Essays on the Economics of Pollution Sensors and Adaptation

Dissertation

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Alexander Janosch Dangel
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Introduction

Do you see particles in the air?
Unguided particles in the air
Do you see particles in the air?
Nobody notices, nobody cares, oh

Andrew Bird, "Puma"
Are You Serious

Motivation

Globally, air pollution is the single greatest environmental health risk, and, as such, its social costs are enormous. The World Health Organization (2016) estimates that annually over seven million people die prematurely from air pollution worldwide and the World Bank values its damages in excess of 6% of global economic output ($8.1 trillion; World Bank, 2022). To reduce its damages, governments regulate air pollution by collecting information about ambient air quality (AQ) with sparse networks of ground-based stationary monitors. Since the latter half of the twentieth century, government AQ monitoring and enforcement policies in high-income countries have led to substantial reductions in ambient pollution concentrations (Aldy et al., 2022; Fowler et al., 2020). In addition to using this AQ information to enforce pollution regulations, governments disclose it directly to researchers for scientific applications and to citizens to promote pollution awareness and, when necessary, to motivate them to take protective actions. A growing body of evidence shows that government AQ information disclosure programs meaningfully impact citizens' exposure perceptions (Oberholzer-Gee and Mitsunari, 2006), certain pollution-related behaviors (Fan, 2024; Gao et al., 2023), and health outcomes (Jha and La Nauze, 2022; Neidell, 2009). Moreover, mounting evidence demonstrates that the net benefits of disclosure far exceed its costs (Barwick et al., 2024). However, three insights indicate that contemporary government AQ information disclosure is suboptimal.

First, new evidence reveals limitations to making policies with AQ information collected and disclosed by governments. Recent studies document substantial errors in regulating air pollution with government monitor data (Fowlie et al., 2019;

Sullivan and Krupnick, 2018), strategic polluter and regulator circumvention of monitoring protocols (Zou, 2021; Mu et al., 2021), and biases in government monitor locations (Muller and Ruud, 2018; Grainger and Schreiber, 2019a). When authorities couple it with appeals for pollution avoidance or mitigation, AQ information can be ineffective or even have adverse effects (Tribby et al., 2013; Sexton, 2012; Sexton Ward and Beatty, 2016). Disparities in exposure also continue to persist despite overall effective policies (Currie et al., 2023; Colmer et al., 2020). Moreover, researchers link recent backsliding on decades of pollution improvements to waning regulatory effectiveness and new pollution risks in the face of global climate change (Clay et al., 2021).

Second, government monitoring networks are not designed to capture variation in air pollution in a way that matters most for individual decisions. In part, this shortcoming is linked to the prohibitive cost of installing sufficient densities of regulatory monitors for capturing ambient air pollution's high degree of spatial variability. Even in countries with widespread government monitoring, many cities do not have a government monitor and, even if they have one, one measurement location is certainly not sufficient to capture differences in pollution within and between neighborhoods. More generally this can be described under the umbrella of the "ecological fallacy," whereby statistical relationships at aggregate levels mask individual-level relationships (Banzhaf et al., 2019).[1] If regulators do install monitors to measure population-level exposure, where regulators decide to site them relative to people and pollution sources can induce measurement error in exposure, which may be particularly pernicious for pollution assessment if it systematically relates to actual pollution levels (Fowlie et al., 2019; Sullivan and Krupnick, 2018; Grainger and Schreiber, 2019b).[2]

Third, the advent of commercially-available low-cost pollution sensors in the 2010s (Snyder et al., 2013) demonstrates widespread demand for more comprehen-

[1] A simple stylized example: if half of a population is exposed to maximum pollution and the other half of the population is exposed to no pollution, observing average population-level exposure would lead one to believe there are no individual level differences in pollution, even though they could not be more different between the two groups.

[2] There are two main types of measurement error: i) classical measurement error when measurement error is uncorrelated with the value of the measured parameter, and ii) non-classical measurement error when measurement error is correlated with the value of the measured parameter.

sive AQ information.[3] These sensors enable citizens to collect and share accurate AQ information independently from governments, and private installations of stationary AQ sensors (SAQS) already outnumber government AQ monitors in most countries. When integrated into non-regulatory networks, SAQS provide more spatially-resolved AQ information than official government networks, in particular in population-dense urban areas. More recently, manufacturers have brought consumer wearable AQ sensors (WAQS) to market, which enable adopters to measure personal pollution exposure wherever they carry their devices. Although the information collected with SAQS and WAQS technologies is not used to enforce environmental regulations, it appears well suited to reduce measurement error and to influence pollution perceptions, inform individual decisions, shape policymaking, and be employed in scientific applications.

In the context of these developments, I empirically study the economics of pollution sensors and adaptation. Previous to this dissertation, sensors had not been rigorously studied from an economic standpoint. Yet, their emergence against the backdrop of (suboptimal) government AQ monitoring and information disclosure raises fundamental questions about their economic value. Who naturally demands these technologies? How much do they value them? How does their real world implementation relate to government AQ information provision? In light of well-documented disparities in pollution exposure (Currie et al., 2023; Colmer et al., 2020; Ehler et al., 2024), my objective is to answer these questions and also evaluate distributional aspects embedded within each of them to understand whether sensors are counteracting environmental injustices or reinforcing them. This dissertation extends to a second main topic: pollution adaption. Purchasing and using sensors is in and of itself a behavioral response to pollution, and I aim to understand additional adaptations: do people protect themselves from pollution? Do their actions differ when they have personal exposure information from sensors? Do prospective sensor adopters respond to spatially-proximate pollution? Can individual pollution mitigation be influenced by highly-relevant pollution information coupled with moral appeals to reduce pollution?

Tackling these questions sheds light on key properties of air quality as a non-

[3]The United States Environmental Protection Agency (EPA) defines a low-cost sensor as priced below $500, but some sensors retail for as little as $15.

market economic good.[4] The air we breathe is a fascinating object of economic inquiry. Introductory economics courses often employ it as a textbook example of a public good, whose consumption can neither be restricted nor prevents another person's consumption. A more careful consideration shows cracks in this classification. First, when polluters "consume" clean air, they exert negative externalities on others, meaning that one person's consumption can hinder another person's enjoyment of it. Second, people prefer to live where the air is cleaner (Banzhaf and Walsh, 2008). This means that air quality is priced into other goods and implies that higher income can grant access to cleaner air. Evidence from this dissertation supplies new evidence that the public goods nature of air quality may be further eroding.

Pollution sensors also have fascinating economic properties because they provide adopters with novel AQ information about true exposure levels that can be shared to benefit others. One of the largest non-regulatory SAQS networks worldwide, Sensor.Community, is a citizen-led initiative that exemplifies the private and public dimensions of sensors on multiple levels. This network first emerged in Germany in the mid 2010s at a time when citizen concern about urban air pollution reached a recent climax. The volunteer organization set out to provide better AQ information through the deployment of inexpensive SAQS that automatically transmit pollution information to publicly-accessible real-time pollution maps and data archives. While the collected information holds no regulatory or legal weight, it perfectly exemplifies informal community monitoring efforts that demand for better environmental quality (Shimshack, 2014; Gray and Shimshack, 2011). Moreover, it is not surprising that this non-regulatory network and other similar ones originated in high-income countries where the demand for environmental quality is presumably highest (Greenstone and Jack, 2015). Adopting a sensor to collect and share AQ information publicly with others represents a private provision of a local impure public good (Bergstrom et al., 1986). The information produced by SAQS can benefit the adopter themselves directly, but its public disclosure may also improve knowledge of pollution levels, sources, and trends for individuals who do not directly participate in the network. The pro-social act of installing and maintaining a SAQS is particularly interesting considering that it

[4] This means that air is not directly bought or sold on the market.

may substitute for government AQ monitoring some in settings and complement it in others. It may also crowd out the contributions of other prospective adopters if they decide to free-ride on the public SAQS network data rather than install their own SAQS. My dissertation provides the first empirical evidence for this phenomenon.

Adaptation to pollution is a response to its enormous social and individual costs. Government policies aimed at correcting this market failure have been remarkably effective in reducing damages from long-term pollution exposure (Aldy et al., 2022). However, even in settings with relatively low pollution levels, costs remain substantial (Deryugina et al., 2019) and unequally distributed (Currie et al., 2023). Individuals mainly bear them in terms of mortality, morbidity, and productivity loss from exposure (Hanna and Oliva, 2015; Chang et al., 2016), but private adaptation also comes at a cost (Aguilar-Gomez et al., 2022). For example, people may choose to purchase an indoor air filter or other defensive products to reduce exposure (Ito and Zhang, 2020; Ahmad et al., 2022) or respond to heightened ambient pollution levels by forgoing other economically-meaningful activities to stay indoors (Fan, 2024; Janke, 2014; Neidell, 2009). Here, governments attempt to guide individual adaptations by providing recommendations for when and how to reduce pollution exposure on poor AQ days.[5] Our current understanding of pollution concentration-response functions suggest that adaptation is not necessarily beneficial for everyone. For this reason, recommendations are often targeted at sensitive population groups (i.e. children, elderly, and individuals with pre-existing health conditions). Coupling these pollution information with certain messaging strategies (Ito et al., 2018; Ferraro et al., 2011) can tap into different motivations to effectively improve decision-making (Gneezy et al., 2011). Parts of my dissertation show that intertemporal substitution,[6] social norms, and other factors are also of paramount importance for influencing these behaviors.

I use state-of-the-art field experimental, quasi-experimental, and spatial methods from economics to evaluate my dissertation's research questions. In particular, my research relies heavily on methods developed for understanding natural eco-

[5]Recommended behaviors include actions such as staying indoors, rescheduling or delaying commutes, refraining from outdoor exercise, and wearing pollution masks.

[6]This refers to the shifting of activities between time periods.

nomic relationships using experimental variation (Levitt and List, 2009). I use price randomization and willingness-to-pay elicitation methods to measure WAQS demand and valuations in the real world (Berry et al., 2020; Cole et al., 2020). To identify causal relationships of interest, I use instrumental variables regression (Angrist and Pischke, 2009), difference-in-differences event studies, and spatiotemporal models of technology diffusion (Rode and Weber, 2016; Graziano and Gillingham, 2014).

The results of my analyses make important contributions to literature on environmental information in economics (Greenstone et al., 2022; Neidell, 2009), but also extend to more general economics discussions on the role of information (Stigler, 1961; Loewenstein et al., 2014; Golman et al., 2017, 2022), the private provision of public goods (Bergstrom et al., 1986; Andreoni, 1990), and the determinants of technology adoption (Griliches, 1957; Bass, 1969). In addition, the interdisciplinary nature of my research can contribute to conversations in political science on citizen participation in environmental governance (Anderson et al., 2019), in social psychology on determinants of pro-environmental behavior (Steg and Vlek, 2009), in human geography on participatory sensing and citizen science (Goodchild, 2007), and in exposure sciences on pollution risks (Lim et al., 2022; Boomhower et al., 2022).

Synopsis

My dissertation consists of four distinct chapters. The first essay constructs and analyzes a dataset that links personal pollution exposure data to ambient monitor data. To do so, I rely on millions of personal exposure readings collected by WAQS consumers who independently measure pollution with their sensors. I find evidence of a gap in $PM_{2.5}$ measurements between these two monitoring approaches. Personal exposure is considerably lower than monitor data suggests, but monitors fail to capture many instances of high personal exposure. Moreover, my analysis reveals that this measurement error correlates with pollution levels, location, and temporal factors. This chapter breaks new ground in economics on how economists measure pollution and also advances the state-of-the-art in epidemiology and other exposure sciences. The dataset I use for this analysis come from WAQS deployed in the field experiment conducted in my second essay.

The second essay conducts a field experiment in the United States on commercially-available WAQS. We partner with a leading WAQS manufacturer to study demand, use, and impacts of this novel monitoring technology among its real-world consumers. We show that demand is low at current market prices, and those who are interested in and purchase sensors come primarily from socioeconomically-advantaged groups. We use naturally occurring pollution variation to help explain WAQS adoptions and user activity trends. Our results indicate that adopters believe they are exposed to less pollution and substitute away from government AQ information but do not change the frequency of their defensive actions. Our results provide guidance on WAQS deployment and have important implications for pollution monitoring and environmental inequality.

The third chapter studies SAQS adoptions in Germany using data from the non-regulatory Sensor.Community network. Our analysis finds considerable disparities in SAQS adoption rates between municipalities and neighborhoods and identifies income and green political preferences as two primary adoption determinants. We also find that SAQS are installed more often near government monitors in Germany, but monitor non-compliance with AQ standards does not appear to drive additional adoptions. In line with its unique local public goods properties, we demonstrate that SAQS installations may discourage subsequent adoptions nearby.

The fourth chapter studies temporal factors influencing the effectiveness of a don't drive appeal (DDA) encouraging motorists in Stuttgart, Germany to voluntarily reduce driving during transitory high pollution episodes. We use a difference-in-difference event study approach to compare traffic data in Stuttgart with nearby Munich and show that DDAs at most lead to an average 1% reduction in overall traffic. However, treatment effects vary along three temporal dimensions. DDAs are most effective at the their onset, again at the tail end of DDA episodes, and following lengthy recovery periods between DDAs. A theoretical model highlights the importance of social norms and intertemporal substitution for these results.

Research Frontiers

This dissertation advances the state of knowledge in economics on pollution sensors and adaptation. It also leads naturally to extensions of my research at the discipline's frontiers. My results suggest that economists must think more carefully about measurement error in the context of airborne pollution exposure and develop methods to recalibrate damage estimates. WAQS provide a unique opportunity to improve the quality of future exposure assessment, but their natural deployment is still hampered by their high cost and disparities in interest and adoption. As future sensor iterations become more affordable, it will be interesting to see how exactly demand grows. Future research must also do more to understand the specific channels connecting exposure, adaptation, and individual benefits. My research demonstrates that adopters believe they benefit considerably from purchasing and using sensors, so identifying opportunities to lower access barriers and incentivize use will be worthwhile.

Furthermore, there may be important future applications for sensors. For example, my research raises questions about the long-term equilibrium effects of sensors on government monitoring. Will private non-regulatory networks crowd out the future installation of government monitors, or will governments step in to provide better information using sensors in a more coordinated fashion? There are recent instances of policy-makers installing SAQS to build hyper-local AQ monitoring networks, but our understanding of the factors that determine these municipal efforts is still in its infancy. Developing methods to employ WAQS for community pollution monitoring is another promising avenue forward. While WAQS track idiosyncratic exposure, applying state-of-the-art statistical models to data from networks of WAQS adopters could tease out background pollution exposure, which may be valuable information for adopters and non-adopters alike.

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Mind the $PM_{2.5}$ Gap! Comparing Exposure Data from Wearable Sensors and Ambient Monitors

Mind the $PM_{2.5}$ Gap! Comparing Exposure Data from Wearable Sensors and Ambient Monitors

Alexander Dangel

Abstract

Biases in pollution exposure estimates from conventional data sources could threaten the validity of existing health impact assessments and corresponding monetary damage estimates. To shed light on this, I construct a novel $PM_{2.5}$ pollution dataset using over 45 million personal exposure readings collected by 594 consumer adopters of wearable air quality sensors in the United States. I then test for the existence and nature of a $PM_{2.5}$ exposure gap between personal measurements and commonly-employed secondary data from ambient monitors. On average, personal exposure is between 7% and 18% less than monitor data suggests, while median differences correspond to nearly 40% less pollution. Moreover, my analysis reveals correlations between this $PM_{2.5}$ gap and pollution levels, location, and temporal factors. Accounting for these discrepancies in the future would recalibrate existing damage functions and previous estimates of environmental policy benefits.

Keywords: Pollution, Exposure, Damages, Concentration-Response Functions, $PM_{2.5}$ Gap, Measurement Error

JEL Classification: D62, D80, Q50, Q53

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1.1 Introduction

Human exposure to airborne fine particulates ($PM_{2.5}$) is a major global public health risk which leads to excess mortality, morbidity, and productivity loss. In the United States, total damages from $PM_{2.5}$ pollution exceed 4% of annual gross domestic product ($790 billion in damages in 2014, Tschofen et al., 2019). Exposure is linked to an estimated 110,000 - 310,000 premature deaths in the US annually (Chan et al., 2023) and significant racial disparities in risk burdens (Currie et al., 2023). Effectively designing and evaluating policies to improve $PM_{2.5}$-related outcomes requires an accurate assessment of exposure and its damages. However, existing estimates suffer from gaps in measurement coverage due to a widespread reliance on ambient pollution data from an insufficient number of non-randomly sited regulatory monitors (henceforth monitors). Biases and uncertainties in these secondary data can skew health impact assessments and compromise their usefulness for environmental policy-makers. The recent emergence of consumer wearable air quality sensors (WAQS) has generated an abundance of new personal $PM_{2.5}$ readings collected by individuals who purchase and carry WAQS with them to track pollution exposure in real-time wherever they go. From an economic standpoint, such improvements in exposure estimates could lead to substantial welfare benefits for the public by advancing the state of knowledge on concentration-response functions (CRFs) and updating other parameters guiding environmental policies. Until now, this new personal pollution monitoring technology, the advantages of its exposure estimates, and its overall implications for scientific and policy applications have been poorly understood.

In this paper, I test empirically for the existence and magnitude of a "personal-ambient $PM_{2.5}$ exposure gap" (henceforth $PM_{2.5}$ gap) and identify intrapersonal and interpersonal factors which affect it. My analysis uses $PM_{2.5}$ readings collected by consumer WAQS adopters from a companion field experiment conducted in the United States (Dangel and Goeschl, 2024a). I begin by distinguishing two exposure assessment approaches that form the basis of my analysis: i) "personal" exposure monitoring conducted independently by consumer WAQS adopters and ii) "ambient" exposure assignment, which researchers use to link individuals to outdoor pollution concentrations measured at or modeled based on the monitor closest to their residence. The latter is a widely accepted approach in epidemiological exposure assessment and social science research (Evangelopoulos et al., 2020; Cain et al., 2024).

This new type of personal data represents a paradigm shift in exposure assessment for three main reasons. First, current exposure assessments typically rely on ambient exposure assignment, but in reality individuals move and adapt to exposure in ways that neither a single monitor nor existing monitoring networks could capture. WAQS technologies overcome this by accurately measuring pollution exposure at the personal level, where it is most relevant to health outcomes, including indoors and in other previously unmonitored micro-environments. Second, involving consumers in the provision of exposure data adds socioeconomic and behavioral dimensions to data collection. As documented in a companion paper (Dangel and Goeschl, 2024a), current WAQS consumers are not a representative group of individuals. They belong to higher socioeconomic groups and are predominantly White and male. Moreover, consumers naturally use their WAQS intensively for several weeks after adoption, but data collection thereafter is varied and correlated with transitory ambient pollution episodes. Third, our current understanding of pollution risks (e.g. CRFs, air quality standards, and regulations) and how these risks are communicated to the public (e.g. air quality indices, alert programs) are based on ambient metrics which may not transfer directly to high-frequency personal WAQS data. Personal WAQS data will presumably enable a reassessment of pollution risks and CRFs, but they may also require a recalibration of pollution-related standards and information strategies.

My analysis empirically constructs two competing exposure assessment approaches. First, I collect personal exposure data from 658 WAQS users who participate in a consumer field experiment from November 2022 through September 2024 (Dangel and Goeschl, 2024a). Second, I assign ambient exposure concentrations to these 594 of these individuals by matching them to the closest continuous $PM_{2.5}$ monitor in the Air Quality System (AQS) database from the US Environmental Protection Agency (EPA). I hold this monitor constant for each individual throughout the analysis. For each individual, I then calculate differences in concurrent $PM_{2.5}$ estimates at the hourly and daily level. In a first step, I characterize the nature and scope of the intrapersonal $PM_{2.5}$ gap. I focus on its relationship to ambient pollution levels, location, and temporal factors. In a second step, I use linked socioeconomic survey data from participants to examine factors which may predict $PM_{2.5}$ gap disparities between participants in our study.

This paper makes three main contributions. First, I construct a state-of-the-art pollution dataset that breaks new ground in economics and exposure assessment science. This novel dataset draws from over 45 million $PM_{2.5}$ exposure readings collected by consumer WAQS adopters in the United States and provides considerable advantages over existing secondary dataset employed by economists and other researchers to measure pollution exposure. In particular, my dataset contains individual level locational data and captures granular exposure variation that is unobservable to studies using ambient monitor data or satellite remote-sensing products (Burke et al., 2023; Colmer et al., 2020; Deryugina et al., 2019). To the best of my knowledge, a comprehensive personal exposure dataset of this size has never before been assembled.[1]

Second, I use this innovative dataset to reveal substantial disparities between personal exposure and existing estimates from ambient regulatory monitors. On average, personal exposure is between 7% and 18% lower than concurrent monitor levels, while median differences corresponds to between 38 and 40% less pollution. My analysis further characterizes the nature of this $PM_{2.5}$ gap and demonstrates that it differs substantially with respect to measured pollution levels, location, time of day, weekday, and season. These findings contribute to a recent literature studying biases in conventional pollution data from regulatory monitors (Mu et al., 2021; Grainger and Schreiber, 2019; Fowlie et al., 2019; Sullivan and Krupnick, 2018). Moreover, I am able to link individual $PM_{2.5}$ gap estimates to individual level information and test for predictors of $PM_{2.5}$ gap disparities between individuals. While my analysis identifies significant heterogeneity in $PM_{2.5}$ discrepancies between individuals participating in this study, aggregate socioeconomic categories explain only a small share of total variation in individual $PM_{2.5}$ gaps. This finding contrasts with studies that demonstrate $PM_{2.5}$ inequities with conventional exposure data and larger population samples (Kerr et al., 2024; Currie et al., 2023; Spiller et al., 2021).

[1] See Lim et al. (2022), Evangelopoulos et al. (2020), and Steinle (2013) for recent reviews covering personal exposure assessment studies.

Third, I contribute to the literature on behavioral adaptation to pollution risks (Fan, 2024; Barwick et al., 2024; Burke et al., 2022; Neidell, 2009) by providing estimates of the defensive properties of residencies and highlighting when pollution risks may not be captured by ambient monitor data. My results shows that individuals are, on average, only exposed to about 29% to 41% of pollution per unit of measured ambient concentration, but being at home further reduces this share to 25%. Furthermore, personal exposure data capture up to 97% of the marginal effect of pollution per unit of personal exposure, suggesting that there are significant exposure risks left unobserved in conventional data. My results show that a considerable this effect is driven by high personal exposure readings above 25 $\mu g/m^3$. These finding contribute novel evidence collected with WAQS to a nascent literature harnessing crowd sourced data from fixed pollution sensors to measure pollution exposure (Kramer et al., 2023; Krebs et al., 2021).

The remainder of this paper is organized in the following manner. The next section provides a brief overview of the literature on $PM_{2.5}$ pollution exposure and damages and exposure monitoring. Section 1.3 then explains how I construct my dataset and provides descriptive statistics. Section 1.4 describes the econometric specifications I use to estimate the results I present in section 1.5. Section 1.6 concludes.

1.2 Background

1.2.1 $PM_{2.5}$ Exposure and Damages

It is well established that, among air pollutants, $PM_{2.5}$ causes the most damage to the US economy (The US Burden of Disease Collaborators et al., 2018). Long-term and short-term exposure inflict enormous negative externalities on human health and the economy that are estimated between 100,000 and 300,000 premature deaths annually (Chan et al., 2023) and exceed 4% of aggregate economic output (Tschofen et al., 2019; Goodkind et al., 2019). Underlying these estimates are CRFs derived from longitudinal studies (Krewski et al., 2009; Lepeule et al., 2012) that measure the mortality impacts associated with marginal increases in $PM_{2.5}$ and the value of a statistical life approach (Viscusi and Aldy, 2003). While federal policies like the Clean Air Act have substantially reduced pollution and its damages in the US since the 1970s (Aldy et al., 2022), researchers link recent increases in $PM_{2.5}$ pollution to elevated burdens from wildfire smoke, increased economic activity, and reduced regulatory enforcement (Clay et al., 2021; Burke et al., 2023, 2021). These findings suggest that some of the progress previously made by policy-makers on $PM_{2.5}$ pollution is being undone. Moreover, a growing literature documents persistent disparities in pollution damages between population subgroups despite overall improvements (Currie et al., 2023; Colmer et al., 2020).

1.2.2 Conventional Exposure Data

Environmental authorities site ambient monitors as the basis for regulatory air pollution monitoring, population pollution exposure tracking, and for other information disclosure programs. Due to high expenses associated with installing and maintaining ambient monitoring networks, these monitors are not installed at sufficient densities to capture actual spatial and temporal variation in pollution. Instead, data collected at these sites acts as inputs for pollution models which statistically estimate ambient concentrations across time and space. This data is also employed as ground truth measurements for validating satellite measurements and other remote-sensed data products. However, it is not entirely clear to what extent regulators' non-random siting decisions and other factors ultimately influence the quality of modeled exposure data. Recent studies have documented substantial policy errors in regulating air pollution with monitor data (Fowlie et al., 2019; Sullivan and Krupnick, 2018), strategic polluter and regulator circumvention of monitoring protocols (Zou, 2021; Mu et al., 2021), biases in government monitor siting decisions (Muller and Ruud, 2018; Grainger and Schreiber, 2019).

Recent advances in satellite image processing have greatly expanded the spatial coverage of ground-level pollution estimates. Conventional gridded datasets can reach spatial resolutions of less than 1-km^2 and often span the globe or entire continents, but their temporal coverage is limited by restricted number of times satellites pass over each grid cell. While state-of-the-art satellite products aim to provide neighborhood-level pollution data in real-time (e.g. Tropospheric Emissions: Monitoring of Pollution project), most conventionally-employed datasets provide at best monthly or yearly estimates (van Donkelaar et al., 2021; Di et al., 2016). These satellite products also carry biases and uncertainties which may distort their estimates (Jain, 2020). Nevertheless, studies increasingly employ this data to shed light on changing $PM_{2.5}$ exposure trends (Burke et al., 2023), long-term pollution exposure disparities (Colmer et al., 2020), and environmental policy effectiveness (Fowlie et al., 2019; Sullivan and Krupnick, 2018).

1.2.3 Sensors

More recently, the emergence of consumer low-cost sensors has created new opportunities to improve measure pollution closer to where it matters most: where people are. The widespread proliferation of stationary air quality sensors (SAQS) has greatly expanded the spatial density of non-regulatory $PM_{2.5}$ measurement sites and has also increased the temporal resolution of $PM_{2.5}$ data through publicly-available real-time maps. However, SAQS are installed in the US in socioeconomically advantaged communities and areas with fewer minority residents (Coury et al., 2024; Zivin et al., 2024), suggesting that these private networks may be reinforcing existing environmental inequalities. Crowd sourced $PM_{2.5}$ measurements at these sites have been used to characterize ambient $PM_{2.5}$ exposure disparities (Kramer et al., 2023) and shed light on the relationship between indoor and outdoor pollution concentrations (Krebs et al., 2021). A new strand of the economics literature analyzes factors driving the SAQS demand (Coury et al., 2024; Zivin et al., 2024; Dangel and Goeschl, 2024b), but otherwise there is a scarcity of rigorous empirical evidence about these technologies and their impacts.

Contemporary epidemiological studies distribute WAQS to volunteer participants to measure personal exposure for limited periods of time (Steinle, 2013; Lim et al., 2022; Boomhower et al., 2022; Evangelopoulos et al., 2020). However, these studies typically have small sample sizes (i.e. less than several dozen participants) and often recruit niche groups of high-risk or highly-exposed individuals (e.g. elderly, cyclists, school children). Several meta studies compare key outcomes across studies such as mean ratios, slopes, and correlations between personal and ambient concentrations. However, it is not clear to what extent sample recruitment, different sensor technologies, and other factors relating to the individual research studies' designs bias these meta results. The recent introduction of new consumer WAQS takes another step closer to personal exposure measurement in real-world settings. The present analysis and its companion paper are the first to explore these from an economic standpoint.

1.3 Data

1.3.1 Wearable Air Quality Sensors

Participants use the Atmotube Pro WAQS in this study. This smartphone-connected portable device measures $PM_{2.5}$ and other environmental parameters (i.e. temperature, relative humidity, atmospheric pressure, PM_{10}, PM_1, and volatile organic compounds) and retails for between \$179 and \$249. When its battery has charge, Atmotube Pro continuously collects readings, which are transmitted via a Bluetooth connection to an accompanying smartphone application (app) where they are displayed in real-time, logged to a database on the smartphone, and transferred to an Atmotube cloud database via the Internet. With default settings, the expected battery life is ten days.

Atmotube Pro is designed to measure personal exposure, so users can wear it on a belt loop, for example, or attach it to a bag using a small carabiner clip. While users need not always wear or carry their devices, the app automatically augments readings with smartphone GPS coordinates if an active Bluetooth connection exists between Atmotube Pro and the user's smartphone. Given that people generally keep their smartphones on hand, users can be assumed to be within Bluetooth range (approximately ten meters) of their Atmotube Pro whenever readings have GPS coordinates.[2] The performance of Atmotube Pro's $PM_{2.5}$ readings has been independently validated in laboratory and field settings by AQ-SPEC, a government-funded sensor evaluation center.

In the companion study (Dangel and Goeschl, 2024a), 829 Atmotube Pros

[2] There may be edge cases where this assumption does not hold. For example, a user could pair Atmotube Pro with another GPS-capable device (e.g. a tablet or second smartphone), which they do not keep close by. However, we can identify these cases based on their fixed location.

are deployed to consumers via a point-of-sale intervention on the manufacturer's online store from November 2022 through September 2023. A pop-up banner invites prospective customers to complete a discount-incentivized survey. The survey collects information about respondents' socioeconomic backgrounds, pollution perceptions, and willingness to pay for Atmotube Pro, and it informs respondents that they agree to share their data for a research study if they participate.[3] Upon survey completion, respondents receive a personalized discount code for Atmotube Pro at one of five randomly-allocated price levels ranging from free ($0, free shipping) to $149. Individual purchase decisions are linked to survey responses and later to WAQS data if they choose to adopt. After participants receive their Atmotube PRO, they can use their devices as they wish.

The present study uses WAQS data from 658 of 829 participants from the companion study who I am able to match to WAQS data stored in the Atmotube cloud database. I exclude an additional 64 participants from our sample in a filter process explained later in this section. Table 1.1 describes the final sample of 594 individuals. Participants are disproportionately between 36 and 55 years old (49%), male (82%), and White (77%) and predominantly come from households with at least one college educated member (85%) and above median household income (78%). On average, they live in households with 2.7 members, 0.5 children below 18 years of age, and 0.3 seniors above 65 years. About 73% of responding participants "somewhat agree" or "strongly agree" that the air they breathe is polluted, and 21% have a household member living with a cardiopulmonary diagnosis. Participants generally reside in urban counties, with the average share of urban households in adopters' counties exceeding 88%, and they live relatively more often in the West (32%) and less frequently in the Midwest (19%). On average, each participant uses their WAQS on 79.4 days in the first year after adoption.

For each of the 594 participants, I observe raw WAQS $PM_{2.5}$ readings stored in the Atmotube cloud database collected between November 2022 and September 2024. Table 1.2 summarizes this data. Panel A shows that participants collect nearly 45 million raw $PM_{2.5}$ readings over this time span. In the raw readings, the average $PM_{2.5}$ is 6.64 $\mu g/m^3$. About 12% of raw readings exceed the EPA

[3]Participants are free at any point in time to opt-out of data sharing via the app or by contacting the research team.

Table 1.1: Summary Statistics: Participants

	Users	Mean	SD
Panel A: Personal Characteristics			
Age: 18-35 (=1)	594	0.27	0.44
Age: 36-55 (=1)	594	0.49	0.50
Age: 56+ (=1)	594	0.24	0.43
Gender: Male (=1)	579	0.82	0.38
Race/Ethn.: White alone (=1)	571	0.77	0.42
HH Education: Bachelor or higher (=1)	583	0.85	0.36
HH Income: Above median (=1)	492	0.78	0.41
HH: # Members (count)	580	2.66	1.24
HH: # Children (count)	577	0.54	0.87
HH: # Seniors (count)	583	0.30	0.63
AQ Belief: Polluted (Likert: 4/5=1)	594	0.73	0.44
HH: Cardiopulmonary diagnosis (=1)	579	0.21	0.41
County: Urban (%)	594	0.88	0.19
Region: West (=1)	594	0.31	0.46
Region: Midwest (=1)	594	0.19	0.39
Region: Northeast (=1)	594	0.25	0.43
Region: South (=1)	594	0.25	0.43
Residence: Distance to monitor (km)	594	15.57	15.76
Residence: Within 15 km of monitor (=1)	594	0.64	0.48
Residence: Within 25 km of monitor (=1)	594	0.84	0.37
Panel B: WAQS Use			
Days active in first year (count)	594	79.45	91.60
Reading within 15 km of residence (=1)	594	0.90	0.16
Reading within 1 km of residence (=1)	594	0.84	0.20
Reading at residence (=1)	594	0.83	0.20

Note: The initial sample includes 658 participants, but we exclude 64 participants more than 100 kilometers from the nearest monitor, leaving 594 participants in the full sample. For each variable in panel A, participants who choose not to respond to the corresponding survey question are excluded from users, mean, and standard deviation statistics.

Table 1.2: Summary Statistics: WAQS Readings

	Count	Mean	SD	Min	Max
Panel A: Raw					
Raw PM2.5 (µg/m3)	45,060,423	6.63	29.84	1	1,000
Exceeds 9 µg/m3 (=1)	45,060,435	0.12	0.33	0	1
Exceeds 35 µg/m3 (=1)	45,060,435	0.02	0.15	0	1
Geocoordinates (=1)	45,060,435	0.10	0.30	0	1
At residence (=1)	4,562,905	0.86	0.35	0	1
Distance from residence (km)	4,562,905	125.72	886.66	0	15,387
Panel B: Hourly					
Hourly mean PM2.5 (µg/m3)	1,093,868	7.01	29.40	1	999
Raw readings per hour (count)	1,093,868	41.14	24.47	4	60
GPS readings per hour (count)	1,093,868	14.23	24.86	0	60
Exceeds 9 µg/m3 (=1)	1,093,868	0.12	0.33	0	1
Exceeds 35 µg/m3 (=1)	1,093,868	0.03	0.16	0	1
Geocoordinates (=1)	1,093,868	0.27	0.45	0	1
At residence (=1)	299,373	0.82	0.38	0	1
Distance from residence (km)	299,373	179.94	1069.58	0	16,455
Matched to monitor reading (=1)	299,373	0.93	0.25	0	1
Panel C: Daily					
Daily mean PM2.5 (µg/m3)	35,262	6.60	18.41	1	627
Raw readings per day (count)	35,262	1036.40	501.32	118	1,440
GPS readings per day (count)	35,262	306.16	501.70	0	1,440
Hourly readings per day (count)	35,262	24.00	0.00	24	24
Exceeds 9 µg/m3 (=1)	35,262	0.14	0.35	0	1
Exceeds 35 µg/m3 (=1)	35,262	0.02	0.14	0	1
Geocoordinates (=1)	35,262	0.48	0.50	0	1
Distance from residence (km)	16,954	206.53	1211.32	0	16,160
Matched to monitor reading (=1)	16,954	0.74	0.44	0	1

Note: Exceedance variables correspond to EPA annual (9 µg/m3) and 24-hour (35 µg/m3) PM2.5 standards.

annual $PM_{2.5}$ standard (9 µg/m^3) and approximately 2% of readings exceed the EPA annual $PM_{2.5}$ guideline (35 µg/m^3).

Approximately 10% of all raw $PM_{2.5}$ readings are accompanied by GPS coordinates. For each participant, I identify the most frequent GPS location within 250 meters and assign this location to the participant as their place of residence.[4] I then calculate the distance between each WAQS reading and each user's corresponding residence location. Statistics in panel A of table 1.2 show that, on average, each reading is taken 126 kilometers from the residence, but this is driven by a very small number of very distant observations. About 86% of all raw readings with GPS coordinates are collected at the individuals' residences (i.e. within 250 meters of their most frequent location). Panel B of table 1.1 shows that, on average, about 90% of each individuals' raw WAQS readings with GPS coordinates are within fifteen kilometers of their residence, 84% are within one kilometer of their residence, and 83% are at their residence.

In order to later compare WAQS data to hourly and daily monitor concentrations, I average raw $PM_{2.5}$ values first to the user-hourly level and then to the user-daily level. When I aggregate to the user-hourly level, I remove hourly readings with fewer than four raw readings per hour.[5] On average, each hourly reading in the final sample consists of 42.4 readings and records 6.95 µg/m^3 of $PM_{2.5}$. I assume for each user-hour that a user is exposed to the hourly $PM_{2.5}$ measured by their WAQS if at least one raw WAQS reading in that hour includes GPS coordinates. I drop the remaining hourly observations because I cannot be sure that the user is in the vicinity of their Atmotube Pro. This leaves a total of 299,328 hourly personal $PM_{2.5}$ exposure readings that I aim to match to monitor data in a later step. I then aggregate to the user-daily level and conservatively exclude daily readings with fewer than twenty-four hourly readings. On average, each daily $PM_{2.5}$ reading consists of 1,036 raw readings and measures a daily mean hourly concentration of 6.61 µg/m^3 of $PM_{2.5}$. Only about 27% of hourly readings

[4]For a subset of 365 participants with WAQS data during nighttime hours (12am - 4am, i.e. when they are presumably at home), I am able to validate for 96% of the subset that their most frequent nighttime location corresponds to their most frequent location.

[5]In the app settings, users can set different $PM_{2.5}$ reading frequencies. The most frequent is once every minute and the least frequent but most energy efficient is once every fifteen minutes. Figure 1.B.3 in appendix 1.B shows the distribution of raw $PM_{2.5}$ readings per hour before removal.

aggregated to the daily level have GPS coordinates, and a smaller share have GPS readings for entirety of the day. Similarly to the raw-to-hourly aggregation process, I only assign daily readings to individuals if they collect at least one GPS reading that day. As a result, 16,944 daily personal exposure readings can be matched to monitor data.

1.3.2 Monitors

To generate a dataset of hourly and daily ambient monitor $PM_{2.5}$ concentrations most relevant for personal exposure assignment, I first download the universe of hourly $PM_{2.5}$ monitor data stored in the EPA AQS database from November 1, 2022 through September 30, 2024.[6] For each monitor site, I calculate the hourly reporting rate across the entire time period and drop monitors reporting less than 75% of possible hourly concentrations. I then use participants' residence locations as identified by the procedure described in section 1.3.1 to match each user to the closest monitor that survives the preceding reporting rate filter. For each participant, I hold this monitor constant throughout the analysis. I drop participants that live further than 100 kilometers from the nearest monitor.[7] Panel A in table 1.1 provides summary statistics on participants' residence location relative to their matched monitor. On average, each participant's residential location is about 15.6 kilometers from the nearest matched monitor. Over 60% of participants live within fifteen kilometers, and nearly 85% live within twenty-five kilometers.

Table 1.3 describes the complete hourly and daily $PM_{2.5}$ pollution data for 320 ambient monitors closest to the 593 WAQS users' residential locations. Across all 6,814,310 hourly observations, the average hourly monitor $PM_{2.5}$ concentration (8.06 $\mu g/m^3$) is about 1.05$\mu g/m^3$ higher than the average measured by participants' WAQS. The average daily $PM_{2.5}$ concentration (8.47 $\mu g/m^3$) is about 1.86 $\mu g/m^3$ greater than average daily WAQS readings. A greater share of hourly monitor concentrations exceeds the EPA annual $PM_{2.5}$ standard than the share of WAQS readings that do (32% vs. 12%), but a smaller share exceeds the EPA 24-hour

[6] At this point in time, 2024 EPA AQS data is still preliminary.

[7] Counties vary greatly in size, so it is not unusual for an individual to live in a monitored county but still live 100 kilometers from their nearest monitor. For example, Los Angeles County spans over 120 kilometers from north to south.

Table 1.3: Summary Statistics: Monitor Concentrations

	Count	Mean	SD	Min	Max
Panel A: Hourly					
Hourly mean PM2.5 (µg/m3)	6,714,310	8.09	7.98	0	623
Exceeds 9 µg/m3 (=1)	6,714,310	0.32	0.47	0	1
Exceeds 35 µg/m3 (=1)	6,714,310	0.01	0.09	0	1
Matched to WAQS reading (=1)	298,044	0.94	0.24	0	1
Panel B: Daily					
Daily mean PM2.5 (µg/m3)	219,500	8.52	6.82	0	238
Hourly readings per day (count)	219,500	24.00	0.00	24	24
Exceeds 9 µg/m3 (=1)	219,500	0.34	0.48	0	1
Exceeds 35 µg/m3 (=1)	219,500	0.01	0.08	0	1
Matched to WAQS reading (=1)	22,686	0.55	0.50	0	1

Note: Exceedance variables correspond to EPA annual (9 µg/m3) and 24-hour (35 µg/m3) PM2.5 standards.

standard compared to WAQS (1% vs. 3%). Figure 1.B.6 in appendix 1.B shows the overall distribution of hourly readings from both technologies. Visual inspection identifies substantial differences in the distribution of ambient versus personal exposure readings. The vast majority of all readings from both technologies is at or below 25 µg/m^3, but a significantly larger mass of WAQS readings are below 5 µg/m^3 compared to monitor readings. There also appears to be a non-trivial surplus of WAQS readings greater than 25 µg/m^3.

1.3.3 Data Matching

I then match concurrent WAQS and monitor data that survive the filter processes described in sections 1.3.1 and 1.3.2. Ultimately, I can match 279,638 hourly mean WAQS readings from 590 participants to 269,097 hourly mean concentrations measured at 310 monitors and 12,476 daily mean WAQS readings from 377 participants to 11,365 daily readings from 231 monitors from November 2022 through September 2024. For each participant-monitor observation, I then calculate the observation-specific $PM_{2.5}$ gap as the difference from subtracting monitor $PM_{2.5}$

from WAQS $PM_{2.5}$. This data forms the basis for my main analysis.

Later, I restrict my sample in two ways to check the sensitivity of my data. First, I exclude from the analysis concentrations above 25 µg/m^3 for monitors but not for WAQS and vice versa. Figure 1.B.6 in appendix 1.B shows that approximately 95% of all unmatched WAQS and monitor readings measure less than 25 µg/m^3, so restricting my sample in this way enables me to describe the $PM_{2.5}$ gap when personal and ambient readings are each most representative of normal pollution conditions. I repeat this process to include only concentrations up to 400 µg/m^3 for one technology but not the other and vice versa. Thereby, I can separately describe the $PM_{2.5}$ gap for a wider range of pollution levels from the vantage point of high personal exposure and high ambient concentrations.[8] Second, I remove participant-monitor pairs where the distance between participant residence and matched monitor exceeds fifteen kilometers. The full matched sample which includes 590 participant-monitor pairs thereby shrinks to 361 participant-monitor pairs.

Figure 1.B.1 and figure 1.B.2 in appendix 1.B display temporal trends in matched $PM_{2.5}$ data by monitoring technology and count the number of active WAQS with geocoordinates at the calendar date and hour-of-day levels, respectively. Two key aspects of our data emerge from visually inspecting trends in the matched data. First, mean monitor concentrations at both the daily and diurnal levels sometimes exceed mean WAQS readings, while the relationship flips at other points in time. However, mean monitor concentrations substantially exceed median WAQS concentrations at all points in time. I leave a more detailed analysis and description of these differences for section 1.5. Second, there is heterogeneity in the number of participants collecting WAQS data over time. Panel B in figure 1.B.1 demonstrates that the number of participants who collect at least twenty-four hourly WAQS measurements per day with at least one GPS reading steadily increases from November 2022, peaks in summer 2023, steadily falls until spring 2024, and then flattens our for the remainder of the study period. This pattern reflects staggered WAQS deployment and natural declines in WAQS user activity

[8]Both WAQS and monitors in our matched data have readings higher than 400 µg/m^3, but there are a very small number of observations at these $PM_{2.5}$ levels, so analyses from the vantage point of these extremely high readings are too limited.

over time after adoption as documented in the companion field experiment (Dangel and Goeschl, 2024a).[9] Panel B in figure 1.B.2 also shows diurnal patterns to data collection. Fewer participants collect geolocated WAQS readings during the night.

1.4 Method

I rely on three ordinary least squares (OLS) regression specifications to characterize: i) the existence and magnitude of a $PM_{2.5}$ gap, ii) the influence of locational and temporal factors on it, and iii) differences in it between individuals. First, I regress the following equation:

$$\Delta PM_{it} = \alpha_0 + \beta PM_{itm} + \delta_t + \gamma_i + \epsilon_{itm}, \tag{1.1}$$

where the dependent variable, ΔPM_{it}, is the $PM_{2.5}$ exposure gap defined as the difference after subtracting monitor $PM_{2.5}$ from WAQS $PM_{2.5}$ for participant i in time period t. The temporal resolution of t can either be hourly or daily as described in section 1.3. The constant term, α_0, describes the magnitude of the $PM_{2.5}$ gap at a measured pollution concentration of 0 µg/m^3.[10] The main explanatory variable of interest, PM_{itm}, is the $PM_{2.5}$ concentration at time t assigned to participant i by monitoring technology m, which can either be: i) the assigned ambient monitor or ii) the participant's own WAQS. The coefficient of interest, β, then captures the marginal effect of a one unit increase in measured pollution on the $PM_{2.5}$ exposure gap.

Equation 1.1 also includes participant (γ_i) and time (δ_t) fixed effects. Previous research documents diurnal, daily, and seasonal differences in $PM_{2.5}$ exposure relating to temporal and atmospheric factors, so the model should account for these factors. While my analysis does not directly include weather data, I flexibly control for diurnal and within-week variation in the $PM_{2.5}$ gap with hour-of-day and day-of-week fixed effects, respectively, and for seasonal exposure gap differences with year-month fixed effects. My regressions also include individual user fixed ef-

[9]In the study, WAQS adoptions peak in summer 2023.

[10]I also estimate intercept-only versions of equation 1.1 that capture the mean $PM_{2.5}$ gap.

fects to account for unobserved idiosyncratic differences in the $PM_{2.5}$ gap between participants that could be related to a wide range of factors such as occupation, preferences for tobacco smoke, or matched monitor characteristics (e.g. distance to participant residence and whether it measures traffic, background, or industrial pollution). I separately predict the magnitude of the $PM_{2.5}$ gap over each of the included sets of fixed effects. Inference relies on heteroskedasticity-robust standard errors that are always clustered on the individual participant.

Second, I augment equation 1.1 by introducing a binary variable indicating whether the participant is at their residence or not and interacting this with the continuous $PM_{2.5}$ term:

$$\Delta PM_{it} = \alpha_0 + \beta_1 PM_{itm} + \beta_2 Home_{it} + \beta_3 PM_{itm} \times Home_{it} + \delta_t + \gamma_i + \epsilon_{itm}, \quad (1.2)$$

where $Home_{it}$ is equal to one when participant i's WAQS readings geocoordinates are within 250 meters of their residential geocoordinates at time t and zero otherwise.[11] The first of the two additional coefficients of interest, β_2, describes the difference in mean exposure at the residence versus not at the residence, holding measured pollution constant. The second added coefficient, β_3, corresponds to the change in the marginal effect of measured pollution on the $PM_{2.5}$ gap when at the residential location, and summing β_1 and β_3 captures the net marginal effect of an additional unit of measured pollution when participants are at their residential locations. I statistically test whether each of these coefficients differs significantly from zero. The remaining components of equation 1.2 are identical to 1.1.

Finally, I predict for each participant their average $PM_{2.5}$ gap using equation 1.1 and regress the predicted value on a vector of individual-level characteristics collected in the baseline survey of the companion field experiment (Dangel and Goeschl, 2024a). This regression is described by:

$$\Delta \hat{P}M_i = \alpha_0 + \beta \mathbf{X}_i + \epsilon_i, \quad (1.3)$$

where β is a vector of characteristic-specific relationships between the included

[11] This 250 meter buffer around the residence accounts for inaccuracies in GPS data, but could likely be decreased in subsequent analysis.

variables of interest, $\mathbf{X}_i$, and the $PM_{2.5}$ gap, holding all else equal. My analysis tests for whether participant age group, gender, racial/ethnic group, education level, income level, household size, ex ante air quality beliefs (i.e. before WAQS adoption), county urban share, distance to monitor, or having children, seniors, or someone with a cardiopulmonary diagnosis living in the household affect the $PM_{2.5}$ gap to an extent that differs statistically from zero.

1.5 Results

1.5.1 $PM_{2.5}$ Gap Estimates

My analysis documents substantial disagreement between personal and ambient $PM_{2.5}$ concentrations. On aggregate, personal exposure is about 0.6 µg/m^3 less than hourly ambient concentrations and about 1.6 µg/m^3 less than daily ambient concentrations. While these differences may appear modest, they respectively correspond to 7% and 18% of mean monitor concentrations sampled by all matched monitors in my dataset over the entire study period. However, averages obscure a more striking result. The median $PM_{2.5}$ gap is about 3.1 µg/m^3 at the hourly level and 3.4 µg/m^3 at the daily level, which respectively correspond to 38% and 40% less than mean hourly and daily ambient monitor concentrations in our dataset. Figures 1.B.4 and 1.B.5 in appendix 1.B highlight how a substantial majority of monitor concentrations exceed personal hourly and daily $PM_{2.5}$ readings. The relatively small magnitude of previously-described overall mean $PM_{2.5}$ gaps appear to be driven by relatively large shares of personal-ambient exposure differences that exceed 25 µg/m^3 (approximately 2.6-3.0%), which offset the general skew in the distributions of the hourly and daily $PM_{2.5}$ gaps.

1.5.2 Intrapersonal Differences

Next, I find that discrepancies in personal and ambient exposure readings relate to levels of measured pollution, location, and temporal factors that differ within individuals. In a first step, I visually inspect the relationship between measured pollution and the $PM_{2.5}$ gap for measured concentrations: i) below 25 µg/m^3 and

ii) below 400 µg/m^3. I then show regression results that quantify the plotted relationships and shed light on the other intrapersonal factors.

Panel A of figure 1.1 depicts the mean, median, interquartile range, and middle 90% of the distribution of hourly $PM_{2.5}$ gap observations from the vantage point of the nearest monitor for monitor exposure levels below 25 µg/m^3. There is a clear negative relationship between monitor $PM_{2.5}$ concentrations and the $PM_{2.5}$ gap. However, at low monitor $PM_{2.5}$ concentrations, the mean $PM_{2.5}$ gap is positive, indicating that personal exposure is, on average, greater than as measured by the nearest monitor at ambient pollution levels below the EPA annual $PM_{2.5}$ standard (9 µg/m^3). As monitor concentrations increase, this relationship flips. The $PM_{2.5}$ gap is negative for the majority of monitor concentrations below 25 µg/m^3, meaning that mean personal exposure is less than as measured by the nearest monitor for monitor concentrations above the EPA annual $PM_{2.5}$ standard. Comparing the average relationship to the spread of the distribution provides further evidence that monitors overestimate personal exposure. For each 1 µg/m^3 monitor $PM_{2.5}$ bin above 4 µg/m^3, over 75% of $PM_{2.5}$ gap observations are negative, and the distribution is increasingly negative over the range of interest.

