side of a negative air-quality shock in which the wildfire component of PM2.5 exceeds 100 $\mu g/\mathrm{m}^3$.

A smoke event in the month of occurrence increases monitor installations by 0.0736. This result is nearly identical to the effect measured in the classic event study of Figure 1 (0.0745 installations). Figure A1, however, shows a small pre-trend in monitor adoptions in the two months before a smoke event. We are interested in explaining this pre-trend and we provide evidence this pre-trend is driven by monitor takeup responding to sub-100 μg threshold events.

First, we note that at the (arbitrary) 100 μg threshold for defining a smoke event, our identification primarily comes from the Camp Fire of November 2018, as shown in Figure A2:

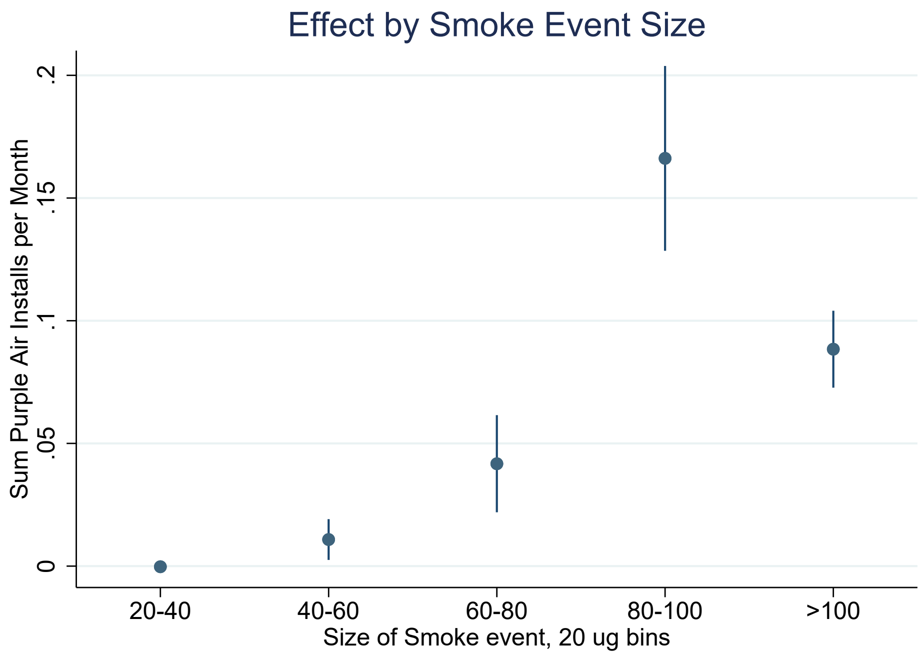
Figure A2: Counts of Purple Air Installations and Smoke Events per Month



Count of new Purple Air installations and counts of 100 μg smoke events by month during study period. The large spike corresponds to the Camp Fire of November 2018.

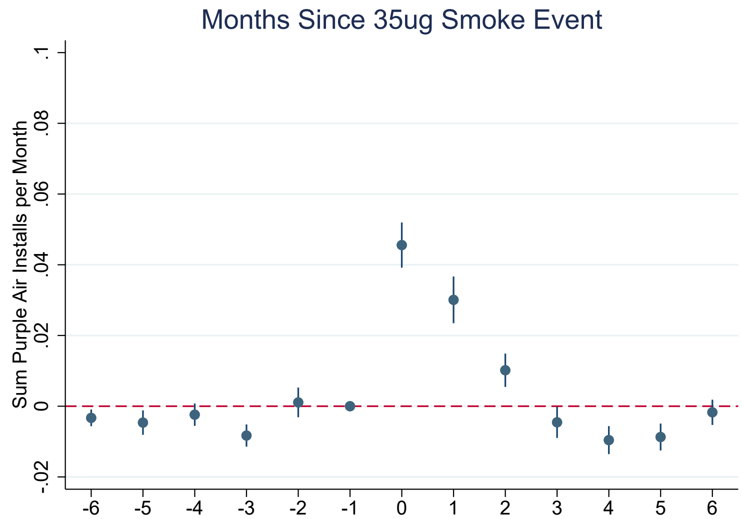
Appendix Figure A2 shows that at the 100 μg threshold for defining a smoke event, our identification primarily comes from the Camp Fire of November 2018. However, major fires such as the Mendocino Complex Fire in August-September 2018 produced widespread smoke below 100 $\mu g/\text{m}^3$. Most of these fires occurred prior to November 2018. To test the threshold at which smoke events begin to produce increases in monitor adoption, we regress the monthly count of Purple Air installations on a set of indicators for bins of smoke event sizes. Bins are 20 μg for concentrations between 0 and 100 μg, with a single bin for smoke events greater than 100 μg. The excluded group is 0-20 μg. The results, plotted in Appendix Figure A3, show that smoke events begin to have a small but statistically significant effect on monitor adoptions around 35 $\mu g/\text{m}^3$. Using this information, we define 35μg as the threshold for a smoke event. Figures A4 and A5 show the classic event study and the Borusyak et al. (2021) event study using this threshold: the pre-trend largely disappears, and the effect size diminishes in magnitude to about 30% of the result using 100 μg threshold.

Figure A3: Effect on Monitor Adoptions by Smoke Event Size



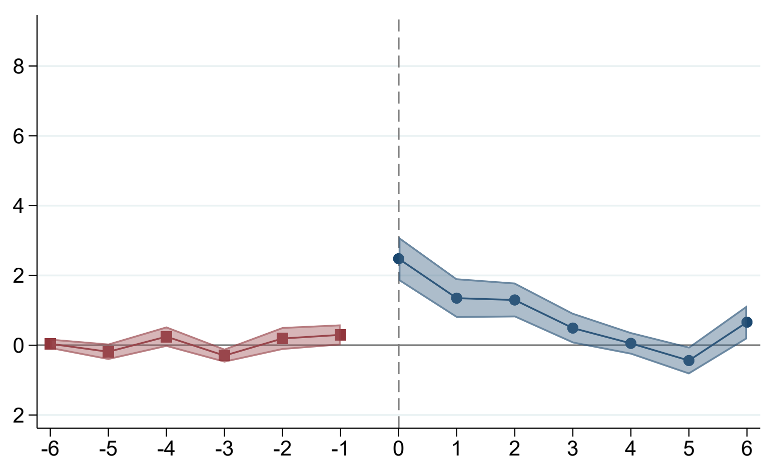
We regress the count of monthly Purple Air installations on a set of treatment indicators for 20 μg bins of smoke events. Excluded: 0-20μg. 95% confidence intervals are shown using robust standard errors clustered at the tract level.

Figure A4: Event Study for the Effect of 35ug Smoke Events on Monitor Adoptions



This figure plots the event study indicators for 35 $\mu g/\mathrm{m}^3$. smoke events. The outcome is the monthly count of monitors installed in a tract. Event window spans 6 months on either side of a negative air-quality shock in which the wildfire component of PM2.5 exceeds 100 $\mu g/\mathrm{m}^3$. 95% confidence intervals are shown using robust standard errors clustered at the tract level.