Panel B relaxes the 25 µg/m^3 restriction on monitor $PM_{2.5}$ observations and plots the corresponding mean and distributional statistics for the range of monitor concentrations up to 400 µg/m^3. Once again, the results are striking. As ambient concentration increase, the $PM_{2.5}$ gap increases dramatically. At $PM_{2.5}$ levels that 24-hour air quality index (AQI) thresholds deem unhealthy for sensitive groups (35 µg/m^3), unhealthy (55 µg/m^3), very unhealthy (125µg/m^3), or hazardous (225µg/m^3) monitors measure substantially more pollution than participants' WAQS. To put just one data point into perspective, at a hazardous monitor concentration of 300 µg/m^3, personal exposure is over 200 µg/m^3 less. Furthermore, in contrast to panel A, the middle 90% of the $PM_{2.5}$ gap distribution also shifts downward considerably as monitor concentrations reach extreme values.

Panel C of figure 1.1 displays the same statistics relating the $PM_{2.5}$ gap to measured exposure but from the perspective of WAQS users for personal exposure levels below 25 µg/m^3. Here, the relationship between personal exposure measurements and the $PM_{2.5}$ gap is positive. At low personal exposures, the average $PM_{2.5}$ gap is negative, meaning that participants' readings are, on average, below ambi-

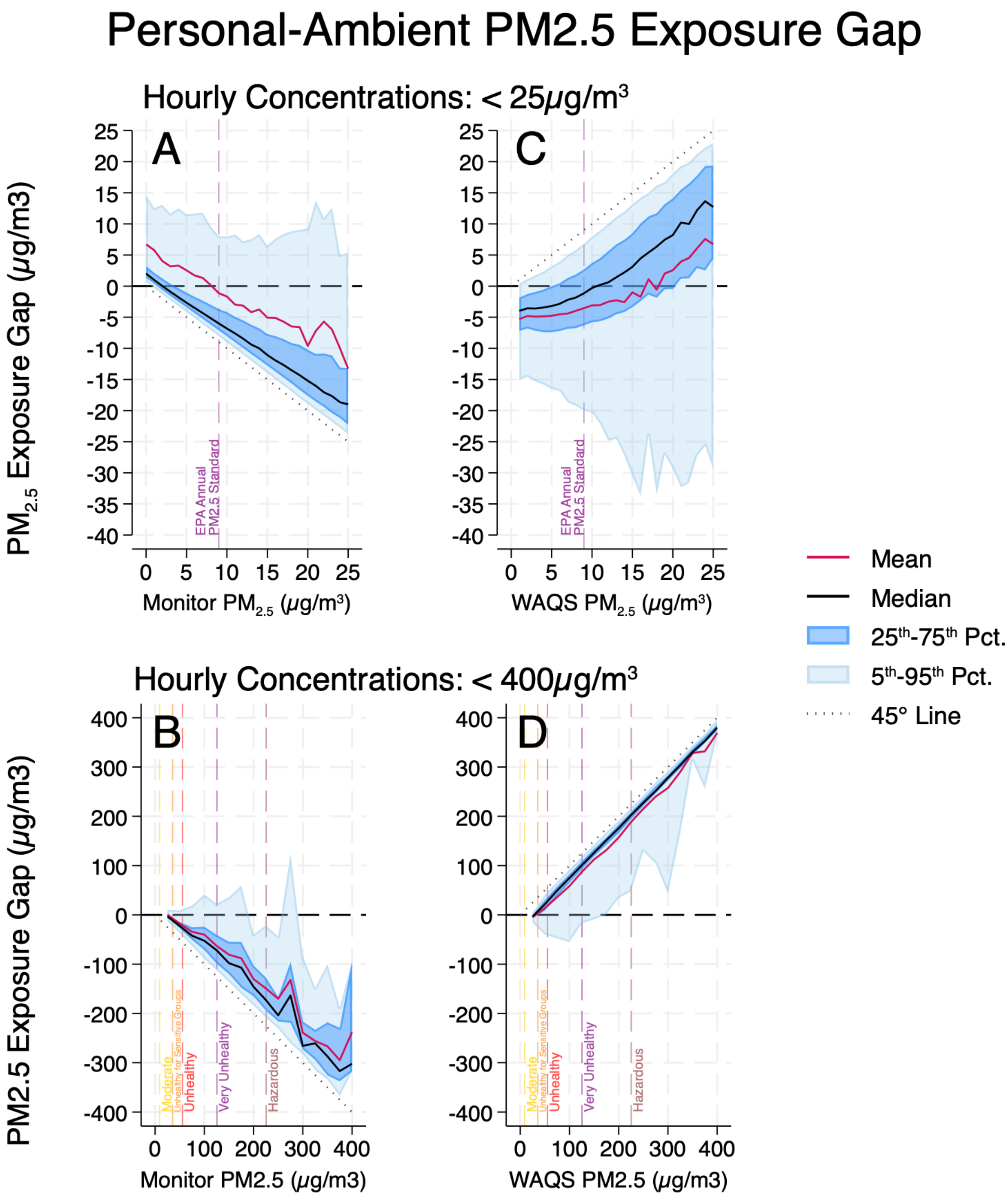

Figure 1.1: This figure characterizes the relationship between the $PM_{2.5}$ exposure gap and hourly mean $PM_{2.5}$ measured at the nearest monitor (panel A and B) or with a wearable air quality sensor (WAQS, plots C and D) for respective x-axis concentrations in 1 μg/m^3 bins below 25 μg/m^3 (panels A and C) or 25 μg/m^3 bins below 400 μg/m^3 (panels B and D). In all panels the $PM_{2.5}$ exposure gap is calculated as the difference from subtracting hourly mean monitor $PM_{2.5}$ from hourly mean WAQS $PM_{2.5}$. Dashed vertical lines mark EPA ambient $PM_{2.5}$ standards in panels A and C and air quality index thresholds in panels B and D.

ent concentrations measured at the nearest monitor. The $PM_{2.5}$ gap flips in sign to positive at about 17-19 µg/m^3, where personal and ambient concentrations are on average equal, but the majority of $PM_{2.5}$ gap observations are already positive. Above 20 µg/m^3 the $PM_{2.5}$ gap is predominantly positive, meaning that higher WAQS readings in this range tend to exceed readings at the nearest monitor.

Panel D of figure 1.1 plots the $PM_{2.5}$ gap mean and distribution for WAQS readings up to 400 µg/m^3 and reveals a strong positive linear relationship between the $PM_{2.5}$ gap and WAQS readings as they reach extreme values. This means that when individuals measure high pollution levels, their exposure levels are not at all or insufficiently captured by ambient monitors.

Table 1.4: OLS Regression Results: Hourly Monitor PM2.5 Gap (<25µg/m^3)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap	(5) PM2.5 Gap
Constant	0.533***	6.272***	5.821***	5.513***	5.713***
	(1.68e-15)	(0.386)	(1.263)	(1.529)	(1.391)
Monitor PM2.5		-0.732***	-0.732***	-0.692***	-0.709***
		(0.0492)	(0.0492)	(0.0863)	(0.0848)
At Residence=1			0.552	0.932	0.894
			(1.449)	(1.857)	(1.679)
At Residence=1				-0.0486	-0.0542
× Monitor PM2.5				(0.105)	(0.106)
Observations	269,098	269,098	269,098	269,098	269,098
Users	551	551	551	551	551
User FE	Yes	Yes	Yes	Yes	Yes
Time FE	No	No	No	No	Yes
Mean WAQS PM2.5	8.88	8.88	8.88	8.88	8.88
Mean Monitor PM2.5	9.46	9.46	9.46	9.46	9.46
R^2	0.20	0.21	0.21	0.21	0.22

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting hourly mean monitor PM2.5 from hourly mean WAQS PM2.5. Time fixed effects include hour-of-day, day-of-week, and year-month indicators. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

I now turn to regression results in table 1.4, which displays point estimates from five specifications linking monitor $PM_{2.5}$ to the $PM_{2.5}$ gap at monitor concentrations below 25 µg/m^3 that validate the results depicted in panel A of figure 1.1. In column 1, the coefficient on the constant-only regression corresponds to a mean $PM_{2.5}$ gap of +0.5 µg/m^3 at monitor concentrations below 25 µg/m^3 and is highly statistically significant. In column 2, I present results from estimating equation 1.1 without time fixed effects. The coefficient on the constant term increases to a $PM_{2.5}$ gap of +6.3 µg/m^3 at monitor concentrations of 0 µg/m^3, and the coefficient on the monitor $PM_{2.5}$ term is equal to a 0.7 µg/m^3 decrease in the $PM_{2.5}$ gap for each additional unit of ambient $PM_{2.5}$ pollution up to 25 µg/m^3. I thereby estimate that the $PM_{2.5}$ gap flips in sign at 8.6 µg/m^3. The regression in column 2 adds a binary variable for observations where the participant is at their residence. The coefficient on this term suggest 0.5 µg/m^3 more pollution at residences than elsewhere but is not statistically significant at the 5% level. For ambient concentrations below 25 µg/m^3, being at the residence also does not appear to meaningfully impact the relationship between ambient monitor concentrations and the $PM_{2.5}$ gap as the interaction term in columns 3 and 4 is of modest magnitude and statistically insignificant at the 5% level.

Table 1.5: OLS Regression Results: Hourly Monitor PM2.5 Gap (<400µg/m^3)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap	(5) PM2.5 Gap
Constant	-0.579***	6.219***	5.908***	4.708***	4.690***
	(1.22e-15)	(0.444)	(1.224)	(1.306)	(1.155)
Monitor PM2.5		-0.719***	-0.719***	-0.594***	-0.591***
		(0.0469)	(0.0469)	(0.0501)	(0.0510)
At Residence=1			0.383	1.893	1.909
			(1.412)	(1.617)	(1.441)
At Residence=1				-0.157*	-0.160**
× Monitor PM2.5				(0.0610)	(0.0600)
Observations	279,606	279,606	279,606	279,606	279,606
Users	558	558	558	558	558
User FE	Yes	Yes	Yes	Yes	Yes
Time FE	No	No	No	No	Yes
Mean WAQS PM2.5	8.88	8.88	8.88	8.88	8.88
Mean Monitor PM2.5	9.46	9.46	9.46	9.46	9.46
R^2	0.19	0.23	0.23	0.23	0.23

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting hourly mean monitor PM2.5 from hourly mean WAQS PM2.5. Time fixed effects include hour-of-day, day-of-week, and year-month indicators. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

Table 1.5 expands the range of considered monitor $PM_{2.5}$ values up to 400 mg/m3 and reveals two notable differences to the estimates in table 1.4. First, the constant term in column 1 flips in sign but is of similar magnitude. Instead of personal exposure exceeding ambient concentrations on average by +0.5 µg/m^3, I estimate that exposure is 0.6 µg/m^3 less on average. However, the nature of $PM_{2.5}$ gap described by estimates in columns 2 and 3 appears very similar. Most strikingly, the interaction terms in columns 4 and 5 are statistically significant and of meaningful magnitude. For each one unit increase in ambient concentrations measured at the nearest monitor, participants are exposed to 0.16 µg/m^3 less of $PM_{2.5}$ when they are at their place of residence. This result is statistically significant at the 5% level in column 4 and at the 1% level in column 5 when I add temporal fixed effects. While the coefficient on being at the residence does not turn statistically significant, it jumps in magnitude from 0.4 µg/m^3 to 1.9 µg/m^3. This implies that the linear relationship between mean ambient concentrations and the $PM_{2.5}$ gap depicted in panel B of figure 1.1 rotates clockwise.

Next, I consider regression estimates measuring the $PM_{2.5}$ gap from the personal exposure perspective for concentrations below 25 µg/m^3. Table 1.6 confirms the relationship plotted in panel C of figure 1.1. The constant term in columns 1 through 5 demonstrates that, on average, personal $PM_{2.5}$ exposures in this range are between 4.6 and 6.6 µg/m^3 less than concentrations measured at the nearest monitor. As personal exposure increases, the $PM_{2.5}$ gap also increases by about 0.3 µg/m^3 for each additional unit of pollution. Coefficients on the interaction terms in columns 4 and 5 once again demonstrate the relevance of residential location, as the marginal impact of an additional unit of personal pollution on the $PM_{2.5}$ gap jumps in column 4 to 0.45 µg/m^3 when participants are not at their residence. The interaction term coefficient shows that the marginal impact of personal pollution on the $PM_{2.5}$ gap is more than halved at participants' residences. When adding temporal fixed effects, the coefficient on the binary residence variable turns statistically significant and implies that the $PM_{2.5}$ gap is 0.93 µg/m^3 greater at home, holding all else equal.

Table 1.6 shows how these estimates change when considering WAQS readings up to 400 µg/m^3. The mean $PM_{2.5}$ gap as identified by the constant term in column 1 shrinks to 0.58 µg/m^3 from 4.6 µg/m^3 in the limited sample. In columns

Table 1.6: OLS Regression Results: Hourly WAQS PM2.5 Gap (<25µg/m^3)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap	(5) PM2.5 Gap
Constant	-4.567***	-5.680***	-5.441***	-6.310***	-6.556***
	(1.82e-15)	(0.234)	(0.377)	(0.404)	(0.372)
WAQS PM2.5		0.262***	0.260***	0.448***	0.486***
		(0.0551)	(0.0556)	(0.0683)	(0.0620)
At Residence=1			-0.288	0.815	0.928*
			(0.270)	(0.420)	(0.399)
At Residence=1				-0.244**	-0.246**
× WAQS PM2.5				(0.0809)	(0.0752)
Observations	266,576	266,576	266,576	266,576	266,576
Users	547	547	547	547	547
User FE	Yes	Yes	Yes	Yes	Yes
Time FE	No	No	No	No	Yes
Mean WAQS PM2.5	8.88	8.88	8.88	8.88	8.88
Mean Monitor PM2.5	9.46	9.46	9.46	9.46	9.46
R^2	0.12	0.13	0.13	0.13	0.16

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting hourly mean monitor PM2.5 from hourly mean WAQS PM2.5. Time fixed effects include hour-of-day, day-of-week, and year-month indicators. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

Table 1.7: OLS Regression Results: Hourly WAQS PM2.5 Gap (<400µg/m^3)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap	(5) PM2.5 Gap
Constant	-0.579***	-9.182***	-9.339***	-9.119***	-9.054***
	(1.22e-15)	(0.142)	(0.268)	(0.285)	(0.281)
WAQS PM2.5		0.969***	0.969***	0.945***	0.949***
		(0.0160)	(0.0159)	(0.0228)	(0.0212)
At Residence=1			0.194	-0.0664	-0.163
			(0.271)	(0.306)	(0.302)
At Residence=1				0.0285	0.0243
× WAQS PM2.5				(0.0205)	(0.0184)
Observations	279,606	279,606	279,606	279,606	279,606
Users	558	558	558	558	558
User FE	Yes	Yes	Yes	Yes	Yes
Time FE	No	No	No	No	Yes
Mean WAQS PM2.5	8.88	8.88	8.88	8.88	8.88
Mean Monitor PM2.5	9.46	9.46	9.46	9.46	9.46
R^2	0.19	0.91	0.91	0.91	0.92

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting hourly mean monitor PM2.5 from hourly mean WAQS PM2.5. Time fixed effects include hour-of-day, day-of-week, and year-month indicators. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

2-5, the mean $PM_{2.5}$ gap increases in magnitude to between 9.1 and 9.3 µg/m^3, but the marginal relationship between WAQS readings and the $PM_{2.5}$ gap increases to nearly one-to-one. For each additional unit of personal exposure, the $PM_{2.5}$ gap grows by between 0.95 and 0.97 µg/m^3. There appears to be little evidence for a statistically meaningful impact of residential location on the marginal impact or overall level of the $PM_{2.5}$ gap for this larger range of $PM_{2.5}$ concentrations. Considered jointly, the results in table 1.6 provide further evidence that monitor data fails to capture personal exposure, in particular when considering high personal exposure levels.

In order to inspect the sensitivity of these results to my parameter and filtering choices, I present several additional regression tables in appendix 1.A. I briefly summarize the results. First, I look at daily $PM_{2.5}$ gap estimates. Table 1.A.1 presents four regression models estimating the relationship between monitor and personal concentrations and the daily $PM_{2.5}$ gap for 343 participants who survive the daily data collection filter. The marginal monitor $PM_{2.5}$ gap (-0.7 µg/m^3) and marginal WAQS $PM_{2.5}$ (+0.9 µg/m^3) are very similar to results presented in tables 1.5 and 1.7, respectively. When I restrict this daily sample to 220 residence-monitor pairs that are separated by less than fifteen kilometers, the $PM_{2.5}$ gap from the vantage point of monitors remains fairly similar (0.68 µg/m^3 per unit of $PM_{2.5}$) while the WAQS $PM_{2.5}$ shrinks from 0.91 µg/m^3 to 0.75 µg/m^3. This suggests that spatial variation in $PM_{2.5}$ may impede the usefulness of WAQS $PM_{2.5}$ concentrations for predicting the $PM_{2.5}$ gap more so than monitor $PM_{2.5}$ concentrations. Next, I consider results in tables 1.A.3 to 1.A.6 that restrict the sample to residence-monitor pairs within fifteen kilometers. For all four of these tables, point estimates from these 355 to 361 residence-monitor pairs are resoundingly similar to corresponding estimates from the full sample in tables 1.4 to 1.7.

In a final step of the intrapersonal analysis, I shed light on three temporal dimensions of the $PM_{2.5}$ gap: i) within-day, ii) day-of-the-week, and iii) seasonal variation. Figure 1.B.7 in appendix 1.B displays the predicted diurnal $PM_{2.5}$ gap after regressing equation 1.1. During the night (11pm to 4am) and in the early morning (7am to 9am), personal exposure is statistically less than as measured at the nearest government monitor. Throughout the day (10am to 6pm) participants are, on average, exposed to concentrations equivalent to ambient pollution levels. In the evenings (7pm to 8pm), however, the $PM_{2.5}$ gap is positive, with individuals exposed to about 1 µg/m^3 of additional pollution compared to the nearest monitor concentration. The figure 1.B.7 in appendix 1.B reveals another important temporal dimension. Participants are slightly more or about as exposed as the nearest monitor measures on weekend days, but on weekdays the exposure gap is negative and statistically significant. Seasonally, figure 1.B.9 shows that there is a considerable statistically significant negative $PM_{2.5}$ gap during summer months, while personal exposure readings are typically higher in the winter month but these differences are not statistically significant.

1.5.3 Interpersonal Differences

In the final step of my analysis, I calculate the predicted $PM_{2.5}$ gap for each of the participants in the study with the aim of identifying individual-level factors which may determine it. Figure 1.B.10 in appendix 1.B depicts the distribution of predicted $PM_{2.5}$ gaps for 479 individuals. Several key insights emerge. First, the mean and median individual-level $PM_{2.5}$ gap diverge. Individuals experience a mean $PM_{2.5}$ gap of +2.7 µg/m^3 and a median $PM_{2.5}$ gap of -2.1 µg/m^3. As with the matched $PM_{2.5}$ data, there is a substantial leftward bias in the distribution of the $PM_{2.5}$ gap, which implies that a substantial share of participants are, on average, less exposed to pollution than ambient concentrations from their nearest government monitor suggest. However, over five percent of participants are exposed to substantially higher average

Table 1.8 shows the results of a model that regresses predicted $PM_{2.5}$ gaps on individual-level characteristics. The point estimates show that there is no statistically-robust evidence for the observed individual-level characteristics affecting the $PM_{2.5}$ gap measured in this study. The only coefficient that is statistically significant individually is on an indicator for high pollution beliefs collected before individuals adopt their WAQS, but an F-test testing for joint significance fails to reject the null hypothesis that all coefficients included in the model are statistically equal to zero. In all likelihood, this study is underpowered for identifying possible differences between groups. The magnitude and sign of several of the estimated relationships do provide some weak suggestive evidence that factors like age, race, income, pollution beliefs, and cardiopulmonary diagnoses may affect the $PM_{2.5}$ gap.

1.6 Discussion

This paper constructs and analyzes a landmark dataset linking ambient monitor concentrations to personal exposure readings collected independently by consumer WAQS adopters. The results of my analysis provide striking insights into the nature and scope of a $PM_{2.5}$ gap between personal and monitored exposure levels. While this study documents the existence of a substantial negative $PM_{2.5}$ gap in the studied population (i.e. self-selecting consumer WAQS adopters), the key implication from this study extends beyond the insight that monitored pollution concentrations may routinely exceed personal exposure while failing to observe high personal exposure levels. The main consequence is that correct estimates of economic damages from air pollution require a more careful consideration of actual exposure and how it systematically deviates from existing secondary data. In other words, economists might be getting key economic relationships (e.g. between exposure and productivity, labor supply, etc.) wrong due to unaccounted factors influencing the $PM_{2.5}$ gap as documented in this paper.

Importantly, my results do not imply that mitigating pollution exposure is any less pressing just because personal exposure is lower than expected from ambient concentrations at the nearest monitor. Future research will need to reassess CRFs, and truly understanding damages will likely require further investment in longitudinal studies. Consumer WAQS data offer a new technological opportunity to advance such efforts in potentially cost-effective ways. However, the relationships measured in the present analysis may already suffice to rescale existing CRFs, and I expect that future analyses will naturally extend in this direction.

Table 1.8: OLS Regressions Results: Participant PM2.5 Gap Predictors

	(1)
Age: 36-55 (=1)	-2.951
	(3.569)
Age: 56+ (=1)	0.0606
	(4.574)
Gender: Male (=1)	0.955
	(3.719)
Race/Ethn.: White alone (=1)	-1.785
	(3.692)
HH Education: Bachelor or higher (=1)	-0.199
	(4.042)
HH Income: Above median (=1)	-4.195
	(3.840)
HH: # Members (count)	2.816
	(1.824)
HH: # Children (count)	-0.819
	(2.595)
HH: # Seniors (count)	-1.144
	(2.755)
AQ Belief: Polluted (Likert: 4/5=1)	-7.584**
	(3.219)
HH: Cardiopulmonary diagnosis (=1)	-3.654
	(3.556)
County: Urban (%)	-9.529
	(8.862)
Residence: Distance to monitor (km)	-0.0746
	(0.101)
Survey Quarter FE	Yes
Region FE	Yes
F-statistic	1.125
R^2	0.05
Participants	479

Note: Dependent variable is the predicted participant hourly $PM_{2.5}$ exposure gap from a regression on participant indicators and hour-of-day, day-of-week, and year-month fixed effects. PM2.5 gap is calculated as the difference from subtracting hourly mean monitor PM2.5 from hourly mean WAQS PM2.5. Standard errors in parentheses. *=p<0.01, **=p<0.05, ***=p<0.01.

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Appendix - Chapter 1

1.A Additional Tables

Table 1.A.1: OLS Regressions Results: Daily PM2.5 Gap (<400µg/m^3)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap
Monitor PM2.5	-0.700***	-0.710***		
	(0.0463)	(0.0467)		
WAQS PM2.5			0.903***	0.912***
			(0.0548)	(0.0500)
Constant	4.727***	4.832***	-9.512***	-9.581***
	(0.475)	(0.480)	(0.428)	(0.390)
Observations	12,442	12,442	12,442	12,442
Users	343	343	343	343
User FE	Yes	Yes	Yes	Yes
Time FE	No	Yes	No	Yes
Mean WAQS PM2.5	7.81	7.81	7.81	7.81
Mean Monitor PM2.5	10.29	10.29	10.29	10.29

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting daily mean monitor PM2.5 from daily mean WAQS PM2.5. Time fixed effects include day-of-week and year-month fixed effects. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

Table 1.A.2: OLS Regressions Results: Daily PM2.5 Gap (<400µg/m^3, <15km)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap
Monitor PM2.5	-0.674***	-0.681***		
	(0.0439)	(0.0423)		
WAQS PM2.5			0.729***	0.751***
			(0.0745)	(0.0680)
Constant	3.951***	4.024***	-8.125***	-8.285***
	(0.443)	(0.426)	(0.539)	(0.493)
Observations	8,018	8,018	8,018	8,018
Users	220	220	220	220
User FE	Yes	Yes	Yes	Yes
Time FE	No	Yes	No	Yes
Mean WAQS PM2.5	7.24	7.24	7.24	7.24
Mean Monitor PM2.5	10.09	10.09	10.09	10.09

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting daily mean monitor PM2.5 from daily mean WAQS PM2.5. Time fixed effects include day-of-week and year-month fixed effects. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

Table 1.A.3: OLS Regression Results: Hourly Monitor PM2.5 Gap (<25µg/m^3, <15km)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap
Monitor PM2.5	-0.768***	-0.768***	-0.786***	-0.813***
	(0.0411)	(0.0410)	(0.0999)	(0.0804)
At Residence=1		0.510	0.331	0.744
		(0.939)	(1.363)	(1.236)
At Residence=1			0.0230	0.0339
× Monitor PM2.5			(0.0957)	(0.0810)
Constant	6.294***	5.883***	6.025***	5.836***
	(0.317)	(0.841)	(1.216)	(1.063)
Observations	170,610	170,610	170,610	170,610
Users	355	355	355	355
User FE	Yes	Yes	Yes	Yes
Time FE	No	No	No	Yes
Mean WAQS PM2.5	8.55	8.55	8.55	8.55
Mean Monitor PM2.5	9.26	9.26	9.26	9.26
R^2	0.23	0.23	0.23	0.23

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting hourly mean monitor PM2.5 from hourly mean WAQS PM2.5. Time fixed effects include hour-of-day, day-of-week, and year-month indicators. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

Table 1.A.4: OLS Regression Results: Hourly WAQS PM2.5 Gap (<25µg/m^3, <15km)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap
WAQS PM2.5	0.311***	0.311***	0.504***	0.541***
	(0.0651)	(0.0653)	(0.0817)	(0.0722)
At Residence=1		-0.0114	1.158*	1.341**
		(0.341)	(0.538)	(0.495)
At Residence=1			-0.253*	-0.253**
× WAQS PM2.5			(0.104)	(0.0963)
Constant	-5.545***	-5.536***	-6.441***	-6.748***
	(0.293)	(0.466)	(0.489)	(0.423)
Observations	168,691	168,691	168,691	168,691
Users	355	355	355	355
User FE	Yes	Yes	Yes	Yes
Time FE	No	No	No	Yes
Mean WAQS PM2.5	8.55	8.55	8.55	8.55
Mean Monitor PM2.5	9.26	9.26	9.26	9.26
R^2	0.14	0.14	0.14	0.17

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting hourly mean monitor PM2.5 from hourly mean WAQS PM2.5. Time fixed effects include hour-of-day, day-of-week, and year-month indicators. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

Table 1.A.5: OLS Regression Results: Hourly Monitor PM2.5 Gap (<400µg/m^3, <15km)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap
Monitor PM2.5	-0.766***	-0.766***	-0.623***	-0.607***
	(0.0570)	(0.0570)	(0.0652)	(0.0657)
At Residence=1		0.433	2.094	2.640*
		(0.933)	(1.216)	(1.339)
At Residence=1 × Monitor PM2.5			-0.176*	-0.176*
			(0.0795)	(0.0761)
Constant	6.382***	6.038***	4.691***	4.100***
	(0.528)	(0.897)	(0.961)	(1.032)
Observations	176,995	176,995	176,995	176,995
Users	361	361	361	361
User FE	Yes	Yes	Yes	Yes
Time FE	No	No	No	Yes
Mean WAQS PM2.5	8.55	8.55	8.55	8.55
Mean Monitor PM2.5	9.26	9.26	9.26	9.26
R^2	0.24	0.24	0.24	0.25

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting hourly mean monitor PM2.5 from hourly mean WAQS PM2.5. Time fixed effects include hour-of-day, day-of-week, and year-month indicators. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

Table 1.A.6: OLS Regression Results: Hourly WAQS PM2.5 Gap (<400µg/m^3, <15km)

	(1) PM2.5 Gap	(2) PM2.5 Gap	(3) PM2.5 Gap	(4) PM2.5 Gap
WAQS PM2.5	0.969***	0.969***	0.914***	0.923***
	(0.0245)	(0.0245)	(0.0309)	(0.0294)
At Residence=1		0.000284	-0.497	-0.349
		(0.320)	(0.449)	(0.410)
At Residence=1 × WAQS PM2.5			0.0620	0.0515
			(0.0348)	(0.0321)
Constant	-8.998***	-8.998***	-8.559***	-8.680***
	(0.209)	(0.342)	(0.388)	(0.364)
Observations	176,995	176,995	176,995	176,995
Users	361	361	361	361
User FE	Yes	Yes	Yes	Yes
Time FE	No	No	No	Yes
Mean WAQS PM2.5	8.55	8.55	8.55	8.55
Mean Monitor PM2.5	9.26	9.26	9.26	9.26
R^2	0.90	0.90	0.90	0.91

Note: Dependent variable is the $PM_{2.5}$ exposure gap calculated as the difference from subtracting hourly mean monitor PM2.5 from hourly mean WAQS PM2.5. Time fixed effects include hour-of-day, day-of-week, and year-month indicators. Robust standard errors clustered on WAQS user in parentheses. *=p<0.05, **=p<0.01, ***=p<0.001.

1.B Additional Graphics

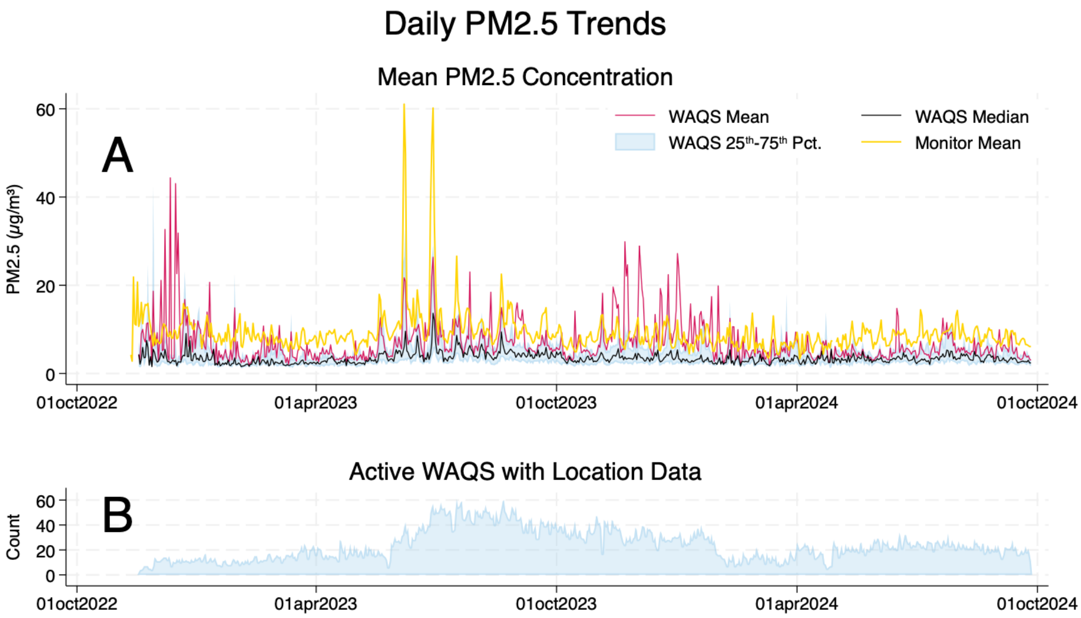

Figure 1.B.1: Daily trends in wearable air quality sensor (WAQS) $PM_{2.5}$ readings. Panel A shows the mean, median, and interquartile range for mean daily $PM_{2.5}$ readings averaged first by user-date-hour, then user-date, and finally across all WAQS active on that date. Panel B displays the total number of WAQS which collect at least one measurement that day.

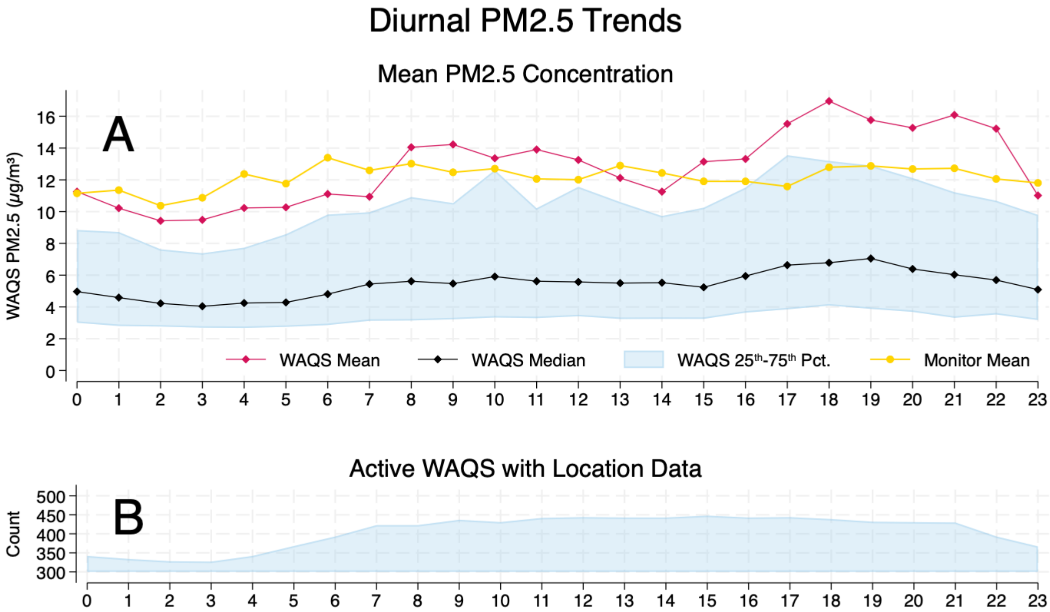

Figure 1.B.2: Diurnal trends in wearable air quality sensor (WAQS) $PM_{2.5}$ readings. Panel A shows the mean, median, and interquartile range for mean $PM_{2.5}$ readings by hour-of-day averaged first by user-date-hour, then user-hour, and finally across all WAQS active each hour-of-day. Panel B displays the total number of WAQS which collect at least one measurement each hour.

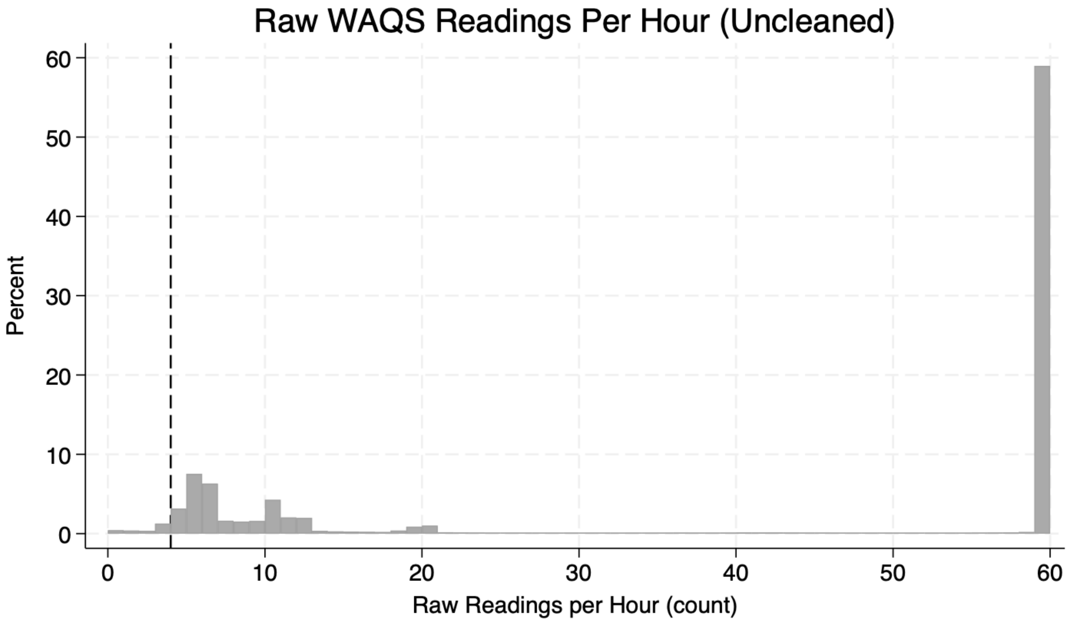

Figure 1.B.3: Raw WAQS readings per hour before data cleaning. Hourly observations with fewer than four readings per hour are removed from the analysis. This threshold is marked with a vertical dashed line.

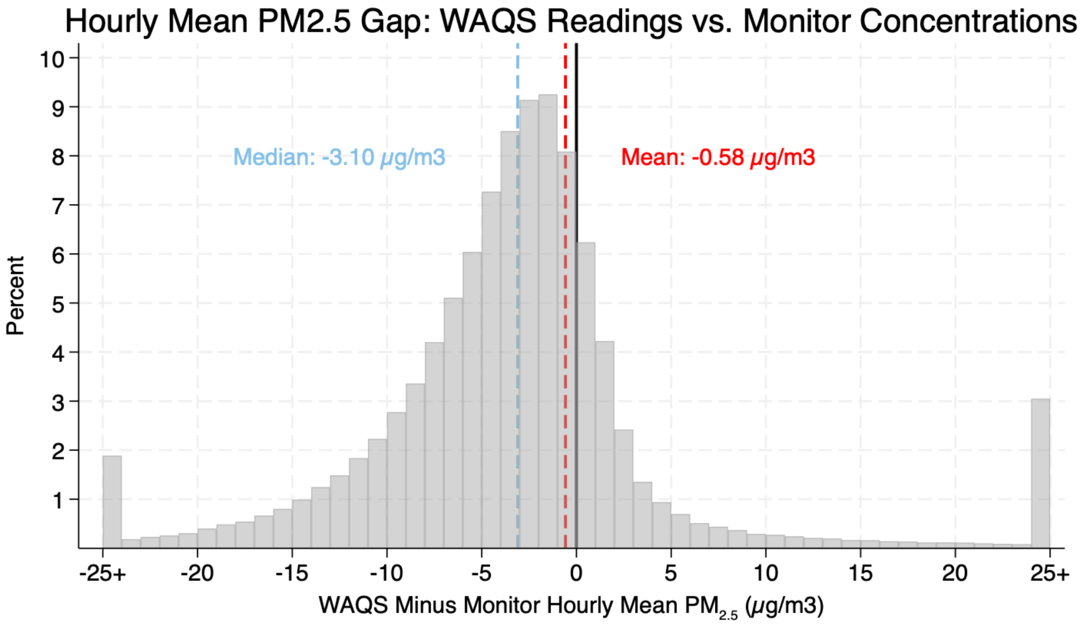

Figure 1.B.4: WAQS users with an active EPA monitor within 100 kilometers included. If none, recorded as missing. A total of 279,638 hourly means from 590 participants were matched to 269,097 hourly readings from 310 monitors.

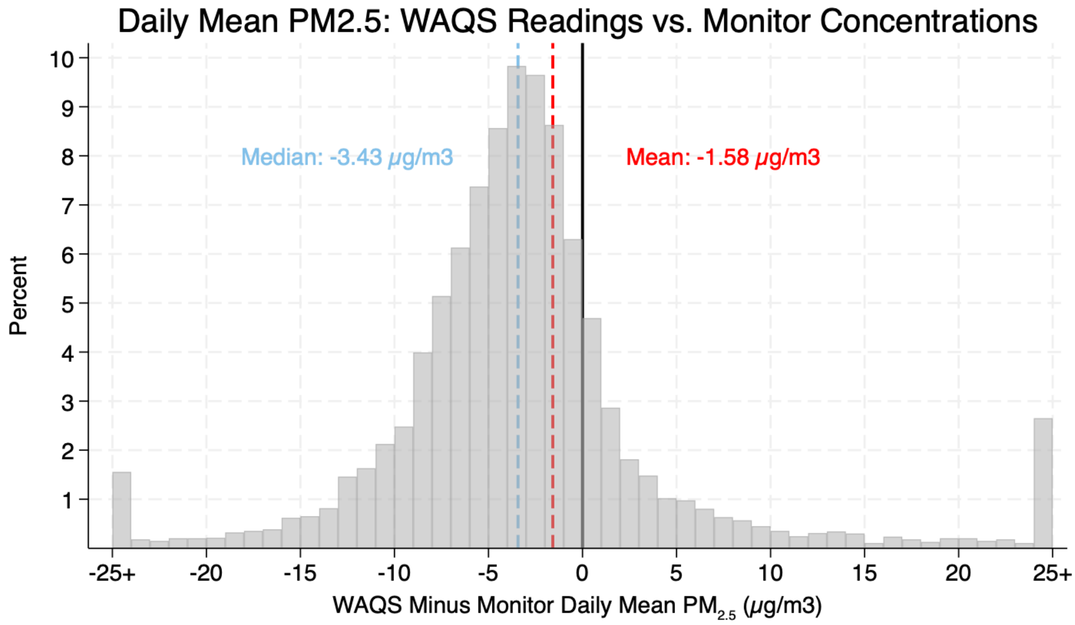

Figure 1.B.5: WAQS users with an active EPA monitor within 100 kilometers included. If none, recorded as missing. A total of 12,476 daily means from 377 participants were matched to 11,365 daily readings from 231 monitors

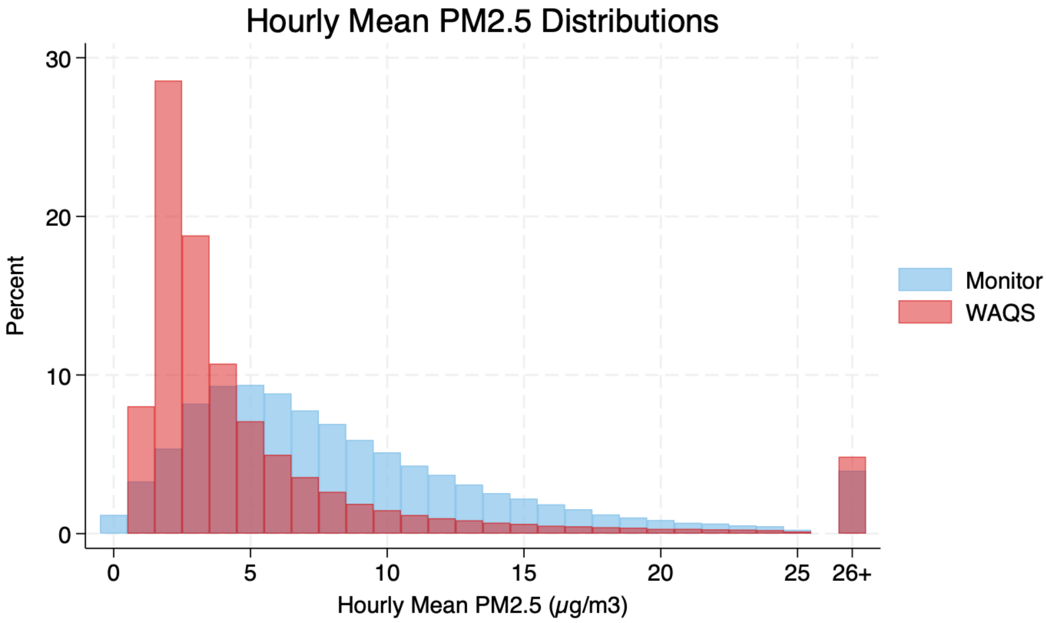

Figure 1.B.6: Hourly Mean $PM_{2.5}$ concentration distributions for all readings before unmatched data is dropped.

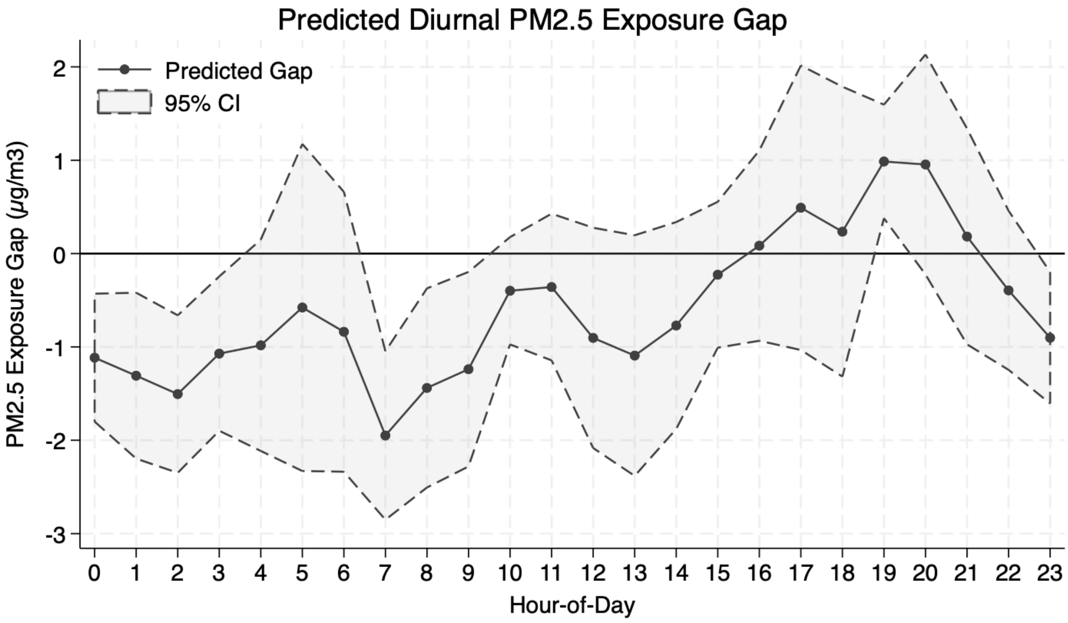

Figure 1.B.7: Predicted $PM_{2.5}$ exposure gap by hour of the day.

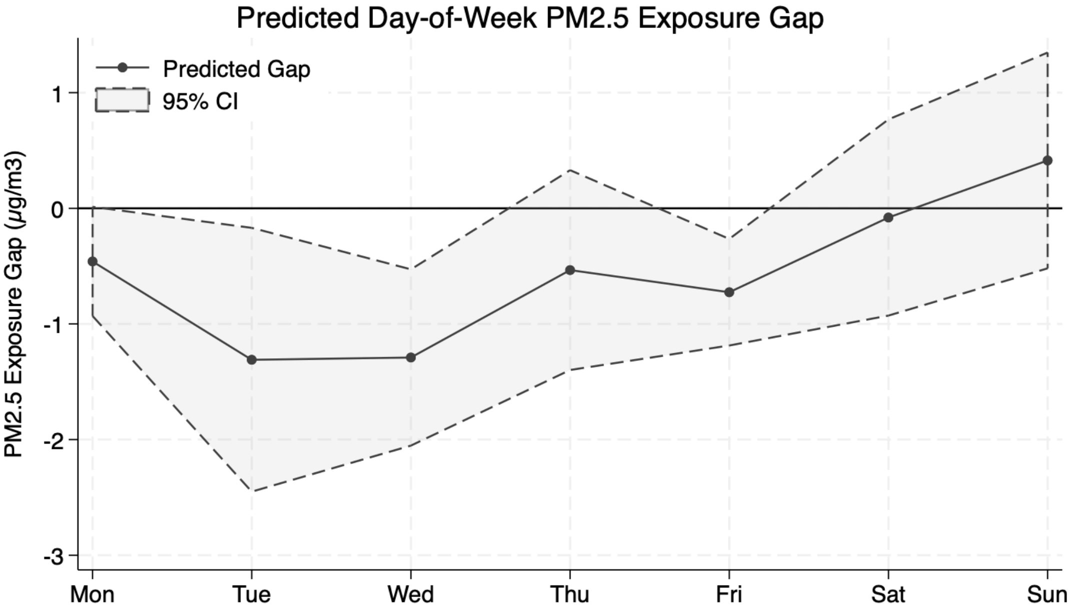

Figure 1.B.8: Predicted $PM_{2.5}$ exposure gap by day of the week.

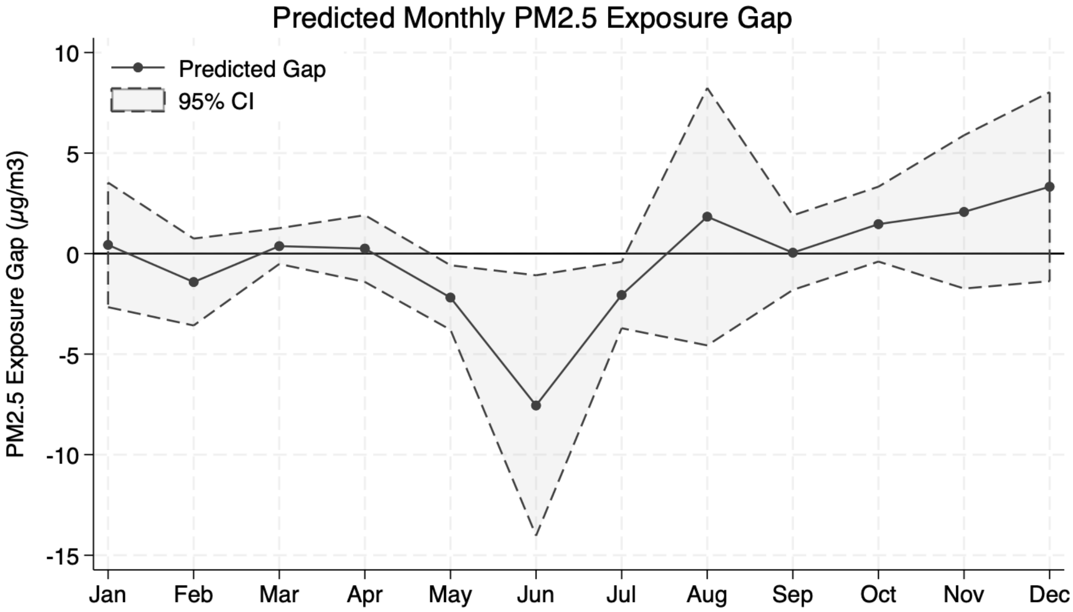

Figure 1.B.9: Predicted $PM_{2.5}$ exposure gap by month of the year.

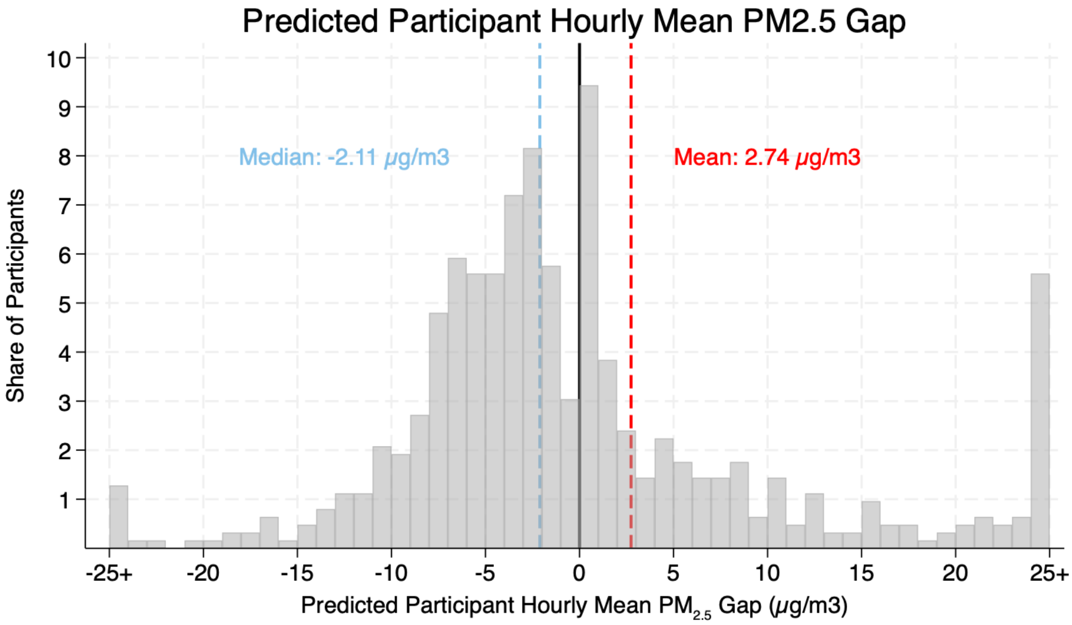

Figure 1.B.10: Histogram of predicted individual level mean $PM_{2.5}$ exposure gaps.