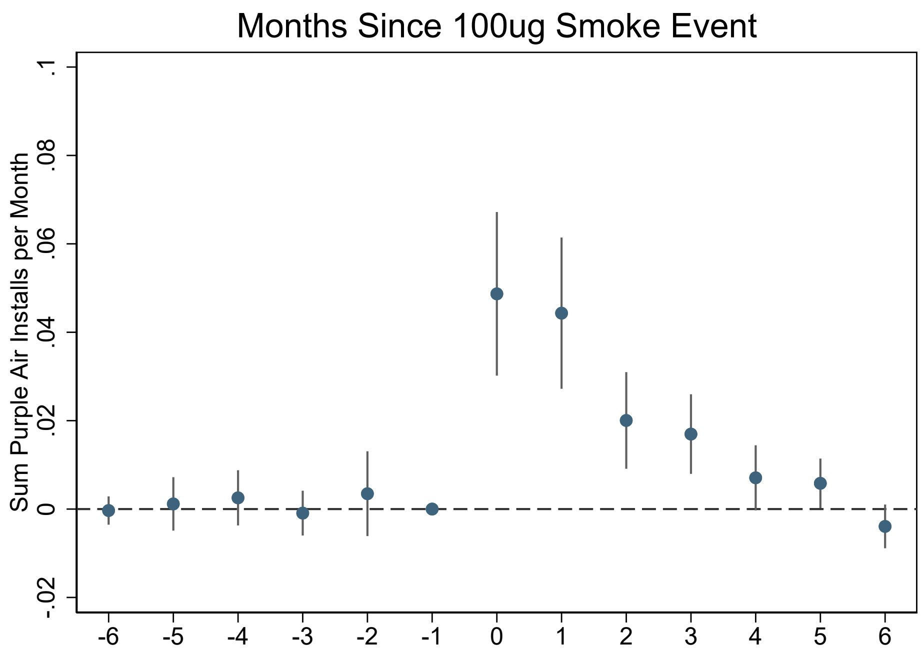
Figure A5: Event Study for the Effect of 35ug Smoke Events on Monitor Adoptions



Event Study using the D-i-D imputation estimator of Borusyak et al. 2021. The outcome is the monthly count of monitors installed in a tract. Event window spans 6 months on either side of a negative air-quality shock in which the wildfire component of PM2.5 exceeds 35 $\mu g/\mathrm{m}^3$.

We also run an event study in which we define the smoke threshold as 100μg, but control for a full set of event study indicators corresponding to events of size 35μg or larger. The coefficients on the 100 μg event study indicators are shown in Figure A6. Again, the pretrend disappears, and the effect size diminishes only slightly from the 100 μg event study without controls for sub-threshold events. On this basis we conclude the pretrend in Figure A1 is most likely a true monitor adoption response to events below the 100μg threshold.

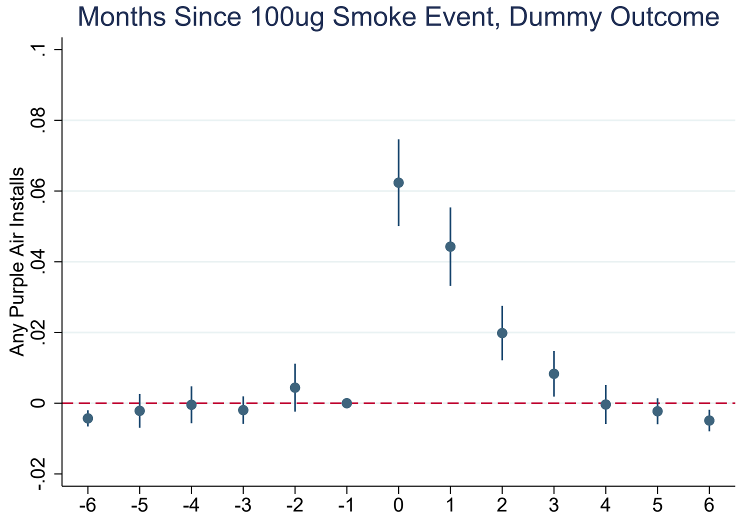
Figure A6: Event Study for the Effect of 100ug Smoke Events on Monitor Adoptions, Controlling for 35ug Events



This figure plots the event study indicators for 100 $\mu g/\mathrm{m}^3$ smoke events, controlling for a second set of indicators for 35 $\mu g/\mathrm{m}^3$. smoke events. The outcome is the monthly count of monitors installed in a tract. Event window spans 6 months on either side of a negative air-quality shock in which the wildfire component of PM2.5 exceeds 100 $\mu g/\mathrm{m}^3$. 95% confidence intervals are shown using robust standard errors clustered at the tract level.

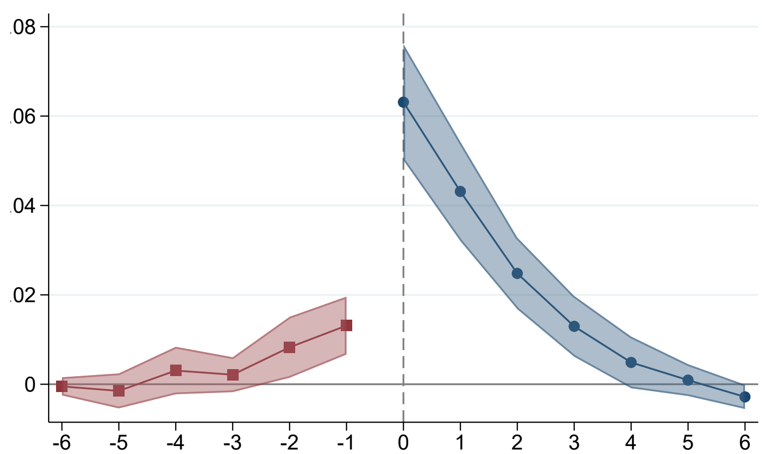
Finally, we conduct an event study in which we define the smoke threshold as 100μg, but we define the outcome variable as a dummy equal to 1 if there is any installation in tract s in month t, and zero otherwise. We do this to explore the effect of potential outliers, since the count of installations in a given tract-month ranges from zero to 14. The classic event study and the Borusyak et al. (2021) event study using this outcome definition are shown in Figures A7 and A8; results are slightly smaller in magnitude as expected but are quite similar to the event study of Figure 1. The effect in the first period after a smoke event is 0.074 using the count of installations as the outcome variable and 0.063 using the dummy.

Figure A7: Event Study, Outcome Variable is Any Installations



This figure plots the event study indicators for 100 $\mu g/\mathrm{m}^3$. smoke events. The outcome is a dummy variable equal to 1 if any installations occur in a tract-month. Event window spans 6 months on either side of a negative air-quality shock in which the wildfire component of PM2.5 exceeds 100 $\mu g/\mathrm{m}^3$. 95% confidence intervals are shown using robust standard errors clustered at the tract level.

Figure A8: Event Study, Outcome Variable is Any Installations



Event Study using the D-i-D imputation estimator of Borusyak et al. 2021. The outcome is a dummy variable equal to 1 if any Purple Air monitors are installed in a tract-month. Event window spans 6 months on either side of a negative air-quality shock in which the wildfire component of PM2.5 exceeds 100 $\mu g/\mathrm{m}^3$.

We expand on Figures 2 and 3 of the main paper to further investigate the private monitor adoption response by neighborhood characteristics. In Figure 2, we show that the size of the adoption response is uncorrelated with distance to the nearest government monitor, average PM2.5 reading, or share of residents over 80 years old, and is negatively correlated with the share of residents under the age of 10.