2

Wearable Air Quality Sensor Demand, Use, and Impacts: Field Experimental Evidence from US Early Adopters

Wearable Air Quality Sensor Demand, Use, and Impacts: Field Experimental Evidence from US Early Adopters

Alexander Dangel and Timo Goeschl

Abstract

Wearable air quality sensors enable consumer adopters to track personal real-time pollution levels and could help communities, policy-makers, and researchers better assess human exposure to harmful pollutants. We partner with a leading manufacturer to conduct a field experiment in the United States to study demand, use, and impacts among real-world consumers of this technology. A point-of-sale survey and pricing experiment show that willingness to pay is low compared to market prices, and advantaged groups dominate among those interested in and purchasing sensors. We use naturally occurring pollution variation to show that unhealthy pollution episodes trigger new adoptions. While detailed statistics document a substantial decline in user activity in the months after adoption, pollution episodes also lead to upticks in user activity. Follow-up data suggests that adopters believe they are exposed to less pollution and seek air quality information less from other sources but do not change how often they take defensive action. Our results have implications for air quality awareness and sensor deployment initiatives, pollution monitoring systems, and for the question of equitable access to environmental information.

Keywords: air pollution exposure; information; sensors; monitoring; field experiment; willingness to pay, demand, adaptation

JEL Classification: D63, D91, Q50, Q53

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2.1 Introduction

Governments have long been tasked with collecting and publicly disclosing air quality (AQ) information for the benefit of their citizens using sparse networks of highly accurate but expensive ground-based monitors. Policy-makers and researchers rely to a great extent on this data for assessing human exposure to pollution and its damages, raising public awareness, and informing the design and enforcement of environmental regulations. Today, individuals can purchase wearable AQ sensors (WAQS) to accurately monitor pollutants independently. The personal, real-time data they collect appears well-suited to impact individual beliefs and pollution-related outcomes. Moreover, consumer WAQS data could revolutionize exposure assessments if made available to policymakers and researchers. However, real-world demand for this novel technology, how individuals actually use it, and its impacts on adopters are not yet well understood.

In this study, we partner with a leading WAQS manufacturer to conduct a field experiment to investigate the demand for, use of, and impact of a portable smartphone-connected sensor that tracks real-time particulate matter (PM) and volatile organic compound (VOC) exposure. Our study implements a point-of-sale intervention on 1,784 prospective WAQS customers in the United States that is designed to link individual-level socioeconomic characteristics with willingness-to-pay (WTP) estimates, actual purchase decisions, user activity statistics, and information about pollution perceptions and related behaviors. At the point of intervention, we implement two randomized treatments. First, we randomly assign participants to one of two commonly employed ex ante product valuation techniques. Second, we assign participants to receive an offer to purchase a WAQS at one of six randomly-drawn prices ranging from zero (free) to an minimum discount price ($149). During our campaign, 47% of prospective customers (n=829) ultimately accept their offer and adopt a WAQS. Approximately 79% of adopters (n=658) opt into data sharing, enabling us to observe i) granular sensor activity, ii) timestamped and geolocated AQ readings, and iii) daily engagement statistics from an accompanying smartphone application (app). In an endline survey, 369 participants (21% of all baseline participants) report changes in pollution-related outcomes since the initial intervention.

Our study contributes four main results to the literature. First, to the best of our knowledge, our study is the first to value perceived benefits from WAQS adoption in a real-world setting and to link these valuations to actual purchasing decisions at randomized prices, contributing to an emerging economics literature valuing AQ information (Hanna et al., 2021; Ahmad et al., 2022; Imtiaz et al., 2023; Barwick et al., 2022). In our study, mean ex ante willingness-to-pay (WTP) is about $98, which is low compared to WAQS market prices ($179) and contrasts in absolute terms with valuations for one year of i) real-time text message air quality alerts in Mexico ($2.83, Hanna et al., 2021) or ii) day-ahead air pollution forecasts in Pakistan ($5.13, Imtiaz et al., 2023; $1.34, Ahmad et al., 2022).[1] We also contribute methodologically to a growing literature on product valuations in the field (Berry et al., 2020; Cole et al., 2020) by showing that subjects' stated, non-incentivized ex ante valuations closely predict actual take-it-or-leave-it (TIOLI) conversion rates and that adopters report ex post valuations just $8.30 lower after having used the device ($111.47).

Second, we believe our study is the first to collect data on WAQS customers' individual-level socioeconomic characteristics in a real-world purchasing scenario. We show that advantaged socioeconomic groups dominate WAQS demand, while disadvantaged socioeconomic groups, who are most vulnerable to risks from air pollution exposure (Hsiang et al., 2019), are significantly under-represented. Participants in our study are predominantly White (72%), male (76%), between the ages of 26 and 45 (55%), above median household income (69%), and highly educated (78% with at least a bachelor's degree), and WAQS take-up further reinforces these disparities. Our results validate and expand upon previous findings that document clusters of private stationary air quality sensor (SAQS) adoptions in areas with a greater population of White residents and higher socioeconomic status in the US (deSouza and Kinney, 2021; Mullen et al., 2022; Burke et al., 2022; Coury et al., 2024; Zivin et al., 2024). We then exploit natural fluctuations in ambient air pollution levels between and within US counties to show that WAQS adoptions increase between 76% and 247% over baseline levels during unhealthy air pollution

[1] In relative terms, our WTP estimate is 0.13% of average US per capita income compared to 0.02% in Mexico (Hanna et al., 2021) and 0.3% (Imtiaz et al., 2023) and 0.08% (Ahmad et al., 2022) in Pakistan.

episodes, contributing to an emerging economics literature evaluating the contemporaneous determinants of private AQ monitoring (Coury et al., 2024; Zivin et al., 2024)

Third, unlike previous WAQS studies from other research fields that recruit volunteer participants, enforce wearing protocols, or track volunteers for limited time spans (see Lim et al. (2022) for a systematic review of the literature), we design our study to capture natural long-term user activity in the real world. We partner with a commercial manufacturer, leverage a common point-of-sale intervention with real prospective customers, and minimize subsequent interactions with subjects. Our approach to studying real-world WAQS use has a clear advantage over previous findings that come from small-scale, qualitative studies that distribute sensors to volunteer participants (Heydon and Chakraborty, 2020; Bales et al., 2019; Oltra et al., 2017), and, to our knowledge, have never before studied WAQS user activity in subjects who purchase their own devices. The WAQS data we collect from participants via an application programming interface (API) shows that sensor activity drops off from an average of 58% of days in the first month to about 17% in the sixth month, and accompanying app engagement data corroborate these trends. Furthermore, we once again employ naturally-occurring pollution variation to show that harmful ambient air pollution episodes lead to contemporaneous increases in user activity, with daily sensor and app activity rates increasing by over 25% in weeks with "unhealthy" pollution levels compared to baseline levels. Endline survey data point to learning about AQ as a possible mechanism explaining WAQS use. Overall, our results resonate with findings from Delmas and Kohli (2019), who demonstrate that engagement with a smartphone app that provides real-time population-level ambient AQ information tapers off quickly.

Fourth, we provide evidence suggesting WAQS adoption causally impacts pollution perceptions and AQ information seeking but not the frequency of defensive actions. Endline survey outcomes show that adoption increases adopters' likelihoods of perceiving less pollution by 47% and checking alternative AQ information sources less often by 29%. Notably, adoption had no impact on the number of defensive purchases adopters make or on the likelihood that they take defensive actions or reduce polluting indoor behaviors. Our impact results are closely related to Greenstone et al. (2021), who find that indoor SAQS adoption does not affect defensive investments or actions in households in Delhi, India, and Roth and Metcalfe (2024), who find that indoor SAQS adoption increases defensive behaviors (e.g. ventilation) in a study with households in London, UK. More generally, these findings contribute novel evidence about personal AQ information to a larger economics literature on the relationship between AQ information and exposure perceptions (Hanna et al., 2021; Oberholzer-Gee and Mitsunari, 2006), pollution avoidance (Fan, 2024; Gao et al., 2023; Janke, 2014; Neidell, 2009), and defensive expenditures (Ahmad et al., 2022; Wang et al., 2021).

The remainder of this paper is laid out in the following manner. Section 2.2 provides background information on pollution exposure assessment and related literature. Section 2.3 describes our study design and data. Section 2.4 explains our estimation approach and discusses our experiment's validity. Section 2.5 details our results in three subsections corresponding to i) demand, ii) use, and iii) impacts. Section 2.6 concludes with a discussion of our results.

2.2 Background

At its core, this study analyzes from an economic standpoint a consumer technology which environmental health experts consider a "gold standard" for assessing personal pollution exposure (Lim et al., 2022). In studying a commercially-available WAQS technology, we relate research from a wide range of disciplines examining links between air pollution exposure and human well-being to epidemiological assessments of personal pollution exposure, qualitative studies exploring the social science aspects of wearable pollution sensors, and economic research on pollution adaptation. Moreover, we test our research hypotheses with quantitative field experimental methods from economics (Harrison and List, 2004) with the goal of advancing our understanding of potential WAQS applications to real-world policy and research settings. In the remainder of this section, we briefly summarize the research domains most closely tied to our study.

Previous studies from epidemiologists, public health researchers, social scientists, and others find overwhelming evidence that airborne pollution exposure has substantial individual and social costs that disproportionately burden disadvantaged and vulnerable populations (Aguilar-Gomez et al., 2022; Rajagopalan et al., 2020). Economists contribute to this body of evidence by studying social welfare and distributional impacts of government pollution policies (Currie et al., 2023; Aldy et al., 2022; Hsiang et al., 2019), measuring causal relationships between exposure, health, and economic outcomes (Künn et al., 2023; Deryugina et al., 2019; Chang et al., 2016; Hanna and Oliva, 2015; Neidell, 2004), and evaluating defensive responses to pollution information programs (Fan, 2024; Barwick et al., 2022; Saberian et al., 2017; Sexton Ward and Beatty, 2016; Graff Zivin and Neidell, 2009). To conduct these analyses, economic and social science research largely relies on modeled, population-level pollution exposure estimates based on data from either sparse networks of stationary regulatory monitors or satellite remote sensing (Cain et al., 2024). While recent methodological developments have strengthened empirical findings, researchers point to non-classical measurement error and the ecological fallacy more generally as two reasons why employing modeled exposure estimates as a proxy for actual exposure threatens the validity of previous results (Cain et al., 2024; Jain, 2020; Fowlie et al., 2019).

In the last decades, however, technological innovations have driven down environmental sensor prices. The commodified collection and disclosure of AQ information through commercially available, low-cost air quality sensors (Snyder et al., 2013) has opened the door for consumers, policy-makers, and researchers to consider incorporating more granular exposure readings into how they assess pollution. These sensors can be broadly classified into two types: i) *fixed* SAQSes, which are intended to be installed at a single indoor or outdoor location (e.g. homes, workplaces, schools, and other locations), and ii) *portable* WAQS, which individuals carry to track personal pollution exposure wherever they go. Economic researchers recently began including SAQS data in study designs (Künn et al., 2023; Imtiaz et al., 2023; Adhvaryu et al., 2022) and more closely studying SAQS technologies (Coury et al., 2024; Zivin et al., 2024; Roth and Metcalfe, 2024; Dangel and Goeschl, 2023; Krebs et al., 2021; Greenstone et al., 2021) but have never before incorporated WAQS data or analysed WAQS technologies previous to this study. Past empirical research into natural SAQS deployment has largely relied on publicly available installation geocoordinates to match adoption locations with census block-level socioeconomic statistics and has not linked natural adoptions to individual-level characteristics (Coury et al., 2024; Zivin et al., 2024; Burke et al., 2021; deSouza and Kinney, 2021; Mullen et al., 2022). Three notable economic field experiments deploy indoor SAQS to volunteer participants to measure their impact on defensive actions (Greenstone et al., 2021; Roth and Metcalfe, 2024) or rely on data from previously-installed outdoor SAQSes to quantify the effect of this AQ information source on pollution beliefs (Imtiaz et al., 2023).

For consumers, policy-makers, and researchers, there are a fundamental differences between measuring personal exposure with WAQS technologies versus using government-monitored ambient concentrations as an exposure proxy. Steinle et al. (2013) and Lim et al. (2022) concisely review previous research, methods, and results on personal exposure assessment and highlight the differences between personal and ambient exposure measurements. A companion paper (Dangel, 2024) contributes empirically to this literature on a "personal-ambient $PM_{2.5}$ exposure gap" by comparing WAQS readings collected by participants from this field experiment with ambient concentrations measured at nearby regulatory monitors.[2] Here we briefly summarize two important findings. A first key insight is that the median difference between $PM_{2.5}$ pollution readings and ambient concentrations is between 3.1 and 3.4 µg/m^3 (over 33% of the annual EPA $PM_{2.5}$ standard), suggesting that participants in our study are, for the majority of the time that they use their devices, significantly less exposed to pollution than regulatory monitor data suggests.[3] However, a second key insight is that a small share of high WAQS readings ($PM_{2.5}$ > 25 µg/m^3) drive up the overall average, so that average personal pollution readings are only about 0.6 µg/m^3 less than concentrations at nearby

[2]In this article, we focus on measuring WAQS demand, use, and impacts and do not perform a detailed analysis of participants' WAQS pollution data.

[3]This first insight shows that WAQS data may be useful to adopters because they provide informational gains over the status quo (government monitoring), in particular because individuals spend most of their time in micro-environments that are not monitored by regulatory pollution monitors. For example, individuals in western countries spend over 90% of their time each day in indoor micro-environments, and for some criteria air pollutants, like PM, indoor concentrations do not necessarily correlate with outdoor concentrations. Indoor spaces can have different pollution sources and outdoor-indoor penetration rates can vary widely (Greenstone et al., 2021; Krebs et al., 2021; Burke et al., 2022). Furthermore, it is well-established that air pollutants can vary outdoors to a high degree both spatially and temporally, meaning that ambient exposure estimates collected at a regulatory monitoring site may not be representative of concurrent exposure in an outdoor micro-environment nearby (Apte et al., 2017; Miller et al., 2020; Wang et al., 2021).

government monitors. Questions about existing pollution damage estimates follow naturally. However, it is unclear at this point to what extent these results are specific to our sample of individuals naturally interested in monitoring pollution exposure and to what extent they can be generalized to the broader population.

While other economists have yet to study WAQS and social scientists have not rigorously studied them with quantitative methods (Hubbell et al., 2018) or in real-world consumer settings, studies with small participant groups, often targeting specific sub-populations, do provide some limited, preliminary evidence. Qualitative social science studies survey and interview volunteer WAQS users to identify how use affects pollution perceptions and behavioral responses. For example, Heydon and Chakraborty (2020), who distribute sensors to the parents of 45 school children in Sheffield, UK for a period of 2 weeks, observe that WAQS use increases or confirms air pollution concerns, raises pollution awareness, and improves pollution source identification, but leads some participants to express feelings of powerlessness, loss of control over their environments, and resignation when behavioral adaptations (e.g. changing routes to school, etc.) are ineffective in reducing WAQS exposure readings. Qualitative findings from other settings (Bales et al., 2019; Oltra et al., 2017) draw similar conclusions showing increased pollution awareness, but also highlight large heterogeneities in impacts, which they suggest may be related to differences in baseline exposure and socioeconomic differences between participants.

Personal exposure assessments with wearable devices are well-established in epidemiology (Steinle et al., 2013), but researchers rarely deploy WAQS in large scale studies. A systematic literature review from Lim et al. (2022) shows that just 32 of 273 reviewed personal exposure assessment studies (11%) recruit over 100 participants to carry a WAQS and only eleven studies (4%) involve over 200 participants. The sample size of our study, with 658 participants reporting WAQS readings, exceeds all but one study reviewed by Lim et al. (2022) with 1,330 participants.[4] Furthermore, epidemiological studies typically ask participants to measure personal exposure for very short sampling periods and may not provide

[4]For context, Rylance et al. (2020) conducted a longitudinal study measuring respiratory health and two-day personal pollution exposure for 1,330 participants in rural Malawi on three separate occasions over a three year period.

real-time feedback on exposure readings to participants. On average, the studies reviewed in Lim et al. (2022) average less than a five-day sampling duration and span at most a four-week-long sampling period.[5] Limitations in terms of study size and duration are likely linked to research budget constraints, development costs for bespoke WAQS technologies, and difficulties recruiting volunteer participants. Our study's cooperation with a commercial sensor manufacturer enables us to achieve participation levels that are at least an order of magnitude larger than previous studies and track personal exposure for a considerably longer duration.

To conclude this section, we highlight two main ways in which we believe consumers can benefit from WAQS adoption through our study. First, because adopters can use their sensors to track AQ wherever they go, including indoors and other areas without government AQ monitoring coverage,[6] we expect participants to update their pollution exposure beliefs after adoption. However, it is not clear ex ante whether adopters will update their pollution perceptions upward or downward as this likely depends on who adopts, how high their baseline exposure is, and where they choose to use their devices. Second, adopters can employ WAQS to test whether pollution-related adaptations effectively reduce personal exposure in real-time. This suggests WAQS adopters could use the technology to identify new defensive strategies and expand their efforts to protect themselves from air pollution.[7] It is unclear, however, whether participants in our study will be able to successfully identify effective pollution-related interventions and whether they will be willing to bear the economic and behavioral costs of these adaptations if they do.

[5]Lim et al. (2022) only consider studies that sampled personal exposure for a minimum of 20 hours.

[6]Previous research indicates that even in places with significant government AQ monitor coverage, personal exposure can be weakly correlated with population-level estimates because individuals move through highly variable outdoor air and diverse indoor spaces (Apte et al., 2017; Miller et al., 2020; Wang et al., 2021; Ashmore and Dimitroulopoulou, 2009).

[7]For example, it is plausible that WAQS users become more aware of air pollution sources in their environments and use this information to adapt to pollution levels. For example, short-term defensive actions include moving indoors during ambient pollution episodes, wearing a protective face mask, or ventilating. Long-term adaptations include installing an air purifier or moving to a new home.

2.3 Study Design

We design a field intervention to learn about WAQS demand, use, and impacts in a real-world setting. To facilitate deployment and generate meaningful results, we partner with Atmotech, Inc. (ATMO), a leading US-based WAQS manufacturer that sells their products directly to consumers online. Our field study begins with a point-of-sale intervention in ATMO's online store and consists of four main stages: i) an online store pop-up and incentivized baseline survey, ii) a pricing experiment leading to real WAQS transactions, iii) adopters' voluntary WAQS use, and iv) an incentivized endline survey.

The following subsection details the technical attributes of the WAQS we use for our study and its accompanying app. We then provide an overview of our field intervention and describe the data we collect in the baseline survey, on customer transactions, from participants' sensors and apps, and in the endline survey. Each data subsection provides summary statistics and attrition information.

2.3.1 Wearable Air Quality Sensor and App

For our study, we use the Atmotube Pro WAQS, which retails at between \$179 and \$249 during our intervention. This handheld device can be worn as an accessory or attached to a bag and measures particulate matter ($PM_{10}/PM_{2.5}/PM_1$), volatile organic compounds (VOC), temperature, relative humidity, and atmospheric pressure. PM and VOC are measured at default frequencies of once per second and once every two seconds, respectively, and are stored to memory every minute.[8] An indicator light on the front of the device displays one of five colors corresponding to the current air quality score (AQS), an open-source index that combines PM and VOC readings into a single parameter with corresponding health

[8]Independent sensor accuracy tests document high correlation between Atmotube Pro PM measurements and reference instrument readings, even under varying temperature and humidity conditions. For details, see South Coast Air Quality Monitoring District's Air Quality Sensor Performance Evaluation Center's Atmotube Pro Summary Report at http://www.aqmd.gov/docs/default-source/aq-spec/summary/atmotube-pro—summary-report.pdf?sfvrsn=8 and the UK Environment Agency's Monitoring Certification Scheme (MCERTS) report on Atmotube Pro's Sensiron SPS30 particulate matter sensor: https://www.csagroup.org/wp-content/uploads/MC-20035001.pdf.

recommendations.[9] AQ data is stored on internal device memory and visualized in the accompanying Atmotube app whenever the device is synchronized with a smartphone. The device's battery life is about ten days with default settings.

The accompanying smartphone app is available to download for free in the iOS and Android app stores. In the app, users have access to detailed sensor data and an air pollution mapping feature. The app saves and displays a complete history of environmental data collected by the device, including current readings as well as previous hourly, daily, weekly, and monthly averages for all parameters (figure 2.D.3 in appendix 2.D). Users can also opt into air pollution mapping by giving the app location services permissions. The device must be connected to the user's smartphone via Bluetooth at the time of data collection to match readings with the smartphone's GPS coordinates. In addition to seeing their own air pollution readings plotted in a "personal" air pollution map, users can opt in to contribute their data to a "global" map where they can also see other users' readings (figure 2.D.4 and figure 2.D.5 in appendix 2.D).

2.3.2 Online Intervention and Baseline Survey

Our field study intervenes when prospective WAQS customers arrive on ATMO's website. On the landing page, we inform them of a discount opportunity and its conditions via a pop-up.[10] Visitors learn that they can receive a 30% discount on Atmotube Pro (40% off during an initial period from November 2022 through January 2023) in exchange for taking a "short survey" (figure 2.C.1 in Appendix 2.C). In addition, various banners on other pages across ATMO's website point users to take the survey (figure 2.C.3 in Appendix 2.C). Clicking on any one of these pop-ups or banners takes visitors to a campaign-specific product page where they see a more detailed offer banner (figure 2.C.2 in Appendix 2.C) and can learn

[9] The Air Quality Score (AQS) is calculated according to a publicly-available algorithm at https://atmotube.com/atmotube-support/what-is-air-quality-score-aqs. AQS ranges from 0 (severely polluted) to 100 (very clean). Each quintile (0-20, 21-40, etc.) corresponds to a different device indicator light color and health recommendations. See figure 2.D.2 in appendix 2.D.

[10] ATMO products, and AQ sensors in general, are sold directly to consumers on the Internet and not in brick-and-mortar stores, so we believe that the landing page is a meaningful point of intervention.

more about device features and technical specifications.

Interested visitors can then fill in a survey in exchange for an instantly redeemable discount code for Atmotube Pro. Clicking on the “take the survey” button on the detailed offer banner takes them to our Qualtrics survey (figure 2.C.4 in appendix 2.C). At this point, we inform them that they will receive a discount on Atmotube Pro for completing the survey and that a limited number of devices will be made available for free. We also explain to them that they will be participating in a research study if they choose to complete the survey and accept its terms and, if they do so, they agree to make their AQ sensor data available for research purposes. We inform them that they are free to opt-out of sharing their data at any point. Finally, we explain that they must be 18 years of age or older and agree to the privacy terms in order to participate and that households may only participate once. Upon survey completion, each respondent receives a unique 16-character Atmotube Pro discount code and information on its value in $USD and as a percentage of the retail price (figure 2.C.5 in appendix 2.C).

Our baseline survey consists of fifteen questions split into five sections: i) personal characteristics, ii) socioeconomic background, iii) air pollution perceptions, iv) product valuation, and v) contact information. Before starting the survey, survey respondents see a survey landing page with study information, must complete a Captcha puzzle to confirm that they are not a bot (survey page 1), and must agree to participation terms to continue (survey page 2). We then ask respondents for the following information: age group, gender, race/ethnicity, household size, number of children in household (younger than 18), number of seniors in household (over 65), if someone in the household has been diagnosed with cardiovascular or lung disease, highest education level in the household, annual pre-tax household income, whether they consider their air “polluted” (Likert scale), AQ information sources, Atmotube Pro valuation (see following subsection), valuation of a monitoring incentive program, email address, and, optionally, phone number. For details on wording, response type, and additional answer options, see figure 2.F.5 in appendix 2.E.

Of 2,581 total responses, 2,184 respondents (84.6%) fully complete the baseline survey and 397 (15.4%) do not finish it. The majority of respondents who do not complete the survey leave the survey on the consent page (6.9% of all respondents) or on the first question page (age question, 1.4% of all respondents, see figure 2.I.2 in appendix 2.I). A relatively large share of participants also do not advance past valuation and contact info questions (in total 3.4% of all respondents). All other questions have attrition rates less than 0.5%. Further, we identify 400 repeat submissions from households because they receive the same discount code from our assignment algorithm.[11] We remove these duplicate submissions from our data set and retain each duplicate household's first submission, leaving 1,784 unique and complete survey responses. Among complete responses, the median survey completion time is 4 minutes and 15 seconds.

In addition to our field intervention, ATMO conducts digital marketing to drive traffic to their online store (e.g. paid promotions in Youtube videos, newsletters, etc.). For each survey respondent who arrives on ATMO's website from one of these sources, our survey automatically records the source. We can thereby distinguish whether prospective WAQS adopters arrive on our survey from organic traffic (e.g. online search) or from one of ATMO's marketing interventions. Figure 2.I.1 gives an overview of these events and the share of traffic that we can link to each source. ATMO also runs three independent sales campaigns during our intervention that take precedence over our survey campaign. During ATMO's 2022 Black Friday sale (November 23, 2022 to November 30, 2022), 2022 holiday sale (December 13, 2022 to January 3, 2023), and 2023 fall sale (September 6, 2023 to September 14, 2023), our intervention's pop-ups and banners do not appear, so we do not receive new survey responses during these periods.

Sensor Valuation Type Treatment

Our intervention's first experimental treatment is the random assignment of one of two WAQS valuation questions as the twelfth survey question. Half of respondents (n=892, 50%) see an open-ended hypothetical WTP question that asks respondents how much they are willing to pay for Atmotube Pro using a numerical input box. We phrase the question so that respondents do not anchor their valuations to a specific price.

The second half of respondents (n=892, 50%) see a series of multiple price list

[11] The discount code assignment algorithm is based on Qualtrics geoIP and duplicate detection.

(MPL) questions, first asking if they would be willing to pay \$50 for Atmotube Pro. They can respond either "Yes" or "No." If they respond in the affirmative for \$50, they are asked in exactly the same way if they would be willing to pay \$100 for Atmotube Pro. The valuation amount increases by \$50 each time the respondent answers "Yes" until \$250. If they respond "Yes" to \$250, the series of sensor valuation questions ends. If a respondent responds "No" to any one of the questions, they are then asked the same question but for \$25 less than the preceding question. If they respond "Yes", the sensor valuation questions ends. If they again respond "No", they are once again asked the same question but for \$25 less than the preceding question (\$50 less than the initial valuation they responded "No" to). The sensor valuation ends with their response after two consecutive "No" responses. This MPL procedure allows us to evaluate the respondent's sensor valuation in \$25 increments on the range from \$0 to \$250.

In both sensor valuation type treatment groups, we frame the question using a cheap talk design (Cummings and Taylor, 1999) by including the statement: "Previous participants have overstated their willingness to pay. Please do not." This design has previously been shown to successfully reduce hypothetical bias. It is plausible that respondents still anchor their responses on previously acquired information or may be affected by other sources of bias. For example, we do not observe whether respondents see the Atmotube Pro retail price on the full product page, conduct AQ sensor price comparisons on the Internet beforehand, or are otherwise naturally exposed to price information (e.g. social interactions). In our intervention, we focus on implementing an economical survey that minimizes survey response time and our demands on respondents, so we do not implement a more cognitively-demanding, incentive-compatible WTP elicitation design like the Becker-Degroot-Marschack method. We cautiously interpret respondents' stated hypothetical valuations in isolation and compare them with incentive-compatible purchase decisions at randomized prices in a subsequent step.

Price Treatment

Our second treatment assigns a random Atmotube Pro offer price in the form of a personalized discount code to respondents who finish the survey at one of

Table 2.3.1: Willingness to Pay and Treatments

	Full Sample		Adopters		Non-Adopters		A-NA
	(1)		(2)		(3)		(4)
	Mean	N	Mean	N	Mean	N	Diff. t-test
Panel A: WTP Elicitation							
Overall ($)	97.75	1,784	110.38	829	86.79	955	23.59**
Open-ended ($)	111.43	892	115.72	422	107.58	470	8.14
Multiple Price List ($)	84.08	892	104.85	407	66.65	485	38.20***
Open-ended (0/1)	0.50	892	0.51	422	0.49	470	0.02
Multiple Price List (0/1)	0.50	892	0.49	407	0.51	485	-0.02
Panel B: Offer Price							
Free (0/1)	0.06	104	0.12	100	0.00	4	0.12***
$50 (0/1)	0.19	344	0.31	254	0.09	90	0.21***
$79 (0/1)	0.01	20	0.01	7	0.01	13	-0.01
$100 (0/1)	0.19	334	0.19	161	0.18	173	0.01
$125 (0/1)	0.45	806	0.31	259	0.57	547	-0.26***
$149 (0/1)	0.10	174	0.06	47	0.13	127	-0.08***
$160 (0/1)	0.00	2	0.00	1	0.00	1	0.00
Observations	1,784		829		955		1,784

Note: Column (4) reports the results from a two-sample t-test statistics comparing means from adopters (2) and non-adopters (3). T-test significance levels: * for $p<0.1$, ** for $p<0.05$, and *** for $p<0.01$.

five main price levels: \$149, \$125, \$100, \$50, and \$0 (free).[12] We choose these price levels because they span the range from free to a minimum discount on the retail price and are situated at potentially sales-relevant levels. Due to budget constraints, assignment protocols, and changes in price levels during the campaign, discount codes are not distributed with equal likelihood across all prices (see table 2.3.1, panel B1). However, participants do not know about the underlying price distribution. Free (\$0) discount codes are, for example, dispatched at most once per calendar day.

2.3.3 Transactions

Once a respondent receives a discount code, they can either follow a link on the final survey page to the ATMO store or independently go to the store and apply the discount code. They receive their discount code, survey responses, and participation agreement via email, so that they can make their purchase later.[13] Shipping is free. For each discount code, ATMO communicates to us whether this discount code was used to purchase an Atmotube Pro or not. Of the 1,784 unique respondents who complete the survey and receive a discount code, 829 (46.5%) ultimately purchase one using their personalized discount code. The unique discount codes later enable us to link individual survey responses to purchase decisions.

Participants who purchase an Atmotube Pro with a discount code receive it by mail and can start using it at will. The typical delivery time is less than a week but may vary depending on shipping location and date. Device packaging is identical to what regular customers receive and instructs customers to download the ATMO smartphone app to access their WAQS data. We use a unique device ID collected before shipping to link WAQS data to each transaction and thereby to individual survey responses. If the user opts-in to sharing their data with Atmotube Cloud in the accompanying app, we can observe uploaded WAQS data via an API.

[12] At the beginning of our study, a sixth (\$79) and seventh price (\$160) were offered to just 20 and two respondents, respectively.

[13] Participants who had not redeemed their discount codes by August 2023 were notified then that their discount codes would expire by September 2024. A limited number of participants purchased one after being reminded.

2.3.4 Sensor Readings

Table 2.3.2: Sensor Readings

	Users	Obs.	Mean	SD	Min	1st	99th	Max
AQS	658	71,458,080	82.80	14.2	0	29	98	100
PM_1 (µg/m^3)	655	49,129,278	5.40	26.4	1	1	66	1,000
$PM_{2.5}$ (µg/m^3)	655	49,129,278	6.83	28.7	1	1	74	1,000
PM_{10} (µg/m^3)	655	49,129,278	8.21	30.2	2	2	79	1,000
VOC (ppm)	658	71,451,726	0.36	1.0	0	0	2.3	60
Temperature (C)	654	69,939,677	22.23	3.5	-125	11	31	127
Humidity (%)	654	69,939,675	43.02	11.3	1	18	77	100
Pressure (mbar)	658	71,454,479	985.21	46.0	588	797	1.0e+03	1,054.78
Geocoordinates (0/1)	658	71,458,554	0.23	0.4	0	0	1	1

Note: Air Quality Score (AQS) is a proprietary, open-source index developed by ATMO that evaluates air quality on a scale from 0 (polluted) to 100 (clean) combining particulate matter (PM) and volatile organic compound (VOC) readings.

Of 829 WAQS adopters in our study, we observe 658 users' (79%) WAQS readings (PM, VOC, temperature, pressure, etc.) through August 2024 via the Atmotube Cloud API.[14] In total, table 2.3.2 shows that adopters collected over 71,000,000 timestamped readings (each consisting of an array of different component readings) from November 2022 through April 2024. Of all logged statistics, 100% contain a VOC reading, 98% contain a temperature and humidity reading, and 69% have PM1, $PM_{2.5}$, and PM_{10} readings.[15] About 23% of readings are accompanied by GPS coordinates.[16]. Across all readings, sensors log on average 5.4µg/m^3 PM_1, 6.8µg/m^3 $PM_{2.5}$, 8.2µg/m^3 PM_{10}, 0.4ppm VOC, and an Air Quality Score (AQS) of 82.8. Approximately of 11.7% of $PM_{2.5}$ readings collected in our study exceed the US EPA's annual national ambient air quality standards, and 2.4% are above the 24-hour standard (see figure 2.I.9 in appendix 2.I).

[14]In order for us to observe sensor readings, subjects in our study had to agree to share their sensor data with us on the survey consent page and opt in to sharing sensor data with Atmotube Cloud in the Atmotube app.

[15]Recording rates vary between components due to different default measurement intervals.

[16]Sensors are not always connected to a smartphone. Without this connection, GPS coordinates are not recorded.

2.3.5 Sensor Activity

To prepare our analysis, we construct user-hourly, user-daily, and user-weekly panels that capture key sensor activity metrics. We first average sensor readings (PM, VOC, temperature, etc.) by user and date-hour to generate user-hour readings. We then average each user's hourly readings each day to aggregate them to the user-day level and then in a similar fashion again to the weekly level. At each aggregation level, we generate a sensor activity dummy variable that equals one if the user has recorded at least one reading in the time period and zero otherwise. We create a balanced sensor activity panel by filling in our sensor activity variables with zeros for all day-hours (days, weeks), which were not included in the raw readings. For each sensor, we then remove all day-hours (days, weeks) preceding the day-hour (day, week) when the user first collected a reading, and treat each user's first sensor day-hour (day, week) as their first user-activity hour (day, week).

Table 2.3.3: User Activity

	Year 1			Month 1		Month 6	
	Users	Mean	SD	Mean	SD	Mean	SD
Panel A: Sensor Activity							
Days Sensor Active (count)	611	83.4	91.8	17.5	10.8	5.4	9.4
Daily Sensor Activity (0/1)	611	0.2	0.3	0.6	0.4	0.2	0.3
Readings per Day (count)	611	263.7	331.3	636.5	569.1	197.4	384.4
Panel B: App Engagement							
Days App Active (count)	548	58.6	59.9	14.0	8.2	4.0	6.8
Daily App Activity (0/1)	548	0.2	0.2	0.5	0.3	0.1	0.2
Events per Day (count)	548	2.1	3.2	10.9	14.9	1.3	4.5
Views per Day (count)	548	0.7	1.3	3.9	5.8	0.5	1.9
Sessions per Day (count)	548	0.3	0.5	1.3	1.3	0.2	0.6

Note: Panels A and B summarize user-activity for participants with at least 365 days of data.

For brevity, we focus on summarizing the user-day sensor activity panel. Panel A in table 2.3.3 shows that in the first year after adoption, 611 participants collect AQ data on an average of 83.4 days (22% of days). On average, each user produces 264 readings per day in the first year. In the first 30 days after adoption, sensors are active on nearly 60% of days (17.5 days of 30 possible days, 58.3%). Figure 2.I.8 in appendix 2.I displays histograms of total sensor activity days per participant for the first year and first month after adoption.

2.3.6 App Engagement

Our collaboration with ATMO enables us to observe WAQS users' daily engagement with the accompanying app, which adopters can use to view data collected by their sensor. We can distinguish between three types of interactions with the app: events, views, and sessions. Events include new screen views, transitions to new screens, and push and in-app notifications. Views are distinct views after switching out of the app. Sessions are defined as unique instances of starting the app after having previously closed out of it.

We create a binary app activity variable at the user-day level that equals one if the user has any interaction with the app (events, views, or sessions) on that day, and zero otherwise. As in section 2.3.5, we create a balanced panel by filling in all missing user-days with zeros and later aggregating to the weekly level. We then merge this daily (weekly) app engagement data to the daily (weekly) sensor activity panel discussed in the previous section.[17] Panel B in table 2.3.3 shows that, on average, WAQS users engage with their app on 58.6 days in the first year after adoption (16% of days) and have an average of 3.2 events, 1.3 views, and 0.3 sessions per day. During the first 30 days after adoption, app users have an average of 10.9 events, 3.9 views, and 1.3 sessions per day. Figure 2.I.8 in appendix 2.I displays histograms of total app engagement days per participant for the first year and first month after adoption.

2.3.7 Endline Survey

In the final stage of our field experiment, we invite baseline survey respondents (i.e. adopters and non-adopters) to fill in a "5-10 minute" endline survey in exchange for an instantly-redeemable $10 Amazon.com gift card. We send invitations and reminders to the email addresses provided in the baseline survey responses. To

[17]Due to a data outage, we are missing app usage data for three weeks in late April 2023 and early May 2023. See figure 2.I.13 in appendix 2.I.

participate, endline respondents must again consent to participation and privacy terms, and we again inform them that they may opt-out of sharing their data at any point.

From December 2023 to April 2024 we collect survey data from 369 participants (21% of our full sample). Appendix 2.F displays the similar but not identical survey questions we present to adopters and non-adopters. In both versions of the endline survey, we ask subjects about their ex post (or current) sensor valuation, other AQS purchases, and changes in air pollution perceptions, defensive expenditures, and behaviors since the baseline survey, but the two versions differ because we also ask adopters about WAQS use locations, patterns, and motivations. For adopters, participants who state that they have not yet used their WAQS themselves are screened out of the survey and are not eligible to receive the prize. For non-adopters, participants who report later purchasing an Atmotube Pro are given questions from the adopter survey. Upon survey completion, participants immediately receive their prize on the survey platform and via email.

2.3.8 Additional Data

In parts of our analysis, we incorporate pollution data from ground-based monitoring stations from the US Environmental Protection Agency's (EPA) AirNow database. We use both the EPA's pre-generated "Daily AQI by County" files and daily monitor-level pollution data accessed via the AirNow API from November 2022 through August 2024. For the latter, we aggregate monitor-level data to the county-day level by calculating the arithmetic mean AQI level for each pollution parameter (PM, ozone, etc.) across all monitors in each monitored county each day (e.g. mean $PM_{2.5}$ AQI across all EPA monitors in Cook County on July 1, 2023).[18] To facilitate our analysis, we use county and state border shapefiles.[19] For our analyses described in sections 2.4.2 and 2.4.4, we then aggregate county-day AQI data to the county-week level by calculating the maximum daily AQI in

[18] According to the EPA datasets, there are 2,772 monitors sited in 1,070 of 3,144 counties in the US during our study. To the best of our knowledge, air quality is not monitored with regulatory ground monitors in the remaining 2,074 counties.

[19] "USA Counties Generalized" shapefiles downloaded from Esri include county population data for 2020 and are based on data from U.S. Census Bureau, Esri, DOC, NOAA, NOS, and NGS.

each county and week for PM and overall (includes ozone, NO_x, SO_2, and CO, if monitored).

We also supplement our analysis with population level statistics from the US Census Bureau. We use data from the 2021 and 2022 Current Population Survey Annual Social and Economic Supplement (CPS ASEC) to calculate the US population share in each bracket corresponding to our baseline survey question response choices for educational attainment (2021), income (2022), and household size (2022). We also add information from the Population Estimates Program (PEP) on the estimated US population by single year of age and sex from 2021. Finally, we incorporate 5-year estimates from the 2021 American Community Survey (ACS) on the racial and ethnic composition of the US population.

2.4 Empirical Strategy

2.4.1 Sampling Approach

We design our study to investigate WAQS in a real-world setting with the goal of i) describing current demand in the United States, ii) analyzing how consumer adopters use the technology, and iii) measuring adoption impacts.[20] A key challenge in studying this emerging technology is its limited natural demand. In a related study (Dangel and Goeschl, 2023), we find that approximately 1 in 16,000 people in the US have adopted a related product (i.e. SAQS), and we expect that WAQS adoptions are even less widespread. Low absolute demand likely reflects i) limited product awareness and ii) low willingness-to-pay relative to market prices in the population. As we face uncertainty about WAQS take-up rates when designing our study, we aim to maximize the likelihood of deploying a large number of WAQS to participants using our limited budget.

To overcome this challenge, our intervention samples 1,784 prospective WAQS customers in the US in a point-of-sale intervention on a WAQS manufacturer's website. We believe these are individuals who i) must be aware of the existence of WAQS, ii) must be aware of Atmotube Pro, iii) have a non-negative willingness-to-pay for it, and iv) are willing to participate in a short research survey for a discount.

[20] See figure 2.A.1 in appendix 2.A for an overview of our research design.

These four criteria lead to varying degrees of selection from the general population and may ultimately affect how applicable our findings are to other groups. As our sample consists of prospective early adopters who organically seek out WAQS (i.e. via Internet search) or react to an ATMO marketing intervention, we expect that they are likely to have greater than average WTP compared to the general population. In particular, we later show our baseline sample is not representative of the US population on observable characteristics, and subjects likely select into our study on expected gains from adoption. Our demand estimates should thus be interpreted as an upper-bound for the United States population and should not be considered representative. While potential selection bias in our subsample of 658 adopters who opt in to sharing their user activity data may further constrain how well our WAQS use findings transfer to groups not involved in our study, we show that our usage results hold independent of user characteristics, suggesting they may be applicable more broadly. Finally, to estimate adoption impacts, we rely on a subsample of 369 baseline survey respondents who also complete an endline survey. This means that our study's adoption impact estimates should also be interpreted with caution. Our impact results are indicative of the effect that WAQS adoption has on prospective early adopters who meet the aforementioned selection criteria and fully participate in our experiment. The following section 2.4.2 explains our demand estimates, section 2.4.3 our use estimation specifications, section 2.4.4 our impact estimation approach, and section 2.4.5 the validity of our estimates more generally.

2.4.2 Demand Estimation

This field experiment aims to evaluate current WAQS demand in the United States along three distinct dimensions. First, we use survey data on prospective customers and data from subsequent transactions to identify key predictors of WAQS interest and adoption. Second, we conduct a hypothetical WAQS product valuation exercise with prospective adopters and then use a pricing experiment to derive demand curves based on their valuations and real purchase decisions. Third, we exploit natural county-level variation in pollution levels to measure the contemporaneous relationship between ambient pollution episodes and WAQS adoptions.

We first gauge how representative our sample of prospective WAQS customers is with respect to the US population. For each socioeconomic variable we collect in our survey, we conduct a one-sided t-test comparing the share of respondents with this characteristic in our sample to the US census population share. We then identify adoption determinants in two ways. We first carry out one-sided t-tests to compare adopters and the US population along each of the surveyed socioeconomic characteristics. Second, we estimate the following ordinary least squares (OLS) regression equation:

$$y_i = \alpha_0 + \beta \mathbf{X}_i + \epsilon_i, \tag{2.1}$$

where y_i is a binary variables that corresponds to whether respondent i adopts a WAQS through our campaign or not. Each term in the $\mathbf{X}_i$ vector refers to one of individual i's socioeconomic characteristics collected in the baseline survey (e.g. age, gender, income). Because we code our socioeconomic variables as binary variables (i.e. from multiple choice or yes/no questions), each variable's β coefficient represents the change in adoption likelihood associated with an affirmative survey response for that characteristic, holding all other characteristics constant.

In a second step, we measure the relationship between WAQS price and demand. We begin with respondents' non-incentivized ex ante WTP responses and create a binary variable $B_{h,w,i}$ for each hypothetical price level h in \$25 price level intervals $h \in \{25, 50, ..., 300\}$ and each randomized WTP elicitation type $w \in \{1, 2\}$ (i.e. open-ended or MPL).[21] $B_{h,w,i}$ is equal to one for survey respondent $i \in \{1, 2, ..., 1784\}$ if respondent i responds to the WTP elicitation of type w and states they intend to buy Atmotube PRO at hypothetical price h, zero if they do not intend to purchase it for h, and missing if they did not receive the

[21] We make several assumptions about the nature of hypothetical WAQS demand. First, we do not allow for negative WTP when conducting our elicitation in our experimental design, so we assume that all participants have at minimum a WTP of zero. While negative WTP is possible in practice, we believe it is unlikely in our setting where individuals seek out the product themselves. However, this assumption implies that we cannot estimate equation 2.2 at a zero price because all $B_{0,w,i}$ are equal to one. Instead, we assume that hypothetical demand is equal to exactly one at a price of zero for both WTP elicitation treatments. Second, we are limited to making statements about hypothetical demand from MPL estimates for the range $h \leq \$250$ because our MPL questions are capped at \$250. We also do not observe any hypothetical adopters in the MPL elicitation treatment at prices above \$200. Accordingly, we assume that demand is exactly zero for $h > \$200$ for MPL respondents.

WTP elicitation w. We then run an intercept-only binary logit regression for each hypothetical price level h and WTP elicitation w described by:

$$B_{p,w,i} = \alpha_0, \tag{2.2}$$

where the constant term α_0 is equal to the log-odds of an intended purchase at hypothetical price h for WTP elicitation w. Predictive margins for each logit regression correspond to the share of participants who intend to purchase at price h and allow us to calculate confidence bands for each point estimate from each WTP elicitation approach. We then continue to our WAQS transactions data. Once again, we estimate an intercept-only binary logit model like in equation 2.2 but the dependent variable corresponds to individual i's purchasing decision at her randomly assigned offer price. Predictive margins then estimate the realized likelihood of WAQS adoption at each TIOLI offer price $p \in \{0, 50, 100, 125, 149\}$. Comparing hypothetical ex ante demand estimates with actual demand estimates enables us to comment on the predictive accuracy of our WTP elicitation approach.

Next, we use our field study's spatial and temporal scope to estimate the impact of local ambient air pollution episodes on new WAQS adoptions.[22] Our specification is described by a standard two-way fixed effects (TWFE) regression model:[23]

$$y_{c,t} = \alpha_0 + \beta_1 EPISODE_{c,t} + \gamma_c + \phi_t + \epsilon_{c,t}, \tag{2.3}$$

where $y_{c,t}$ is the number of study participants who purchase a WAQS using a discount code from our campaign in county c and year-week t, $EPISODE_{c,t}$ is a binary variable corresponding to whether an "unhealthy" air pollution episode

[22]See figure 2.B.1 in appendix 2.B for the geographic distribution of WAQS adoptions in the contiguous US and figure 2.I.10 in appendix 2.I for temporal trends.

[23]We acknowledge that the literature has identified significant shortcomings to TWFE estimators implemented in settings that deviate from the canonical setup with two time periods and two groups (Borusyak et al., 2024; Roth et al., 2023; Goodman-Bacon, 2021; De Chaisemartin and D'Haultfœuille, 2020). These revelations presumably negatively affect the quality of our estimates in equation 2.3 because we analyze a setting with many units, multiple time periods, variations in treatment timing between treated units, and repeated treatments. To resolve similar complications when studying the impact of wildfire smoke on SAQS adoptions, Coury et al. (2024) use the imputation method from Borusyak et al. (2024). However, a crucial assumption of this approach unsuitable for our setting is that treatment is fully absorbed after treatment and does not allow for repeat treatments of the same unit.

occurs in county c and week t, γ_c are county fixed effects, and ϕ_t are year-week fixed effects. In our baseline model, we define an "unhealthy air pollution episode" as a week with at least one day with a mean county-wide AQI above 100 (i.e. "Unhealthy for Sensitive Groups"). The coefficient of interest, β_1, captures the effect of an air pollution episode on new WAQS adoptions in the same week after controlling for county-specific adoption rates and overall time trends.

2.4.3 Use Estimation

We then turn to our daily user activity data to analyze natural usage trends. In a first step, we measure its time-invariant determinants and in a second step we evaluate the impact of local ambient pollution levels on contemporaneous user activity. We begin by calculating for each week during the first year post-adoption the likelihood that a WAQS user records a pollution reading and the likelihood that they engage with the app. For each adopter, we sum the number of days they use their sensor (app) in the first six months and in the latter six months of the first year after adoption. We employ these aggregate counts as the main outcomes to evaluate short-term and long-term WAQS use determinants using the following regression equation:

$$y_i = \alpha_0 + \beta \mathbf{X}_i + \epsilon_i, \tag{2.4}$$

where y_i can either be the total number of days user i has recorded at least one sensor reading or the total number of days that user i has interacted with his app. The vector of β coefficients captures how each adopter characteristic in $\mathbf{X}_i$ relates to user activity in the first or second six-month period after adoption.

To measure the relationship between ambient air pollution episodes and user activity, we estimate a slightly modified version of equation 2.3:[24]

$$y_{i,t} = \alpha_0 + \beta_1 EPISODE_{i,t} + \eta_w + \gamma_i + \phi_t + \epsilon_{i,t}, \tag{2.5}$$

where $y_{i,t}$ is one of four user activity outcomes for WAQS user i in calendar week t:

[24] Here, we again acknowledge potential limitations in this setting of using a classical TWFE estimator as described in footnote 23.

i) a binary variable for whether they record at least one pollution measurement or not, ii) the number of days they record at least one pollution measurement, iii) a binary variable for whether they engage with their app at least once or not, and iv) the number of days they engage at least once with their app. We include η_w, a fixed effect specific to the user-week $w \in \{1, 2, ..., 52\}$ in the first year after adoption, to capture common usage trends among all adopters each week after adoption w over time. All other variables are defined as in equation 2.3. The coefficient of interest, β_1, then isolates the impact of an air pollution episode on our four user activity outcomes after accounting for individual time-invariant factors and flexibly controlling for week-to-week trends in user activity over calendar time and post-adoption.

2.4.4 Impact Estimation

In our endline survey, we ask respondents to report changes since baseline to five groups of pollution-related outcomes: i) pollution perceptions, ii) defensive purchases, iii) AQ information seeking, iv) defensive actions, and v) indoor pollution mitigation. For each of these categories, we create binary outcome variables to capture the following six hypotheses about WAQS adoption:

- **Hypothesis 1** *Adoption leads individuals to believe that at least one of eight primary micro-environments is significantly less polluted than previously thought.*[25]

- **Hypothesis 2** *Adoption leads individuals to believe that at least one of eight primary micro-environments is significantly more polluted than previously thought.*

- **Hypothesis 3** *Adoption leads individuals to make additional defensive purchases to protect themselves from air pollution.*

[25] We ask individuals about pollution levels in the following eight micro-environments: (I) at home indoors, (II) at home outdoors, (III) at work indoors, (IV) at work outdoors, (V) in transit indoors, (VI) in transit outdoors, (VII) in indoor recreational spaces, and (VIII) in outdoor recreational spaces.