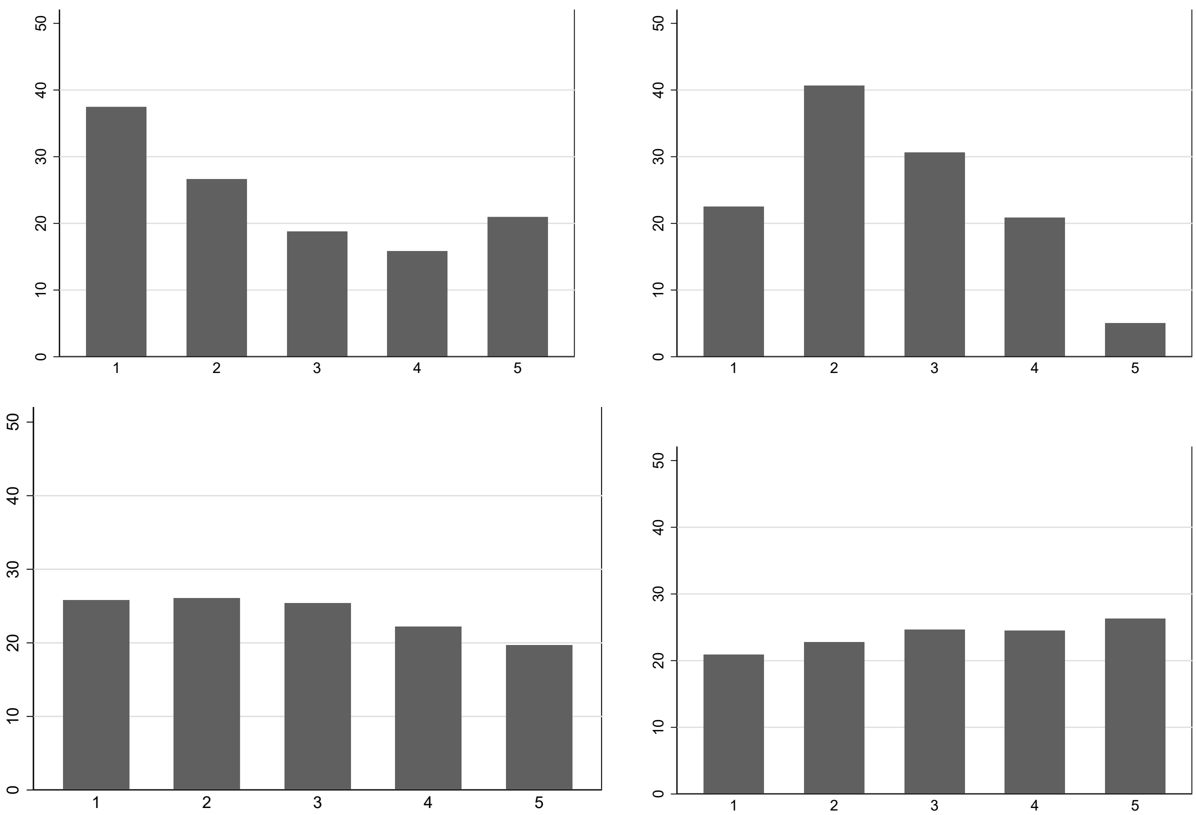
One notable feature of Figure 2 is in Panel B, where we regress the number of new Purple Air installations on smoke events with the sample restricted by quintiles of average annual PM2.5 reading. The estimate of the effect of smoke events in the highest quintile of pollution is both large and poorly estimated. Seeking to understand this result, in Appendix Figure A9 we first plot the share of ever-treated tracts across each quintile for each of the variables of interest in Figures 2 and 3. We find that for each set of quintiles, the share of ever-treated tracts ranges between 10% and 40% of total tracts. It is also relatively consistent across quintiles. The exception is the highest quintile for PM2.5 pollution, which has an ever-treated share below 5%, leading to the imprecise estimate in Figure 2 Panel B.

In Appendix Table A1 we continue to explore different definitions of high-pollution locations by splitting the sample by above and below median pollution levels at the nearest government monitor, and by Clean Air Act attainment vs. non-attainment counties. There is no difference in effect size when splitting the sample by above vs. below median air pollution. The effect is concentrated in non-attainment counties; however, nearly all California counties are in nonattainment status for some measure of air pollution, so this sample contains the vast majority of observations. We then estimate the interaction terms following equation:

$$Y_{st} = \alpha + \beta_1 Smoke\_Event_{st} + \beta_2 Smoke\_Event_{st} * Heterogenity + \delta_s + \gamma_t + \epsilon_{st} \qquad (3)$$