- **Hypothesis 4** *Adoption leads individuals to less frequently check other sources of AQ information (e.g. governmental and non-governmental sources).*

- **Hypothesis 5** *Adoption leads individuals to engage more often in defensive behaviors to protect themselves from air pollution.*

- **Hypothesis 6** *Adoption leads individuals to more often take measures to reduce pollution in indoor micro-environments.*

To test these six hypotheses about WAQS adoption impacts, we estimate:

$$Y_i = \beta_0 + \beta_1 T_i + \epsilon_i \tag{2.6}$$

where Y_i is one of the six endline outcomes of interest corresponding to each of our six hypotheses for respondent i, T_i is a binary variable that conveys whether respondent i adopted a WAQS or not, and ϵ_i is an idiosyncratic error term. Because we do not randomly allocate WAQS to participants and instead allow subjects to select into adoption, treatment effect estimates from equation 2.6 will be biased. To generate unbiased estimates, we instrument for the treatment variable T_i by estimating in a first-stage:

$$T_i = \alpha_0 + \alpha_1 P_i + \rho_i \tag{2.7}$$

where P_i is the WAQS offer price individual i receives (\$149, \$125, \$100, \$50, \$0). Our instrument, P_i, meets the relevance and exclusion criteria for a valid instrument because: i) it induces changes in the likelihood that individual i adopts a WAQS and ii) it was randomly assigned and therefore does not correlate with the error term ϵ_i in equation 2.6.

As a result of this two-stage least squares (2SLS) estimation approach, the treatment effects we identify are local average treatment effects (LATE) as originally defined by Imbens and Angrist (1994). The coefficient of interest, β_1, is therefore interpreted as the average treatment effect for individuals who were induced into treatment (i.e. decided to purchase a WAQS) by the instrument (i.e. lower offer prices).[26] While this eliminates bias present in a simple comparison of mean outcomes between adopters and non-adopters, it has some consequences for

[26] These are called "compliers" in the instrumental variables literature.

the external validity of our estimates. Namely, we cannot draw conclusions about adoption impacts for individuals who will always purchase a WAQS independent of the offer price (i.e. "always-takers"), nor for individuals who would never purchase a WAQS regardless of the offer price (i.e. "never-takers").

2.4.5 Experimental Validity

To check the internal validity of our demand estimates, we conduct balance tests for differences in observable characteristics across our two treatments. Our WTP elicitation type assignment is well-balanced across groups as coefficients from only four of 48 included variables differ significantly from zero in a regression of the MPL treatment indicator on baseline characteristics (table 2.G.1 in Appendix 2.G). We do find that respondents from households with low-education (high school or less, -16%, $p < 0.05$), who have at least one PhD-educated household member (-8%, $p < 0.1$), and who acquire AQ information from private organizations (-6%, $p < 0.1$) are less likely to receive MPL than OE, while respondents from low-income households are more likely to receive the MPL elicitation question (+11%, < \$25,000 in household income). Similarly, our offer price randomization is balanced across most dimensions as just five of 47 variables differ significantly from zero in a regression of offer price on baseline characteristics (table 2.G.2 in Appendix 2.G). Respondents received a statistically higher offer price if they somewhat disagreed (+\$9, $p < 0.05$) that their air is polluted, and younger age groups (-\$9, 18-25; -\$9 26-35; -\$8 36-45) and respondents with an associate's degree (-\$11, $p < 0.01$) received a statistically lower offer price.

Overall, treatment balance tests show some evidence of compositional differences between treatment groups for individual variable coefficients. This is not unusual considering our relatively modest sample size and that we control for at least 47 variables in each regression. F-tests for joint significance fail to reject the null hypothesis for the WTP elicitation in table 2.G.1 in Appendix 2.G and for the offer price treatment regression in table 2.G.2 in Appendix 2.G, confirming that both randomizations were effective overall.[27]

[27] Joint hypothesis tests assume a null hypothesis that all coefficients except the intercept are statistically equivalent to zero.

We expect that subjects differently decide whether to adopt a WAQS based on observable and unobservable characteristics, potentially threatening the validity of a simple comparison of mean outcomes between adopters and non-adopters at endline, as discussed in the previous section. We test for observable differences in adoption rates by regressing a binary adoption variable on baseline characteristics and offer price levels and find that 17 of 56 baseline characteristics differ significantly in adopters compared to non-adopters (table 2.G.3 in Appendix 2.G). Study participants who are aged 18-25 (-22%, $p < 0.01$) or 26-35 (-9%, $p < 0.01$), identify as female (-9%, $p < 0.01$), or live in a household with at most a high school diploma (-15%, $p < 0.05$) or associate's degree (-12%, $p < 0.05$) are less likely to adopt an WAQS through our campaign. Those who belong to high-income groups (\$125k-\$250k: +9%, $p < 0.01$; \$250k+: +18%, $p < 0.01$), who acquire AQ information from government (+5%, $p < 0.05$) or social media (+7%, $p < 0.05$), or who have a higher stated WTP (+1% for every additional \$100 in WTP, $p < 0.05$) are more likely to adopt conditional on completing our baseline survey and after controlling for treatment group assignments. It is not surprising that an F-test for joint significance rejects the null in this regression in table 2.G.3 in Appendix 2.G. To alleviate concerns about observable and unobservable selection into adoption, we leverage the randomized offer price to instrument for adoption when testing impacts with the 2SLS estimation strategy described in section 2.4.4. We provide ordinary least squares (OLS) regression results for comparison and add controls for observable differences in alternate 2SLS specifications.

Systematic differences in the types of participants who decide to share data with Atmotube Cloud (i.e. make user activity observable to the experimenter) or complete the endline survey would also affect how well our results can be generalized. To test for selection into WAQS data-sharing, we regress a binary variable for having ever uploaded data to Atmotube Cloud on baseline characteristics and treatment group indicators conditional on having adopted. Table 2.G.4 in Appendix 2.G shows that the coefficients on ten of the 67 included variables differ significantly from zero. Conditional on adopting a WAQS, females (-9%, $p < 0.05$), Hispanic and Latino people (-18%, $p < 0.05$), participants from households with more children (-8% for each additional child, $p < 0.01$), and those who acquired AQ information from newspapers (-13%, $p < 0.05$) are less likely to share data, while those who previously acquired AQ information using smartphone apps (+11%, $p < 0.01$) or reside in the Midwest (+16%, $p < 0.01$), Northeast (+11%, $p < 0.01$) or South (+7%, $p < 0.1$) were more likely do so. We also find that two of our treatment group coefficients indicate statistical differences in the likelihood of observing WAQS if a user received an offer price of $149 (-16%, $p < 0.1$) and if they received MPL elicitation questions in the baseline survey (-6%, $p < 0.5$). The F-test for joint significance rejects with a p-value below the 1% significance level, suggesting that there are indeed some statistically robust differences between observed and unobserved users in our study.

We then test for selection into our endline survey by adoption status. We separately regress for adopters and non-adopters a dummy variable for endline survey completion on baseline characteristics and offer price levels. Column 5 in table 2.G.5 in Appendix 2.G shows the balance test results for adopters. Adopters are more likely to respond to the endline survey if they are aged 18-25 (+36%, $p < 0.01$), live with more children (+9% per additional child, $p < 0.01$) or someone with a cardiopulmonary diagnosis (+10%, $p < 0.05$), arrived on our baseline survey from an external source (+8%, $p < 0.05$), or reside in the Northeast (+12%, $p < 0.01$). Adopters are less likely to respond if they have more household members (-6% for each additional member, $p < 0.05$), low household education levels (-35% if high school or less, $p < 0.05$) or use social media as an AQ information source (-9%, $p < 0.05$). The F-test for this regression rejects the null hypothesis that all variables are jointly equal to zero at the 5% significance level, so it appears that adopters who complete the endline survey are not necessarily representative of adopters in our study along all the included dimensions. In column 6 of table 2.G.5, we include additional regressors for WAQS use and app engagement, and find that adopters who use their devices more are statistically more likely to respond (+1.5% for every additional ten days of use in the first six months, $p < 0.05$).

The second regression in table 2.G.6 shows that non-adopting endline respondents who live in larger households (-2% for each additional household member, $p < 0.1$), are from a household with at most a master's degree (-5%, $p < 0.01$) or PhD (-7%, $p < 0.1$) or who completed the baseline survey in 2022 are less likely complete the endline survey. Non-adopters aged 56-65 (+7%, $p < 0.1$) or had previously acquired AQ information from sensors (+8%, $p < 0.01$) were more likely to complete the endline survey. Here, the F-test for joint significance rejects at the 5% significance level, suggesting that endline responding non-adopters may also differ from the full sample of non-adopters. While there is some evidence for observable differences between baseline and endline participants and very little evidence of systematic patterns across balance tests, controlling for observable differences in our regressions helps to mitigate validity concerns.

The latter stages of our study rely on subject participation. While the previous balance tests show that sample composition is well balanced across treatments and to a lesser degree across study stages, we observe attrition rates that may limit the degree to which our results can be generalized. Table 2.G.7 in appendix 2.G reports attrition rates by study stage. We focus here on the shares of adopters for whom we observe user activity and endline outcomes. Of 829 adopters in our study, we are able to observe sensor activity via the API for 658 of them (79% of all adopters). While 71 of these users could not be matched to app engagement data, we are able to link individuals, sensor activity, and app engagement for a total of 587 adopters (71% of all adopters). The 171 adopters for whom we do not observe user activity either did not upload data to Atmotube Cloud (148, 17.9% of all adopters) or could not be matched to a specific WAQS device because of logistical data processing errors (23, 3% of all adopters). At endline, we observe outcomes for 251 adopters who pass the pre-survey filter questions and complete the endline survey (about 30% of all adopters), and we are able to match baseline, user activity, and endline data for 222 of them (27% of all adopters).

2.5 Results

2.5.1 WAQS Demand

Socioeconomic Adoption Determinants

Baseline survey responses show that prospective WAQS adopters are more likely to come from advantaged population groups and are not representative of the overall US population in terms of socioeconomic characteristics. Column 1 in table 2.5.1 shows that a majority of baseline survey respondents are between the ages of 26

Table 2.5.1: Baseline Socioeconomic Characteristics

	Census	Full Sample		Adopters		Non-Adopters		Regression
	(1)	(2)		(3)		(4)		(5)
	Mean	Mean	Δ(2-1)	Mean	Δ(3-1)	Mean	Δ(4-1)	Adopt
Panel A: Age								
18-25	0.13	0.07	-0.06***	0.03	-0.10***	0.10	-0.04***	-0.17***
26-35	0.18	0.25	0.08***	0.24	0.06***	0.27	0.09***	-0.03
36-45	0.17	0.29	0.13***	0.30	0.14***	0.29	0.12***	.
46-55	0.16	0.18	0.02***	0.20	0.04***	0.17	0.01	0.01
56-65	0.16	0.12	-0.04***	0.13	-0.04***	0.12	-0.05***	0.02
Over 65	0.20	0.08	-0.12***	0.10	-0.10***	0.06	-0.14***	0.11*
Panel B: Gender								
Male	0.48	0.77	0.29***	0.81	0.33***	0.74	0.25***	.
Female	0.52	0.20	-0.31***	0.16	-0.35***	0.24	-0.28***	-0.09***
Prefer to Self-Describe	.	0.02	.	0.02	.	0.03	.	0.03
No Response	.	0.03	.	0.03	.	0.03	.	0.01
Panel C: Race/Ethnicity								
White Alone	0.59	0.72	0.12***	0.74	0.15***	0.69	0.10***	.
Hispanic or Latino	0.18	0.04	-0.14***	0.05	-0.14***	0.04	-0.14***	0.08
Black Alone	0.12	0.03	-0.09***	0.03	-0.10***	0.04	-0.08***	-0.03
Asian Alone	0.06	0.11	0.06***	0.11	0.06***	0.11	0.06***	-0.00
Two or More Races	0.03	0.07	0.04***	0.06	0.02***	0.09	0.06***	-0.08*
No Response	.	0.06	.	0.05	.	0.07	.	-0.07
Panel D: Education								
12th, No HS Dip.	0.10	0.01	-0.09***	0.00	-0.09***	0.01	-0.09***	0.04
HS Diploma	0.28	0.03	-0.25***	0.01	-0.27***	0.04	-0.24***	-0.20***
College, No Deg.	0.10	0.08	-0.02***	0.07	-0.03***	0.09	-0.01	-0.03
Associate's Degree	0.17	0.07	-0.10***	0.06	-0.11***	0.09	-0.08***	-0.05
Bachelor's Degree	0.22	0.35	0.13***	0.37	0.14***	0.34	0.12***	.
Master's Degree	0.10	0.28	0.19***	0.30	0.21***	0.26	0.17***	-0.01
Professional Degree	0.01	0.06	0.05***	0.07	0.06***	0.05	0.04***	0.01
PhD	0.02	0.09	0.07***	0.10	0.08***	0.07	0.05***	0.03
No Response	.	0.03	.	0.02	.	0.03	.	-0.10
Panel E: Income								
<$25,000	0.17	0.06	-0.11***	0.04	-0.13***	0.09	-0.09***	-0.07
$25,000 - $50,000	0.19	0.11	-0.08***	0.08	-0.11***	0.13	-0.06***	-0.04
$50,000 - $75,000	0.16	0.14	-0.02**	0.12	-0.04***	0.16	-0.01	0.01
$75,000 - $125,000	0.21	0.24	0.03**	0.21	0.00	0.26	0.05***	.
$125,000 - $250,000	0.19	0.28	0.09***	0.32	0.13***	0.25	0.05***	0.10***
$250,000+	0.07	0.17	0.10***	0.22	0.15***	0.12	0.05***	0.18***
No Response	.	0.17	.	0.17	.	0.16	.	0.07*
Observations	1,784	1,784		829		955		1,784
Additional controls								Yes
F-stat								3.26
F-stat p-value								0.00

Note: In panels B-E, columns (2)-(4) report the share of all responses excluding "No response" answers as the mean. Columns (2)-(4) report differences in means (Δ) and one-sided t-test statistics comparing the full sample, adopter, and non-adopter means respectively with census population means in column (1). Column (5) reports coefficients from a linear probability model where a binary adoption variable (adopt=1) is regressed on a full set of socioeconomic variables. Additional control regressors are shown in table **??** in appendix **??**. "." marks the omitted baseline variable for each panel in column (5). T-test and regression coefficient significance levels: * for p<0.1, ** for p<0.05, and *** for p<0.01. Panels A, B, and C correspond to the individual survey respondent, while panel D refers to the highest education level in the respondent's household and panel E refers to their mean annual pre-tax household income.

and 45 (55%), male (79%), White (72%), have at least one college-educated household member (78% with a bachelor's, master's, professional, or doctorate degree), and belong to an above median income household (over 69% above $70,784, US Census 2021). One-sample t-test comparisons with US Census population shares in panels A2, B2, and C2 confirm that 26 to 45 year-olds, males, and Whites are significantly over-represented in our sample compared to the US population distribution (+20%, +33%, +14%, respectively), while female (-34%), Hispanic or Latino (-14%), Black (-11%), 18 to 25 year-old (-8%), and 65 and older (-11%) subjects are significantly under-represented. Panel D2 shows that households with at least one college-educated member are significantly over-represented relative to the population share (+46%), while households without a college education are significantly under-represented. Above median income households make up a 22 percentage point larger share of our sample who shared their income information than the US population (calculation excludes 301 "No response" in panel E2). We also query households on whether someone who belongs to an air pollution risk group lives in the household. We document that a large share of prospective WAQS customers do not have children (66%), seniors (80%), or anyone with a diagnosed cardiopulmonary disease (78%) living in their homes.[28]

Table 2.5.2 shows baseline survey respondents' air pollution perceptions and preferences over previously-used AQ information sources. Over two-thirds of baseline respondents either "somewhat agree" or "strongly agree" that the air they breathe is polluted (71%), while less than one in five "somewhat disagree" or "strongly disagree" with that statement (13%). Table 2.G.3 in the appendix shows that there appear to be no statistically detectable differences in our general measure of air pollution perceptions between adopters and non-adopters. Prospective WAQS adopters report using smartphone applications (56%) and government institutions (45%) most to inform themselves about air pollution, and about 26% of respondents reported previously using information collected with AQ sensors.[29]

[28]We do not have US population statistics for these variables and cannot gauge their representativeness.

[29]For the full sample, we do not know whether the participants who reported having used information from an AQ sensor in the baseline survey owned one themselves or used publicly available information from a private sensor network. We follow up with endline respondents to learn more about their AQ sensor ownership history previous to our campaign.

Table 2.5.2: Baseline Pollution Perceptions and Air Quality Information Sources

	Full Sample	Adopters	Non-Adopters	A-NA	Regression
	(1)	(2)	(3)	(4)	(5)
	Mean	Mean	Mean	Diff t-test	Adopt
Panel A: "The air I breathe is polluted."					
Strongly Disagree	0.03	0.03	0.03	0.00	-0.01
Somewhat Disagree	0.10	0.10	0.10	-0.00	-0.02
Neither Agree nor Disagree	0.15	0.15	0.15	-0.01	
Somewhat Agree	0.46	0.46	0.46	0.00	0.01
Strongly Agree	0.25	0.25	0.25	0.00	0.03
Panel B: AQ Information Sources					
Newspaper	0.11	0.11	0.10	0.01	0.01
Television	0.20	0.18	0.21	-0.03	-0.04
Radio	0.12	0.11	0.12	-0.01	-0.02
Government Institutions	0.45	0.49	0.41	0.08***	0.07**
Private Organizations	0.22	0.23	0.20	0.03	0.03
Smartphone Apps	0.56	0.56	0.56	-0.00	-0.02
Social Media	0.19	0.20	0.18	0.02	0.07**
AQ Sensor	0.26	0.28	0.24	0.04*	0.02
None	0.08	0.08	0.09	-0.01	0.02
Observations	1,784	829	955	1,784	1,784
Additional controls					Yes
F-stat					3.26
F-stat p-value					0.00

Note: Columns (4) reports the results from a two-sample t-test statistics comparing means from adopters (2) and non-adopters (3). Column (5) reports coefficients from a linear probability model where a binary adoption variable (adopt=1) is regressed on a full set of socioeconomic variables. Additional control regressors are shown in table **??** in the appendix. "." marks the omitted baseline variable for each panel in column (4). T-test and regression coefficient significance levels: * for p<0.1, ** for p<0.05, and *** for p<0.01.

Only a small share of subjects report not using any AQ information sources at all (8%). With the exception of a slightly larger share of adopters relying on government institutions (+8%), there do not appear to be any significant differences in how adopters and non-adopters inform themselves about air pollution.

In column 3 of table 2.5.1, one-sample t-tests of differences in means between adopters and the US population show that adopters are even more dissimilar to the US population than prospective adopters along many of surveyed socioeconomic dimensions. With few exceptions, differences between adopters and the full sample of respondents appear fairly modest, although disparities in income, education, and gender are clearly reinforced. Column 5 shows, based on coefficients of a linear probability model, that differences in income levels may help to explain differences in adoption likelihood, with higher than median income levels significantly more likely to adopt than the baseline, holding all else equal ($125,000-$250,000: + 10%, $250,000+: +18%). Furthermore, 18-25 year olds (-17%), women (-9%), and respondents from households with at most a high school education (-20%) are significantly less likely to adopt than the respective baseline after accounting for our full set of controls. Baseline air pollution perceptions and AQ information sources seem to have a less clear effect on adoption likelihood. Regression results in column 4 of table 2.5.2 show that differences in baseline perceptions do not have a statistically detectable effect on adoption likelihood, while previous use of AQ information from government sources (+7%) and social media (+7%) significantly increase adoption likelihood, holding all else equal.

We use adopting households' ZIP codes to identify sensor adoption counties.[30] Figure 2.B.1 maps adoption locations for the 829 sensor adopters who purchased a device through our campaign. Table 2.H.3 in appendix 2.H shows that adoptions are concentrated in the western and northeastern US, in more urban areas, in counties with government monitors, and in counties with a greater number of government AQ monitors.

[30] ZIP code and county borders do not necessarily overlap. In case of multiple possibilities, we first try to assign each adoption to the county where the purchased WAQS is most frequently used based on GPS information. If this is not possible (e.g. due to lacking GPS data or inconsistencies), we assign the adopter to the county with the largest area in the adopter ZIP code.

Valuations and Demand

Our two experimental variations enable us to measure prospective WAQS adopters' expected benefits from WAQS adoption and estimate the relationship between WAQS demand and price. We first discuss valuation results before proceeding to demand estimates.

Subjects in our study are, on average, willing to pay less than half of the original WAQS market retail price ($249 and later $179). Table 2.3.1 shows that baseline survey respondents report an average willingness to pay of $97.75. Adopters and non-adopters state average willingness to pay of $110.38 and $86.79, respectively, and subjects' valuations differ significantly depending on how they were asked to value the sensor. Open-ended WTP elicitation leads prospective adopters to report WAQS valuations $27.35 greater than those from MPL questions.

Ex ante WAQS valuations also differ along socioeconomic dimensions, but many of these differences disappear after controlling for respondents' other socioeconomic characteristics. Figure 2.I.5 in appendix 2.I shows predicted ex ante valuations from a linear regression on respondent and household characteristics. Higher income groups report statistically greater sensor valuations than lower income groups, with individuals in the highest income bracket reporting almost twice the WTP of individuals in the lowest income bracket. Middle aged respondents (46 to 55 years old) report the highest WTP by age group, and males are willing to pay a higher price than females, but these differences are not statistically significant at the 95% level. Finally, there are some indications that WTP decreases in household size. Importantly, with the exception of respondents living alone, living in a household with at most an associate's degree, or belonging to two or more racial or ethnic groups, average WTP falls short of WAQS market retail prices.

Next, we derive sensor demand curves using subjects' ex ante sensor valuations and purchase data. Figure 2.5.1 depicts intended and realized purchasing rates reported in table 2.H.1 in appendix 2.H. Stated sensor demand is relatively low at current market sensor prices, with just 7.3% of MPL respondents and 8.3% of OE respondents intending to purchase at a price of $179. At a price of $149, the highest price level we actually offer to participants in our campaign, 27.0 % of respondents purchase the sensor, while only 15.4% of MPL respondents and

22.5% of OE respondents intend to purchase at that price. In line with demand theory, actual purchasing rates increase as prices decreases to \$125 (32.1%), \$100 (48.2%), \$50 (73.8%), and \$0 (96.2%). Stated purchasing intentions most closely predict actual conversions at \$125, \$100, and \$0, while they under-predict demand at \$149 and over-estimate demand at \$50. Across all non-zero prices, OE sensor valuations exceed MPL valuations and the mean absolute difference between MPL estimates and actual conversion rates (7.5%) is somewhat larger than the mean absolute difference for OE estimates (6.8%).

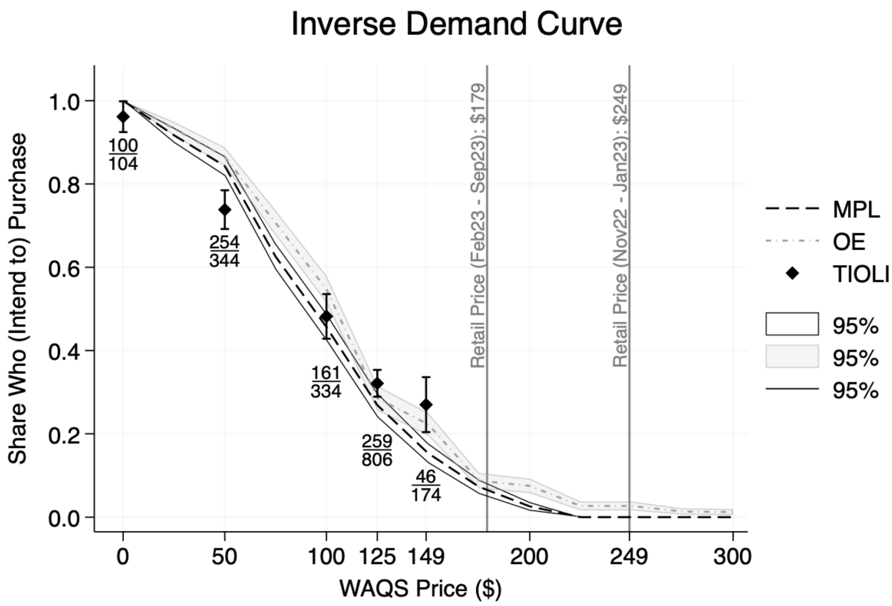

Figure 2.5.1: Inverse wearable air quality sensor (WAQS) demand curve comparing take-it-or-leave-it (TIOLI) conversion rates at randomized prices with purchasing intentions across the price range from multiple price list (MPL) and open-ended (OE) stated ex ante valuations. TIOLI fractions correspond to the number of conversions over the number of offers at each price level. Confidence intervals calculated from predicted margins after separate logit regressions at each TIOLI offer price level (0, \$50, \$100, \$125, \$149) and at each \$25 interval from \$25 to \$300 for both MPL and OE. TIOLI data from \$79 (n=20) and \$160 (n=2) price levels omitted.

We also inspect our valuation and transaction data for inconsistencies between purchasing intentions and actual purchase (or non-purchase) decisions. Figure 2.I.4 in Appendix 2.I shows the spread between respondents' WTP and the price they were offered, split by purchasing decision and WTP elicitation type. Of respondents who received an open-ended sensor valuation question and ultimately purchased a device, 291 of 422 (68.9%) purchased a device at a price when it was consistent with their stated valuation, and the remaining 131 adopters who responded to the open-ended question (31.1%) under-estimated their WTP. With the MPL approach, a slightly smaller share of adopters under-estimated their WTP (121 of 407 adopters, 29.7%). However, among non-adopters, we find that fewer respondents over-estimate their WTP with MPL (10.5%, 51 of 485 non-adopters) than with the OE question (21.9%, 103 of 470 non-adopters "should have" purchased).

In our endline survey, we elicit participants' ex post WAQS valuations and find evidence that valuations remain stable or increase for a majority of respondents. Table 2.H.2 in appendix 2.H shows that, on average, adopters and non-adopters report ex post WTP of about \$11.46 and \$3.23 less at endline than at baseline. However, only about 31% of adopters and 25% of non-adopters decreased their valuations. A linear fit on scattered adopter valuations (figure 2.I.7 in appendix 2.I) rotates downward relative to the non-adopter linear fit, suggesting that WAQS take-up converges valuations. The standard deviation of adopter valuations decreases substantially from \$72.01 at baseline to \$51.14 at endline for adopters while remaining relatively stable for non-adopters (\$46.93 to \$52.53).

Adoptions and Ambient Pollution

Table 2.5.3 shows regression results for six specifications demonstrating a positive relationship between air pollution episodes and contemporaneous adoptions in counties where government monitors report real-time AQI. Column 1 shows that county-level WAQS adoptions are about 111% higher during weeks when the maximum overall AQI exceeds 100 (AQI is "Unhealthy for sensitive populations" or worse) compared to weeks when it does not (AQI is "Good" or "Moderate"). In absolute terms, this result corresponds to 0.019 additional adoptions over a baseline mean of 0.017 adoptions per county and week. Column 2 shows that this effect grows to a 217% increase when comparing weeks with a maximum overall AQI above 150 (AQI is "Unhealthy" or worse) to those below 150 (AQI is

"Unhealthy for Sensitive Populations" or better).

Column 3 highlights which overall AQI levels are driving additional adoptions. Compared to weeks with at most "Good" or "Moderate" overall AQI, adoptions do not change during weeks with a maximum "Unhealthy for Sensitive Groups" AQI, while adoptions increase by a statistically significant 76% during weeks with at most "Unhealthy" AQI (151-200) and 205% during weeks with "Hazardous" AQI (300+). Although the point estimate on "Very Unhealthy" also suggests a 247% increase in weekly adoptions (0.042 additional adoptions per week), this coefficient is not statistically significant at the 10% level.

Three additional regressions in columns 4-6 analyze the relationship between adoptions and ambient particulate matter pollution episodes in 668 counties that monitor and report PM AQI. We focus on PM AQI in these regressions because this is the primary criteria pollutant measured by the WAQS offered in our campaign. We find very similar estimates to the results for overall AQI from columns 1-3, except that the coefficient for weeks with a maximum "Very Unhealthy" AQI is also statistically significant at the 10% level.

Overall, our results point to higher maximum AQI driving an increasing number of additional adoptions, with the highest AQI category showing a slight decline relative to the second highest category. Considering that the magnitude of our point estimates does not differ significantly between columns 3 and 6, we believe that it is primarily week-to-week PM variation driving new adoptions, rather than harmful levels of other AQI parameters like ozone, NO_2, or CO that are also captured by the AQI values in columns 1-3.

2.5.2 WAQS Use

Our study design enables us to examine key aspects of WAQS user activity with device statistics from actual consumer adopters. We first report general trends in user activity observed in our study before evaluating which factors affect usage rates and providing some preliminary evidence on the underlying motivations.

Table 2.5.3: Adoptions & Air Pollution Episodes

	(1)	(2)	(3)	(4)	(5)	(6)
AQI>100	0.019***			0.020***		
	(0.006)			(0.006)		
AQI>150		0.037**			0.040**	
		(0.018)			(0.018)	
AQI:101-150			-0.002			-0.002
			(0.003)			(0.005)
AQI:151-200			0.013**			0.013**
			(0.005)			(0.005)
AQI:201-300			0.042			0.051*
			(0.028)			(0.029)
AQI>300			0.035*			0.034*
			(0.019)			(0.019)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-week FE	Yes	Yes	Yes	Yes	Yes	Yes
AQI parameter	All	All	All	PM	PM	PM
Baseline mean	0.017	0.017	0.017	0.020	0.021	0.020
Baseline AQI	<100	<150	<100	<100	<150	<100
Observations	41,607	41,607	41,607	31,054	31,054	31,054
Counties	915	915	915	668	668	668

Note: Dependent variables is the number of WAQS adoptions through our campaign in a given county and week. Each explanatory variable is a binary indicator corresponding to whether that Air Quality Index (AQI) level was the highest observed in a given county and week. Standard errors clustered on county in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

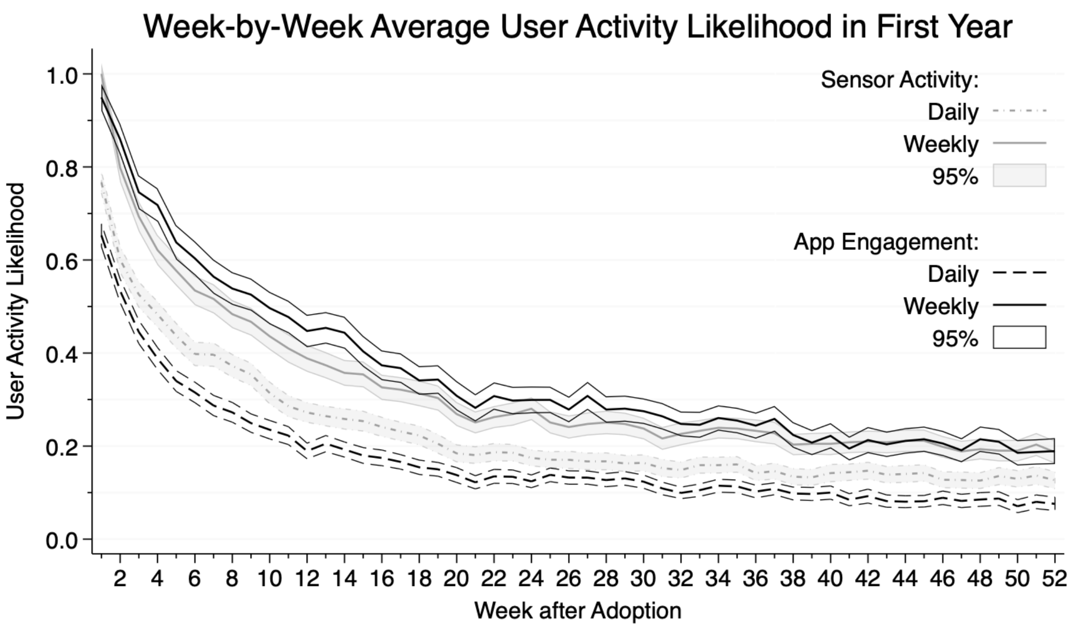

Figure 2.5.2: Daily and weekly user activity statistics calculated for each week over the first year with 95% confidence intervals.

General User Activity Trends

Figure 2.5.2 documents week-by-week average user activity rates over the first year after adoption for four distinct metrics: i) daily sensor activity likelihood (i.e. the average share of days each week that users have at least one reading), weekly sensor activity likelihood (i.e. the share of users that have at least one reading each week), iii) daily app engagement likelihood (i.e. the average share of days each week that users engage with their app at least once), and iv) weekly app engagement likelihood (i.e. the share of users that engage with their app at least once each week). Across all four metrics, average user activity trends show a very similar pattern. User activity rates are initially high but already declining in the first month, fall at a decreasing rate in the remaining first half year, and flatten out and decline at a much lower rate in the second six-month period.

Throughout the first year, weekly average daily sensor activity is higher than weekly average daily app engagement.[31] In the first week, daily sensor activity rates average close to 80% (between five and six days), while daily engagement rates are around 65% (slightly less than five days). In the second month, daily sensor (app) activity falls to around 40% (30%), and by the sixth month below 20% (15%). One year after adoption, weekly average daily sensor (app) activity has stabilized to about 15% (<10%). Average weekly sensor and app activity rates are in general much higher (because the threshold is lower) and more similar to each other than daily rates, although average weekly app engagement is about five percentage points higher than average weekly sensor activity from month two through month six after adoption.[32] After average weekly sensor (app) activity rates start at 100% (95%) in the first week, they drop to between 50-60% (60-70%) in the second month, about 30% (35%) in the sixth month, and about 20% (20%) after one year.

[31] One plausible explanation for this is that sensors can remain active passively as long as they are charged (i.e. multiple days), while app engagement requires users to deliberately engage with the app.

[32] It may be that individuals check their WAQS readings history or the global map in the app even when their sensor is not active.

User Activity Determinants

Linked adopter characteristics, transactions, and valuations enable us to inspect which factors influence WAQS user activity. Table 2.H.4 in appendix 2.H reports regression results from four models where the dependent variable is the total number of sensor activity days in either i) months one through six after adoption (column 1) or ii) months seven through twelve after adoption (columns 2-4). Our results in column 1 identify female gender (-14 days, p <0.05), non-White racial/ethnic group (-10 days, $p < 0.1$), larger household size (-6 days per person, $p < 0.01$), newspapers as an AQ info source (-16 days, $p < 0.05$), no previous AQ info sources (-16 days $p < 0.1$), and baseline survey completion quarter as factors statistically associated with fewer WAQS use days in the first six months. Over the same period, prior use of AQ information from government organizations and smartphone apps are each linked to about eight additional WAQS use days ($p <$ 0.1 each). Importantly, the regression in column 1 demonstrates that user activity is not sensitive to price paid or expected adoption benefits (i.e. ex ante WTP) as neither coefficient is statistically significant. We provide further evidence for this in figure 2.I.6 in appendix 2.I, where we plot the relationships between average WAQS use rates and WTP (binned in $25 intervals) and offer price for months one, one through six, and seven through twelve after adoption.

Column 2 in table 2.H.4 shows that these same regressors are not effective in explaining differences in long-term use. WAQS activity in months 7-12 after adoption is also linked statistically to some of the same factors for individual model coefficients, but the F-test for overall regression significance fails to reject the null hypothesis that all coefficients are zero ($p > 0.1$). In the regressions in columns 3 and 4 we include variables for sensor activity and app engagement in the first six-month period and find that these are significantly better predictors of long-term WAQS activity than the time-invariant adopter characteristics in columns 1 and 2. It is not surprising that previous user activity correlates strongly with later user activity, but the fact that they explain a significantly larger share of the variation (an R^2 of 0.54 vs. an R^2 of 0.08) and that none of the time-invariant factors (i.e. socioeconomic factors) are statistically significant after controlling for past WAQS use suggests that other factors than the observed characteristics may play a more significant role in determining both short-term and long-term sensor activity.

Table 2.H.5 in appendix 2.H shows that many of the same time-invariant factors that predict WAQS use are also statistically linked to app engagement activity. We highlight two key differences. Higher pollution perceptions are associated with additional app engagement in the first six-month period (+5 days, $p < 0.1$), and Non-White adopters engage four to six days less with their apps in the second half-year even after controlling for user activity in the first half-year ($p < 0.5$).

We now explore one key time-varying factor that may affect user activity by testing the relationship between user activity and contemporaneous ambient pollution levels. Table 2.5.4 demonstrates that user activity increases during local ambient air pollution episodes. We first note that the regressions in columns 1-4 account for the general week-by-week trends demonstrated in figure 2.5.2 with week-since-adoption fixed effects, observed and unobserved differences between individuals with user fixed effects, and seasonal trends in user activity with calendar-week fixed effects. Column 1 shows that average weekly sensor activity rates are not affected in county-weeks where the maximum overall AQI is "Unhealthy for Sensitive Populations," but do statistically increase by 2.2 percentage points (7.5% increase over a baseline mean of 29.3%) in county-weeks with an "Unhealthy" overall AQI, 8.6 percentage points (29.3% higher than baseline) in county-weeks with a "Very Unhealthy" overall AQI (201-300), and 5.7 percentage points (19.4% higher than baseline) in county-weeks with "Hazardous" overall AQI (300+) compared to county-weeks with "Good" or "Moderate" overall AQI (<100). Point estimates in column 3 shows that weekly app engagement rates evolve in a very similar fashion during weeks with higher maximum overall AQI levels, except that the coefficient on an "Unhealthy" overall AQI is not statistically significant at the 10% level.

Columns 2 and 4 show that participants use their sensors and apps more intensively during weeks with higher maximum overall AQI levels. On average, sensors are used for an additional 0.13 days per week (8.9% increase over a baseline of 1.42 days per week) during weeks with a maximum overall AQI that is "Unhealthy for Sensitive Populations" (AQI:151-200) an additional 0.37 days per week (25.8% increase) when there is a maximum "Very Unhealthy" overall AQI (201-300), and an additional 0.24 days per week (16.9% increase) during weeks with "Hazardous" overall AQI (300+) compared to county-weeks with at worst "Good" or "Mod-

Table 2.5.4: User Activity & Air Pollution Episodes

	Sensor Activity		App Engagement	
	(1)	(2)	(3)	(4)
	In Week	Days/Week	In Week	Days/Week
AQI:101-150	-0.000	0.006	0.005	0.021
	(0.008)	(0.039)	(0.008)	(0.032)
AQI:151-200	0.022**	0.127**	0.018	0.143***
	(0.010)	(0.062)	(0.012)	(0.051)
AQI:201-300	0.086***	0.366***	0.082***	0.315***
	(0.023)	(0.112)	(0.023)	(0.095)
AQI:301+	0.057**	0.244	0.059**	0.182*
	(0.024)	(0.153)	(0.026)	(0.102)
User FE	Yes	Yes	Yes	Yes
Year-Week FE	Yes	Yes	Yes	Yes
Week since Adoption FE	Yes	Yes	Yes	Yes
Baseline mean (AQI=Good or Moderate)	0.293	1.419	0.327	1.066
Observations	27,845	27,845	24,177	24,177
Users	522	522	473	473

Note: Dependent variables in columns (1) and (3) are binary variables for whether a user's sensor and app are active in a given week. Dependent variables in columns (2) and (4) are the number of days that a user's sensor and app are active in a given week. Each explanatory variable is a binary indicator corresponding to whether that Air Quality Index (AQI) level was the highest observed in a given county and week. Standard errors clustered on user in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

erate" overall AQI (<100). With the exception of the final point estimate on "Hazardous" AQI levels, these increases in weekly sensor activity are significant at the 5% significance level or lower. Again, similar patterns emerge for the average number of weekly app engagement days. Column 4 shows that effect sizes are of similar magnitude in absolute terms but somewhat larger relatively due to the lower baseline mean weekly app engagement days (1.07 engagement days per week).

User Activity Motivations

In our endline survey, we ask adopters to self-report how often they use their WAQS relative to when they first started using it and query them about motivations underlying their current usage frequency. Overall, we document substantial evidence that adopters learn about AQ by using their WAQS. Of all 251 adopters who respond to the endline survey, 147 (58%) report decreased WAQS use, fifteen (6%) report increased WAQS use, and 89 (35%) report using their WAQS at endline just as often as when they first adopted. Table 2.5.5 shows that among those who report decreased WAQS use, 32% report having learning that their AQ is good, 35% report learning that their AQ does not vary much, and 6% report learning that their AQ is bad but cannot improve it. Jointly, 54% (79 of 147 respondents) report learning that their AQ either is good or does not vary significantly, and 58% (85 respondents) report learning in some way about AQ. Importantly, just 10% of those who report lower WAQS use attribute this to a belief that the collected AQ data is not useful. Among the fifteen adopters who report higher WAQS use, ten (67%) report learning that AQ varies a lot and they feel that they are benefiting from additional use, six (40%) report learning that their AQ is bad and wanting to improve it, and three (20%) say they have learned that their AQ is good. All fifteen respondents who report increases in WAQS use attribute this to some type of learning about AQ, and nine (60%) report needing time to form a routine to use their WAQS more often. For the 89 adopters who report similar WAQS use frequency at endline, 48 (59%) report using their WAQS at least once per day and 41 (41%) report using their WAQS less than once a day.[33]

[33] We also collect free text responses from all 89 of these respondents about why they use their devices at these rates. We omit this data from our analysis for the time being.

Table 2.5.5: Self-Reported Reasons for Using Sensor Less

	Mean
Panel A: AQ Information Reasons	
I have learned that my AQ is good, so there is nothing I need to do...	0.32
I have learned that my AQ is bad, and I cannot improve it.	0.06
I have learned that my AQ doesn't vary much, and I'm not benefiting...	0.35
I don't think the AQ data Atmotube PRO collects is very useful.	0.10
None of the above	0.35
Panel B: Product Reasons	
Atmotube PRO device specifications are not good enough.	0.11
Atmotube App is not good enough.	0.15
I have another AQ sensor that I prefer using.	0.08
I have sold or permanently given away my Atmotube PRO.	0.01
My Atmotube PRO stopped working.	0.03
None of the above	0.67

Note: Responses from adopters who self-reported decreased WAQS use since first adopting (147 of 251 adopters responding to the endline survey).

2.5.3 WAQS Adoption Impacts

Table 2.5.6 reports two-stage-least-squares (2SLS) regression results estimating the impact of WAQS adoption on endline outcomes. First, we demonstrate that the randomized offer price participants receive is a valid instrument for adoption. Across six first-stage regressions where the dependent variable is a binary variable for adoption, the likelihood a participant adopts a WAQS decreases by between 0.34% and 0.39% for each additional dollar the price increases, and this relationship is highly statistically significant ($p<0.01$). For each regression, the Kleibergen-Paap Wald F-statistic sufficiently exceeds the rule-of-thumb value of ten for a strong instrument (Staiger and Stock, 1997) and is substantially greater than the critical values from a Stock-Yogo weak identification test (Stock and Yogo, 2005).

We then carry out six second-stage regressions corresponding to hypotheses 1-6 about WAQS adoption impacts laid out in section 2.4.4. First, column 1 confirms hypothesis 1. Adoption leads to a statistically significant increase in the likelihood of perceiving less pollution by 47 percentage points (137% increase over the non-adopter mean). Second, we find no statistically detectable effect of adoption on the likelihood of perceiving more pollution, failing to confirm hypothesis 2. The

Table 2.5.6: 2SLS Adoption Impact Estimates

Dependent Variable:	(1) Perceive less pollution	(2) Perceive more pollution	(3) Number defensive purchases	(4) Check AQ info less	(5) Protect self more	(6) Pollute less indoors
Second-stage:						
Adoption	0.468***	0.083	0.000	0.290*	0.016	-0.002
	(0.171)	(0.174)	(0.419)	(0.154)	(0.158)	(0.173)
Non-adopter mean	0.341	0.681	1.451	0.092	0.724	0.573
	Dependent Variable: Adoption					
First-stage:						
Offer price	-0.0034***	-0.0034***	-0.0034***	-0.0035***	-0.0035***	-0.0039***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0005)
Control variables	No	No	No	No	No	No
FWER-adjusted p	0.042	0.974	1.000	0.265	1.000	1.000
F-statistic	70.2	70.2	70.5	62.1	71.7	70.4
Respondents	341	341	342	279	324	268

Note: Dependent variables are binary variables corresponding to endline survey responses. Familywise error rate (FWER)-adjusted p-values are calculated with the STATA rwolf2 package. Heteroskedasticity-robust standard errors in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

point estimate on the adoption coefficient in column 2 is positive, as hypothesized, but of rather modest magnitude, so we are likely to be under-powered for detecting an effect, if there indeed is one. We note that the non-adopter mean shows that nearly 70% of non-adopters perceived significantly more pollution in at least one of the surveyed micro-environments, suggesting that there were large upward shifts in perceptions among adopters and non-adopters alike during our field study that may be related to external circumstances such as widespread wildfire events in North America in 2023. Third, column 3 shows that adoption has no effect on the number of defensive purchases individuals make to protect themselves from air pollution. The coefficient is negligible in magnitude and statistical significance, leading us to reject hypothesis 3. Fourth, column 4 provides evidence suggesting that adopters check AQ information from governmental and non-governmental sources less after having adopted. Adoption leads to a 29 percentage point increase in the likelihood of checking AQ information less (315% increase over non-adopter mean), and the coefficient is statistically significant at the 10% significance level.

Fifth, we find little evidence in support of hypothesis 5 that adopters engage more often in defensive behaviors to protect themselves from pollution. The coefficient on our variable of interest in column 5 is neither statistically nor economically meaningful. Finally, we reject hypothesis 6 that adoption leads individuals to pollute less in indoor spaces because the adoption coefficient in column 6 is of negligible magnitude and statistically insignificant.

In order to account for multiple hypothesis testing (List et al., 2019), we group hypotheses 1-6 and calculate familywise error rate (FWER) adjusted p-values using the approach laid out in Romano and Wolf (2016) with the rwolf2 STATA package (Clarke, 2021). The FWER-adjusted p-values reported in column 1 of table 2.5.6 demonstrates that our result confirming hypothesis 1 is robust to corrections for multiple hypothesis testing at the 5% significance level (FWER-adjusted p =0.042), while our result for hypothesis 4 in column 4 is no longer statistically significant (FWER-adjusted p >0.1).

In appendix 2.H, we provide two alternate regression specifications that support our results. First, we add a vector of adopter characteristics as control variables to our 2SLS model in table 2.H.9 and find similar effect sizes as our 2SLS model without individual-level controls. While the coefficients that previously confirmed hypotheses 1 and 4 are statistically significant at the 5% level, their FWER-adjusted p-values are statistically insignificant after accounting for multiple hypothesis testing. Second, we carry out six OLS regressions where we regress outcomes corresponding to hypotheses 1-6 on a binary adoption variable and the same vector of controls as in table 2.H.9. Here, the model coefficients again support hypotheses 1 and 4. While the coefficients are of slightly smaller magnitude (hypothesis 1: +27 percentage points for perceiving less pollution; hypothesis 4: +15 percentage points for checking AQ information less), they are both statistically significant even after correcting for multiple hypothesis testing (hypothesis 1: $p < 0.01$; hypothesis 4 $p < 0.05$). Unlike our 2SLS estimates, the adoption coefficient in column 3 is negative, of meaningful magnitude, and statistically significant at the 5% level. Considering participants' non-random decisions to adopt, this is not substantial evidence in favor of hypothesis 3. We do note, however, that a considerably smaller share of adopters report purchasing an air filter since baseline (table 2.H.7 in appendix 2.H).

2.6 Discussion

Adapting to airborne pollution risks often involves informing oneself about current ambient pollution levels with government-monitored information. This field experiment studies an emerging consumer wearable that enables adopters to independently monitor pollution exposure in real-time wherever they go, greatly expanding the number and types of places where they can perceive and respond to risks. In designing our study, we leverage a collaboration with a leading WAQS manufacturer to measure natural properties of this technology with real-world consumers, while aiming to avoid drawbacks common to existing research in this domain. We focus on three broad but closely intertwined aspects of this technology: i) how consumers value it, ii) how they use it in their own environments, and iii) how it affects them.

Our results show that socioeconomically-advantaged consumers drive current WAQS demand in the US and that these early adopters are willing to pay significantly less than current market prices for it. While our estimates presumably overpredict current market demand because of self-selection from the US population, our findings provide clear evidence that lower market prices from foreseeable technological progress will substantially increase WAQS take-up. We also find evidence that people respond to local ambient pollution events by purchasing this technology to learn more about and respond to pollution in their personal environments. This suggests that WAQS technologies will be demanded by advantaged individuals at times when new environmental risk factors emerge, for example in the current context of global climate warming and the associated threat of wildfire smoke exposure (Burke et al., 2022). The socioeconomic disparities in WAQS demand that we identify raise concerns about already substantial disparities in pollution exposure and environmental information provision (Grainger and Schreiber, 2019), which private solutions may be failing to address (Coury et al., 2024). If personal pollution exposure assessments are beneficial for disadvantaged groups, more research would be required to pinpoint WAQS access barriers and explore potential targeting policies.

The user activity trends we document in real WAQS adopters reveal several important insights about the benefits of personal pollution assessment. For a majority of adopters, user activity is concentrated in a period of less than a few months after adoption, when benefits from learning about personal pollution exposure are presumably highest. Over time, effort costs (e.g. charging and carrying the WAQS) may outweigh diminishing returns to continuous personal AQ monitoring. This is not particularly surprising given that many of the indoor spaces people routinely visit are controlled or predictable environments. However, we do find evidence that adopters increase WAQS use and app engagement in response to unexpected ambient pollution episodes, suggesting there are long-term benefits to owning a WAQS. At endline, we find two striking results supporting this claim. First, WAQS valuations are stable or increasing ex post for a vast majority of responding adopters. Second, over 96% of adopters (241 of 251) say they would not have liked to rent their WAQS instead of purchasing it. This implies that WAQS adopters exhaustively extract their expected benefits or even discover additional unanticipated benefits. Although this paper focuses on inherent adopter characteristics and the influence of external pollution on WAQS use, understanding how WAQS readings affect subsequent use and benefits is an important question for further research. While the presumably endogenous relationships between WAQS readings, use, and benefits makes analyzing these factors more difficult, exploiting unexpected air pollution shocks as an instrument seems like a fruitful avenue for future work, perhaps enabling an analysis linking habit formation to long-term user activity like in Harris and Kessler (2019).

In confirming two of our six hypotheses about WAQS impacts, we demonstrate how learning about AQ, demand, and use are related in our study. First, adopters report perceiving significantly less pollution in their surroundings than expected after using their WAQS. This aligns with a key insight from a companion paper (Dangel, 2024) that for a majority of WAQS readings, adopters in our study measure less $PM_{2.5}$ pollution than the nearest government monitor at the same point in time. Our finding in this paper may be associated with who purchases a WAQS through our campaign, their baseline exposure, and their willingness and ability to make costly pollution adaptations to lower exposure levels. In particular, with ample evidence of a negative relationship between socioeconomic status and pollution exposure, WAQS adoption may impact individuals from groups that are poorer, less educated, and more exposed very differently than the early adopters in our sample. Second, adopters in our study report seeking out other sources of AQ information less frequently. This is novel evidence of individuals substituting away from government AQ information sources to private AQ information and differs from Imtiaz et al. (2023) who document indifference between government and private sources in a field experiment in Pakistan. While we fail to confirm three hypotheses about increased defensive expenditures and actions after adoption, these null results align with previous research on a related technology from another context (Greenstone et al., 2021). We point out that our endline questionnaire focuses primarily on the extensive margin of these actions and that users may actually be changing behavior on different margins. For example, WAQS readings may better enable individuals to minimize exposure by shifting *when* they decide to do certain activities.[34]

One untested facet of our results is WAQS platform dependence. It is unclear if and to what extent our results are unique to the ways users interact with the specific product we study (e.g. data visualizations, app notifications, wearable form factor). We note that many of the general product attributes are common to other wearable technologies currently on the market (i.e. companion smartphone app, requires charging and carrying effort, etc.) and that raw pollution readings are readily presented to users. Moreover, only a small share of adopters reported dissatisfaction with the product as a reason for using it less at endline. We believe testing WAQS product design is an important avenue for future research, in particular with an eye to habit formation and belief updating. Furthermore, we expect that other WAQS technologies that measure pollutants distributed differently across space and time, such as NO_X or CO_2, will induce different user activity trends.