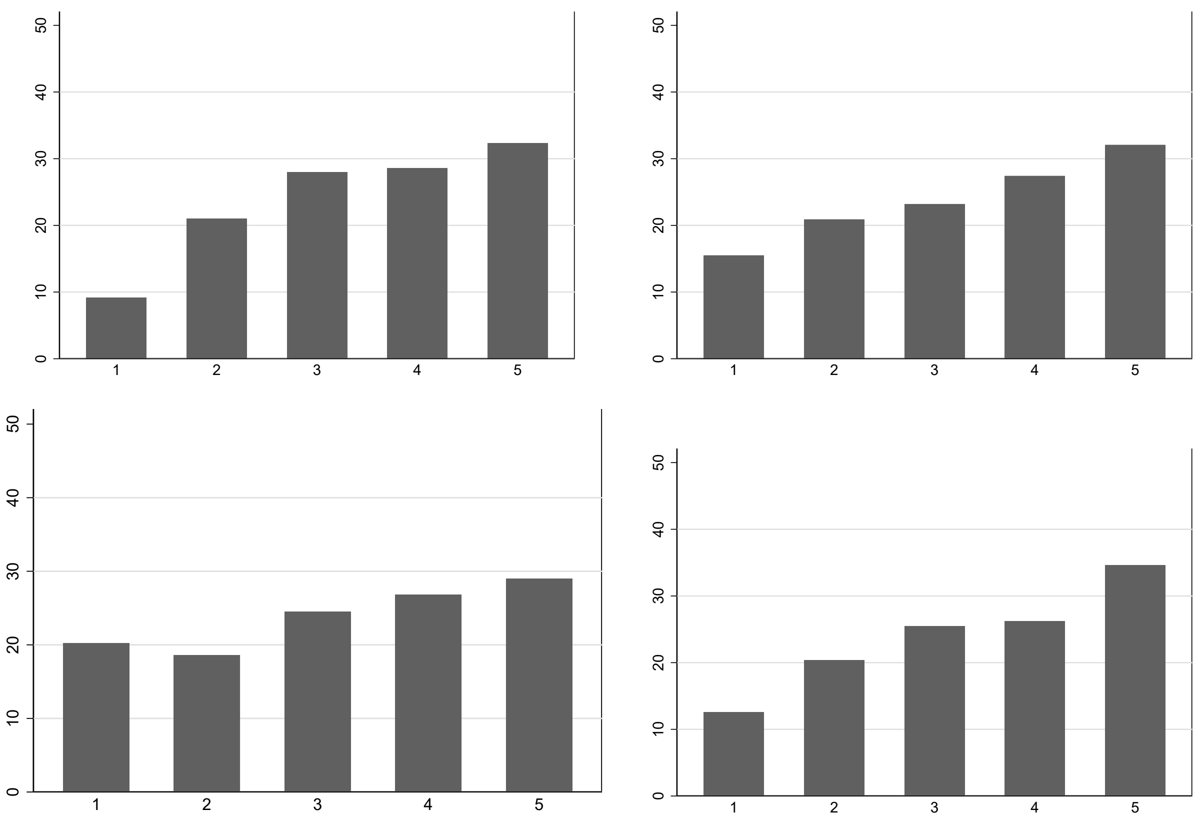
Where *heterogeneity* is one of the eight variables in Figures 2 and 3. The results are shown in Table A2. There is a positive and significant relationship between share of highly educated residents, share of registered Democrats, share of white residents, and per capita income in a tract and the size of the adoption response. There is no significant relationship between the adoption response and distance to monitor, average PM2.5, or share of residents over 80 years old. The interaction term on share of residents under 10 is negative. On this basis, we conclude that private monitor adoptions are unlikely to reduce inequalities in pollution exposure.

Figure A9: Share of Ever-Treated Tracts by Quintiles of Pollution Exposure



Each panel plots the share of ever-treated tracts within each quintile of the variable of interest. Clockwise from top left: quintiles of distance to monitor, PM2.5, Share of residents under 10 years old, Share of residents over 80 years old.

Figure A10: Share of Ever-Treated Tracts by Quintiles of Demographic Characteristics



Each panel plots the share of ever-treated tracts within each quintile of the variable of interest. Clockwise from top left: Quintiles of share of white residents, share of residents with a PhD or professional educated, share of registered Democrats, per capita income.