We conclude by discussing implications of our research for large-scale consumer WAQS deployment in two plausible real-world applications: i) real-time exposure assessment and ii) longitudinal damage assessment. Our study breaks new ground at the nexus of economics and exposure science by involving hundreds of consumer adopters in the decentralized collection of tens of millions of personal pollution ex-

[34]We do find that a higher share of adopters report rescheduling or cancelling trips more often at endline than non-adopters, although we do not test this specific relationship statistically (see table 2.H.8 in appendix 2.H).

posure readings. Lower-cost technologies on the horizon will likely lead to further proliferation of consumer WAQS information. Even though government monitors will presumably maintain their monopoly on regulatory monitoring, this information could prove useful to policymakers interested in disseminating it as part of public pollution awareness programs or to researchers aiming to better assess the relationship between exposure and health damages. Our results show, however, that private informational benefits from continuous WAQS use diminish quickly and are soon overwhelmed by its behavioral costs, so adopters stop using them consistently. The social benefits associated with sharing consumer WAQS data with communities or researchers may be large enough to justify incentives for users to collect data continuously. Our research warns of two potential pitfalls to such approaches. First, there appears to be a base of consistent users in our study who are intrinsically motivated, and we wonder if and how their monitoring effort would be affected by extrinsic incentives. Second, promoting decentralized monitoring without addressing the existing disparities in WAQS demand that we observe could lead to problematic biases in exposure assessments.

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Appendix - Chapter 2

2.A Research Design

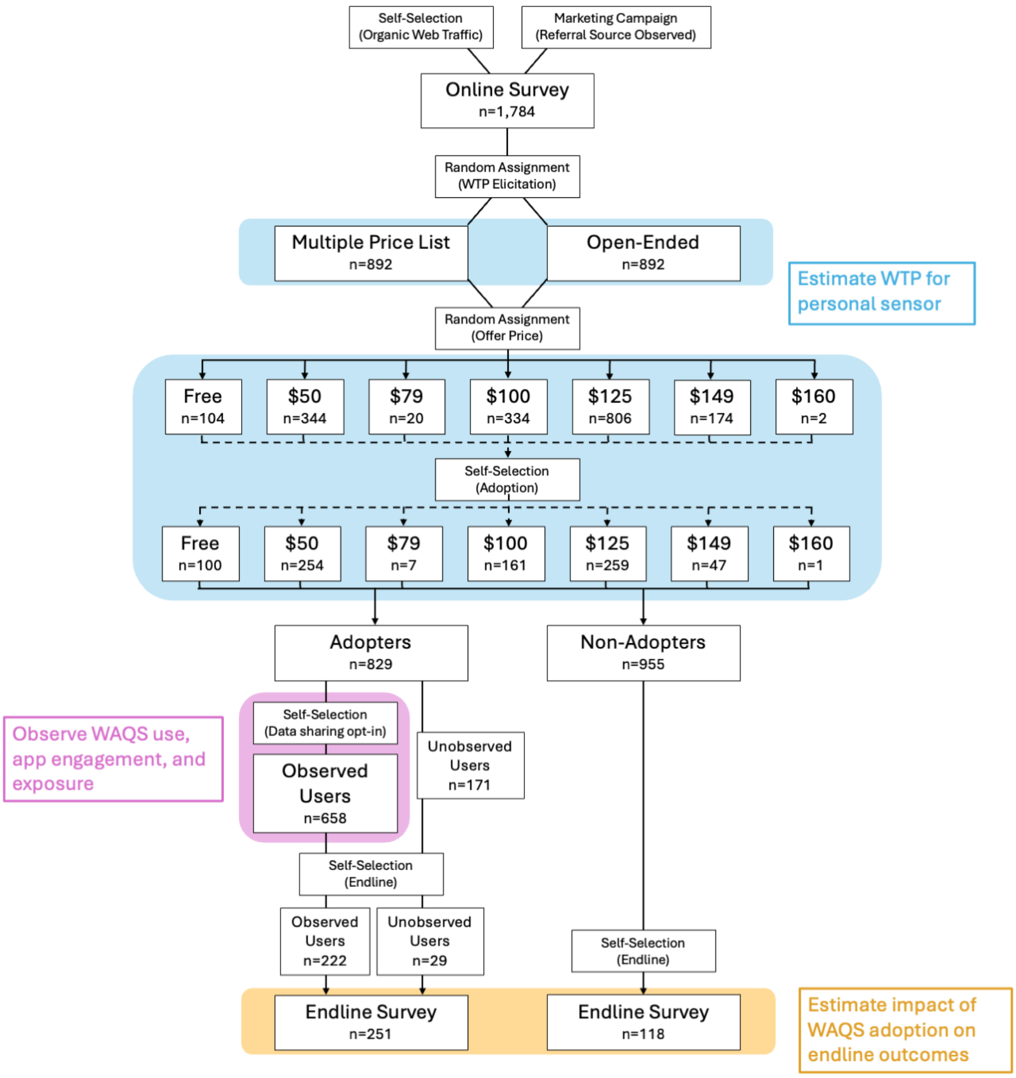

Figure 2.A.1: Schematic diagram of research design.

2.B Maps

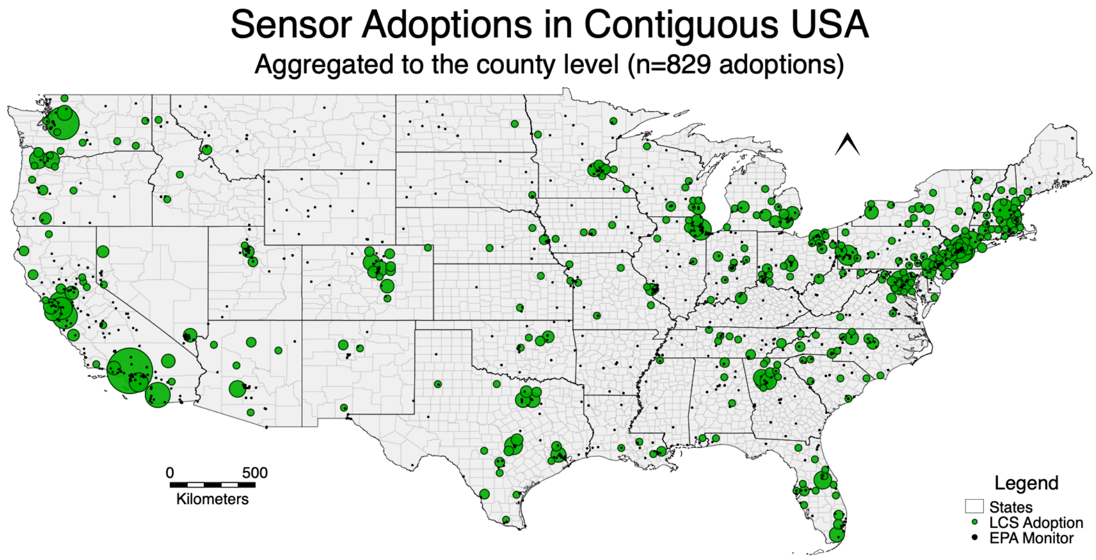

Figure 2.B.1: Sensor adoption locations in contiguous USA.

2.C Online Intervention

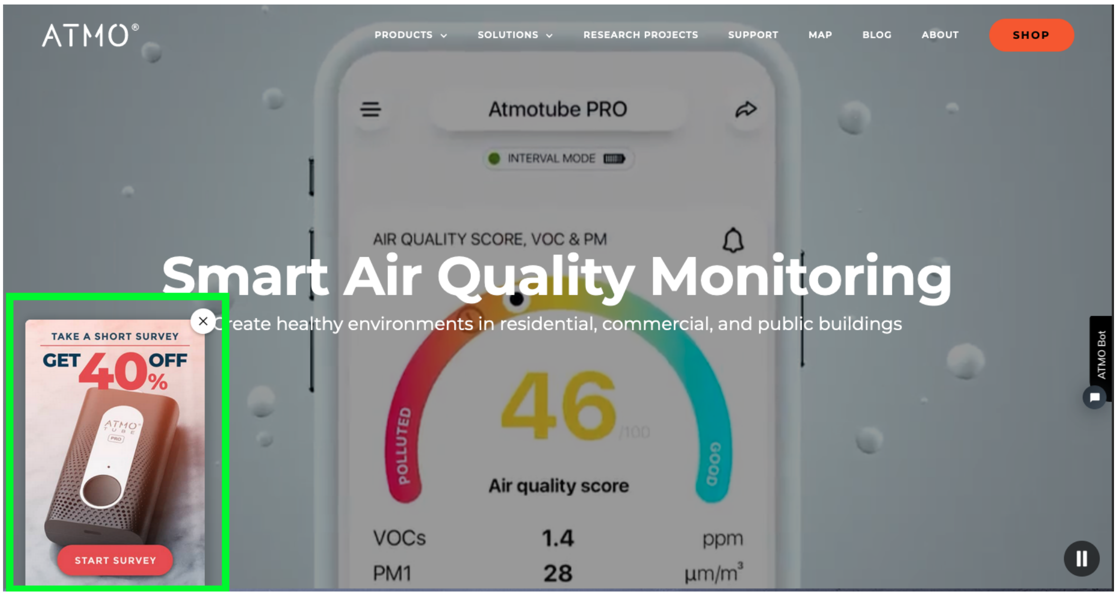

Figure 2.C.1: Desktop version of AQ sensor manufacturer's website with survey pop-up (green rectangle around pop-up added for clarity).

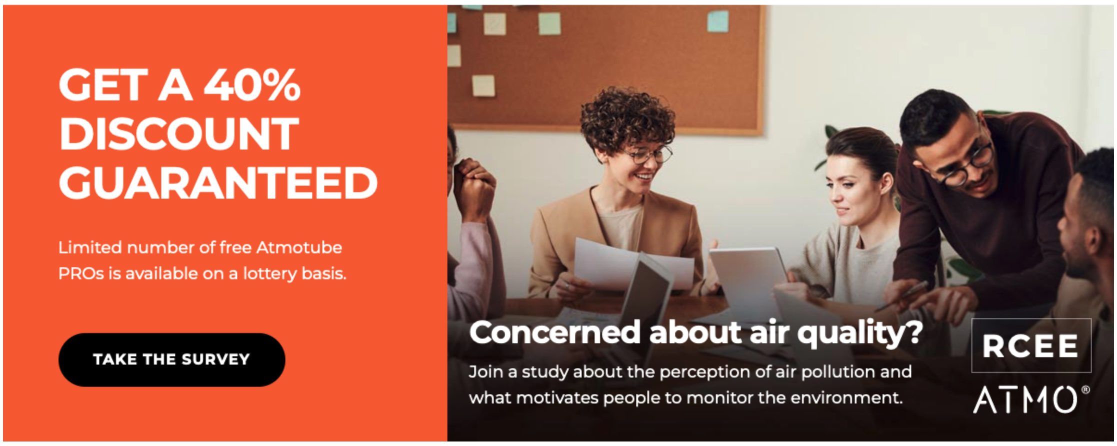

Figure 2.C.2: Detailed offer screen with link to take the survey.

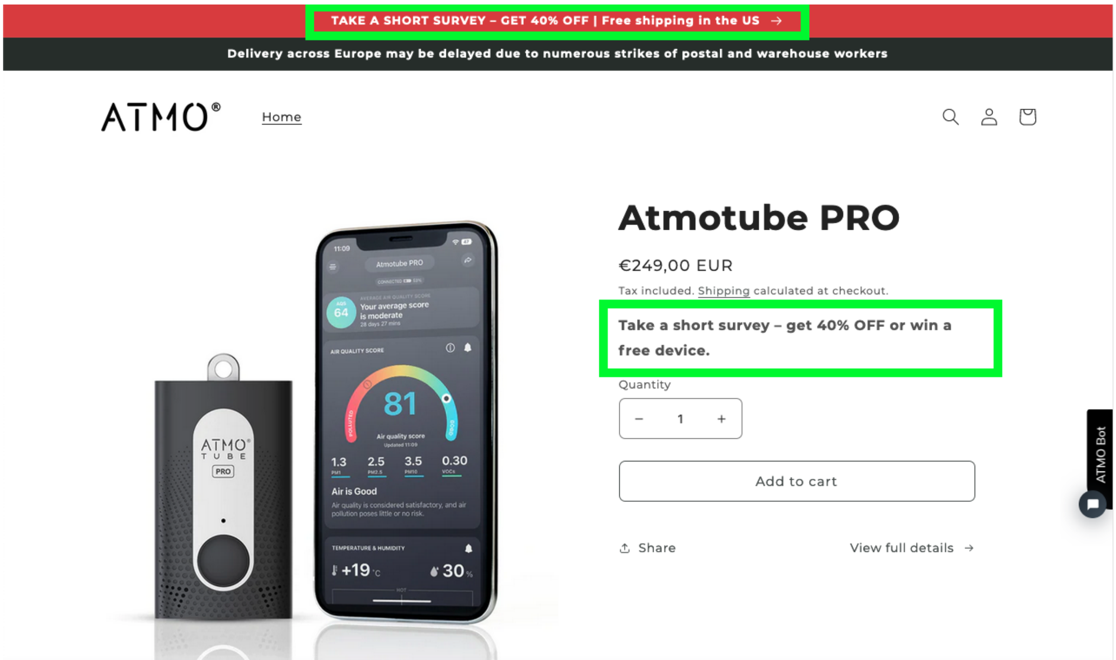

Figure 2.C.3: Desktop version of Atmotube Pro product page with clickable survey banner and text reminder below price (green rectangles added around banner and text for clarity).

RCEE-ATMO Discount Survey

Hi there,

The Research Center for Environmental Economics (RCEE) at Heidelberg University in Germany, in collaboration with ATMO®, conducts research on various drivers for people to monitor indoor and outdoor air quality.

After you complete this 10-min survey, you will immediately receive a promo code for a minimum 30% discount for an Atmotube PRO portable air quality sensor. In addition, a limited number of devices will be made available for free.

The survey will ask you to provide personal information such as gender, ethnicity, education level, etc. This data will be anonymized and won't be shared with third parties.

If you purchase an Atmotube PRO using a promo code, RCEE will receive your sensor air quality logs and Atmotube app usage statistics for research purposes (see privacy terms). However, this data will be anonymized for the research team.

We appreciate your contribution to the study.

Thank you for your time and input!
RCEE, in cooperation with ATMO®

Please note: To participate, you must be 18 years of age or older and agree to the privacy terms. Each household may only fill in this questionnaire once.

Figure 2.C.4: Qualtrics survey screen.

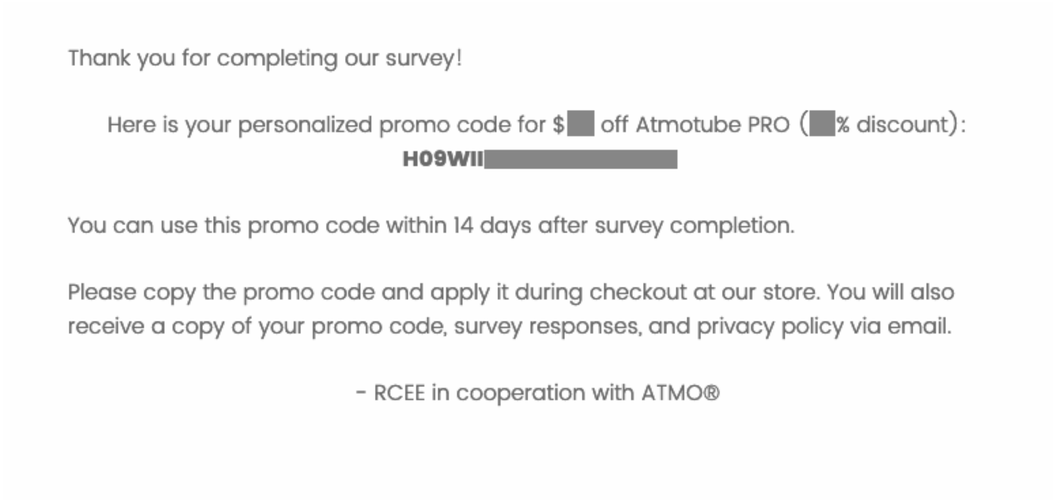

Figure 2.C.5: Final survey screen with discount code.

2.D Hardware and Software

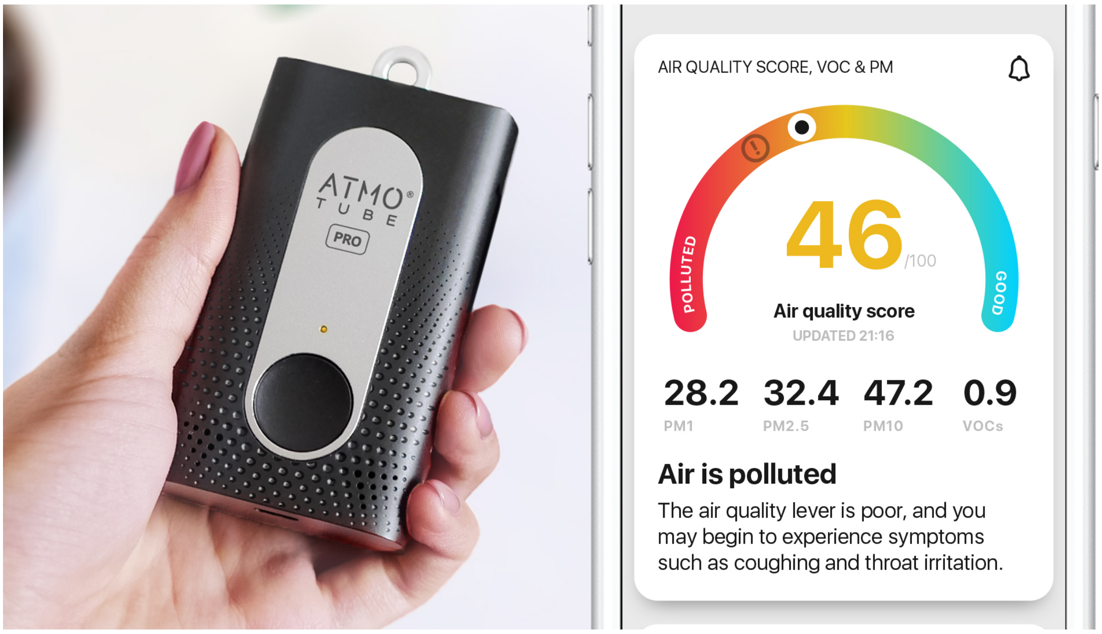

Figure 2.D.1: Atmotube Pro AQ sensor (left) and screenshot from accompanying Atmotube smartphone app (right). Image from Atmotube Media Kit.

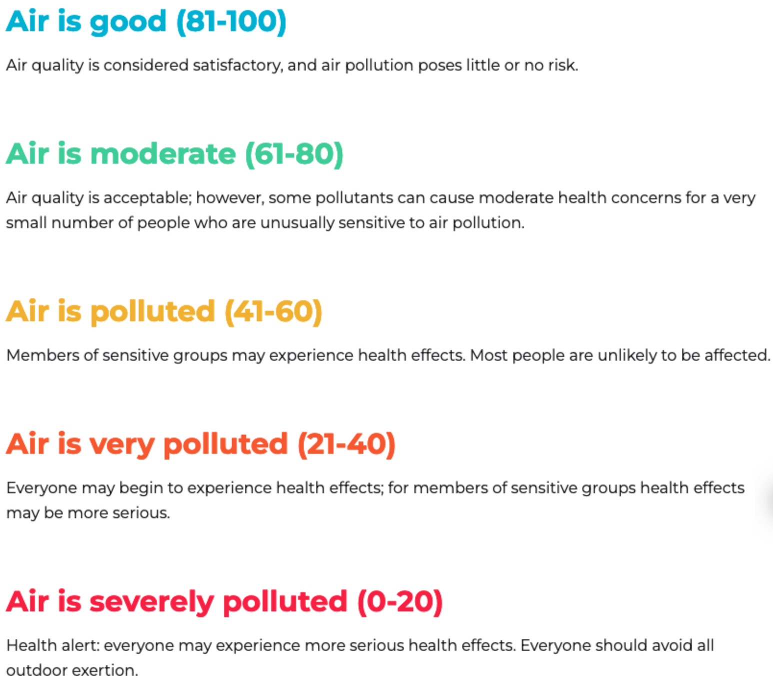

Figure 2.D.2: ATMO air quality score (AQS) ranges with corresponding indicator light colors and health recommendations.

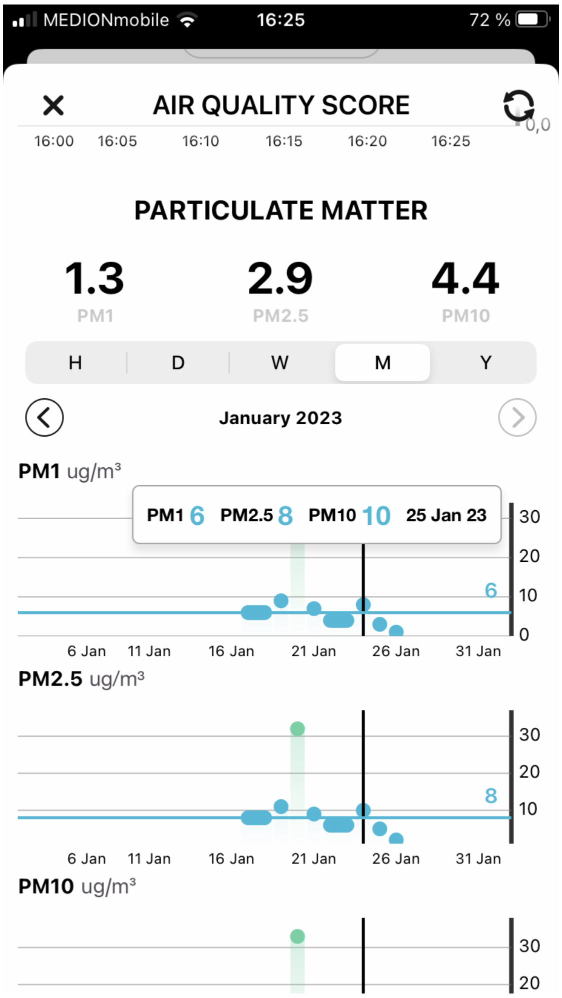

Figure 2.D.3: Atmotube app screenshot of PM data with current readings (top) and historical daily averages by parameter.

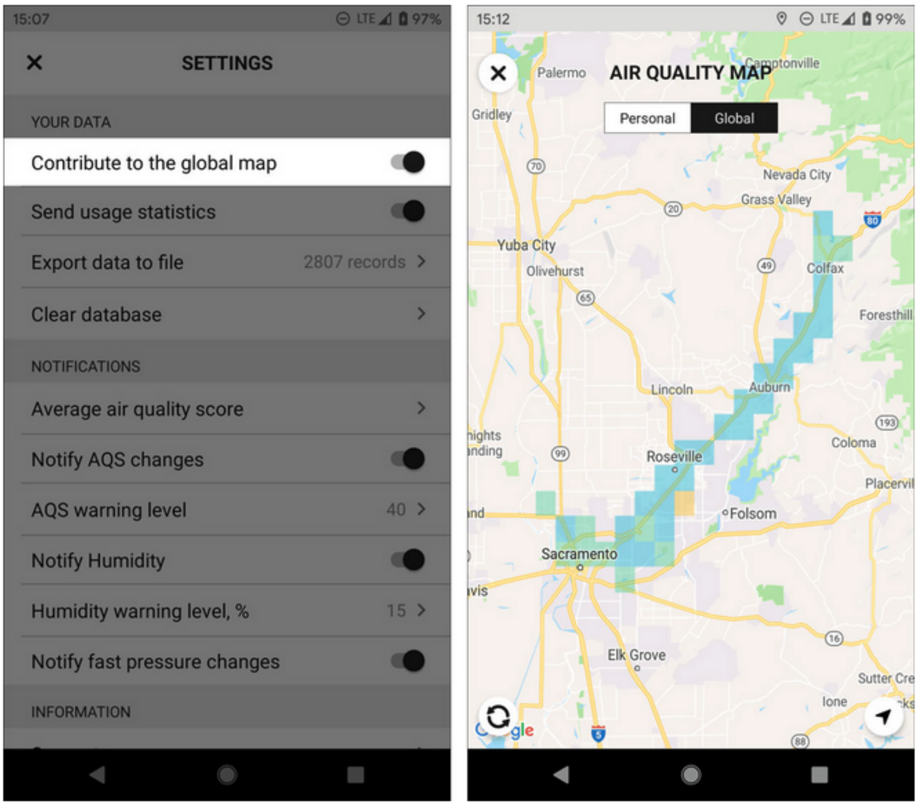

Figure 2.D.4: Atmotube app screenshots with opt in to contributing to the global map (left) and mapped data on the personal map (right).

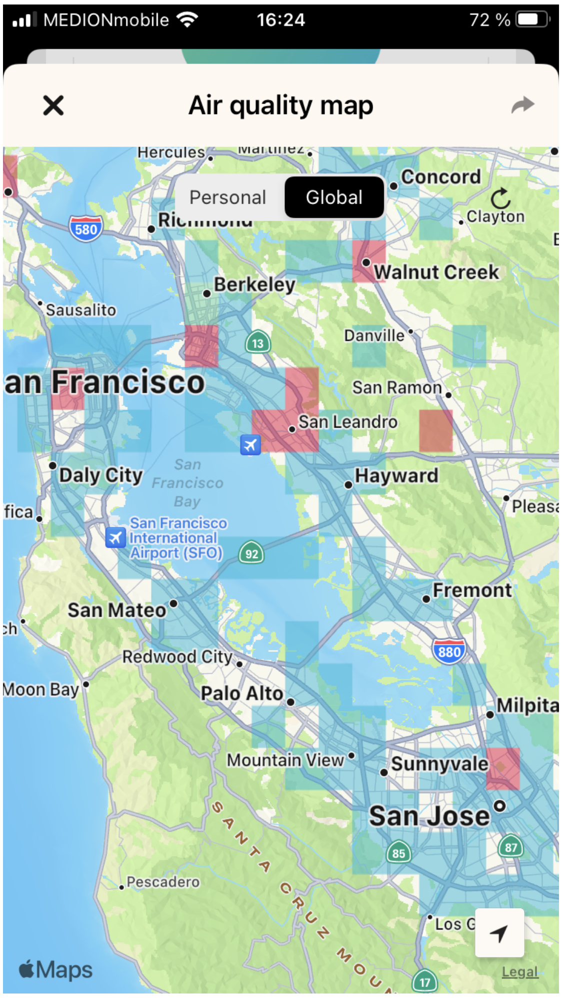

Figure 2.D.5: Atmotube app screenshot of the global map for San Francisco Bay Area.

2.E Baseline Survey

Number: Shorthand	Question	Type	"Other"?	“Prefer not to answer”?
1: Age	Please select your age group	Multiple Choice	No	No
2: Gender	Please select your gender	Multiple Choice	No	Yes
3: Race / Ethnicity	Which of the following (race/ethnicity) best describes you?	Select all that apply	Yes	Yes
4: HH Size	How many people live in your household?	Multiple Choice	No	Yes
5: # Children in HH	How many children (younger than 18) live in your household?	Multiple Choice	No	Yes
6: # Seniors in HH	How many seniors (over 65 years old) live in your household?	Multiple Choice	No	Yes
7: CP Dx in HH	Has anyone living in your household been diagnosed with a cardiovascular or lung disease?	Yes/No	No	Yes
8: High. Educ in HH	What is the highest degree or level of school a person in your household has completed?	Multiple Choice	Yes	Yes
9: HH Income	What is your annual pre-tax household income?	Multiple Choice	No	Yes
10: Air Polluted?	Please evaluate the following statement: "The air I breathe is polluted."	Likert	No	No
11: AQ Info. Sources	Do you use any of the following to inform yourself about air pollution levels?	Select all that apply	Yes	No
12A: Open-ended WTP (OE) [Randomized with 12B]	**Previous participants have overstated their willingness to pay. Please do not.** What is the maximum price (in $USD) you would be willing and able to pay for Atmotube PRO?	Numerical input	No	No
12B: Multiple Price List (MPL) [Randomized with 12A]	**Previous participants have overstated their willingness to pay. Please do not.** Would you be willing and able to buy Atmotube PRO for $X?	Yes/No	No	No
13A: Data-sharing WTP $5 per Month [Randomized with 13B]	**Previous participants have overstated their willingness to pay. Please do not.** If you could earn $5 per month for regularly collecting and sharing Atmotube PRO air quality data with ATMO, what would be the maximum price you would be willing and able to pay for Atmotube PRO?	Numerical input	No	No
13B: Data-sharing WTP $10 per Month [Randomized with 13A]	**Previous participants have overstated their willingness to pay. Please do not.** If you could earn $10 per month for regularly collecting and sharing Atmotube PRO air quality data with ATMO, what would be the maximum price you would be willing and able to pay for Atmotube PRO?	Numerical input	No	No
14: Email	Please enter your email address. Please confirm your email address.	Text input with validation check	No	No
15: Phone	Please enter your phone number (optional)	Phone number input	No	Yes

Figure 2.E.1: Baseline Survey Questions

2.F Endline Survey

Number	**Question**	**Type**	**"Other"?**	**"Prefer not to answer"?**
	Pre-Survey Filter Questions			
Pre-Survey Filter #1	Who did you purchase Atmotube PRO for? Please select all that apply.	Multiple Choice	No	No
Pre-Survey Filter #2	Have you personally used the Atmotube PRO you purchased?	Yes/No	No	No
	Section 1: Purchase and Intentions			
1	Imagine you had paid **[BASELINE WTP]** for Atmotube PRO, and this was the maximum price you had been willing to pay. Given your experience with Atmotube PRO, what would you think about having paid **[BASELINE WTP]**?	Multiple Choice	No	No
1A **[Show if 1 is "It was worth it"]**	Given your experience with Atmotube PRO, would it have been worth purchasing at a higher price than **[BASELINE WTP]**?	Yes/No	No	No
1B **[Show if 1 is "It was not worth it" OR if 1A is "No"]**	Given your experience with Atmotube PRO, what is the maximum price you should have been willing and able to pay for it?	Numerical Input	No	No
2	When you purchased Atmotube PRO, were you interested in using it to monitor wildfire smoke?	Yes/No	No	No
	Section 2: Use and Perceptions			
3	Where did you actually use Atmotube PRO?	Select all that apply	Yes	No
3A **[Order randomized with 4B]**	Since starting to use Atmotube PRO, do you believe any of the following spaces are **significantly less polluted** than you previously thought?	Select all that apply	Yes	No
3B **[Order randomized with 4A]**	Since starting to use Atmotube PRO, do you believe any of the following spaces are **significantly more polluted** than you previously thought?	Select all that apply	Yes	No
4	Compared to when you first started using Atmotube PRO, how often do you use it now?	Multiple Choice	No	No
4A **[Show if 4 is "Less often"]**	Do any of the following **air quality reasons** explain why you now use Atmotube PRO less than before?	Select all that apply	No	No

Figure 2.F.1: Endline Survey Questions (Adopters, Part 1)

Number	Question	Type	"Other"?	"Prefer not to answer"?
4B **[Show if 4 is "Less often"]**	Do any of the following **product reasons** explain why you now use Atmotube PRO less than before?	Select all that apply	No	No
4C **[Show if 4 is "Less often"]**	Are there any other reasons why you now use Atmotube PRO less than before?	Yes (Please Specify) /No	No	No
4D **[Show if 4 is "More often"]**	Do any of the following **air quality reasons** explain why you now use Atmotube PRO more than before?	Select all that apply	No	No
4E **[Show if 4 is "More often"]**	Do any of the following **product reasons** explain why you now use Atmotube PRO more than before?	Select all that apply	No	No
4F **[Show if 4 is "More often"]**	Are there other reasons why you now use Atmotube PRO more than before?	Yes (Please Specify) /No	No	No
4G **[Show if 4 is "I'm not sure"]**	In general, how often do you use Atmotube PRO?	Multiple Choice	No	No
4G **[Show if 4 is "About as often" AND 4G is "Less than once per day"]**	Could you briefly explain why you use Atmotube PRO infrequently (less than once per day)?	Text Input	No	No
4G **[Show if 4 is "About as often" AND 4G is "At least once per day"]**	Could you briefly explain why you use Atmotube PRO frequently (at least once per day)?	Text Input	No	No
Section 3: Use & Behaviors				
10	Since starting to use Atmotube PRO, have you purchased any of these to try to improve your air quality?	Select all that apply	Yes	No
11	Have you ever, even before purchasing Atmotube PRO, protected yourself from air pollution by doing any of the following?	Select all that apply	No	No

Figure 2.F.2: Endline Survey Questions (Adopters, Part 2)

Number	Question	Type	"Other"?	"Prefer not to answer"?
11A **[Order randomized with 11B]**	Since starting to use Atmotube PRO, are you **less likely** to do any of the following to protect yourself from air pollution?	Select all that apply	No	No
11B **[Order randomized with 11A]**	Since starting to use Atmotube PRO, are you **more likely** to do any of the following to protect yourself from air pollution?	Select all that apply	No	No
12	Have you ever, even before purchasing Atmotube PRO, done any of the following indoors?	Select all that apply	No	No
12A **[Order randomized with 12B]**	Since starting to use Atmotube PRO, are you **less likely** to do any of the following indoors?	Select all that apply	No	No
12B **[Order randomized with 12A]**	Since starting to use Atmotube PRO, are you **more likely** to do any of the following indoors?	Select all that apply	No	No
13A **[Order randomized with 13B]**	Since starting to use Atmotube PRO, are you **less likely** to do any of the following to try to improve your air quality?	Select all that apply	No	No
13B **[Order randomized with 13A]**	Since starting to use Atmotube PRO, are you **more likely** to do any of the following to try to improve your air quality?	Select all that apply	No	No
Section 4: Ownership & App				
14	Do you own any other air quality sensors **besides** Atmotube PRO?	Yes/No	No	No
14A **[Show if 14 is "Yes"]**	Did you own any air quality sensors **before** purchasing Atmotube PRO?	Select all that apply	Yes	No
14B **[Show if 14 is "Yes]**	Did you purchase any air quality sensors **after** purchasing Atmotube PRO?	Select all that apply	Yes	No
15	Would you have preferred to rent Atmotube PRO instead of buying it?	Yes/No/"I'm not sure"	No	No
16	Which smartphone or tablet apps did you use with your Atmotube PRO?	Multiple Choice	Yes	No

Figure 2.F.3: Endline Survey Questions (Adopters, Part 3)

Number	Question	Type	"Other"?	"Prefer not to answer"?
Section 1: Sensor Ownership				
1	Have you ever owned an air quality sensor? (Stationary indoor, stationary outdoor, or portable sensor. For example: Atmotube PRO, Aranet4, IQAir, PurpleAir, Qingping Lite, etc.)	Yes/No	No	No
1A **[Show if 1 is "Yes"]**	Did you own any air quality sensors **before** filling in our Atmotube PRO survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**?	Select all that apply/No	Yes	No
1B **[Show if 1 is "Yes"]**	Did you purchase any air quality sensors **after** filling in our Atmotube PRO survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**?	Select all that apply/No	Yes	No
Note: If 1A is "Yes, Atmotube PRO" OR 1B is "Yes, Atmotube PRO", respondent does not advance to Question 2 of the non-adopter endline survey but instead advances to Section 1 of the endline survey for adopters. If this is the case, respondent does not see questions 14, 14A, or 14B.				
2	Imagine you would be offered Atmotube PRO for **[BASELINE SURVEY WTP]**. What would you think about paying **[BASELINE SURVEY WTP]**?	Multiple Choice	No	No
2A **[Show if 2 is "It is worth it."]**	Would you pay a higher price than **[BASELINE SURVEY WTP]**?	Yes/No	No	No
2B **[Show if 2 is "It is not worth it."]**	What is the maximum price you are willing and able to pay for Atmotube PRO?	Numerical Input	No	No
Section 2: Air Quality Perceptions				
3	When you completed our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, were you interested in monitoring wildfire smoke?	Yes/No	No	No
4A **[Order randomized with 4B]**	Since filling in our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, do you believe any of the following spaces are **significantly less polluted** than you thought then?	Select all that apply	No	No

Figure 2.F.4: Endline Survey Questions (Non-adopters, Part 1)

Number	Question	Type	"Other"?	"Prefer not to answer"?
4B **[Order randomized with 4A]**	Since filling in our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, do you believe any of the following spaces are significantly more polluted than you thought then?	Select all that apply	No	No
Section 3: Purchases & Behavior				
5	Since filling in our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, have you purchased any of these to try to improve your air quality?	Select all that apply	Yes	No
6	Have you ever protected yourself from air pollution by doing any of the following?	Select all that apply	No	No
6A **[Order randomized with 6B]**	Since filling in our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, are you **less likely** to do any of the following to protect yourself from air pollution?	Select all that apply	No	No
6B **[Order randomized with 6A]**	Since filling in our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, are you **more likely** to do any of the following to protect yourself from air pollution?	Select all that apply	No	No
7	Have you ever done any of the following indoors?	Select all that apply	No	No
7A **[Order randomized with 7B]**	Since filling in our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, are you **less likely** to do any of the following indoors?	Select all that apply	No	No
7B **[Order randomized with 7A]**	Since filling in our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, are you **more likely** to do any of the following indoors?	Select all that apply	No	No
8A **[Order randomized with 8B]**	Since filling in our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, are you **less likely** to do any of the following to try to improve your air quality?	Select all that apply	No	No
8B **[Order randomized with 8A]**	Since filling in our first survey in **[BASELINE SURVEY COMPLETION MONTH, YEAR]**, are you **more likely** to do any of the following to try to improve your air quality?	Select all that apply	No	No

Figure 2.F.5: Endline Survey Questions (Non-adopters, Part 2)

2.G Balance

Table 2.G.1: WTP Elicitation Type Balance

	Mean			Difference	Regression
	(1) Full Sample	(2) MPL	(3) OE	(4) MPL-OE	(5) MPL
Age: 18-25	0.07	0.06	0.08	-0.02	0.04
Age: 26-35	0.25	0.25	0.26	-0.01	0.01
Age: 36-45	0.29	0.31	0.28	0.04*	-0.03
Age: 46-55	0.18	0.18	0.18	0.00	.
Age: 56-65	0.12	0.12	0.12	0.00	-0.01
Age: 65+	0.08	0.07	0.08	-0.01	0.06
Gender: Male	0.75	0.75	0.74	0.01	.
Gender: Female	0.20	0.20	0.20	0.00	-0.01
Gender: Self-describe	0.02	0.02	0.03	-0.01**	0.12
Race/Ethn.: White alone	0.67	0.67	0.67	0.00	.
Race/Ethn.: Hispanic or Latino	0.04	0.04	0.04	0.00	-0.03
Race/Ethn.: Black Alone	0.03	0.03	0.03	0.00	-0.01
Race/Ethn.: Asian Alone	0.11	0.10	0.11	-0.01	0.01
Race/Ethn.: Two or more	0.07	0.06	0.08	-0.01	0.04
HH: # Members	2.96	3.00	2.92	0.08	0.00
HH: # Children	1.75	1.80	1.71	0.09	-0.01
HH: # Seniors	1.39	1.39	1.39	0.00	-0.00
HH: No cardiopulminary diagnosis	0.74	0.75	0.73	0.01	.
HH: Cardiopulminary diagnosis	0.22	0.22	0.22	0.00	-0.00
Educ: High School or less	0.03	0.04	0.03	0.01*	-0.16**
Educ: College, no degree	0.08	0.08	0.09	-0.01	0.02
Educ: Associate	0.07	0.07	0.08	-0.01	0.03
Educ: Bachelor	0.34	0.34	0.35	-0.01	.
Educ: Master	0.27	0.27	0.28	-0.01	0.02
Educ: Professional	0.06	0.06	0.06	0.01	-0.02
Educ: PhD	0.08	0.10	0.07	0.03**	-0.08*
HH Income: <$25,000	0.05	0.04	0.07	-0.02**	0.11*
HH Income: $25,000 - $50,000	0.09	0.08	0.10	-0.02	0.03
HH Income: $50,000 - $75,000	0.11	0.12	0.11	0.01	-0.03
HH Income: $75,000 - $125,000	0.20	0.20	0.20	-0.00	.
HH Income: $125,000 - $250,000	0.23	0.26	0.21	0.05**	-0.05
HH Income: $250,000+	0.14	0.14	0.14	0.00	0.01
Air Polluted: Strongly disagree	0.03	0.04	0.03	0.00	0.01
Air Polluted: Somewhat disagree	0.10	0.11	0.09	0.01	0.00
Air Polluted: Neither agree nor disagree	0.15	0.16	0.14	0.01	.
Air Polluted: Somewhat agree	0.46	0.46	0.47	-0.01	0.04
Air Polluted: Strongly agree	0.25	0.24	0.26	-0.02	0.05
AQ Info Source: Television	0.20	0.21	0.18	0.02	-0.04
AQ Info Source: Radio	0.12	0.12	0.11	0.01	0.01
AQ Info Source: Government	0.45	0.46	0.44	0.02	-0.01
AQ Info Source: Private orgs.	0.22	0.23	0.20	0.03*	-0.06*
AQ Info Source: Smartphone apps	0.56	0.56	0.56	0.01	-0.02
AQ Info Source: Social media	0.19	0.19	0.19	-0.01	0.00
AQ Info Source: AQ sensor	0.26	0.27	0.26	0.01	-0.00
AQ Info Source: None	0.08	0.09	0.08	0.01	-0.06
AQ Info Source: Newspaper	0.11	0.12	0.09	0.03**	-0.07
N	1,784	892	892	1,784	1,784
F-stat					1.11
F-stat p-value					0.28

Note: "No response" variables are included in the regression in column 5 but omitted from the table due to space constraints. In column 5, "." designates the baseline category for each socioeconomic variable.

Table 2.G.2: Offer Price Balance

	Mean						Regression
	(1) Full Sample	(2) Free	(3) $50	(4) $100	(5) $125	(6) $149	(7) Offer Price
Age: 18-25	0.07	0.10	0.07	0.06	0.07	0.06	-8.79*
Age: 26-35	0.25	0.31	0.28	0.23	0.26	0.20	-9.07***
Age: 36-45	0.29	0.31	0.31	0.30	0.28	0.29	-7.98***
Age: 46-55	0.18	0.11	0.15	0.22	0.18	0.25	.
Age: 56-65	0.12	0.12	0.11	0.13	0.13	0.12	-4.77
Age: 65+	0.08	0.06	0.08	0.07	0.08	0.09	-3.39
Gender: Male	0.75	0.71	0.77	0.76	0.74	0.78	.
Gender: Female	0.20	0.18	0.18	0.18	0.21	0.19	0.77
Gender: Self-describe	0.02	0.06	0.02	0.02	0.02	0.02	-9.74
Race/Ethn.: White alone	0.67	0.70	0.67	0.68	0.67	0.67	.
Race/Ethn.: Hispanic or Latino	0.04	0.04	0.06	0.02	0.04	0.02	-2.42
Race/Ethn.: Black Alone	0.03	0.03	0.03	0.04	0.02	0.07	3.23
Race/Ethn.: Asian Alone	0.11	0.11	0.12	0.12	0.10	0.10	-1.40
Race/Ethn.: Two or more	0.07	0.07	0.06	0.07	0.07	0.08	5.75
HH: # Members	2.96	3.12	2.87	2.96	3.00	2.82	-0.97
HH: # Children	1.75	1.74	1.69	1.79	1.77	1.71	2.38
HH: # Seniors	1.39	1.44	1.37	1.37	1.40	1.40	-0.32
HH: No cardiopulminary diagnosis	0.74	0.70	0.74	0.76	0.73	0.73	.
HH: Cardiopulminary diagnosis	0.22	0.25	0.22	0.19	0.23	0.24	0.64
Educ: High School or less	0.03	0.04	0.03	0.01	0.04	0.06	1.59
Educ: College, no degree	0.08	0.06	0.10	0.09	0.08	0.07	-2.25
Educ: Associate	0.07	0.12	0.09	0.06	0.06	0.08	-11.08***
Educ: Bachelor	0.34	0.32	0.35	0.38	0.34	0.33	.
Educ: Master	0.27	0.20	0.28	0.25	0.29	0.30	2.38
Educ: Professional	0.06	0.12	0.03	0.07	0.06	0.07	-2.22
Educ: PhD	0.08	0.08	0.09	0.09	0.08	0.07	-4.15
HH Income: <$25,000	0.05	0.05	0.06	0.03	0.05	0.09	3.34
HH Income: $25,000 - $50,000	0.09	0.11	0.08	0.08	0.10	0.10	4.29
HH Income: $50,000 - $75,000	0.11	0.08	0.14	0.12	0.11	0.12	0.88
HH Income: $75,000 - $125,000	0.20	0.21	0.19	0.20	0.19	0.22	.
HH Income: $125,000 - $250,000	0.23	0.25	0.22	0.28	0.21	0.24	-1.42
HH Income: $250,000+	0.14	0.12	0.15	0.14	0.14	0.10	-2.61
Air Polluted: Strongly disagree	0.03	0.04	0.01	0.04	0.04	0.04	9.44
Air Polluted: Somewhat disagree	0.10	0.06	0.09	0.08	0.11	0.13	8.62**
Air Polluted: Neither agree nor disagree	0.15	0.20	0.15	0.14	0.15	0.15	.
Air Polluted: Somewhat agree	0.46	0.47	0.50	0.47	0.46	0.41	1.67
Air Polluted: Strongly agree	0.25	0.23	0.25	0.27	0.24	0.26	2.76
AQ Info Source: Television	0.20	0.19	0.19	0.22	0.19	0.21	0.34
AQ Info Source: Radio	0.12	0.10	0.12	0.14	0.11	0.11	0.96
AQ Info Source: Government	0.45	0.52	0.45	0.44	0.44	0.45	-2.70
AQ Info Source: Private orgs.	0.22	0.21	0.23	0.19	0.23	0.18	1.05
AQ Info Source: Smartphone apps	0.56	0.52	0.56	0.60	0.58	0.44	-0.23
AQ Info Source: Social media	0.19	0.20	0.20	0.20	0.18	0.20	-0.43
AQ Info Source: AQ sensor	0.26	0.27	0.27	0.25	0.24	0.35	0.63
AQ Info Source: None	0.08	0.08	0.09	0.08	0.08	0.10	0.27
AQ Info Source: Newspaper	0.11	0.12	0.09	0.10	0.11	0.10	-0.15
N	1,784	104	344	334	806	174	1,784
F-stat							0.97
F-stat p-value							0.53

Note: “No response” variables are included in the regression in column 7 but omitted from the table due to space constraints. In column 7, “.” designates the baseline category for each socioeconomic variable.

Table 2.G.3: Adoption Balance

	Mean			Difference	Regression
	(1) Full Sample	(2) Adopters	(3) Non-adopters	(4) A-NA	(5) Adopt
Age: 18-25	0.07	0.03	0.10	-0.06***	-0.22***
Age: 26-35	0.25	0.24	0.27	-0.03	-0.09***
Age: 36-45	0.29	0.30	0.29	0.02	-0.05
Age: 46-55	0.18	0.20	0.17	0.02	.
Age: 56-65	0.12	0.13	0.12	0.01	-0.02
Age: 65+	0.08	0.10	0.06	0.04***	0.08
Gender: Male	0.75	0.79	0.71	0.08***	.
Gender: Female	0.20	0.16	0.23	-0.07***	-0.08***
Gender: Self-describe	0.02	0.02	0.03	-0.00	0.00
Race/Ethn.: White alone	0.67	0.70	0.65	0.06**	.
Race/Ethn.: Hispanic or Latino	0.04	0.04	0.04	0.00	0.08
Race/Ethn.: Black Alone	0.03	0.02	0.04	-0.01	-0.02
Race/Ethn.: Asian Alone	0.11	0.11	0.10	0.00	-0.00
Race/Ethn.: Two or more	0.07	0.05	0.08	-0.03**	-0.06
HH: # Members	2.96	2.88	3.03	-0.15*	-0.02*
HH: # Children	1.75	1.71	1.79	-0.08	0.01
HH: # Seniors	1.39	1.39	1.39	0.01	0.02
HH: No cardiopulminary diagnosis	0.74	0.76	0.72	0.04**	.
HH: Cardiopulminary diagnosis	0.22	0.21	0.23	-0.03	-0.02
Educ: High School or less	0.03	0.02	0.05	-0.03***	-0.15**
Educ: College, no degree	0.08	0.07	0.09	-0.02	-0.04
Educ: Associate	0.07	0.06	0.08	-0.03**	-0.12***
Educ: Bachelor	0.34	0.36	0.33	0.03	.
Educ: Master	0.27	0.29	0.26	0.04*	0.00
Educ: Professional	0.06	0.07	0.05	0.02	0.00
Educ: PhD	0.08	0.10	0.07	0.03**	0.01
HH Income: <$25,000	0.05	0.03	0.07	-0.04***	-0.05
HH Income: $25,000 - $50,000	0.09	0.07	0.11	-0.04***	-0.02
HH Income: $50,000 - $75,000	0.11	0.10	0.13	-0.03**	0.01
HH Income: $75,000 - $125,000	0.20	0.18	0.22	-0.04**	.
HH Income: $125,000 - $250,000	0.23	0.27	0.21	0.06***	0.09***
HH Income: $250,000+	0.14	0.18	0.10	0.08***	0.18***
Air Polluted: Strongly disagree	0.03	0.03	0.03	0.00	0.03
Air Polluted: Somewhat disagree	0.10	0.10	0.10	-0.00	0.02
Air Polluted: Neither agree nor disagree	0.15	0.15	0.15	-0.01	.
Air Polluted: Somewhat agree	0.46	0.46	0.46	0.00	0.01
Air Polluted: Strongly agree	0.25	0.25	0.25	0.00	0.04
AQ Info Source: Television	0.20	0.18	0.21	-0.03	-0.04
AQ Info Source: Radio	0.12	0.11	0.12	-0.01	-0.01
AQ Info Source: Government	0.45	0.49	0.41	0.08***	0.05**
AQ Info Source: Private orgs.	0.22	0.23	0.20	0.03	0.04
AQ Info Source: Smartphone apps	0.56	0.56	0.56	-0.00	-0.02
AQ Info Source: Social media	0.19	0.20	0.18	0.02	0.07**
AQ Info Source: AQ sensor	0.26	0.28	0.24	0.04*	0.01
AQ Info Source: None	0.08	0.08	0.09	-0.01	0.02
AQ Info Source: Newspaper	0.11	0.11	0.10	0.01	0.00
Offer Price: Free	0.06	0.12	0.00	0.12***	.
Offer Price: $50	0.19	0.31	0.09	0.21***	-0.24***
Offer Price: $79	0.01	0.01	0.01	-0.01	-0.62***
Offer Price: $100	0.19	0.19	0.18	0.01	-0.50***
Offer Price: $125	0.45	0.31	0.57	-0.26***	-0.66***
Offer Price: $149	0.10	0.06	0.13	-0.08***	-0.71***
Offer Price: $160	0.00	0.00	0.00	0.00	-0.55*
WTP: Elicitation Method	1.50	1.51	1.49	0.02	-0.00
WTP: $	97.75	110.38	86.79	23.59**	0.00**
N	1,784	829	955	1,784	1,784
F-stat					10.40
F-stat p-value					0.00

Note: "No response" variables are included in the regression in column 7 but omitted from the table due to space constraints. In column 7, "." designates the baseline category for each socioeconomic variable.

Table 2.G.4: Observed Use Balance

	Mean			Difference	Regression
	(1) Adopters	(2) Observed Users	(3) Unobserved Users	(4) OU-UU	(5) Observed Use
Age: 18-25	0.03	0.03	0.05	-0.01	-0.11
Age: 26-35	0.24	0.24	0.22	0.03	-0.02
Age: 36-45	0.30	0.30	0.32	-0.02	0.00
Age: 46-55	0.20	0.19	0.20	-0.00	.
Age: 56-65	0.13	0.12	0.15	-0.02	-0.06
Age: 65+	0.10	0.11	0.07	0.04	0.09
Gender: Male	0.79	0.81	0.73	0.08**	.
Gender: Female	0.16	0.15	0.20	-0.05	-0.09**
Gender: Self-describe	0.02	0.02	0.03	-0.01	-0.03
Race/Ethn.: White alone	0.70	0.73	0.61	0.12***	.
Race/Ethn.: Hispanic or Latino	0.04	0.03	0.08	-0.05***	-0.15**
Race/Ethn.: Black Alone	0.02	0.03	0.02	0.01	0.12
Race/Ethn.: Asian Alone	0.11	0.11	0.12	-0.01	-0.02
Race/Ethn.: Two or more	0.05	0.05	0.06	-0.01	0.03
HH: # Members	2.88	2.81	3.15	-0.34**	0.03
HH: # Children	1.71	1.66	1.92	-0.26***	-0.08***
HH: # Seniors	1.39	1.38	1.45	-0.07	-0.02
HH: No cardiopulminary diagnosis	0.76	0.76	0.75	0.02	.
HH: Cardiopulminary diagnosis	0.21	0.21	0.19	0.02	-0.00
Educ: High School or less	0.02	0.01	0.03	-0.02	-0.12
Educ: College, no degree	0.07	0.08	0.05	0.02	0.05
Educ: Associate	0.06	0.05	0.08	-0.03	-0.09
Educ: Bachelor	0.36	0.36	0.35	0.02	.
Educ: Master	0.29	0.29	0.30	-0.01	-0.02
Educ: Professional	0.07	0.07	0.05	0.03	0.05
Educ: PhD	0.10	0.10	0.09	0.01	-0.02
HH Income: <$25,000	0.03	0.03	0.03	0.00	0.11
HH Income: $25,000 - $50,000	0.07	0.07	0.08	-0.01	0.02
HH Income: $50,000 - $75,000	0.10	0.09	0.12	-0.03	-0.02
HH Income: $75,000 - $125,000	0.18	0.18	0.16	0.02	.
HH Income: $125,000 - $250,000	0.27	0.27	0.27	-0.00	-0.02
HH Income: $250,000+	0.18	0.19	0.16	0.03	0.04
Air Polluted: Strongly disagree	0.03	0.03	0.06	-0.03*	-0.09
Air Polluted: Somewhat disagree	0.10	0.11	0.08	0.03	0.07
Air Polluted: Neither agree nor disagree	0.15	0.14	0.19	-0.05	.
Air Polluted: Somewhat agree	0.46	0.47	0.43	0.04	0.06
Air Polluted: Strongly agree	0.25	0.26	0.25	0.01	0.04
AQ Info Source: Television	0.18	0.18	0.19	-0.00	-0.00
AQ Info Source: Radio	0.11	0.11	0.12	-0.00	-0.01
AQ Info Source: Government	0.49	0.51	0.42	0.09**	0.03
AQ Info Source: Private orgs.	0.23	0.24	0.22	0.01	-0.02
AQ Info Source: Smartphone apps	0.56	0.60	0.41	0.19***	0.11***
AQ Info Source: Social media	0.20	0.19	0.23	-0.04	-0.06
AQ Info Source: AQ sensor	0.28	0.29	0.25	0.05	0.03
AQ Info Source: None	0.08	0.07	0.13	-0.06***	-0.10
AQ Info Source: Newspaper	0.11	0.10	0.15	-0.05*	-0.13***
Referral Source: Not Atmotube	0.26	0.26	0.26	-0.00	-0.02
Offer Price: Free	0.12	0.13	0.09	0.03	.
Offer Price: $50	0.31	0.30	0.32	-0.01	-0.08
Offer Price: $79	0.01	0.01	0.00	0.01	0.10
Offer Price: $100	0.19	0.20	0.19	0.01	-0.09
Offer Price: $125	0.31	0.31	0.33	-0.03	-0.08
Offer Price: $149	0.06	0.05	0.07	-0.02	-0.16*
Offer Price: $160	0.00	0.00	0.00	0.00	-0.14
Multiple Price List (0/1)	0.49	0.47	0.57	-0.10**	-0.06**
WTP: $	110.38	112.67	101.59	11.08**	0.00
Adopter County: Urban (%)	0.87	0.88	0.87	0.01	0.04
Adopter Region: West	0.34	0.32	0.44	-0.13***	.
Adopter Region: Midwest	0.16	0.18	0.09	0.09***	0.16***
Adopter Region: Northeast	0.23	0.25	0.19	0.06	0.11***
Adopter Region: South	0.26	0.25	0.26	-0.01	0.07*
Survey Completed: 2022Q4	0.10	0.10	0.10	-0.00	.
Survey Completed: 2023Q1	0.19	0.19	0.19	0.01	-0.01
Survey Completed: 2023Q2	0.45	0.43	0.49	-0.05	-0.04
Survey Completed: 2023Q3	0.26	0.27	0.22	0.05	-0.00
Survey Completed: 2023Q4	0.00	0.00	0.01	-0.00	-0.15
N	829	658	171	829	829
F-stat					2.00
F-stat p-value					0.00

Note: "No response" variables are included in the regression in column 5 but omitted from the table due to space constraints. In column 5, "." designates the baseline category for each socioeconomic variable.

Table 2.G.5: Endline Survey Response Balance for Adopters

	Adopters					
	(1) Baseline Mean	(2) Endline Respondent Mean	(3) Endline Non-respondent Mean	(4) R-NR Difference	(5) All Adopters Regression	(6) Observed Users Regression
Age: 18-25	0.03	0.06	0.02	0.03**	0.37***	0.36***
Age: 26-35	0.24	0.29	0.22	0.07**	0.08	0.09
Age: 36-45	0.30	0.27	0.32	-0.05	-0.02	-0.01
Age: 46-55	0.20	0.16	0.21	-0.05*	.	.
Age: 56-65	0.13	0.12	0.13	-0.02	0.01	0.06
Age: 65+	0.10	0.11	0.10	0.01	0.04	0.02
Gender: Male	0.79	0.81	0.78	0.03	.	.
Gender: Female	0.16	0.16	0.16	-0.01	-0.02	0.01
Gender: Self-describe	0.02	0.02	0.02	-0.01	-0.03	-0.14
Race/Ethn.: White alone	0.70	0.73	0.69	0.04	.	.
Race/Ethn.: Hispanic or Latino	0.04	0.05	0.04	0.01	0.05	0.13
Race/Ethn.: Black Alone	0.02	0.02	0.02	-0.00	-0.01	0.08
Race/Ethn.: Asian Alone	0.11	0.11	0.11	-0.01	-0.01	0.05
Race/Ethn.: Two or more	0.05	0.03	0.06	-0.03*	-0.09	-0.10
HH: # Members	2.88	2.70	2.96	-0.26**	-0.06***	-0.06**
HH: # Children	1.71	1.65	1.74	-0.09	0.09***	0.12***
HH: # Seniors	1.39	1.32	1.42	-0.10	0.01	0.02
HH: No cardiopulminary diagnosis	0.76	0.72	0.78	-0.05*	.	.
HH: Cardiopulminary diagnosis	0.21	0.26	0.18	0.08***	0.10**	0.06
Educ: High School or less	0.02	0.00	0.02	-0.02*	-0.35**	-0.41**
Educ: College, no degree	0.07	0.07	0.07	-0.00	-0.06	-0.06
Educ: Associate	0.06	0.06	0.05	0.01	0.01	0.02
Educ: Bachelor	0.36	0.39	0.34	0.04	.	.
Educ: Master	0.29	0.28	0.30	-0.01	-0.00	0.01
Educ: Professional	0.07	0.07	0.07	-0.00	0.00	0.00
Educ: PhD	0.10	0.11	0.09	0.01	0.01	0.03
HH Income: <$25,000	0.03	0.03	0.03	-0.00	-0.01	-0.06
HH Income: $25,000 - $50,000	0.07	0.09	0.06	0.04**	0.05	0.12
HH Income: $50,000 - $75,000	0.10	0.11	0.09	0.01	-0.01	0.03
HH Income: $75,000 - $125,000	0.18	0.20	0.17	0.03	.	.
HH Income: $125,000 - $250,000	0.27	0.25	0.27	-0.02	-0.04	-0.00
HH Income: $250,000+	0.18	0.17	0.19	-0.02	-0.04	-0.04
Air Polluted: Strongly disagree	0.03	0.02	0.04	-0.03**	-0.12	-0.15
Air Polluted: Somewhat disagree	0.10	0.10	0.10	-0.00	0.05	0.10
Air Polluted: Neither agree nor disagree	0.15	0.14	0.15	-0.02	.	.
Air Polluted: Somewhat agree	0.46	0.47	0.46	0.01	0.04	0.06
Air Polluted: Strongly agree	0.25	0.28	0.24	0.04	0.08	0.12*
AQ Info Source: Television	0.18	0.16	0.19	-0.03	-0.03	-0.04
AQ Info Source: Radio	0.11	0.09	0.12	-0.03	-0.05	-0.05
AQ Info Source: Government	0.49	0.54	0.47	0.07**	0.05	0.04
AQ Info Source: Private orgs.	0.23	0.25	0.23	0.02	-0.01	-0.02
AQ Info Source: Smartphone apps	0.56	0.59	0.54	0.05	-0.00	-0.06
AQ Info Source: Social media	0.20	0.17	0.22	-0.04	-0.09**	-0.08*
AQ Info Source: AQ sensor	0.28	0.31	0.27	0.03	0.02	0.02
AQ Info Source: None	0.08	0.04	0.10	-0.05***	-0.11	-0.13
AQ Info Source: Newspaper	0.11	0.09	0.12	-0.03	-0.06	-0.07
Referral Source: Not Atmotube	0.26	0.29	0.24	0.05	0.08**	0.08*
Offer Price: Free	0.12	0.14	0.11	0.03	.	.
Offer Price: $50	0.31	0.33	0.30	0.04	-0.04	-0.01
Offer Price: $79	0.01	0.00	0.01	-0.01	-0.30	-0.28
Offer Price: $100	0.19	0.17	0.21	-0.04	-0.10	-0.07
Offer Price: $125	0.31	0.30	0.32	-0.01	-0.09	-0.05
Offer Price: $149	0.06	0.05	0.06	-0.01	-0.08	-0.00
Offer Price: $160	0.00	0.00	0.00	-0.00	-0.14	-0.06
Multiple Price List (0/1)	0.49	0.50	0.49	0.02	0.02	-0.01
WTP: $	110.38	111.32	109.96	1.36	0.00	0.00
Adopter County: Urban (%)	0.87	0.88	0.87	0.00	0.11	0.05
Adopter Region: West	0.34	0.30	0.37	-0.07*	.	.
Adopter Region: Midwest	0.16	0.16	0.17	-0.01	0.03	-0.04
Adopter Region: Northeast	0.23	0.28	0.21	0.07**	0.12***	0.07
Adopter Region: South	0.26	0.26	0.26	0.00	0.04	0.03
Survey Completed: 2022Q4	0.10	0.08	0.10	-0.03	.	.
Survey Completed: 2023Q1	0.19	0.20	0.19	0.01	0.04	0.06
Survey Completed: 2023Q2	0.45	0.44	0.45	-0.00	0.00	0.04
Survey Completed: 2023Q3	0.26	0.28	0.26	0.02	0.07	0.08
Survey Completed: 2023Q4	0.00	0.00	0.00	-0.00	-0.20	-0.32
Sensor Activity Days: Months 1-6	25.10	34.99	19.90	15.09***	.	0.00**
App Engagement Days: Months 1-6	15.25	20.84	12.30	8.54***	.	0.00
N	829	257	572	829	829	658
F-stat				.	1.38	1.39
F-stat p-value				.	0.03	0.03

Table 2.G.6: Endline Balance for Non-Adopters

	Non-Adopters				
	(1) Baseline Mean	(2) Endline Respondent Mean	(3) Endline Non-respondent Mean	(4) Difference	(5) Regression
Age: 18-25	0.10	0.08	0.10	-0.01	0.01
Age: 26-35	0.27	0.26	0.27	-0.01	0.00
Age: 36-45	0.29	0.28	0.29	-0.01	0.01
Age: 46-55	0.17	0.15	0.18	-0.02	.
Age: 56-65	0.12	0.18	0.11	0.07*	0.07*
Age: 65+	0.06	0.04	0.06	-0.02	-0.03
Gender: Male	0.71	0.72	0.71	0.01	.
Gender: Female	0.23	0.24	0.23	0.01	0.01
Gender: Self-describe	0.03	0.01	0.03	-0.02**	-0.07
Race/Ethn.: White alone	0.65	0.73	0.63	0.09**	.
Race/Ethn.: Hispanic or Latino	0.04	0.02	0.04	-0.03*	-0.06
Race/Ethn.: Black Alone	0.04	0.02	0.04	-0.02*	-0.06
Race/Ethn.: Asian Alone	0.10	0.10	0.11	-0.00	-0.01
Race/Ethn.: Two or more	0.08	0.04	0.09	-0.05**	-0.06
HH: # Members	3.03	2.73	3.07	-0.34**	-0.02*
HH: # Children	1.79	1.71	1.80	-0.09	0.02
HH: # Seniors	1.39	1.31	1.40	-0.08	0.00
HH: No cardiopulminary diagnosis	0.72	0.68	0.72	-0.05	.
HH: Cardiopulminary diagnosis	0.23	0.25	0.23	0.03	0.00
Educ: High School or less	0.05	0.03	0.05	-0.03	-0.07
Educ: College, no degree	0.09	0.07	0.09	-0.02	-0.06
Educ: Associate	0.08	0.08	0.08	0.00	-0.01
Educ: Bachelor	0.33	0.45	0.32	0.13***	.
Educ: Master	0.26	0.25	0.26	-0.01	-0.05*
Educ: Professional	0.05	0.05	0.05	-0.00	-0.05
Educ: PhD	0.07	0.06	0.07	-0.01	-0.07*
HH Income: <$25,000	0.07	0.06	0.07	-0.01	-0.02
HH Income: $25,000 - $50,000	0.11	0.12	0.11	0.01	0.02
HH Income: $50,000 - $75,000	0.13	0.09	0.14	-0.04	-0.04
HH Income: $75,000 - $125,000	0.22	0.22	0.22	0.00	.
HH Income: $125,000 - $250,000	0.21	0.31	0.19	0.11**	0.05
HH Income: $250,000+	0.10	0.08	0.10	-0.02	-0.02
Air Polluted: Strongly disagree	0.03	0.03	0.03	-0.01	-0.04
Air Polluted: Somewhat disagree	0.10	0.12	0.10	0.02	0.04
Air Polluted: Neither agree nor disagree	0.15	0.12	0.16	-0.04	.
Air Polluted: Somewhat agree	0.46	0.43	0.46	-0.03	0.00
Air Polluted: Strongly agree	0.25	0.31	0.24	0.06	0.05
AQ Info Source: Television	0.21	0.21	0.21	0.00	0.00
AQ Info Source: Radio	0.12	0.14	0.12	0.02	0.02
AQ Info Source: Government	0.41	0.47	0.41	0.06	0.01
AQ Info Source: Private orgs.	0.20	0.26	0.19	0.07	0.04
AQ Info Source: Smartphone apps	0.56	0.62	0.56	0.06	0.01
AQ Info Source: Social media	0.18	0.19	0.18	0.01	0.01
AQ Info Source: AQ sensor	0.24	0.34	0.23	0.11**	0.08***
AQ Info Source: None	0.09	0.08	0.09	-0.00	0.04
AQ Info Source: Newspaper	0.10	0.09	0.10	-0.01	-0.01
Referral Source: Not Atmotube	0.23	0.26	0.22	0.04	0.02
Offer Price: Free	0.00	0.00	0.00	-0.00**	.
Offer Price: $50	0.09	0.10	0.09	0.01	0.08
Offer Price: $79	0.01	0.00	0.02	-0.02***	-0.06
Offer Price: $100	0.18	0.25	0.17	0.08*	0.10
Offer Price: $125	0.57	0.60	0.57	0.03	0.06
Offer Price: $149	0.13	0.04	0.15	-0.10***	0.13
Offer Price: $160	0.00	0.00	0.00	-0.00	0.11
Multiple Price List (0/1)	0.51	0.50	0.51	-0.01	-0.00
WTP: $	86.79	73.47	88.67	-15.20	-0.00
Adopter County: Urban (%)	0.00	0.00	0.00	0.00	0.00
Adopter Region: West	0.00	0.00	0.00	0.00	.
Adopter Region: Midwest	0.00	0.00	0.00	0.00	0.00
Adopter Region: Northeast	0.00	0.00	0.00	0.00	0.00
Adopter Region: South	0.00	0.00	0.00	0.00	0.00
Survey Completed: 2022Q4	0.13	0.00	0.15	-0.15***	-0.18***
Survey Completed: 2023Q1	0.18	0.18	0.19	-0.01	.
Survey Completed: 2023Q2	0.40	0.45	0.39	0.06	0.02
Survey Completed: 2023Q3	0.28	0.36	0.27	0.09**	0.05
Survey Completed: 2023Q4	0.00	0.01	0.00	0.01	0.27
N	955	118	837	955	955
F-stat					1.50
F-stat p-value					0.01

Table 2.G.7: Attrition

	N	Share of sample	Share of adopters	Share of non-adopters
Stage 1: Baseline				
Surveys started	2,581	.	.	.
Surveys completed	2,184	.	.	.
Unique	1,784	100.0	.	.
Duplicates	400	.	.	.
Stage 2: Adoption				
Adopters	829	46.5	100.0	.
Non-adopters	955	53.5	.	100.0
Stage 3: User Activity				
Observed adopters (users)	658	36.9	79.4	.
w/ app engagement	587	32.9	70.8	.
w/out app engagement	71	4.0	8.6	.
Unobserved adopters	171	9.6	20.6	.
No data from API	148	8.3	17.9	.
Warehouse errors	23	1.3	2.8	.
Stage 4: Endline				
Surveys started	426	23.9	.	.
Surveys completed	375	21.0	.	.
Adopters	257	14.4	31.0	.
Observed users	222	12.4	26.8	.
Unobserved users	29	1.6	3.5	.
Failed pre-survey filters	6	0.3	0.7	.
Non-adopters	118	6.6	.	12.4

2.H Additional Tables

Table 2.H.1: Share Purchasing or Intending to Purchase

Price	TIOLI	MPL	OE	TIOLI - MPL	TIOLI - OE	OE - MPL
0	0.962	1.000	1.000	-0.038	-0.038	0.000
25	.	0.917	0.933	.	.	0.016
50	0.738	0.843	0.865	-0.105	-0.127	0.022
75	.	0.626	0.706	.	.	0.081
100	0.482	0.456	0.547	0.026	-0.065	0.091
125	0.321	0.269	0.286	0.052	0.035	0.017
149	0.270	0.154	0.225	0.117	0.045	0.072
150	.	0.154	0.223	.	.	0.070
175	.	0.073	0.087	.	.	0.015
200	.	0.026	0.075	.	.	0.049
225	.	0.000	0.027	.	.	0.027
250	.	0.000	0.027	.	.	0.027
275	.	0.000	0.013	.	.	0.013
300	.	0.000	0.012	.	.	0.012

Table 2.H.2: Endline WAQS Valuations

	Adopters			Non-Adopters			A-NA
	N	Mean	SD	N	Mean	SD	SD
Baseline WTP (\$)	251	111.47	72.01	118	73.47	46.93	38.00***
Endline WTP (\$)	251	100.01	51.14	118	70.25	52.53	29.76***
Δ WTP (\$)	251	-11.46	65.45	118	-3.23	33.90	-8.23
Δ WTP > 0 (=1)	251	0.20	0.40	118	0.23	0.42	-0.03
Δ WTP < 0 (=1)	251	0.31	0.46	118	0.25	0.43	0.06
Δ WTP $= 0$ (=1)	251	0.49	0.50	118	0.52	0.50	-0.03

Table 2.H.3: Regression Results: County-level Adoption

	(1) Adoptions	(2) Adoptions	(3) Adoptions
Population (10,000s)	0.004***	0.004***	0.003***
	(0.000)	(0.000)	(0.000)
Population Urban (%)	0.056***	0.059***	0.049***
	(0.004)	(0.004)	(0.004)
Midwest	-0.574***		
	(0.164)		
Northeast	0.267		
	(0.199)		
South	-0.768***		
	(0.174)		
County Monitored (0/1)			0.966***
			(0.174)
Government Monitors (count)			0.023*
			(0.013)
State FE	No	Yes	Yes
Counties	3,126	3,080	3,080

Note: Pseudo-poisson maximum likelihood regression results from a regression where the dependent variables is the number of WAQS adoptions through our campaign per county. In column (1), western counties are the baseline. Standard errors in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

Table 2.H.4: User Activity Determinants: Total Sensor Activity Days

	(1) Months 1-6		(2) Months 7-12		(3) Months 7-12		(4) Months 7-12	
	b	se	b	se	b	se	b	se
Age: 18-25	-7.40	(12.91)	1.54	(10.82)	6.09	(7.36)	5.95	(7.34)
Age: 26-35	-4.78	(6.65)	-2.27	(5.57)	0.66	(3.79)	0.68	(3.78)
Age: 36-45	-8.87	(6.12)	-10.03*	(5.13)	-4.58	(3.50)	-4.50	(3.49)
Age: 56-65	-2.66	(7.67)	-8.42	(6.43)	-6.79	(4.37)	-6.64	(4.36)
Age: 65+	6.37	(9.99)	-4.04	(8.37)	-7.95	(5.70)	-8.28	(5.69)
Gender: Female	-13.66**	(5.98)	-5.66	(5.01)	2.73	(3.42)	2.66	(3.42)
Race/Ethn.: Non-White	-10.06*	(5.28)	-10.19**	(4.43)	-4.01	(3.02)	-4.15	(3.02)
HH: Size	-6.10***	(2.23)	-3.69**	(1.87)	0.05	(1.28)	0.16	(1.28)
HH: # Children	2.99	(2.91)	0.48	(2.43)	-1.36	(1.66)	-1.50	(1.66)
HH: # Seniors	3.21	(3.73)	3.15	(3.12)	1.18	(2.13)	1.14	(2.12)
HH: Cardiopulminary Diagnosis	-2.85	(5.34)	-0.15	(4.48)	1.60	(3.05)	1.62	(3.04)
HH Education: Bachelor or Higher	1.10	(6.02)	2.59	(5.04)	1.91	(3.43)	1.83	(3.42)
HH Income: Above Median	-0.80	(5.90)	-0.64	(4.94)	-0.15	(3.36)	0.26	(3.36)
AQ Belief: Polluted (Likert: 4 or 5)	-3.09	(4.79)	-0.24	(4.02)	1.66	(2.73)	1.08	(2.74)
AQ Info Source: Television	-2.46	(5.97)	3.65	(5.00)	5.16	(3.40)	5.35	(3.40)
AQ Info Source: Radio	6.63	(7.27)	5.64	(6.09)	1.57	(4.15)	1.01	(4.15)
AQ Info Source: Government	8.39*	(4.60)	3.39	(3.86)	-1.77	(2.63)	-1.64	(2.63)
AQ Info Source: Private Orgs.	8.32	(5.43)	3.69	(4.55)	-1.43	(3.10)	-1.07	(3.10)
AQ Info Source: Smartphone Apps	7.66*	(4.51)	6.95*	(3.78)	2.24	(2.58)	1.99	(2.58)
AQ Info Source: Social Media	-3.56	(5.41)	-5.17	(4.54)	-2.99	(3.09)	-2.67	(3.08)
AQ Info Source: AQ Sensor	-0.16	(4.69)	-0.12	(3.93)	-0.01	(2.68)	-0.38	(2.68)
AQ Info Source: None	-18.02*	(9.39)	-5.66	(7.86)	5.42	(5.37)	5.75	(5.36)
AQ Info Source: Newspaper	-16.13**	(7.34)	-4.92	(6.15)	4.99	(4.20)	5.56	(4.20)
Referral Source: Not Atmotube	-4.64	(5.01)	-5.41	(4.20)	-2.56	(2.86)	-2.35	(2.86)
Adopter County: Urban (%)	3.88	(10.85)	4.85	(9.09)	2.47	(6.19)	1.56	(6.19)
Adopter Region: Midwest	1.98	(6.27)	-2.96	(5.26)	-4.18	(3.58)	-4.02	(3.57)
Adopter Region: Northeast	5.52	(5.69)	-0.62	(4.77)	-4.01	(3.25)	-4.22	(3.24)
Adopter Region: South	0.53	(5.62)	-1.68	(4.71)	-2.01	(3.20)	-1.83	(3.20)
Offer Price ($)	0.06	(0.05)	0.02	(0.04)	-0.01	(0.03)	-0.02	(0.03)
WTP ($)	0.00	(0.04)	0.00	(0.03)	0.00	(0.02)	0.00	(0.02)
Survey Completed: 2023Q1	-5.04	(8.31)	-0.30	(6.96)	2.80	(4.74)	2.18	(4.74)
Survey Completed: 2023Q2	-18.12**	(7.63)	-2.63	(6.39)	8.50*	(4.37)	6.06	(4.54)
Survey Completed: 2023Q3	-15.72**	(7.82)	0.91	(6.55)	10.57**	(4.47)	8.50*	(4.59)
Sensor Activity Days: Months 1-6					0.61***	(0.02)	0.58***	(0.03)
App Engagement Days: Months 1-6							0.09*	(0.05)
R^2	0.11		0.06		0.57		0.57	
F-stat	1.95		1.07		20.21		19.90	
F-stat p-value	0.00		0.35		0.00		0.00	
Users	658		658		658		658	

Table 2.H.5: User Activity Determinants: Total App Engagement Days

	(1) Months 1-6		(2) Months 7-12		(3) Months 7-12		(4) Months 7-12	
	b	se	b	se	b	se	b	se
Age: 18-25	-1.42	(8.50)	6.25	(6.71)	7.05	(4.72)	7.37	(4.70)
Age: 26-35	-2.18	(4.38)	0.60	(3.46)	1.83	(2.43)	1.97	(2.42)
Age: 36-45	-4.60	(4.03)	-4.24	(3.18)	-1.65	(2.24)	-1.42	(2.23)
Age: 56-65	-2.74	(5.05)	-1.93	(3.99)	-0.38	(2.81)	-0.38	(2.79)
Age: 65+	6.50	(6.57)	-1.36	(5.19)	-5.01	(3.66)	-5.01	(3.64)
Gender: Female	-4.83	(3.94)	-2.32	(3.11)	0.40	(2.19)	0.87	(2.18)
Race/Ethn.: Non-White	-2.69	(3.48)	-6.53**	(2.75)	-5.01***	(1.93)	-4.62**	(1.93)
HH: Size	-3.83***	(1.47)	-1.84	(1.16)	0.31	(0.82)	0.43	(0.82)
HH: # Children	2.87	(1.91)	1.28	(1.51)	-0.33	(1.06)	-0.34	(1.06)
HH: # Seniors	1.75	(2.45)	1.83	(1.94)	0.84	(1.36)	0.76	(1.36)
HH: Cardiopulminary Diagnosis	-1.50	(3.52)	-1.75	(2.78)	-0.91	(1.95)	-0.83	(1.94)
HH Education: Bachelor or Higher	1.41	(3.96)	-1.35	(3.13)	-2.14	(2.20)	-2.12	(2.19)
HH Income: Above Median	-5.01	(3.88)	-3.48	(3.07)	-0.67	(2.16)	-0.88	(2.15)
AQ Belief: Polluted (Likert: 4 or 5)	5.43*	(3.16)	4.22*	(2.49)	1.17	(1.76)	1.61	(1.75)
AQ Info Source: Television	-3.24	(3.93)	0.19	(3.10)	2.01	(2.18)	1.98	(2.17)
AQ Info Source: Radio	9.16*	(4.79)	3.60	(3.78)	-1.54	(2.67)	-1.43	(2.65)
AQ Info Source: Government	2.11	(3.03)	0.92	(2.39)	-0.27	(1.68)	-0.60	(1.68)
AQ Info Source: Private Orgs.	-0.54	(3.58)	1.15	(2.82)	1.46	(1.99)	0.99	(1.98)
AQ Info Source: Smartphone Apps	6.05**	(2.97)	5.05**	(2.35)	1.65	(1.66)	1.56	(1.65)
AQ Info Source: Social Media	-5.16	(3.56)	-3.13	(2.81)	-0.23	(1.98)	-0.31	(1.97)
AQ Info Source: AQ Sensor	4.05	(3.09)	1.85	(2.44)	-0.43	(1.72)	-0.21	(1.71)
AQ Info Source: None	-11.32*	(6.18)	-3.68	(4.88)	2.68	(3.44)	3.05	(3.43)
AQ Info Source: Newspaper	-13.30***	(4.83)	-2.74	(3.82)	4.74*	(2.70)	4.91*	(2.69)
Referral Source: Not Atmotube	-4.30	(3.30)	-3.44	(2.61)	-1.02	(1.84)	-0.99	(1.83)
Adopter County: Urban (%)	11.98*	(7.14)	9.63*	(5.64)	2.90	(3.98)	3.30	(3.96)
Adopter Region: Midwest	-0.97	(4.13)	-3.39	(3.26)	-2.84	(2.29)	-3.00	(2.28)
Adopter Region: Northeast	4.70	(3.75)	1.69	(2.96)	-0.95	(2.08)	-1.00	(2.07)
Adopter Region: South	-1.80	(3.70)	-2.77	(2.92)	-1.76	(2.05)	-1.88	(2.04)
Offer Price ($)	0.05*	(0.03)	0.04	(0.02)	0.01	(0.02)	0.01	(0.02)
WTP ($)	0.02	(0.02)	0.00	(0.02)	-0.01	(0.01)	-0.01	(0.01)
Survey Completed: 2023Q1	4.93	(5.47)	-1.44	(4.32)	-4.21	(3.04)	-3.69	(3.03)
Survey Completed: 2023Q2	20.32***	(5.02)	1.81	(3.97)	-9.61***	(2.83)	-7.62***	(2.90)
Survey Completed: 2023Q3	17.11***	(5.15)	0.34	(4.07)	-9.27***	(2.89)	-7.57**	(2.93)
Sensor Activity Days: Months 1-6					0.56***	(0.02)	0.51***	(0.03)
App Engagement Days: Months 1-6							0.05***	(0.02)
R^2	0.14		0.08		0.54		0.55	
F-stat	2.63		1.35		18.47		18.40	
F-stat p-value	0.00		0.08		0.00		0.00	
Users	658		658		658		658	

Table 2.H.6: Endline Use Micro-environments and Changes in Perceived Pollution

	Adopters (n=251)					Non-Adopters (n=118)		
	(1) Self-reported users		(2) Less	(3) More	(4) Same	(5) Less	(6) More	(7) Same
	N	Mean	Mean	Mean	Mean	Mean	Mean	Mean
Panel A: Indoors								
Home (I)	234	0.93	0.35	0.38	0.27	0.25	0.40	0.36
Work or School (II)	122	0.49	0.30	0.27	0.43	0.06	0.22	0.72
Traveling (III)	129	0.51	0.19	0.39	0.42	0.03	0.25	0.73
Recreation (IV)	65	0.26	0.08	0.42	0.51	0.03	0.18	0.80
None (I-IV)	9	0.04	0.51	0.47	0.45	0.75	0.42	0.08
At least one (I-IV)	242	0.96	0.51	0.55	0.17	0.25	0.58	0.29
Number (I-IV)	.	2.19	0.61	0.82	0.84	0.36	1.04	2.60
Panel B: Outdoors								
Home (V)	136	0.54	0.21	0.19	0.60	0.09	0.18	0.73
Work or School (VI)	60	0.24	0.08	0.17	0.75	0.05	0.09	0.86
Traveling (VII)	108	0.43	0.17	0.19	0.64	0.09	0.20	0.70
Recreation (VIII)	77	0.31	0.16	0.17	0.68	0.06	0.12	0.82
None (V-VIII)	72	0.29	0.81	0.78	0.45	0.83	0.66	0.05
At least one (V-VIII)	179	0.71	0.26	0.31	0.48	0.17	0.34	0.52
Number (V-VIII)	.	1.52	0.36	0.39	1.38	0.30	0.59	3.11
Panel C: Overall								
Other (IX)	26	0.10						
None (I-VIII)	1	0.00	0.40	0.38	0.75	0.64	0.33	0.33
At least one (I-VIII)	250	1.00	0.60	0.62	0.75	0.36	0.67	0.96
Number (I-VIII)	.	3.71	0.85	1.07	1.80	0.65	1.64	5.71

Note: Column 1 reports the number ("N") and share ("Mean") of adopters that self-report using their sensor in each of the eight surveyed micro-environments (I-VIII) in the endline survey. Columns 2 and 5 (3 and 6) report the respective shares of adopters and non-adopters who believe that the air is significantly "Less" ("More") polluted than at baseline. Columns 4 and 6 report the respective shares of adopters and non-adopters who believe that the air is neither significantly less nor significantly more polluted than at baseline ("Same"). For adopters, the shares in columns 2, 3, and 4 are conditional on self-reporting sensor use in each micro-environment (column 1). For each column, panels A and B also present aggregate statistics for the share of all adopters (or all non-adopters) for whom the column never applies ("None"), applies in at least one micro-environment ("At least one"), and the average total number of micro-environments for which the column applies ("Number"). Panel C presents the same aggregate statistics for all eight micro-environments and the number and share of adopters who report using their sensor in another micro-environment (IX).

Table 2.H.7: Endline Defensive Expenditures

	Adopters	Non-Adopters
	(1)	(2)
	Mean	Mean
Air filter or purifier	0.46	0.60
Air humidifier or dehumidifier	0.18	0.18
Air pollution masks	0.14	0.21
Electric vehicle	0.03	0.03
Electric or hand-powered lawn care equipment	0.03	0.08
HVAC installation or maintenance	0.14	0.17
Heat pump	0.02	0.04
Induction or eletrical stove	0.03	0.05
Solar panel	0.04	0.06
Other	0.06	0.03
None	0.36	0.24
At least one	0.60	0.75
Number	1.07	1.42
Observations	251	118

Note: Column 1 reports the share of adopters and column 2 the share of non-adopters who reported making this purchase to improve air quality since baseline.

Table 2.H.8: Endline AQ Information Seeking and Defensive Actions

	Adopters (n=251)			Non-Adopters (n=118)		
	(1) Ever	(2) Less	(3) More	(4) Ever	(5) Less	(6) More
	Mean	Mean	Mean	Mean	Mean	Mean
Panel A: AQ Information Seeking						
Check govt AQ info.	0.78	0.19	0.43	0.81	0.11	0.58
Check non-govt AQ info.	0.43	0.22	0.35	0.48	0.05	0.60
None of the above	0.21	0.76	0.55	0.18	0.90	0.37
At least one	0.79	0.24	0.45	0.82	0.10	0.63
Number	1.18	0.31	0.61	1.29	0.13	0.92
Panel B: Defensive Behaviors						
Wear pollution masks	0.41	0.03	0.42	0.42	0.10	0.44
Limit time outdoors	0.54	0.08	0.40	0.56	0.11	0.47
Refrain from outdoor exercise	0.47	0.04	0.45	0.48	0.09	0.46
Reschedule or cancel trips	0.16	0.05	0.29	0.16	0.05	0.16
Open windows to ventilate	0.76	0.08	0.60	0.83	0.07	0.60
Keep windows intentionally shut	0.77	0.05	0.46	0.73	0.12	0.51
None of the above	0.06	0.65	0.27	0.03	0.68	0.30
At least one	0.94	0.13	0.73	0.97	0.19	0.70
Number	3.12	0.20	1.54	3.19	0.31	1.62
Panel C: Indoor Pollution						
Burn candles or incense	0.50	0.52	0.05	0.44	0.46	0.08
Light fires (e.g. fireplace)	0.30	0.41	0.03	0.25	0.45	0.07
Cook emissions-intensive meals	0.54	0.51	0.01	0.50	0.44	0.03
Use chemical cleaning products	0.59	0.46	0.02	0.63	0.43	0.08
None of the above	0.23	0.39	0.95	0.18	0.42	0.89
At least one	0.77	0.61	0.05	0.82	0.58	0.11
Number	1.93	1.21	0.06	1.81	0.98	0.14
Panel D: Other						
Adjust commuting route		0.05	0.10		0.09	0.10
Contact policymakers or officials		0.05	0.17		0.05	0.15
Engage in environmental activism		0.05	0.17		0.03	0.13
Move to a new home		0.09	0.10		0.14	0.16
Switch employers		0.05	0.03		0.09	0.08
Switch careers		0.06	0.03		0.13	0.07
Switch schools (or children's schools)		0.04	0.02		0.08	0.04
None of the above		0.83	0.69		0.72	0.64
At least one		0.17	0.31		0.28	0.36
Number		0.37	0.58		0.62	0.74

Note: Columns 1 and 4 in panels A, B, and C report the share of respondents (mean) who self-reported ever having done each action. Panels A, B, and C report, conditional on having ever done the action, the share who reported having done the action less often since baseline in columns 2 and 5 and the share who reported having done the action more often in columns 3 and 6. For each column in each panel, we report the average total number of variables that apply and the share for whom none and at least one of the variables apply. For variables in panel D, we do not collect information on whether respondents have ever done the action but instead ask them whether they are less (columns 2 and 5) or more likely (columns 3 and 6) to do it since baseline.

Table 2.H.9: 2SLS Adoption Impact Estimates with Control Variables

Dependent Variable:	(1) Perceive less pollution	(2) Perceive more pollution	(3) Number defensive purchases	(4) Check AQ info less	(5) Protect self more	(6) Pollute less indoors
Second-stage:						
Adoption	0.373**	0.024	-0.048	0.303**	0.011	-0.019
	(0.167)	(0.163)	(0.379)	(0.148)	(0.148)	(0.169)
Non-adopter mean	0.341	0.681	1.451	0.092	0.724	0.573
	Dependent Variable: Adoption					
First-stage:						
Offer price	-0.0036***	-0.0036***	-0.0036***	-0.0039***	-0.0037***	-0.0041***
	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0004)	(0.0005)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
FWER-adjusted p	0.160	1.000	1.000	0.232	1.000	1.000
F-statistic	68.3	68.3	68.5	63.3	69.7	69.9
Respondents	341	341	342	279	324	268

Note: Dependent variables are binary variables corresponding to endline survey responses. Familywise error rate (FWER)-adjusted p-values are calculated with the STATA rwolf2 package. Heteroskedasticity-robust standard errors in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

Table 2.H.10: OLS Adoption Impact Estimates with Control Variables

Dependent Variable:	(1) Perceive less pollution	(2) Perceive more pollution	(3) Number defensive purchases	(4) Check AQ info less	(5) Protect self more	(6) Pollute less indoors
Adoption	0.271***	-0.074	-0.304**	0.153***	-0.003	0.093
	(0.067)	(0.061)	(0.149)	(0.055)	(0.062)	(0.074)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Non-adopter mean	0.349	0.686	1.407	0.097	0.723	0.556
FWER-adjusted p	0.004	0.521	0.170	0.030	0.956	0.521
Respondents	323	323	324	262	307	254

Note: Dependent variables are binary variables corresponding to endline survey responses. Familywise error rate (FWER)-adjusted p-values are calculated with the STATA rwolf2 package. Heteroskedasticity-robust standard errors in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

2.I Additional Graphics

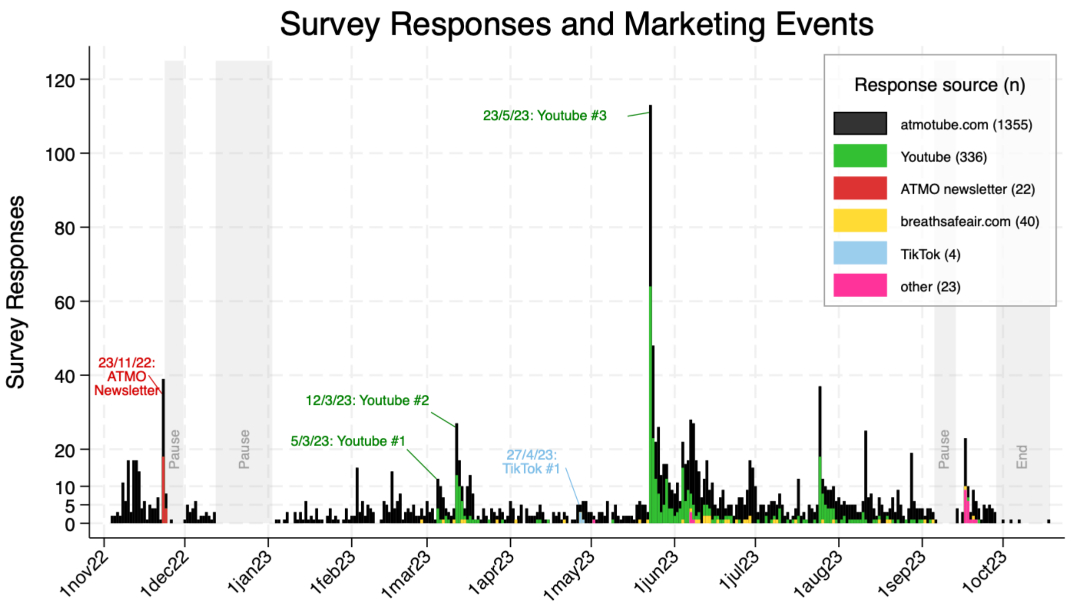

Figure 2.I.1: Survey responses by survey start date and response source. Response source categories are stacked in each bar so that each bar's total height corresponds to the total number of survey responses on a given day. Our intervention was paused during Black Friday, Holiday, and Fall sales (marked in grey).

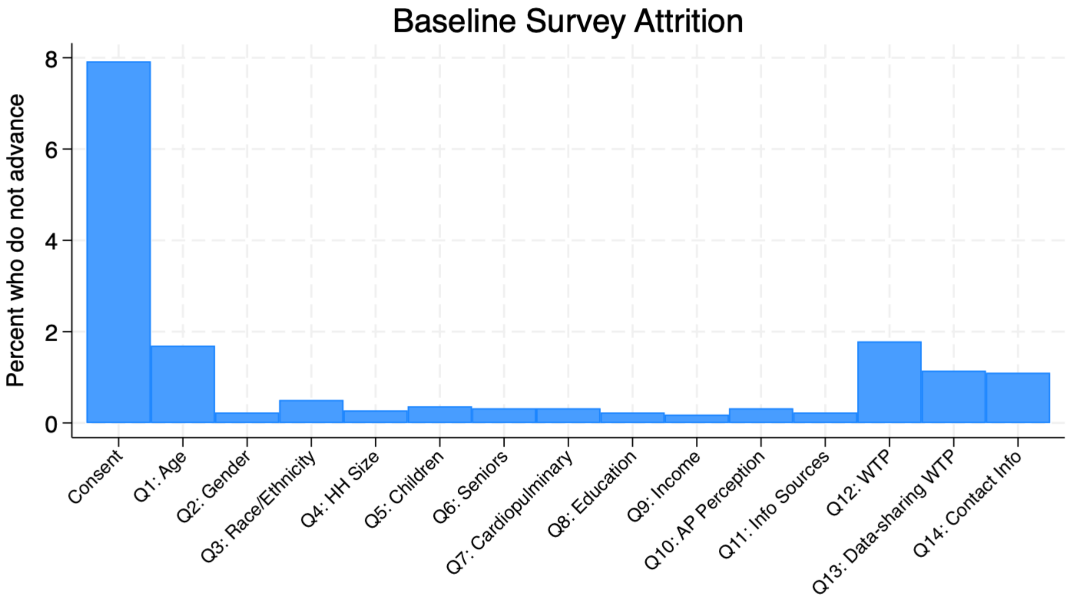

Figure 2.I.2: Baseline survey attrition by question. Of 2,581 survey participants who started the survey, 2,184 completed it (84.6%).

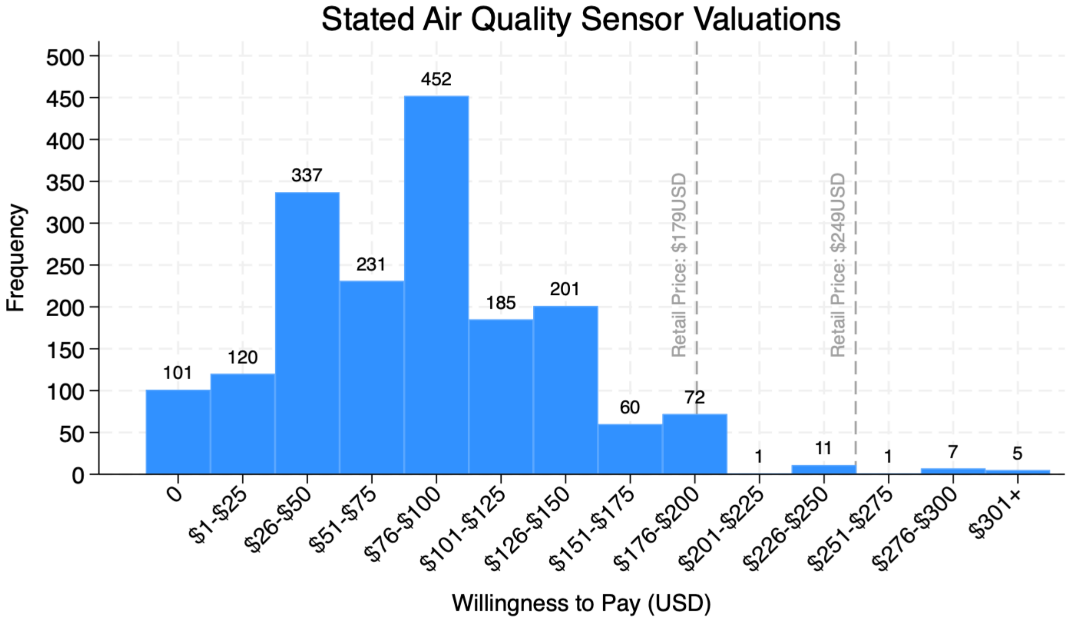

Figure 2.I.3: Stated sensor valuation histogram (n=1,784 survey respondents). Retail price marked with dashed line.

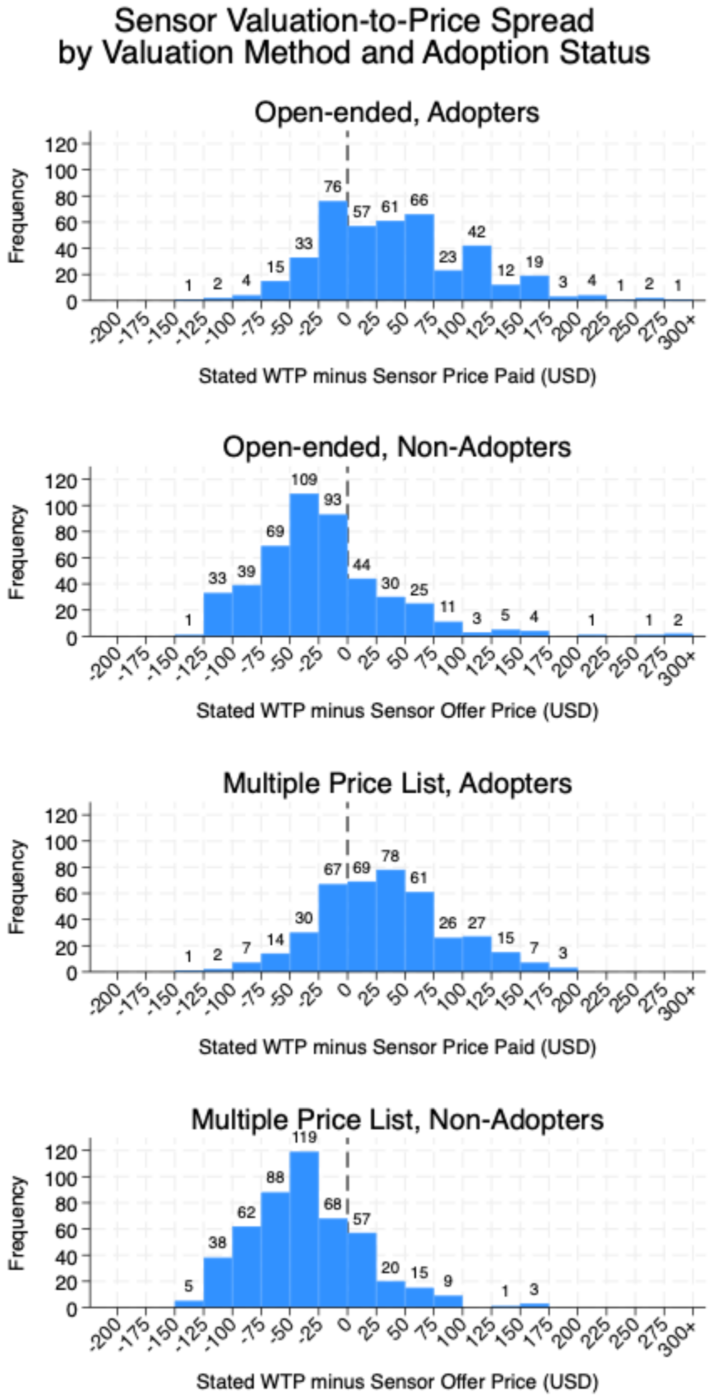

Figure 2.I.4: Comparison of sensor WTP-price spread for adopters by WTP elicitation method.