Table A1: Effect of Smoke Events on Monitor Adoptions by Pollution Status

VARIABLES	(1) Above Median PM2.5 Count Installs	(2) Below Median PM2.5 Count Installs	(3) Non-Attainment Counties Count Installs	(4) Attainment Counties Count Installs
Smoke Event = 1	0.0655***	0.0648***	0.100***	-0.0152
	(0.0123)	(0.0110)	(0.0105)	(0.0119)
Observations	157,521	156,702	285,090	29,133
R-squared	0.046	0.066	0.060	0.051

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Effect of smoke events on monthly monitor adoptions with the sample split by long term air pollution status. Columns 1 and 2 split the sample into tracts with above and below median yearly average PM2.5 readings. Columns 3 and 4 split the sample by Clean Air Act non-attainment status. Robust standard errors are clustered at the tract level.

Table A2: Effect of Smoke Events on Monitor Adoptions with Interactions

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Count Installs				
Smoke Event = 1	0.199***	0.0744***	-0.0229**	0.0796***	-0.0937***	0.118***	-0.0194	-0.0903***
	(0.0268)	(0.0110)	(0.0109)	(0.0121)	(0.0261)	(0.0377)	(0.0138)	(0.0200)
Smoke Event = 1 ×								
Share under 10 years old	-0.0108***							
	(0.00190)							
Share over 80 years old		-6.95e-05						
		(0.00184)						
Share Doctorate and Professional			0.0272***					
			(0.00367)					
Distance Centroid to Monitor				-0.000562				
				(0.000775)				
Share Democrats					0.266***			
					(0.0460)			
Mean Yearly PM2.5						-0.00435		
						(0.00365)		
Share Non-Hispanic White							0.00191***	
							(0.000316)	
Per Capita Income								0.00377***
								-5.39E-04
Observations	312,468	312,468	312,468	314,223	312,663	314,223	312,468	312,234
R-squared	0.059	0.058	0.063	0.058	0.059	0.058	0.059	0.062

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Each column reports the coefficients from a regression of the number of new monthly Purple Air installations on an indicator for 100 μg smoke event, an interaction term between the smoke event indicator and the heterogeneity variable of interest from Figures 2 and 3, and the fixed effects. There is no statistically significant effect on the interaction term for average yearly PM2.5, proximity to government monitor, or share of residents above 80 years old; monitor adoptions are increasing in per capita income, share of highly educated residents, share white, and share Democrat. Robust standard errors are clustered at the tract level.

In Table A3, we follow Gallagher (2014) and investigate the role of TV media coverage in mediating the adoption response. In column 1, we show that the existence of smoke anywhere in the media market is associated with an increase in monitor adoptions. The effect size is less than 10% of the magnitude of our main estimate of the direct effect of smoke in a tract at 0.07 installations per smoke event. In column 2, we include separate indicators for a tract that directly experiences a smoke event, and an indicator for a tract in a media market with smoke but did not itself experience a smoke event ("media neighbors" in the parlance of Gallagher). It is clear that the direct effect of smoke is what is driving the results- TV media coverage does not seem to play a role in increasing monitor adoptions.

Table A3: Effect of Media Market Smoke Events on Monitor Adoptions

VARIABLES	(1) Count Installs	(2) Count Installs
Smoke in Media Market =1	0.00430***	
	(0.00163)	
Smoke in Tract = 1		0.0725***
		(0.00815)
Smoke in Media Market =1, Smoke in Tract = 0		-0.00197
		(0.00163)
Observations	314,223	314,223
R-squared	0.056	0.058

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Following Gallagher (2014), we define a media market neighbor as a census tract that is within the same media market as a tract experiencing a 100 μg smoke event but did not itself experience a smoke event. Column 1 regresses the number of Purple Air installations on an indicator for the presence of 100 μg smoke event anywhere in the TV media market; Column 2 includes separate indicators for a tract experiencing a 100 μg smoke event and media neighbors. The effect of smoke on adoptions is a direct effect; media neighbors do not show increases in adoptions. Robust standard errors are clustered at the tract level.