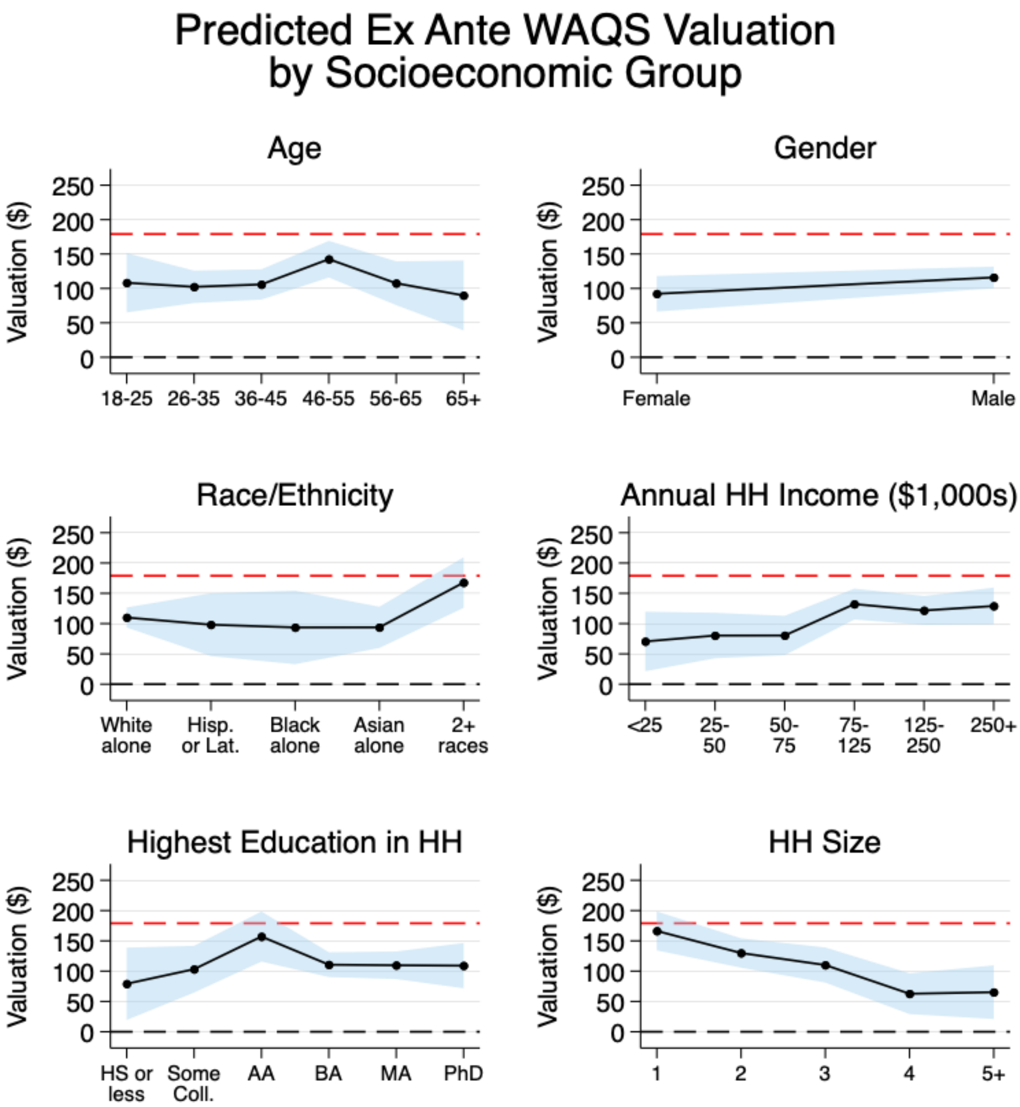

Figure 2.I.5: Predicted ex ante WAQS valuation from a linear regression on respondent and household (HH) characteristics for 1,784 baseline survey respondents. Red dashed line marks current WAQS market price ($179). Margins estimated for MPL with 95% confidence intervals.

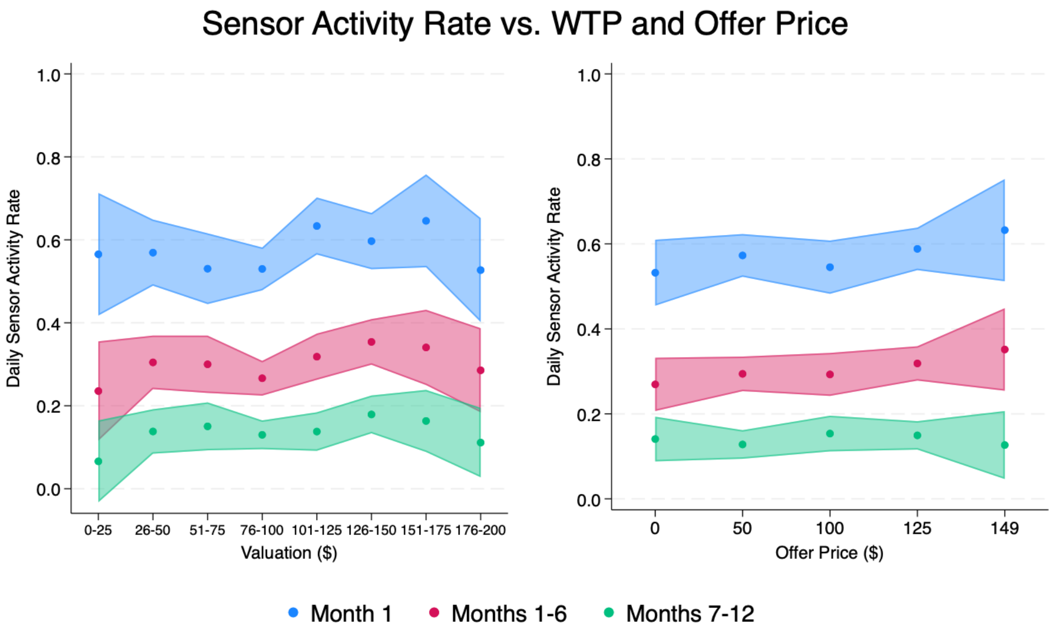

Figure 2.I.6: Daily sensor activity rates by ex-ante willingness to pay and randomized offer price with 95% confidence intervals.

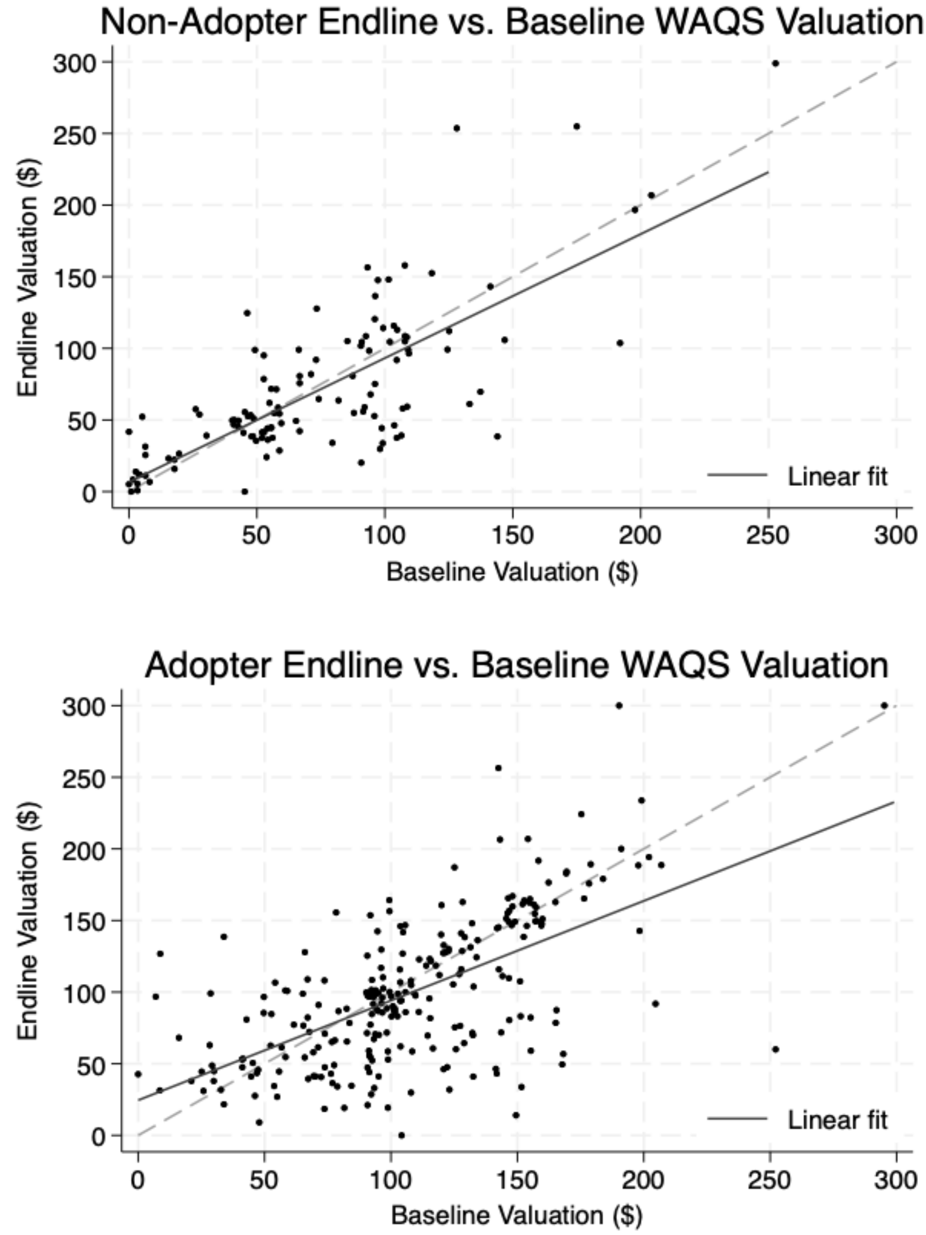

Figure 2.I.7: Scatter plots of endline vs. baseline sensor valuations for non-adopter and adopter endline respondents. Linear fit (solid) and 45 degree line (dashed). Points jittered and one outlier (1000, 200) removed for clarity.

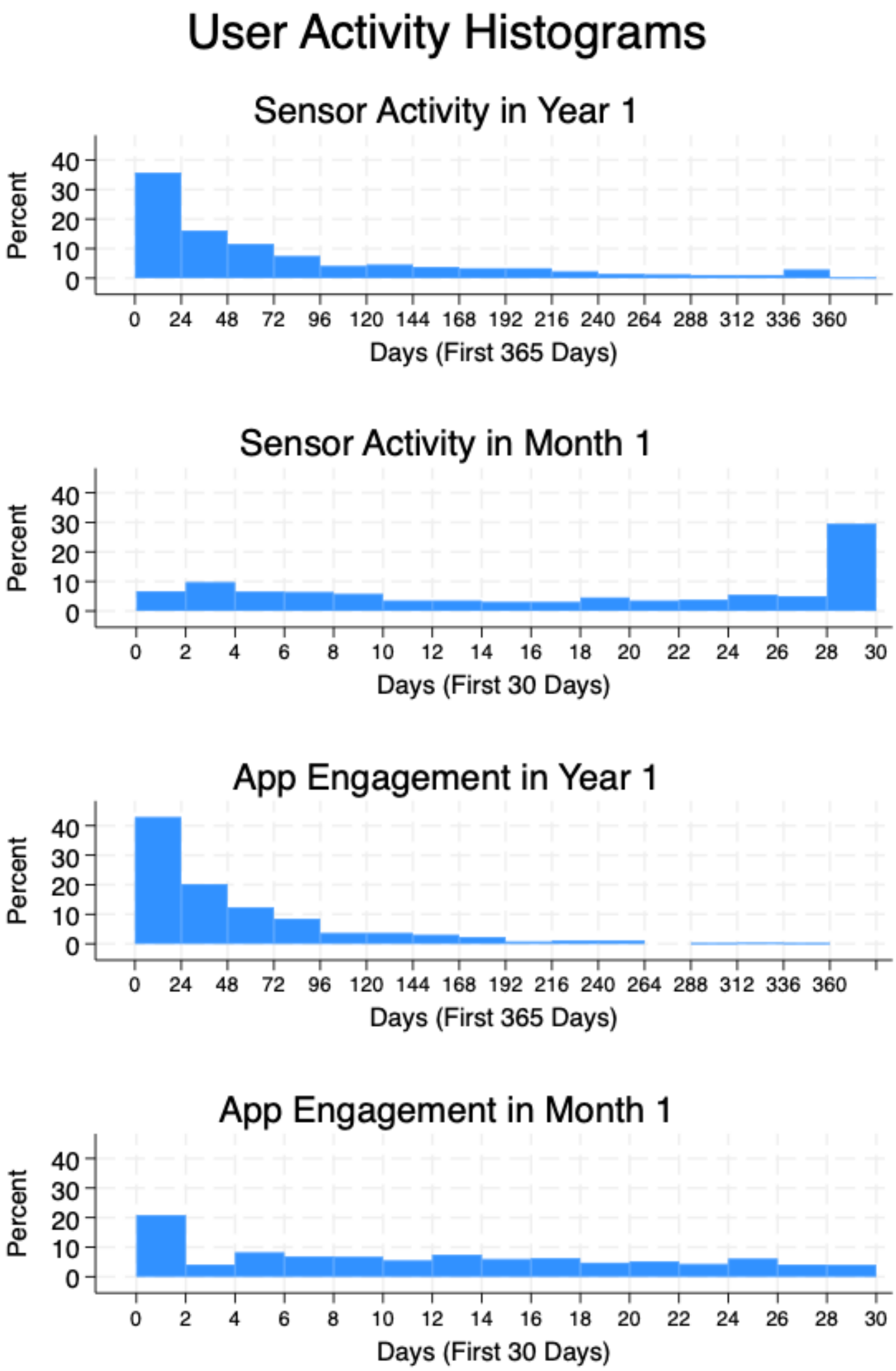

Figure 2.I.8: User activity histograms displaying the number of days observed adopters (n=658) use their WAQS and engage with the app in the first year and in the first month.

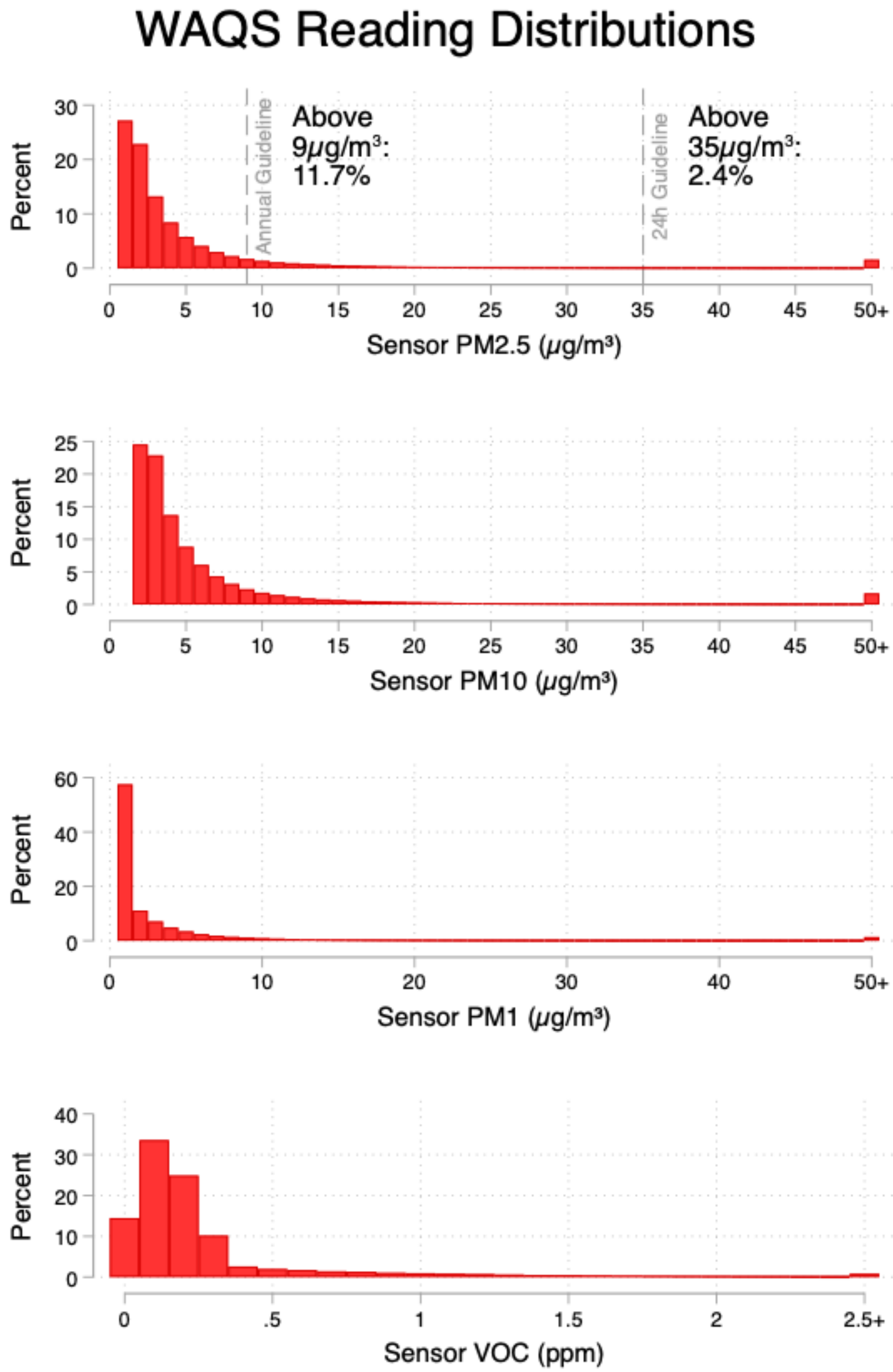

Figure 2.I.9: PM and VOC readings histograms winsorized at 50µg/m^3 and 2.5ppm, respectively. PM readings are capped at 999µg/m^3. US EPA $PM_{2.5}$ guidelines for 24-hour periods (35µg/m^3) and annually (9µg/m^3) indicated with vertical lines.

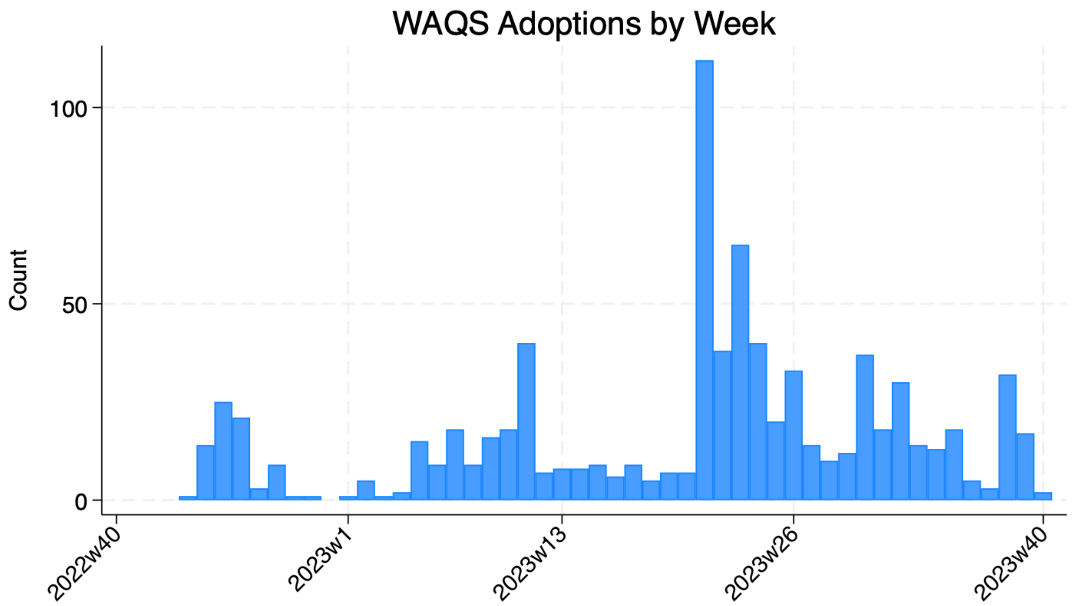

Figure 2.I.10: Weekly WAQS adoptions through our campaign.

AQI Basics for Ozone and Particle Pollution

Daily AQI Color	Levels of Concern	Values of Index	Description of Air Quality
Green	Good	0 to 50	Air quality is satisfactory, and air pollution poses little or no risk.
Yellow	Moderate	51 to 100	Air quality is acceptable. However, there may be a risk for some people, particularly those who are unusually sensitive to air pollution.
Orange	Unhealthy for Sensitive Groups	101 to 150	Members of sensitive groups may experience health effects. The general public is less likely to be affected.
Red	Unhealthy	151 to 200	Some members of the general public may experience health effects; members of sensitive groups may experience more serious health effects.
Purple	Very Unhealthy	201 to 300	Health alert: The risk of health effects is increased for everyone.
Maroon	Hazardous	301 and higher	Health warning of emergency conditions: everyone is more likely to be affected.

Figure 2.I.11: EPA AQI classification with cut-off values and general health recommendations.

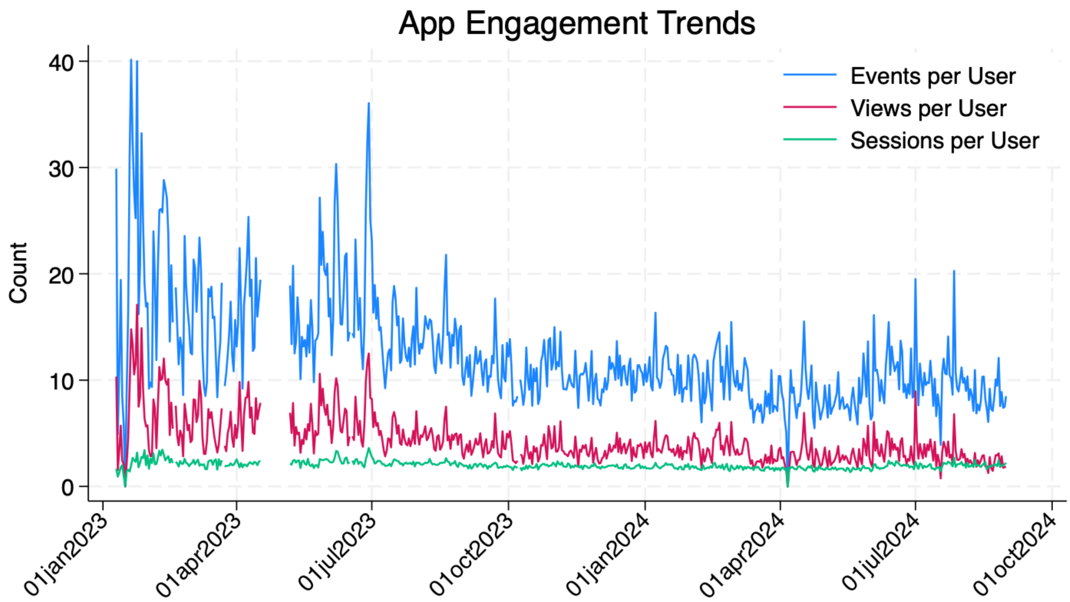

Figure 2.I.12: Daily app engagement statistics. Data missing from April 18, 2024 to May 6, 2024

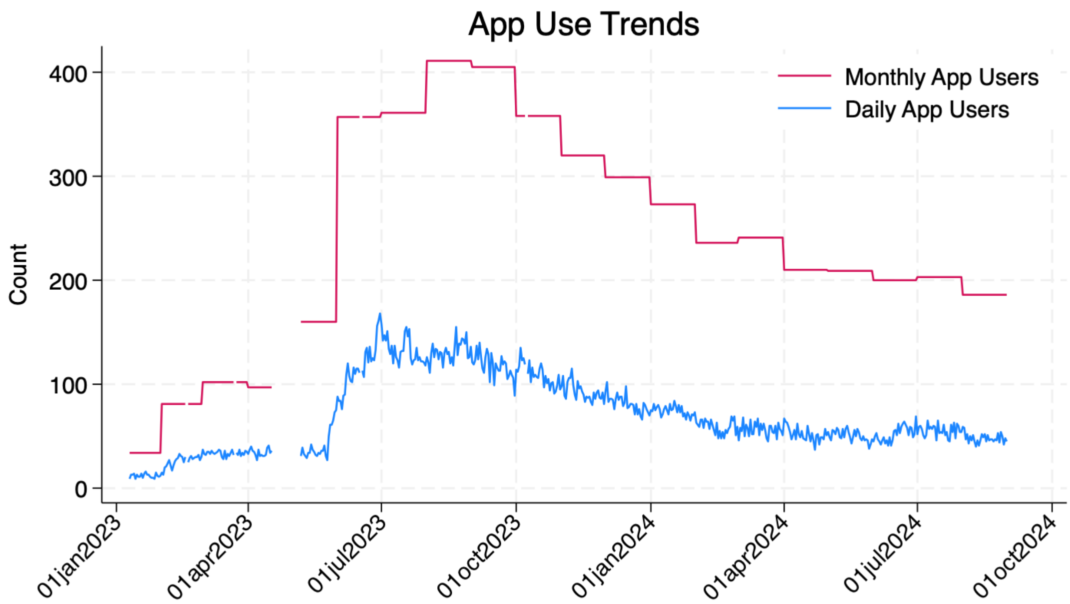

Figure 2.I.13: Number of daily and monthly app users. Data missing from April 18, 2024 to May 6, 2024

3
Stationary Air Quality Sensor Adoption: The Role of Socioeconomics, Government Monitors, and Nearby Sensors

Stationary Air Quality Sensor Adoption: The Role of Socioeconomics, Government Monitors, and Nearby Sensors

Alexander Dangel and Timo Goeschl

Abstract

Citizens have greatly expanded ground-based air quality data coverage by purchasing and installing stationary air quality sensors (SAQS) to collect and publicly disclose information about air pollution levels in real-time. We study the adoption of this emerging non-regulatory monitoring technology using data through 2022 from one of the two largest SAQS networks globally. Our analysis closely examines adoption determinants and spatiotemporal diffusion patterns in Germany, a global leader in SAQS adoptions. Regression results show that income and green political preferences are two primary adoption determinants. Moreover, SAQS are installed more often near government monitors in Germany, but evidence that monitor non-compliance drives additional adoptions is weak. In line with its unique local public goods properties, we demonstrate that SAQS installations have local spatial spillovers by employing fixed effects panel models with spatiotemporal neighbor variables. Our findings reveal private air quality data coverage disparities and shed light on the relationship between government and private monitoring.

Keywords: air pollution information; technology adoption; pollution sensors; socioeconomic status; government monitoring; compliance; peer effects

JEL Classification: Q50, Q53, Q55, D63

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3.1 Introduction

Starting in the 1960s, a limited number of governments began collecting and disclosing to the public ambient air pollution information using spatially sparse networks of stationary air quality (AQ) monitors. In the 2010s, citizens around the world upended this paradigm when they started purchasing and installing stationary AQ sensors (SAQS) to independently monitor pollution and publish readings to public maps on the Internet. From an economic standpoint, the emergence of SAQS presents two novel developments. First, in most countries, non-regulatory monitoring networks now provide an order of magnitude increase in AQ data coverage compared to existing regulatory networks. This data could contribute to improving pollution exposure assessments and AQ information programs. Second, SAQS technologies produce publicly available pollution information, a local impure public good, which make understanding the factors affecting its diffusion of key interest for future deployments. Despite its potential future applications and unique public good characteristics, private AQ monitoring is not yet well understood.

This paper aims to answer three main questions about SAQS adoption. First, are there geographic or socioeconomic disparities in SAQS coverage? Second, are SAQS installed at different rates near existing government monitors? And third, do existing SAQS installations have spillover effects on new SAQS adoption decisions? To address these questions, we closely examine SAQS adoption determinants and spatiotemporal diffusion patterns in Germany, one of the countries with the most adoptions worldwide (over 12,000 SAQS installed through 2022).[1] We construct panel datasets using SAQS installation data for Germany from 2016 to 2022, municipal (*Gemeinde*) and neighborhood (1-km^2 grid cell) socioeconomic information, government AQ monitoring data, and remote-sensed satellite pollution data. We then use Poisson pseudo maximum likelihood (PPML) regression to characterize the relationship between SAQS adoption and socioeconomic and geographic factors. Following the technology adoption literature, we augment our

[1]Sensor.Community is one of the world's largest private AQ monitoring networks and originated in Germany in 2015. In July 2022, 40.6% of global Sensor.Community sensor adoptions (11,873 of 29,213) were located in Germany. For comparison, there are about 1,000 active government AQ monitors in Germany.

neighborhood panel dataset with spatiotemporal neighbor variables to estimate the causal effect of existing nearby SAQS installations on new adoptions.

Our paper provides three main contributions. First, we provide empirical evidence about SAQS adoption disparities and determinants outside the United States for the first time. We show that SAQS demand is skewed towards municipalities and neighborhoods with higher socioeconomic status and higher pollution levels. Municipalities that are more urban, younger, more green-party voting, and have higher average incomes demonstrate higher SAQS adoption rates. Using fine-grained income data, we find, however, that mean household income is not consistently associated with neighborhood SAQS adoption rates, in particular when comparing neighborhoods within the same municipality. This finding from Germany contrasts with previous evidence from Mullen et al. (2022) who document lower SAQS adoption rates in socioeconomically disadvantaged neighborhoods within Los Angeles County but aligns with results from the US that SAQS are a normal good at coarser spatial scales (Coury et al., 2024; Zivin et al., 2024; Burke et al., 2022; deSouza and Kinney, 2021).

Second, we document that neighborhoods closer to government monitors experience more SAQS adoptions, suggesting a complementary relationship between government and private monitoring initiatives. For each additional kilometer that a neighborhood is located from a government monitor, per capita SAQS adoptions decrease by 1%-3% all else equal. Although we also observe substantially higher SAQS adoption rates in municipalities with government monitors and even higher SAQS adoption rates in municipalities with government monitors that are non-compliant with European Union AQ standards (Directive 2008/50/EC), these differences fail to survive regression analyses that control for socioeconomic factors. For comparison, Coury et al. (2024) find no evidence for a relationship between government monitors and SAQS adoptions, while deSouza and Kinney (2021) find a relationship at the national level in the US but not in California, and Zivin et al. (2024) demonstrate a substitutive relationship between SAQS adoptions and government monitors.

Third, we contribute to a growing literature on peer effects in green technology adoption (Bigler and Janzen, 2023; Rode and Weber, 2016; Graziano and Gillingham, 2014) with evidence of a novel spatial peer effect in SAQS adoption in line with a theorized crowding out effect from the provision of a local impure public good. Our analysis shows that recent SAQS installations have a negative effect on new sensor adoptions nearby. For each additional SAQS installation within 2,500 meters, per capita SAQS adoptions decrease by between 18% and 27%. However, this effect flips in sign to +13.3% from 2,500 meters to five kilometers and becomes inconsistent and generally statistically insignificant beyond five kilometers. These findings are robust to alternative distance band specifications, time windows, and temporal lags.

In the following section, we provide readers with background information about existing AQ monitoring programs, SAQS hardware, and global SAQS adoption trends. Section 3.3 lays out our hypotheses about factors influencing SAQS demand. Section 3.4 summarizes our data, and section 3.5 describes our empirical approach. Section 3.6 details our results before section 3.7 concludes.

3.2 Background

3.2.1 Government Air Quality Monitoring

Governments in high-income countries and a growing number of middle and low-income countries conduct ambient AQ monitoring for key health-relevant pollutants such as particulate matter (PM), nitrogen dioxide (NO_2), and ozone (O_3) using networks of ground based monitors with highly accurate reference instruments.[2] Government monitoring has two main objectives: i) to provide ground truth data for regulating emissions from polluting sectors of the economy and ii) to publicly disclose representative pollution exposure information for numerous

[2]The United Kingdom established its pioneering national ambient air monitoring network in 1961. Other high-income countries created similar air monitoring programs in the following decades. China deployed a national network starting in 2013, and other middle and low-income countries are developing their own programs. The World Health Organization Ambient Air Quality Database (2023), Martin et al. (2019) and Larkin et al. (2017) collect data from governments on $PM_{2.5}$ and NO_2 monitor locations worldwide and find large global disparities in government AQ information provision. Martin et al. (2019) demonstrate that most countries have not installed ambient $PM_{2.5}$ monitors (141 of 234 countries have none) and only few have achieved $PM_{2.5}$ monitor densities above 3 monitors per million inhabitants (24 of 234 countries). Larkin et al. (2017) show that NO_2 monitors are installed at the highest rates in Europe, North America, and Asia, while countries in Africa and Oceania have little to no NO_2 coverage.

purposes including warning at-risk populations during air pollution events, raising general pollution awareness, and making data available for research and pollution exposure assessments. Mounting evidence shows that government AQ monitoring and information programs improve health and economic outcomes (Barwick et al., 2024; Jha and La Nauze, 2022; Saberian et al., 2017; Graff Zivin and Neidell, 2009), but recent evidence suggests that government AQ information collection and disclosure is suboptimal.

One major challenge confronting policy-makers is that ambient air pollution can vary highly across geographic space, but current government networks are not designed to measure pollution at fine spatial scales (Carter et al., 2023). Ambient pollution levels can, for example, vary substantially between adjacent city blocks within neighborhoods (Apte et al., 2017; Wang et al., 2021b), meaning that pollution readings recorded at one location may not be representative of concentrations less than a few hundred meters away. Installing additional government monitors to capture this spatial variation is considered prohibitively expensive. Instead, authorities use monitor data and statistical modeling to generate pollution estimates for unmonitored locations.[3] In turn, modeled pollution estimates are aggregated spatially and employed in pollution assessments and AQ information programs (e.g. metropolitan AQ alerts). While exposure modeling is useful in many settings, two main factors threaten its suitability for specific policy-relevant applications.

First, biases introduced by underlying pollution measurement protocols and reinforced by statistical modeling may generate substantial non-classical measurement error in pollution assessments based on government data.[4] Previous research has documented how sparse government monitoring (Sullivan and Krupnick, 2018; Fowlie et al., 2019), endogenous monitor siting (Grainger and Schreiber, 2019; Muller and Ruud, 2018), and strategic monitoring (Mu et al., 2021; Zou, 2021) limit the spatial and temporal scope of government AQ data. It is unclear ex ante, in which direction a given bias will push exposure assessments. However, these

[3]Increasingly complex modeling has improved predictive performance using supplementary remote sensing, geographic information systems (GIS), traffic, and weather data.

[4]Non-classical measurement error occurs when measurement error is correlated with the true value of the parameter. In contrast, classical measurement error occurs when measurement error is independent of the true value of the parameter.

shortcomings could be particularly undesirable for policy-makers when they lead to pollution underestimates. For example, if monitors are more frequently sited in less polluted areas (Grainger and Schreiber, 2019), and policy-makers mistakenly assign these low exposures to nearby unmonitored populations, policy-makers risk under-regulating the pollution harming these populations. If populations are, in fact, differently (more or less) exposed compared to official estimates, inaccuracies would distort policy design relative to the social optimum and potentially undermine policy-makers' distributional intentions.

Second, aggregate pollution information may not adequately reflect individual pollution exposure,[5] jeopardizing individual pollution adaptations and the effectiveness of interventions targeting them. Individual pollution avoidance, mitigation, and characteristics like place of residence or occupation can lead to substantial disagreement between estimates and actual exposure (Lim et al., 2022; Steinle, 2013). Combined with idiosyncratic beliefs about pollution exposure and its damages, individuals weighing the costs and benefits of pollution adaptation may be biased in deciding if and when to adapt based on potentially inaccurate or irrelevant aggregate exposure information. An emerging literature studies whether and to what extent aggregate AQ information induces behavioral change, finding that sensitive populations (e.g. children and elderly) do respond to ambient pollution alerts by avoiding outdoor exposure (Saberian et al., 2017; Noonan, 2014; Graff Zivin and Neidell, 2009), but these programs fail to engage general population groups during dangerous pollution episodes (Sexton Ward and Beatty, 2016) and at-risk populations on consecutive alert days (Saberian et al., 2017; Graff Zivin and Neidell, 2009). This underscores the behavioral costs of adapting to pollution exposure and inefficiencies in existing information programs.

While further expanding government monitoring coverage in countries with high monitor densities remains excessively expensive, more comprehensive pollution information could help to alleviate some concerns and create knock-on effects. In China, for example, air quality monitoring programs positively impact citizen happiness through their role in lowering pollution levels (Wang et al., 2021a). In Germany, municipalities have responded to widespread non-compliance with EU AQ standards by implementing low emissions zones (LEZs) and other pol-

[5]This is known as the ecological fallacy (Banzhaf et al., 2019).

icy measures that reduce pollution and impact life satisfaction (Sarmiento et al., 2023).[6] In other contexts, new monitoring approaches, relying for example on aerial and remote sensing technologies, may shift policy-makers' optimal blend of AQ monitoring technologies. Existing pollution monitoring networks could increase coverage by complementing ground-based monitors with more cost-effective alternatives.[7]

3.2.2 Stationary Air Quality Sensors

In recent years, technological innovations have created the opportunity for manufacturers to introduce consumer stationary air quality sensors (SAQS) priced between \$25 and \$500 to market.[8] These devices measure and display readings of real-time concentrations for key air pollutants such as PM (PM_{10}, $PM_{2.5}$, PM_1), NO_2, or CO_2. Readings are usually stored on local memory and can then be uploaded to network databases via a wireless connection. Hardware specifications can differ in terms of sensor components (target pollutant, sensor technology, and accuracy), display (on sensor or another connected device), data connectivity (Bluetooth, WiFi, mobile data), power source (battery, outlet, solar), design, size, and network compatibility (e.g. Sensor.Community, PurpleAir, etc.). SAQS can be installed outside when designed to be weather-proof to measure ambient air pollution (e.g. mounted to window sills, balconies, lamp posts, etc.) or indoors to track indoor air pollution. This paper focuses on the adoption of outdoor, outlet-powered PM SAQS that display pollution readings on Wi-Fi connected devices and automatically upload pollution readings to publicly-available online network

[6] As depicted in figure 3.D.3, pollution concentrations measured at a large share of German regulatory monitors were not in compliance with EU ambient AQ standards from 2001 to 2021. Although PM_{10} targets were met for the majority of PM_{10} monitors starting in 2012, many NO_2 monitors remained non-compliant until recently.

[7] For examples in US, see `https://www.epa.gov/sciencematters/tempo-new-era-air-quality-monitoring-space` or `https://www.epa.gov/sciencematters/future-emissions-testing-looking-how-epa-using-drones-test-air-quality`

[8] The United States Environmental Protection Agency (EPA) defines a low-cost air quality sensor as priced below \$500. See Snyder et al. (2013); Kumar et al. (2015); Castell et al. (2017) for a review of technical details of these innovations. The EPA's Air Sensor Toolbox and AQ-SPEC (Air Quality Sensor Performance Evaluation Center) maintain lists and field evaluations of commercially-available SAQS. This paper focuses on outdoor SAQS, but indoor SAQS and wearable AQ sensors (WAQS) are also increasingly available.

maps and databases.

PM SAQS rely on light scattering technology to count the number and size of airborne particles passing an optical sensor drawn in via an intake fan. Sensor components are small and can continuously measure accurately despite their low cost. PM SAQS performance differs between sensor products and in comparison to reference grade instruments in laboratory and field settings.[9] Readings from the most accurate SAQS and professional reference instruments correlate strongly in laboratory settings (R^2 above 0.95) and in field settings (R^2 above 0.90). Humidity, extreme temperatures, and other environmental factors are known to negatively affect sensor performance in the field. For data quality assurance, installation protocols from manufacturers, non-regulatory network operators (e.g. PurpleAir or Sensor.Community), and government institutions (e.g. EPA) encourage adopters to site sensors according to a set of guidelines, but adherence is often self-enforced.[10] Adopters may choose to forgo maintenance, to opt-out of publicly sharing their readings, or to uninstall their sensors, at which point their sensors cease to upload data to public data archives.[11]

This paper analyzes data from one of two well-established global private monitoring networks: Sensor.Community (formerly Luftdaten.info).[12] Sensor.Community was founded in Stuttgart, Germany in 2015. The open data, volunteer-led initiative uses a DIY ("do-it-yourself") sensor kit that costs approximately \$25.[13] Adopters can either purchase a pre-assembled sensor kit or purchase the individual components and assemble it themselves with instructions and firmware provided free of charge by Sensor.Community. Installation requires plugging the device into an electrical outlet and connecting it to Wi-Fi. After installation, Sensor.Community

[9] The coefficient of determination between PM sensor readings and reference instruments readings ranges from ∼0.0 to 0.99 (EPA's Air Sensor Toolbox, AQ-SPEC).

[10] For example, Sensor.Community recommends locating sensors 1.5 to 3.5 meters above street-level in a well-ventilated location and asks for a picture of the sensor installation location upon registration to confirm correct installation.

[11] Figure 3.D.2 in appendix 3.D demonstrates that Sensor.Community sensor activity status peaks at installation and falls below 80% within the first year after installation. Around 40% of Sensor.Community installations remain active five years after installation.

[12] The largest non-regulatory network is PurpleAir, and there are other smaller networks like Airly and AirGradient.

[13] The sensor kit includes the Nova Fitness SDS011 PM sensor, temperature and humidity sensors, an Arduino microprocessor, a Wi-Fi chip, and weather-resistant sensor housing. Adopters can opt-in to contributing their AQ data to a publicly available online map and data archive.

SAQS continuously measure PM and share readings with a centralized database and publicly-available map when connected to the Internet.[14] Sensor.Community estimates the total annual electricity costs between approximately \$2-\$4, and the expected sensor lifetime is five years.[15]

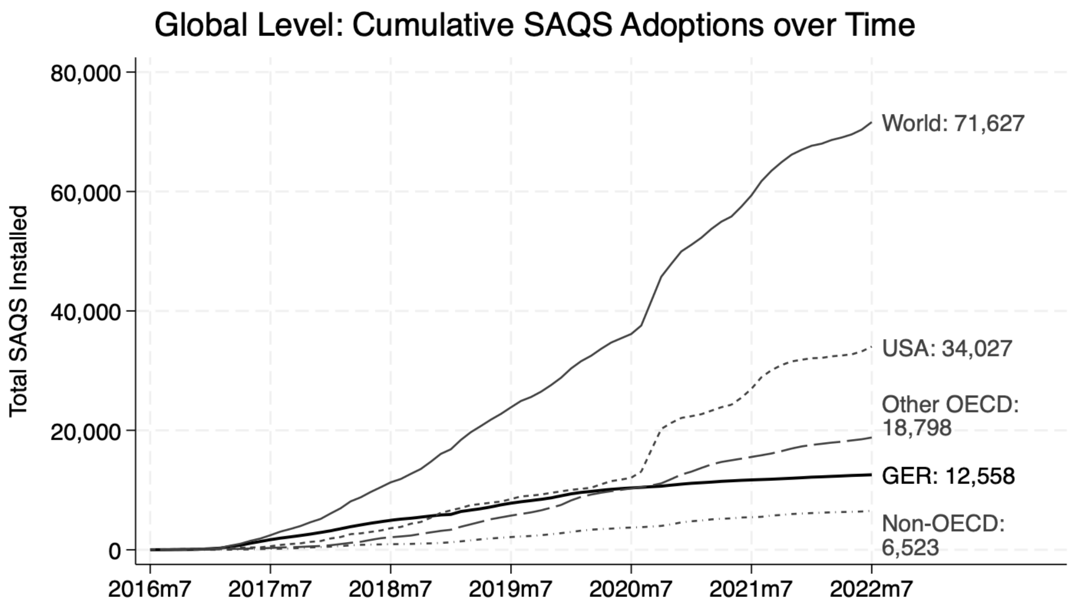

Figure 3.2.1: Cumulative stationary air quality sensor (SAQS) installations in the Sensor.Community and PurpleAir networks through July 2022 worldwide, in USA and Germany, 36 other OECD countries, and non-OECD countries.

The emergence of SAQS demonstrates technological progress and falling sensor costs, but also reveals a latent demand for better AQ information. Figure 3.2.1 shows that SAQS propagated rapidly in high-income countries like Germany, the USA, and other OECD countries with substantial government AQ monitoring networks and relatively low air pollution levels compared to middle and low-income

[14]The Sensor.Community online map displays real-time $PM_{2.5}$ and PM_{10} 5-minute averages and 24 hour trends for each individual sensor and averages readings within increasingly large hexagonal grid cells at greater spatial scales (see figure 3.D.1 in appendix 3.D). Measurements are not stored on local device memory, so if the Internet connection is interrupted readings will not be uploaded to the central database.

[15]The estimated sensor power consumption is 1 watt, totalling 8.76 kWh annually. With an average 2021 German household electricity price of €0.32/kWh, this amounts to approximately €2.80 annually and €14.00 over the sensor's lifetime.

countries.[16] USA and Germany stand out as the two countries with the highest absolute number of SAQS adoptions in each network, with the USA having a larger share of all PurpleAir adoptions than Germany has of Sensor.Community adoptions.

Recent evidence from the USA on SAQS adoptions indicates a positive relationship between local socioeconomic factors and adoption. Using locations of active PurpleAir sensors in the USA, deSouza and Kinney (2021) find that AQ sensor are more often located in census tracts where i) residents have higher average incomes and higher education levels, ii) a greater share of residents are White, iii) existing regulatory monitors are located, and iv) $PM_{2.5}$ concentrations are higher. Mullen et al. (2022) find similar results in a small-scale study of PurpleAir adoptions in Los Angeles County, documenting that neighborhoods with lower average incomes and greater shares of Black and Hispanic residents have lower sensor installation densities. To the best of our knowledge, SAQS adoption has not previously been evaluated outside the USA.

3.3 Technology Adoption

Theories explain new technology diffusion as the product of individual and social processes occurring simultaneously across geographic space (Griliches, 1957; Bass, 1969; Geroski, 2000). Information flows and social interactions determine who learns about the existence of an innovation and its potential benefits. After becoming aware of it, prospective adopters' individual characteristics and surrounding social and informational environments play into their adoption decisions. For those who adopt, experiences with the technology contribute in turn to evolving informational and social landscapes that later affect subsequent prospective adopters' decisions. Over time, this social learning process determines a technology's final market penetration rate and eventually informs the design of subsequent innova-

[16] Figure 3.A.1 in appendix 3.A shows that countries in the Global North dominate global SAQS adoptions through July 2022 (63,502). Sensor.Community installations (31,394) are concentrated in European countries like Germany (12,489), the Netherlands (3,410), Belgium (2,241), Bulgaria (1,795), and Poland (1,731), while the greatest concentrations of PurpleAir installations (46,405) are in non-European countries like USA (33,784), Canada (1,825), India (626), Mexico (595), and Australia (457).

tions. As with other technologies, a model explaining SAQS adoptions is governed by these general processes.[17] However, several unique SAQS properties presumably factor into its diffusion. In this section, we discuss our hypotheses for how individual and social factors determine SAQS adoption.

3.3.1 Income and Socioeconomic Factors

When considered in isolation, SAQS are consumer goods that enable adopters to collect AQ information and assess pollution exposure. Prospective SAQS adopters presumably weigh expected informational benefits from SAQS adoption (i.e. expected utility change from learning the difference between known exposure and true exposure) against the price of adoption.[18] Adoption may be particularly valuable to individuals who believe they can benefit from revealing unknown information about personal pollution exposure, for example, by monitoring emissions from a nearby pollution source or tracking the effectiveness of pollution-related adaptations.[19] Previous economics research on preferences for green consumption (Welsch and Kühling, 2009), hybrid vehicles (Narayanan and Nair, 2013), and solar panels (Rode and Weber, 2016; Graziano and Gillingham, 2014; Müller and Rode, 2013; Bollinger and Gillingham, 2012) show that income is a key adoption determinant of other environmental products and technologies.[20] Because SAQS adoption is closely tied to demand for environmental quality (Greenstone and Jack, 2015), we expect income to be a main adoption determinant. Higher-income groups may be more likely to adopt SAQS because they have the time and financial resources to take defensive actions to protect themselves if true exposure is higher than known exposure (i.e. individuals who are willing and able to purchase air purifiers, switch jobs, or move to a different home). Previous research from California (Burke et al.,

[17] Accordingly, we expect SAQS adoptions to trace an S-shaped logarithmic curve over time as documented previously for the diffusion of other new technologies (Griliches, 1957; Geroski, 2000). This is confirmed by figure 3.2.1.

[18] Hausman and Stolper (2021) derive a housing choice model that explains disparities in pollution exposure through information disparities. Their analysis is a useful starting point for thinking about known and true (unknown) exposure.

[19] Prospective adopters may also consider private benefits from information on real-time pollution trends, pollution source attribution, adoption-related environmental status signaling, or other secondary benefits.

[20] One notable exception is heat pumps (Davis, 2024).

2022; Mullen et al., 2022) and the USA more generally (deSouza and Kinney, 2021) shows that SAQS are a normal good (positively associated with income), and we anticipate that the same relationship holds in Germany. Accordingly, we formulate our first hypothesis about SAQS adoption:

Hypothesis 1 *Higher income households disproportionately adopt SAQS, and household income is positively associated with SAQS adoption rates, holding all else equal.*

Green political preferences are also likely to determine SAQS adoption, in part because of their association with individual income, but also due to their relationship to education and its role in shaping individual air quality knowledge and beliefs. Limited knowledge about pollution exposure, its health impacts, and effective adaptation approaches might considerably restrict the population share for whom adoption has expected net benefits. Individuals who support environmental policies politically are likely to be disproportionately informed about pollution exposure and its impacts, suggesting that their benefits from adoption may be relatively large. More specifically, these could be individuals who would like policymakers to implement stricter pollution control policies, who face high damages from not minimizing pollution exposure (e.g. individuals sensitive to pollution or with pre-existing health conditions), or who are curious about true exposure. We believe this makes green voters more likely to adopt SAQS and postulate the following hypothesis:

Hypothesis 2 *Green voters disproportionately adopt SAQS, and green voting behavior is positively associated with SAQS adoption rates, holding all else equal.*

3.3.2 Pollution

In line with previous empirical research about pollution and residential sorting (Banzhaf and Walsh, 2008), SAQS adopters' geographic locations should relate to spatial patterns in ambient pollution. However, the exact nature of this relationship is unclear. On the one hand, households who value air quality more highly and are presumably more likely to adopt SAQS *ex ante* might choose to live in areas further from pollution sources where they are less exposed. This could lead to an inverse relationship between SAQS adoptions and ambient pollution concentrations (and other correlated disamenities such as population density, noise, and traffic congestion).[21] On the other hand, assumptions underlying residential sorting (full information and frictionless moving) are unlikely to be fulfilled, and adopting a SAQS could precede relocation efforts. In this case, it is possible that ambient pollution and SAQS adoptions are positively related. Previous evidence shows that the salience of pollution sources can mediate the relative strength of income and pollution effects. In the United States, for example, highly salient wildfire events drive new SAQS adoptions but only in areas with higher socioeconomic status (Coury et al., 2024). We posit:

Hypothesis 3 *Households exposed to higher pollution levels disproportionally adopt SAQS, but the direction of the relationship between pollution and SAQS adoptions is ambiguous, holding all else equal.*

3.3.3 Government Monitoring

We believe SAQS adoptions relate to spatial patterns in government monitoring because monitor locations reveal information about ambient pollution concentrations and government AQ information quality. For adopters, information collected by SAQS competes with AQ information supplied by governments. SAQS data still likely improves upon existing information by providing more personally relevant information than the nearest government monitor.[22] In complete absence of proximate government monitoring and disclosure programs, SAQS will produce entirely novel information.[23] When present, government monitors provide both an

[21] Air pollution sources can be highly localized due to factors like emissions from nearby restaurants, street-specific traffic conditions, etc.

[22] Because air pollution can vary highly over short distances, individuals who live further from government monitors might expect their nearest monitor to be less representative of their personal exposure than individuals who live in direct proximity to the next monitor. Direct comparisons between SAQS and monitors require assessments of measurement performance. If, for example, monitors are poorly sited, they may not reflect population exposure very well.

[23] Although certain air quality parameters can, to a very limited extent, be perceived directly through sight, smell, and physiological responses, many harmful pollutants are imperceptible because they are odorless, invisible, and only produce physiological responses with delay.

indicator of baseline AQ levels through the data they supply and, when targeted at specific polluters (e.g. traffic or industrial plants), capture regulatory knowledge about where pollution levels might be highest. Hence, we postulate that individuals are more likely to adopt a SAQS if they are located closer to government monitors, and formulate the following hypothesis:

Hypothesis 4 *SAQS adoptions are negatively associated with distance to government AQ monitors.*

Furthermore, we expect monitor non-compliance with EU AQ regulations to amplify the salience of local pollution and thereby increase SAQS demand. Thus, we formulate a second hypothesis relating government monitoring and SAQS adoptions:

Hypothesis 5 *SAQS adoptions are positively associated with government AQ monitor non-compliance.*

3.3.4 Adoption Spillovers

Thus far, we have assumed SAQS adoptions occur in isolation. Empirically, SAQS adoptions can affect subsequent adoptions through three interrelated pathways: i) peer effects, ii) local AQ information spillovers, and iii) network effects.

First, SAQS adoptions can influence future adoptions through social interactions between adopters and non-adopters. These peer effects capture word-of-mouth communication, imitation, and other types of bidirectional feedback between adopters and their peers.[24]. Due to the green technology characteristics of SAQS, which link adoption with high socioeconomic status, we expect peer effects to exist and to be strongest between spatially proximate individuals who live in population-dense, high-income, and green voting areas. Social networks in these areas are likely to promote the highest number of interactions between individuals who value air quality improvements. We aim to test the following hypothesis:

Hypothesis 6 *SAQS adoptions are positively associated with socioeconomic status and population density within cities.*

[24] The literature also commonly refers to these as social interaction effects, installed base effects, or neighbor effects.

Second, certain SAQS like those from Sensor.Community or PurpleAir enable adopters to automatically share AQ information to publicly available data archives and real-time maps, providing non-adopters and other adopters with information about true exposure across geographic space. Thus, in addition to generating private informational benefits, SAQS can produce information with local impure public good characteristics.[25] Due to the public nature of the information produced, adopters may associate additional warm glow benefits with contributing to the private provision of this public good, as has been documented for charitable giving and other prosocial behaviors. For example, adopters may consider the act of contributing to open AQ data commons a worthwhile endeavor on its own, or, more specifically, they may consider the public benefits of measuring AQ in unmonitored areas. In particular, they may wish to exert pressure on policymakers to improve AQ information provision and to reduce ambient pollution. AQ information produced by nearby SAQS installations may also substitute for new adoptions because non-adopters can free ride on existing adoptions' public AQ information, effectively crowding out subsequent installations. However, it is not clear at which distance from existing SAQS installations prospective adopters feel compelled to install their own SAQS and whether these preferences are homogenous in the population or across different urban and rural morphologies. Moreover, it is ambiguous how prospective adopters weigh the private benefits from installing a SAQS themselves versus free riding on other installations. We aim to test for the presence of these free-riding effects and postulate the following hypothesis:

Hypothesis 7 *SAQS adoptions are less likely in direct proximity to existing adoptions.*

Third, each SAQS adoption can act as a node in a non-regulatory monitoring network, with each additional adoption adding value to the network (e.g. Sensor.Community or PurpleAir). While network benefits from a marginal adoption are initially high when there are a small number of nodes, adoptions can reach a saturation point where an additional installation provides little marginal benefit (i.e. marginal informational gain). Network effects likely exist on both the local

[25] This information has *local* value because it may be particularly relevant to individuals located closest to the installed SAQS.

and global levels, but it is unclear at what adoption density they might take effect. Hence, we refrain from formulating a hypothesis about their existence for the present analysis.

3.4 Data

We construct panel datasets using SAQS installation data for Germany from 2016 to 2022 (geocoordinates and installation dates), municipal and neighborhood level socioeconomic information (income, demographics, voting records, etc.), government monitoring data (monitor geocoodinates, compliance history, and annual pollution concentrations), and satellite remote sensing pollution data (annual concentrations). The following subsections provide an overview of the data we have collected.

3.4.1 Stationary Air Quality Sensor Data

To prepare our analysis, we download sensor data from the Sensor.Community data archive.[26] Each timestamped SAQS reading is accompanied by the sensor's unique identifier, latitude, and longitude. We first create a data set with all global sensor records from July 2016 through July 2022, then query a list of unique sensor identifier, latitude, and longitude triplets, and perform a spatial join to identify unique sensor locations in Germany. We then average each German sensor's PM readings first to the hourly and thereafter to the daily level, creating a panel with each installation's daily mean $PM_{2.5}$ and PM_{10} readings and the number of raw and hourly observations.[27] We record each sensor's first date with PM readings as its installation date and count the number of new installations each month in Germany, in each municipality, and in each grid cell.

Table 3.4.1 shows summary statistics for SAQS adoptions aggregated to the national, municipal, and grid cell levels. Through July 2022, a total of 12,560 SAQS are installed in Germany in the Sensor.Community network. The average

[26] This data is available here: https://archive.sensor.community/csv˙per˙month

[27] If fewer than ten raw SAQS readings are taken in a given hour (less than once every six minutes), we record that hourly mean as missing. Similarly, if fewer than twenty-three hourly means are observed for a given day, we record the daily mean as missing.

Table 3.4.1: Summary Statistics: SAQS Adoptions in Germany through July 2022

	Germany	Municipality			1-km^2 Grid Cell		
	Total	Mean	SD	Max	Mean	SD	Max
Cumulative Adoptions	12,560	1.16	11.80	687	0.08	0.46	25
per 10,000	1.49	0.96	3.66	131.58	1.12	12.79	816.33
per km^2	0.04	0.03	0.09	2.69	0.08	0.46	25
Units	1	10,817			152,881		

municipality had about 1.2 total installations and each grid cell has an average of 0.08 total installations. The municipality with the highest absolute number of installations (Stuttgart) has a total of 682 adoptions. In Germany, about 1.5 in 10,000 individuals adopts a SAQS, while the highest municipal per capita SAQS adoption rate (Stuttgart) corresponds to about one installation per 1,127 residents. The highest spatial density of SAQS adoptions is 25 installations per km^2.

3.4.2 Socioeconomic Data

We acquire socioeconomic data from three sources. First, we use INKAR (*Indikatoren und Karten zur Raum- und Stadtentwicklung*) for municipal population, mean purchasing power (post-tax disposable income including transfers), mean age, municipal area (km^2), and urbanization status (1=urban, 0=rural) annually from 2016 to 2022. Second, we gather data from the German Federal Statistical Office (DESTATIS) on municipal green voting shares as the share of all voters who voted for the Green Party (*Zweitstimmen*) in the 2021 German Federal Election. Third, we use RWI-GEO-GRID for neighborhood level (1-km^2 grid cell) population and mean household income from 2016 to 2019.

3.4.3 Air Pollution Data

We obtain air pollution data from two sources. First, we use information on ground based AQ monitoring in Germany. We collect monitor meta data (coordinates, pollutants monitored, monitor type (i.e. background, traffic, or industrial), instal-

Table 3.4.2: Summary Statistics: Socioeconomic Variables

	Municipality				1-km^2 Grid Cell			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Pop., 10,000s	0.76	4.97	0.0	367.7	0.05	0.11	0.0	2.6
Area, km^2	32.56	41.10	0.0	891.0	1.00	0.00	1.0	1.0
Pop. Density, pop/km^2	0.02	0.03	0.0	0.5	0.05	0.11	0.0	2.6
Purchasing Power, 10,000	2.34	0.30	0.9	6.4	.	.	.	.
Income, 10,000	.	.	.	.	4.74	0.98	0.8	18.9
Green Voting, %BTW21	9.85	5.13	0.0	39.5	.	.	.	.
Mean Age, years	45.29	2.69	26.3	66.5	.	.	.	.
Rural, dummy	0.67	0.47	0.0	1.0	.	.	.	.
Observations	65,802				1,446,736			
Units	10,980				361,684			
Years	2016-2021				2016-2019			
Source	INKAR				RWI-GEO-GRID			

Table 3.4.3: Summary Statistics: Government Ground Monitored Pollution (2016-2022)

	Municipalities	Mean	SD	Min	Max
Panel A: Municipal Mean Pollution					
Annual PM_{10}, µg/m^3	272	16.04	3.3	4.0	26
Annual $PM_{2.5}$, µg/m^3	168	10.85	1.7	4.8	14
Annual NO_2, µg/m^3	335	22.02	11.3	1.8	46
Panel B: Municipal Mean Monitors					
PM_{10}, count	10,817	0.04	0.3	0.0	12
$PM_{2.5}$, count	10,817	0.02	0.2	0.0	11
NO_2, count	10,817	0.04	0.4	0.0	18
Panel C: Municipal EU AQ Directive					
PM_{10} non-compliant, years	257	0.01	0.1	0.0	2
$PM_{2.5}$ non-compliant, years	206	0.00	0.0	0.0	0
NO_2 non-compliant, years	330	0.75	1.4	0.0	6
Total Municipalities	10,817				

lation and removal dates) for 1,090 ground-based government PM_{10}, $PM_{2.5}$, and NO_2 monitors from the German Federal Environmental Agency (UBA, *Umweltbundesamt*). We then add information from UBA about each monitor's annual pollution concentrations (PM_{10}, $PM_{2.5}$, and NO_2) and annual compliance status. We aggregate annual pollution concentrations to the municipal level by calculating the arithmetic mean across all active monitors in a municipality. For each municipality and year, we calculate the mean number of non-compliance years by calculating sum of non-compliance years in the previous five years (at least one monitor in the municipality not compliant). Table 3.4.3 shows the average of annual mean concentrations and average cumulative number of years of non-compliance for monitors of each type of pollutant.

Table 3.4.4: Summary Statistics: Satellite-Monitored Pollution

	Observations	Mean	SD	Min	Max
Panel A: Grid Cell Mean Pollution					
Annual $PM_{2.5}$, µg/m^3	1,807,510	10.56	1.65	3.95	17.50
Annual NO_2, µg/m^3	1,446,683	5.49	3.23	0.00	29.16
Panel B: Municipal Mean Pollution					
Annual $PM_{2.5}$, µg/m^3	16,211	10.84	1.35	5.54	16.61
Annual NO_2, µg/m^3	16,211	5.83	3.08	0.27	27.56

Note: Satellite $PM_{2.5}$ data for the years 2016-2020 comes from van Donkelaar et al. (2021). Satellite NO_2 data for the years 2016-2019 comes from Cooper (2022). Municipal statistics in panel B are population-weighted averages across all 1-km^2 grid cells within a municipality.

Second, we use annual satellite-based estimates at the 1-km^2 raster level for $PM_{2.5}$ from 2016-2021 from van Donkelaar et al. (2021) and for NO_2 from 2016 to 2019 from Cooper (2022). For each year, we calculate the population-weighted average of each pollutant's annual concentration across grid cells in each municipality to use them as an alternative to ground based $PM_{2.5}$ and NO_2 pollution estimates.[28] Table 3.4.4 shows that average annual municipal $PM_{2.5}$ concentrations range from 5.5 µg/m^3 to 16.6 µg/m^3. Average annual municipal NO_2 concentrations range from 0.27 µg/m^3 to 27.56 µg/m^3. For our analysis, we average municipal mean

[28] We do not obtain satellite PM_{10} estimates for this analysis.

satellite-monitored pollution over observed years to generate variables indicative of average pollution levels in the period of interest.

3.5 Empirical Approach

3.5.1 Disparities

In our empirical analysis, we first aim to characterize how SAQS adoptions relate to socioeconomic and geographic factors with the goal of identifying disparities in private AQ monitoring coverage. In line with hypotheses 1-6 in section 3.3, we inspect relationships between presumable adoption determinants and SAQS adoptions using quintile mean scatter plots at the municipal and grid cell level. We focus here on per capita SAQS adoptions as the primary outcome.

3.5.2 Adoption Determinants

To further analyze these correlations, we then estimate various specifications of the following PPML[29] regression model:

$$Adoptions_{i,y} = \alpha + X_{i,y}\beta + \epsilon_{i,y} \tag{3.1}$$

where $Adoptions_{i,y}$ is the number of cumulative SAQS installations per capita in spatial unit i (i.e. municipality or grid cell) in year y, $X_{i,y}$ is a vector of socioeconomic and geographic variables, and ϵ_d, t is the error term. The parameters of interest, β, capture the number of additional cumulative SAQS adoptions per capita associated with a one unit increase in the variable of interest while holding the other included socioeconomic variables constant.

3.5.3 Spillover Effects

In the second part of our analysis, we test for the existence of spatial peer effects in SAQS adoption. Namely, we examine whether and to what extent the number

[29] We use PPML regression for two main reasons: i) we are modeling nonnegative count data that is not distributed normally, and ii) our dataset has a large number of zeros (e.g. municipalities or grid cells with zero observations).

of existing, nearby SAQS installations affects new adoptions. Our identification strategy aims to overcome three well-known challenges to identifying peer effects: i) endogenous group formation, ii) simultaneity, and iii) correlated unobservables (Hartmann et al., 2008). We use a similar estimation strategy as spatiotemporal diffusion models developed by Graziano and Gillingham (2014) and Rode and Weber (2016) to evaluate the effect of neighboring photovoltaic installations on new adoptions. We draw on Rode and Weber (2016) to use the centroid of each 1-km^2 RWI-GEO-GRID grid cell as the scanning point (i.e. focal center) for counting the number of nearby SAQS installations in increasingly wide distance bands. In our preferred regression model, we use grid cell fixed effects to control for endogenous group formation (i.e. residential sorting), lagged spatiotemporal neighbor variables to account for possible simultaneity in adoption, and grid-cell-year fixed effects account for correlated unobservables (i.e. local marketing interventions, air pollution-related news events, etc.).

The spatial neighbor variables are constructed in the following manner. For each grid cell and quarter, we count the number of SAQS installed two quarters earlier in 2,500 meter radial distance bands from the grid cell centroid to 10,000 meters. Formally, for each grid cell centroid g, we count the number of neighboring SAQS installations S, such that:

$$d_{g,j} < D_{outer},$$

$$d_{g,j} \geq D_{inner},$$

$$t_g - t_j > T,$$

$$t_g - t_j < T + W + 1,$$

where $d_{g,j}$ is the distance in kilometers between grid cell centroid g and installation j, D_{inner} is the spatial band's inner radius (0, 2.5km, 5km, 7.5km), D_{outer} is the spatial band's outer radius (2.5km, 5km, 7.5km, 10km), t_g is a scanning quarter between 2016Q3 and 2022Q2, t_j is sensor j's installation quarter, T is our selected time lag between the installation quarter and the current scanning quarter (one quarter in our main specification), and W is a one-quarter window. Figure 3.5.1 provides a visual example.

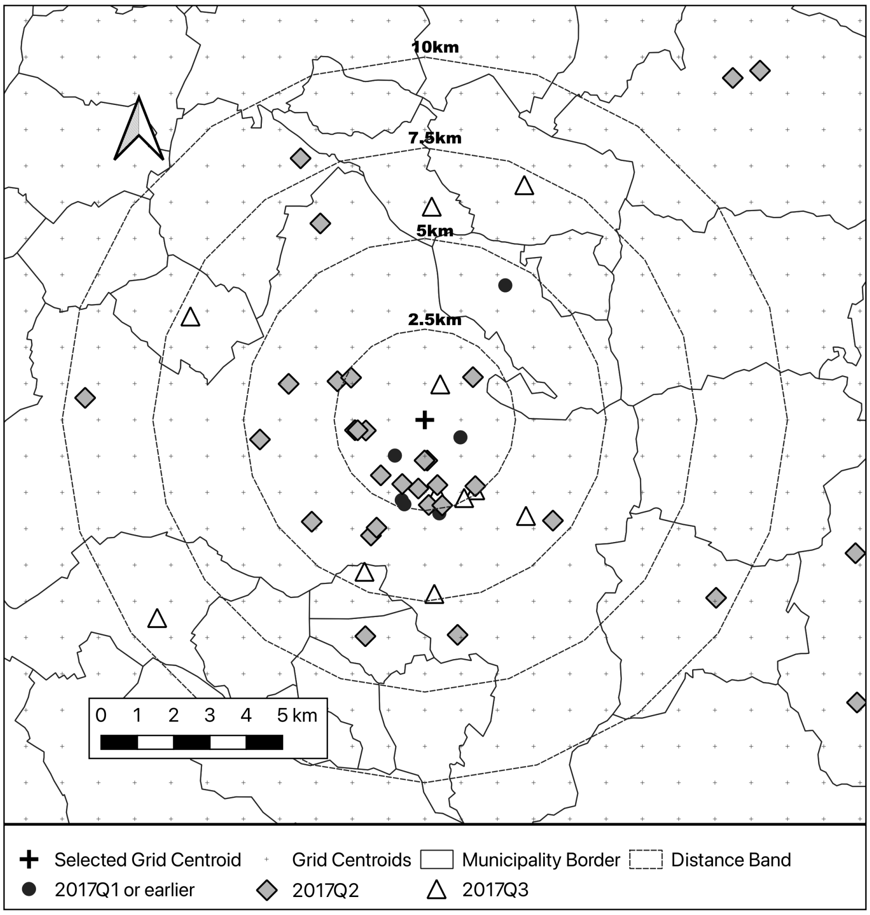

Figure 3.5.1: Example setup for spatiotemporal variable calculation in scanning quarter 2017Q4. Diamond markers designate all considered adoptions in 2017Q2 (two quarters earlier). We count the number of 2017Q2 adoptions in each of the concentric 2,500 meter distance bands. Circle markers indicate all adoptions before 2017Q2 (three or more quarters earlier) which are not counted in the baseline spatiotemporal variables. Triangle markers correspond to adoptions in 2017Q3 which fall in the one quarter lag window and are also excluded. Plus marker is the centroid of the 1-km^2 scanning grid cell of interest.

We then estimate SAQS demand in grid cell i and quarter t using the following PPML specification:

$$Adoptions_{i,t} = \alpha + \beta S_{i,t} + \mu_i + \phi_t + \epsilon_i, t \tag{3.2}$$

where $Adoptions_{d,t}$ is the number of new SAQS adoptions per capita in a municipality-quarter, $S_{i,t}$ is a vector of recent neighboring SAQS adoptions in consecutive spatial bands (0-2.5 km, 2.5-5 km, 5-7.5 km, and 7.5-10 km), μ_d are grid cell fixed effects, ϕ_t are year-quarter time fixed effects, and ϵ_d, t is the error term. The parameter of interest, β, is a vector that captures the average influence of recent SAQS installations on new sensor adoptions at different distances. We estimate this regression equation with alternative distance band, time lag, and window specifications.

3.6 Results

3.6.1 Disparities

Our correlational analysis indicates that socioeconomic factors, pollution levels, and government monitoring at the municipal level are associated with per capita SAQS installation rates. As a result, different socioeconomic groups in Germany experience very different SAQS monitoring coverage. We begin by inspecting in isolation the role of individual municipal-level socioeconomic factors without controls on cumulative SAQS adoptions per capita over time. Figure 3.6.1 plots the mean municipal population share adopting SAQS by municipal socioeconomic quintile for 2017, 2019, and 2022. Year after year, cumulative SAQS adoption rates positively relate to municipal level mean purchasing power, population density, and Green Party voting quintile, while they relate negatively to mean age quintile. In 2022, the percent difference between adoption rates in the highest and lowest adoption quintiles is largest for purchasing power (+329.2%) and green voting (+244.6%), less substantial for population density (+86.3%), and smallest for age (+38.9%). By 2022, the percent difference between highest and lowest quintile adoption rates shrinks by 55.9% for purchasing power, 122.6% for population density, 536.2% for green voting, and 289.2% for mean age, suggesting that in 2017 early adopters were considerably more concentrated in municipalities with high green voting and low mean age than in municipalities with higher population density or higher purchasing power compared to their distribution in 2022. These results confirm hypotheses 1 and 2 that municipalities with high income and green voting disproportionately adopt SAQS.

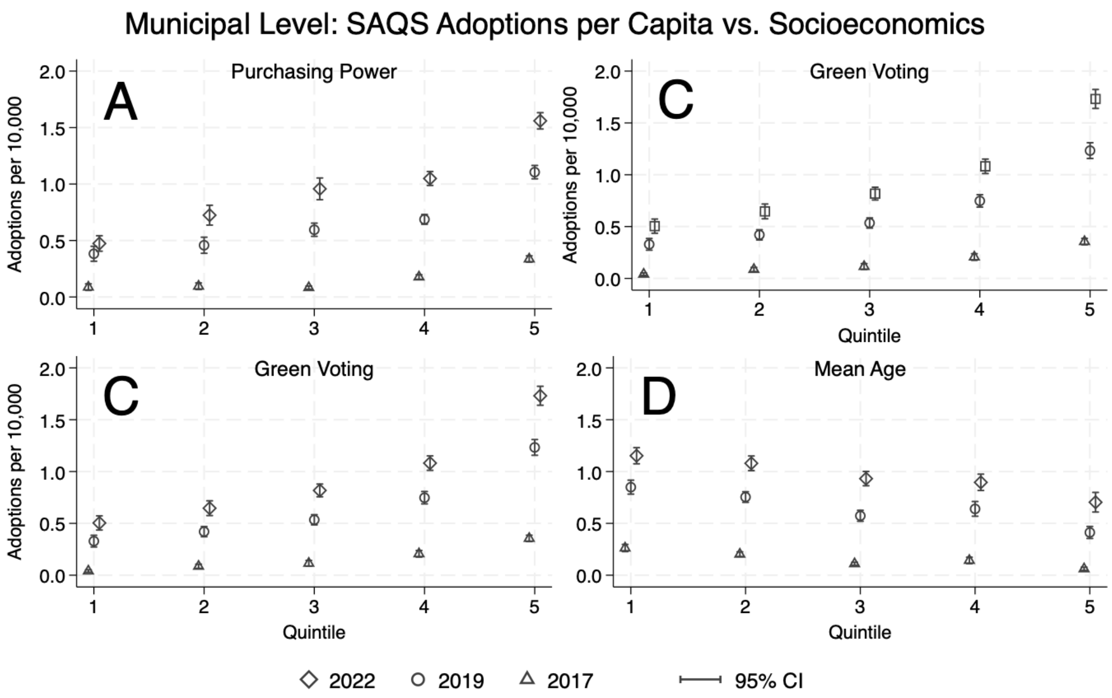

Figure 3.6.1: Mean municipal level cumulative SAQS adoptions per capita in years 2017, 2019, and 2022 plotted against municipal socioeconomic quintile for 2016-2022 averages of A) mean household purchasing power (excludes 2022), B) population density, C) Green Party voting (2021 federal German election), and D) mean age. Quintile mean confidence intervals calculated at the 95% level. All German municipalities included.

We then shed light on how SAQS adoption rates relate to existing pollution estimates from government monitors and satellites in connection with hypothesis 3. Panels A, B, and C of figure 3.6.2 respectively plot the relationship between per capita SAQS adoptions and mean annual government-monitored PM_{10}, $PM_{2.5}$, and NO_2 pollution levels measured over the years 2016 to 2022, conditional on a municipality having at least one active monitor in this time frame. A strictly positive relationship is most apparent between government-monitored NO_2 concentrations and adoptions, while adoptions do not monotonically increase with pollution for the PM parameters. Compared to the respective lowest quintile, municipalities in the highest $PM_{2.5}$, PM_{10}, and NO_2 quintile have 77.3%, 163.2%, and 140.7% higher adoptions in 2022. Although the PM_{10} relationship appears weakly monotonic, this seems less the case for $PM_{2.5}$, where the highest SAQS adoption rates are actually in the fourth $PM_{2.5}$ quintile. Comparing each pollution parameter between 2017 and 2022 demonstrates that the gap between the lowest and highest adoption quintiles shrinks by 89.6% for PM_{10}, 171.8% for $PM_{2.5}$, and 1,161.9% for NO_2. This provides evidence that early adopters were highly concentrated in government-monitored municipalities with high NO_2 concentrations, and suggests that households who are exposed to more pollution disproportionately purchase and install SAQS in line with hypothesis 3.

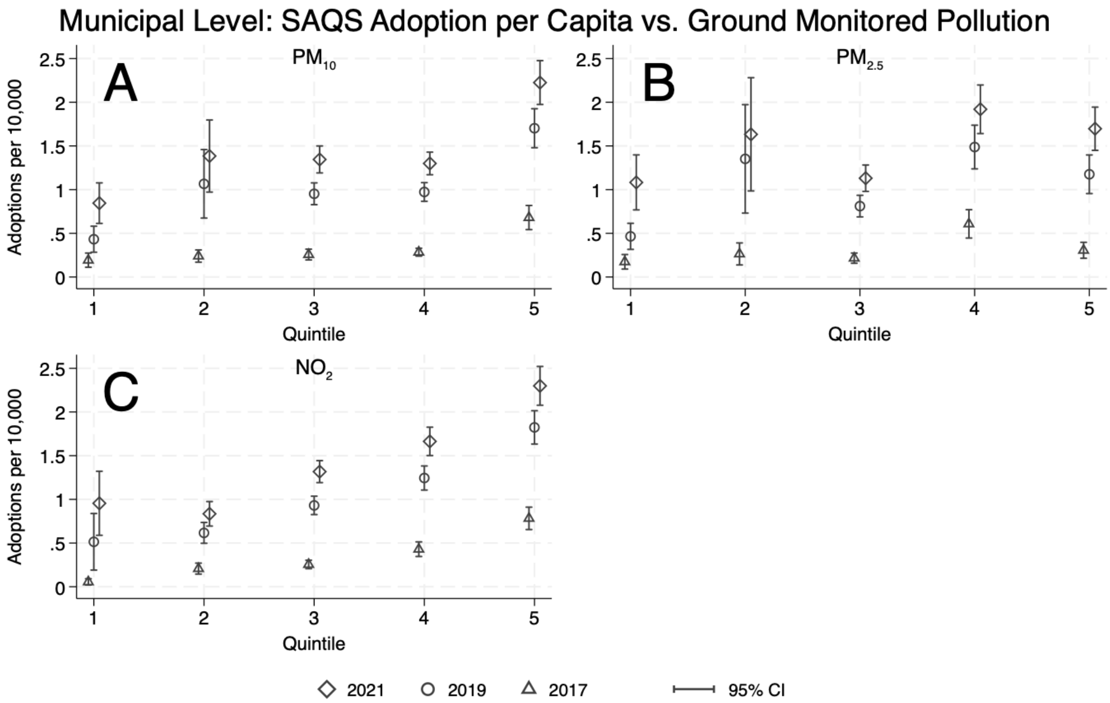

Figure 3.6.2: Mean municipal level cumulative SAQS adoptions per capita in years 2017, 2019, and 2022 versus municipal pollution quintile for 2016-2022 average annual ground monitored PM_{10}, $PM_{2.5}$, and NO_2. Quintile mean confidence intervals calculated at the 95% level. Includes only municipalities with active government monitors from 2016-2021. See figure 3.B.1 in appendix for municipalities that met this criteria in 2022.

For comparison, figure 3.6.3 plots the relationship between satellite-monitored pollution concentrations and SAQS adoptions in all German municipalities, validating the previous NO_2 relationship depicted in figure 3.6.2 for monitored municipalities.[30] The consistency between the two figures probably reflects high NO_2 concentrations in cities but also the regional hotspots in northwest and southern Germany where adoptions are also highest (see figure 3.C.1 in appendix 3.C). However, satellite $PM_{2.5}$ measurements point to a very different relationship between municipal level pollution and adoptions than depicted in figure 3.6.2, with the highest SAQS adoption rates in the second quintile and lower adoption rates in the fourth and fifth quintiles. Non-random government monitor assignment and different regional trends in $PM_{2.5}$ pollution may help to explain the differences in associations between figures 3.6.2 and 3.6.3. In particular, figure 3.C.2 in appendix 3.C shows that more rural municipalities in the east have among the highest $PM_{2.5}$ concentrations, while overall adoptions there are relatively low. Interpreted together with the results from figure 3.6.2, these findings suggest that disparities in SAQS adoptions may be linked to more salient pollution levels like NO_2, which is associated with widespread monitor non-compliance, and PM in monitored counties. Our evidence does not universally support hypothesis 3 that household exposed to more pollution on the municipal level adopt more SAQS.

Figure 3.6.2 demonstrates a clear positive relationship between SAQS adoptions and two important government-monitoring outcomes: i) government monitoring intensity (i.e. the number of active monitors per municipality) in panel A and ii) years of municipal non-compliance with AQ regulations in panel B. Panel A indicates a complementary relationship between government and private monitoring initiatives at the municipal level in support of hypothesis 4, while panel B suggests that non-compliance may be associated with additional private monitoring demand in line with hypothesis 5. In particular, we note the modest differences between municipalities with no monitors and those with zero years of non-compliance (those counties that had at least one government monitor but were never non-compliant from 2016-2022) versus municipalities with one or more years of non-compliance in the same time frame.

[30] We do not incorporate satellite PM_{10} data into our analysis.

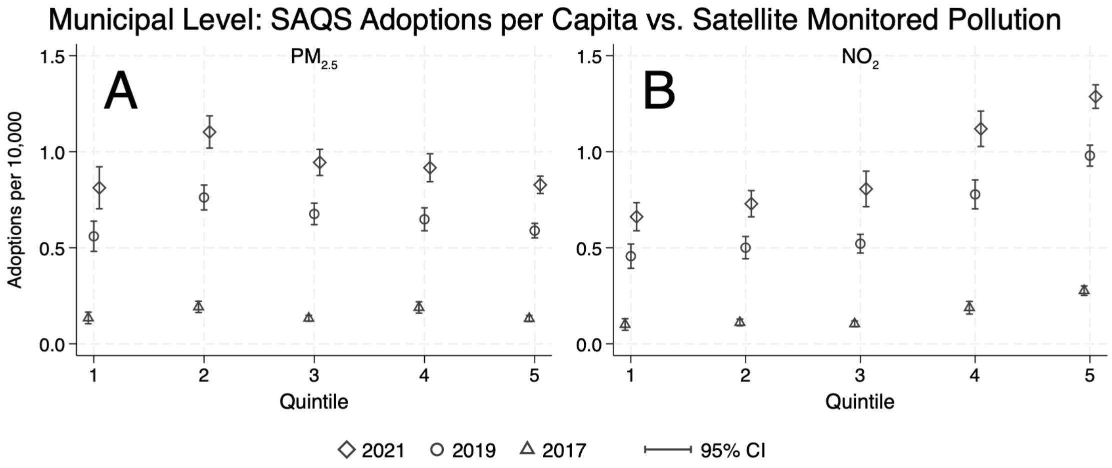

Figure 3.6.3: Mean municipal level cumulative SAQS adoptions per capita in years 2017, 2019, and 2022 plotted against municipal mean annual satellite monitored pollution quintile for A) $PM_{2.5}$ (2016-2021) and B) NO_2 (2016-2019). Annual satellite pollution estimates are calculated as the population-weighted average across 1-km^2 grid cells in a municipality. Quintile mean confidence intervals calculated at the 95% level. Includes all German municipalities.

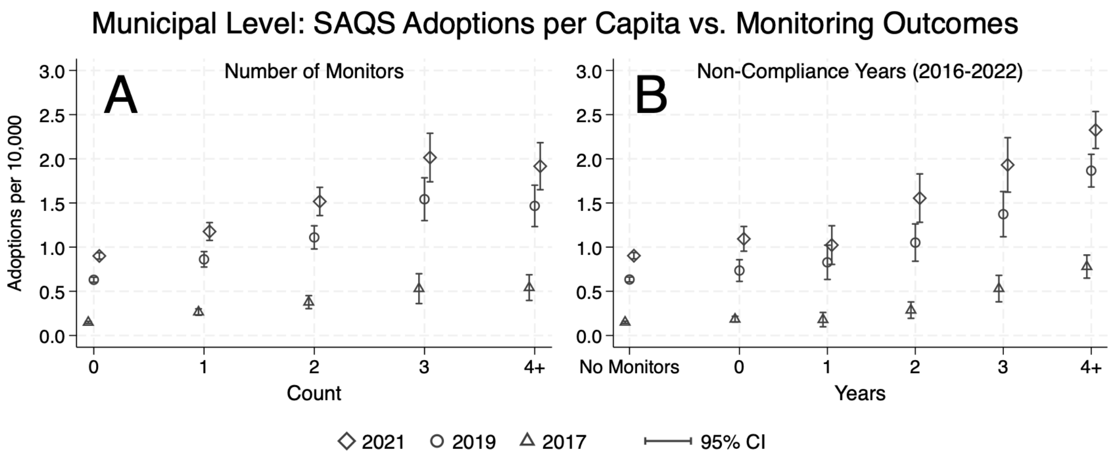

Figure 3.6.4: Mean municipal level cumulative SAQS adoptions per capita in years 2017, 2019, and 2022 plotted against A) number of active government monitors and B) number of years non-compliant with EU air quality standards. Quintile mean confidence intervals calculated at the 95% level. Includes all German municipalities.

3.6.2 Adoption Determinants

Socioeconomic Factors and Pollution

Table 3.6.1 presents estimates from three PPML models at the municipal level, where the cumulative number of per capita SAQS adoptions at the end of 2022 are regressed on municipal average socioeconomic characteristics and pollution levels over the preceding years. The coefficients in column 1 indicate statistically significant positive associations between per capita SAQS adoptions and municipal mean income and green voting, while SAQS adoptions are negatively associated with rural status. The coefficient on mean purchasing power corresponds to an increase in per capita SAQS adoptions by 36% for an increase in mean municipal purchasing power by €10,000, and the coefficient on green voting corresponds to a 6% increase in per capita SAQS adoptions for every additional percent of municipal voters who vote for the Green Party, holding all else equal. These coefficients are in line with hypotheses 1 and 2. In opposition to hypothesis 3, we do not find a statistically significant relationship between satellite-monitored pollution levels and per capita SAQS adoptions in column 1.

The regressions in columns 2 and 3 of table 3.6.1 add state and district fixed effects to respectively control for unobserved differences between municipalities located in different states or districts.[31] We use these fixed effects to test which factors are associated with differences in per capita SAQS adoptions between municipalities in the same state or district. In the regression column 2, we find very similar results to column 1 after adding state fixed effects, except that municipalities with higher mean annual satellite-monitored NO_2 have 3% more per capita SAQS adoptions for each additional unit of NO_2 pollution, and the coefficient on rural status is no longer statistically significant. When we add district fixed effects in column 3, the coefficients on mean purchasing power, green voting, and satellite-monitored NO_2 remain statistically significant, positive, and of similar magnitude, and the coefficient on population density is also statistically significant at the 5% level. Surprisingly the coefficient on population density in column 3 is negative and corresponds to a 187% decrease in per capita SAQS adoption from an additional

[31] There are 16 federal states and 401 districts in Germany.

10,000 residents per km^2.

Table 3.6.1: Municipal Per Capita SAQS Adoption Determinants

	(1)	(2)	(3)
Mean Purchasing Power, 10,000	0.36***	0.36***	0.29*
	(0.10)	(0.11)	(0.14)
Population, 10,000s	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.01)
Population Density, 10,000s per km^2	0.19	-0.36	-1.87*
	(0.70)	(0.74)	(0.86)
Green Voting, %	0.06***	0.07***	0.07***
	(0.01)	(0.01)	(0.02)
Mean Age, years	-0.01	-0.02	-0.02
	(0.02)	(0.02)	(0.02)
Annual Satellite $PM_{2.5}$, µg/m^3	0.01	-0.02	-0.06
	(0.04)	(0.05)	(0.09)
Annual Satellite NO_2, µg/m^3	0.00	0.03*	0.08*
	(0.01)	(0.01)	(0.04)
Rural, dummy	-0.20*	-0.14	-0.13
	(0.08)	(0.10)	(0.09)
State FE	No	Yes	No
District FE	No	No	Yes
Obs.	10,426	10,426	10,322

Notes: This table reports Poisson pseudo maximum likelihood regression estimates and standard errors. In each regression, the dependent variable is cumulative municipal SAQS adoptions per capita in December 2022. Explanatory variables are municipal averages from 2016 to 2022. Significance level: + = $p < 0.1$, * = $p < 0.05$, ** = $p < 0.01$, and *** = $p < 0.001$

In table 3.6.2, we present results from four PPML regressions at the 1-km^2 grid cell level where we test for grid cell per capita SAQS adoption determinants and iteratively add fixed effects at increasingly small spatial scales like in table 3.6.1. Our results in column 1 show that, on the national level, grid cells with higher average household incomes, larger population, and higher mean annual satellite-monitored NO_2 pollution levels have a statistically greater number of per capita SAQS adoptions, providing support for hypotheses 1 and 3. However, mean annual satellite-monitored $PM_{2.5}$ pollution levels are not associated with grid cell per capita SAQS adoptions, holding all else equal. After accounting for mean differences in per capita SAQS adoption rates between grid cells in different states, we find that many of the same statistical relationships hold in column 2 as in column 1, except that the coefficient on mean household income is no longer statistically significant at the 5% level. This implies that within states, grid cells with higher income levels experience no higher per capita SAQS adoption rates after controlling for grid cell population and pollution levels. The relationship between grid cell mean household income and per capita SAQS adoptions also breaks down statistically after adding district level fixed effects in column 3 and municipal level fixed effects in column 4. The relationship between grid cell population counts and per capita SAQS adoptions flips from positive to negative in columns 3 and 4 compared to columns 1 and 2. This means that within districts and municipalities, grid cells that have higher population counts have statistically fewer SAQS adoptions per capita, after controlling for the other observable factors. Jointly, the results in columns 3 and 4 lead us to reject hypothesis 6 that SAQS adoption rates are positively associated with income and population density within municipalities, holding all else equal.

Table 3.6.2: Grid Cell Per Capita SAQS Adoption Determinants

	(1)	(2)	(3)	(4)
Mean Household Income, 10,000	0.09^{**}	0.06	-0.02	-0.01
	(0.03)	(0.03)	(0.04)	(0.05)
Population, 10,000s	0.61^{***}	0.32^{**}	-0.35^{*}	-0.58^{***}
	(0.10)	(0.12)	(0.14)	(0.16)
Annual Satellite $PM_{2.5}$, µg/m^3	-0.02	-0.04	0.01	0.27^{*}
	(0.03)	(0.05)	(0.08)	(0.13)
Annual Satellite NO_2, µg/m^3	0.05^{***}	0.07^{***}	0.14^{***}	0.11
	(0.01)	(0.01)	(0.03)	(0.06)
State FE	No	Yes	No	No
District FE	No	No	Yes	No
Municipality FE	No	No	No	Yes
Obs.	152,879	152,879	152,839	73,088

Notes: This table reports Poisson pseudo maximum likelihood regression estimates and standard errors. In each regression, the dependent variable is cumulative 1-km^2 grid cell SAQS adoptions per capita through 2022. Explanatory variables are grid-cell averages from 2016 to 2022. Significance level: * = $p < 0.05$, ** = $p < 0.01$, and *** = $p < 0.001$

When, in column 3, we add district level fixed effects, we find the effect of annual satellite-monitored NO_2 pollution levels is stronger between grid cells in the same district than within the same state or on the national level. For each additional unit of NO_2 pollution, grid cell per capita SAQS adoptions increase by 14% compared to grid cells within the same district. While the coefficient on NO_2 is of similar magnitude in column 4 when compare grid cells within the same municipality, it is no longer statistically significant at the 5% level. However, we find that, unlike at the three other spatial scales, higher $PM_{2.5}$ pollution within municipalities is associated with statistically greater grid cell per capita SAQS adoptions, holding all else equal. These results are too mixed to identify a clear relationship between pollution and SAQS adoptions in line with hypothesis 3, but at each spatial scale there is at least some evidence of a positive relationship.

Government Monitoring

Regression results in table 3.6.3 shows that there is no statistical difference in per capita SAQS adoptions in municipalities with an active government monitor in the years 2016-2022 compared to municipalities without one. While all three point estimates in columns 1-3 are positive, the result is consistent across the three models while controlling for the socioeconomic factors and pollution levels included in table 3.6.1 and iteratively adding state and district fixed effects. This result does not directly contradict hypothesis 4, that SAQS adoptions are negatively associated with distance to government monitors, but if this hypothesis is true, municipalities with government monitors would presumably have a greater number of SAQS adoptions per capita, but this does not hold.

In table 3.6.4, however, we demonstrate a negative relationship between per capita SAQS adoptions and the distance from grid cell centroids to the nearest government monitor. This implies that grid cells located closer to government monitors have higher per capita SAQS adoption rates in support of hypothesis 4. The coefficient of interest in column 1 shows that for every additional kilometer that a grid cell is located from the nearest government monitor, per capita SAQS adoptions decrease by 1.3%. While the coefficient is not statistically significant at the 5% level when comparing grid cells within in the same district, this relationship

Table 3.6.3: Regression Results: Municipal Level Government Monitoring

	(1)	(2)	(3)
Monitor active 2016-2022, dummy	0.08	0.06	0.03
	(0.08)	(0.08)	(0.10)
State FE	No	Yes	No
District FE	No	No	Yes
SES Controls	Yes	Yes	Yes
Sat. Pollution Controls	Yes	Yes	Yes
Obs.	10,426	10,426	10,322

Notes: This table reports Poisson pseudo maximum likelihood regression estimates and standard errors. In each regression, the dependent variable is cumulative municipal SAQS adoptions per capita through 2022. Explanatory variables are grid-cell averages from 2016 to 2022. Significance level: * = $p < 0.05$, ** = $p < 0.01$, and *** = $p < 0.001$

does hold when comparing grid cells in the same state and in the same municipality. In fact, the effect size strengthens to a 3% reduction in per capita SAQS adoptions per additional kilometer from the nearest government monitor when comparing grid cells within municipalities.

3.6.3 Spillover Effects

Table 3.6.5 shows our main PPML regression results estimating the impact of nearby SAQS installations on new grid cell SAQS adoptions. These models are estimated with a vector of spatiotemporal neighbor variables that count the number of previous SAQS adopters two quarters previous in 2,500 meter distance bands.[32] In column 1, where the model includes grid cell and year-quarter fixed effects, we find a statistically insignificant relationship between new adoptions and SAQS installed within 2,500 meters two quarters earlier. Column 2 shows that the effect remains statistically insignificant but flips negative when we add a second distance band from 2,500 meters to five kilometers. The coefficient on the second distance

[32] Note that there is a two quarter lag between the outcome of interest, contemporaneous adoptions (time period: t), and the explanatory variables, and the spatiotemporal variables, which only considers adoptions in the time period two quarters previously (time period: t-2). In this model, the spatiotemporal window is a single quarter.

Table 3.6.4: 1-km^2 Grid Cell Government Monitoring

	(1)	(2)	(3)	(4)
Distance to Nearest Monitor, km	-0.013***	-0.013**	-0.010	-0.030*
	(0.004)	(0.004)	(0.005)	(0.015)
State FE	No	Yes	No	No
District FE	No	No	Yes	No
Municipality FE	No	No	No	Yes
Obs.	152,385	152,385	152,345	72,850

Notes: This table reports Poisson pseudo maximum likelihood regression estimates and standard errors. In each regression, the dependent variable is cumulative grid cell SAQS adoptions per capita through 2022. Explanatory variables are grid-cell averages from 2016 to 2022. Significance level: * = $p < 0.05$, ** = $p < 0.01$, and *** = $p < 0.001$

Table 3.6.5: Regression Results: Adoption Spillovers

Distance Band	(1)	(2)	(3)	(4)	(5)
0 - 2.5km	0.010	-0.029	-0.041^{+}	-0.040^{+}	-0.179***
	(0.016)	(0.024)	(0.023)	(0.023)	(0.038)
2.5 - 5km		0.042**	0.030^{+}	0.033^{+}	0.053**
		(0.015)	(0.018)	(0.018)	(0.020)
5 - 7.5km			0.027^{+}	0.033^{+}	0.032
			(0.016)	(0.018)	(0.027)
7.5 - 10km				-0.016	-0.045
				(0.020)	(0.031)
Grid FE	Yes	Yes	Yes	Yes	No
Year-quarter FE	Yes	Yes	Yes	Yes	Yes
Grid-year FE	No	No	No	No	Yes
Observations	46,662	46,662	46,662	46,662	12,588

Notes: This table reports Poisson pseudo maximum likelihood regression estimates and standard errors. In each regression, the dependent variable is quarterly grid cell SAQS adoptions per capita. Explanatory variables are spatiotemporal SAQ neighbor counts in the designated distance band installed in the previous quarter. Standard errors clustered on grid cell in columns 1-4 and on grid-year in column 5. Significance level: + = $p < 0.1$, * = $p < 0.05$, ** = $p < 0.01$, and *** = $p < 0.001$

band is statistically significant at the 1% level and corresponds to a 4.2% increase in the number of grid cell per capita SAQS adoptions for each additional SAQS installed two quarters previously from 2,500 meters to five kilometers from the grid cell centroid. As we add additional distance bands in columns 3 and 4, the coefficient on the closest distance band becomes statistically significant at the 10% level and corresponds to about a 4% decrease in grid cell per capita SAQS adoptions for every additional SAQS adoption two quarters previously within 2,500 meters, while the coefficient on the second two distance bands is still positive, but of slightly smaller magnitude than in column 2, and statistically significant at the 10% level. The coefficient on the furthest distance band from 7.5 kilometers to 10 kilometers in column is statistically insignificant at the 10% level.

Our preferred PPML regression specification in column 5 of table 3.6.5 includes grid-year fixed effects to control for unobservable time-varying differences between grid cells. In this regression, the relationship between recent previous adopters within 2,500 meters is strongest and highly statistically significant. For each additional SAQS installed within 2,500 meters of a grid cell centroid two quarters previously, per capita SAQS installations decrease by 17.9%. Taken together, the results on the coefficient for the nearest distance band in columns 1-5 support hypothesis 7, that spatially proximate SAQS adoptions have a negative impact on new SAQS adoptions and prospective SAQS adopters may be free-riding on recent adoptions nearby. Our results in column 5 also support the existence of positive peer effects from previous adoptions between 2.5 and five kilometers from the grid cell centroid, as evidenced by the consistent and statistically meaningful coefficient on the second distance band variable.

In appendix 3.E, we provide several alternative specifications to test the robustness of the results presented in table 3.6.5. First, in table 3.E.1 we expand the window of the spatiotemporal neighbor variables from one quarter to four quarters, meaning that they consider adoptions in the preceding five quarters but excluding the previous quarter to avoid simultaneity in SAQS adoptions. The results are largely in line with table 3.6.5, but statistically significant at lower significance levels and of greater magnitude. In our preferred specification in column 5, the effect size grows to 27.4% fewer per capita SAQS adoptions for each additional adoption within 2,500 meters in the four quarter window, while the effect size in the second distance band remains stable at approximately a 5.4% increase. We also compare our results to models with a different distance band specification. In table 3.E.2 and 3.E.4 we present results for specifications with one kilometer distance bands for the one-quarter and four-quarter windows, respectively. We again find support for our hypothesis that nearby SAQS adoptions crowd out additional adoptions and gain the insight that these effects are concentrated within one kilometer of the grid cell centroid. However, the evidence for positive spillover effects beyond the closest distance band are less consistent in the one kilometer distance band models. Finally, in table 3.E.3 we focus our analysis on the years 2018 to 2022 to omit early adopters who may have been less sensitive to nearby adopters. Again, our results confirm the patterns we observe in the previous regressions.

3.7 Discussion

The recent emergence of non-regulatory AQ monitoring networks represents a paradigm-shift in how pollution information is collected and disclosed to the public. In many countries, thousands of new private monitoring sites now supplement government monitoring initiatives, with citizen adopters installing and maintaining SAQS that automatically share ambient pollution readings with the public in real-time. Our empirical analysis of Sensor.Community, one of the world's largest non-regulatory AQ monitoring networks, provides novel evidence about the factors underlying the diffusion of this novel monitoring technology and supplies important insights about economic factors that may affect its future deployment.

In conducting an analysis of global SAQS adoptions and a more comprehensive analysis of Sensor.Community adoptions in Germany, we provide evidence about non-regulatory monitoring networks outside the United States for the first time. Our analysis studies three main aspects of SAQS adoptions in greater detail: i) disparities in adoption rates, ii) its relationship to government monitoring, and iii) spillovers on subsequent adoption decisions. We thereby confirm in a new context previous findings from California (Coury et al., 2024; Zivin et al., 2024) and the United States (deSouza and Kinney, 2021) and explore entirely novel aspects of SAQS adoptions.

We find that SAQS are installed at greater rates in municipalities that are, on average, younger and more urban, have higher incomes, and vote at higher rates for the Green Party. Disparities in SAQS adoption rates are perhaps most apparent for purchasing power and green voting, where the highest quintiles have nearly 3.3 and 2.4 times the per capita SAQS adoption rates of their corresponding lowest quintiles, respectively. After controlling for socioeconomic factors and pollution, we find that income and green voting are key determinants of SAQS adoptions at the municipal level, while differences by mean age can be explained by differences in purchasing power and green voting. Our results at the neighborhood level indicate that per capita adoptions are higher in neighborhoods with greater PM pollution levels holding all else equal, but neighborhood income levels may not be a meaningful determinant of adoptions when comparing neighborhoods within the same state, district, or municipality. Richer data may provide opportunities for future research to more carefully identify local SAQS adoption determinants. For example, examining differences in green voting behavior between neighborhoods for even a subset of German cities may be illuminating.[33] Furthermore, although previous research points to migratory background as a determinant of pollution exposure in Germany (Ehler et al., 2024; Rüttenauer, 2018), we do not consider disparities in SAQS adoptions related to the share of migrant minorities for this analysis due to a lack of consistent data at the municipal and neighborhood levels.

While our analysis does not directly contribute to this emerging literature on ambient pollution exposure disparities in Germany (Ehler et al., 2024; Rüttenauer, 2018), our finding that SAQS adoptions are greatest in municipalities where pollution is highest shows that the demand for better AQ information is greatest in cities where adopters may be most harmed by pollution. We find this is the case nationally for NO_2 pollution using satellite data and within municipalities with government monitors for NO_2, PM_{10}, and, to a lesser extent, $PM_{2.5}$. Considering existing socioeconomic inequalities in air pollution exposure in other contexts, private air quality monitoring could exacerbate existing environmental inequalities rather than mitigate them (Coury et al., 2024), but our evidence from Germany suggests that more polluted locations are better covered by private SAQS. Another potential avenue for future research would be to use geographic information on the

[33]Spatial data on sub-municipal German Federal Election voting is not publicly available for the entire country but is accessible on open data platforms for a limited number of cities.

location and type of pollution emitters (e.g. industrial facilities, traffic emissions, etc.) in Germany to test whether individuals adopt SAQS in response to specific pollution risks.

We find evidence of a complementary relationship between government and private monitoring, suggesting that private initiatives might reinforce existing differences in AQ information coverage, rather than broadening coverage to underserved areas. Our analysis demonstrates that municipalities with higher government monitor intensity and non-compliance with EU AQ directives have higher per capita SAQS adoption rates. However, these differences fade after controlling for other observable differences between municipalities. The relatively modest number of non-compliant municipalities presumably affects our ability to detect statistically relevant effects, but future research could more carefully consider the existence of a (causal) relationship between regulatory monitor non-compliance and private monitoring.

Results from this paper can contribute to future discussions on the optimal design of pollution monitoring networks in a world with private AQ monitoring. For example, in finding evidence for negative spatial spillovers in SAQS adoption, we document an important property of this novel technology. Recent SAQS adoptions within 1 - 2,500 meters reduce subsequent adoptions nearby between 18% and 27%, providing evidence that existing adoptions effectively crowd out new adoptions nearby. Our analysis also points to spillovers from further away adopters ($>$ 2,500 meters) as another factor driving additional per capita SAQS adoptions. Our spillover results may influence how private monitoring initiatives like environmental justice organizations, network operators, or policy-makers choose to seed SAQS in the population, given that they may not promote new adoptions in their direct vicinity.

While adoptions are a key starting point for studying the emergence of private AQ monitoring, future research should expand its scope to analyze other dimensions of SAQS deployment. For example, aspects such as SAQS activity and reading performance determine whether SAQS actually continuously produce information after installation and to what extent this information is useful to adopters. Moreover, little is known about the impacts of SAQS information on individuals and who benefits from the information they produce.

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Appendix - Chapter 3

3.A Adoption Maps

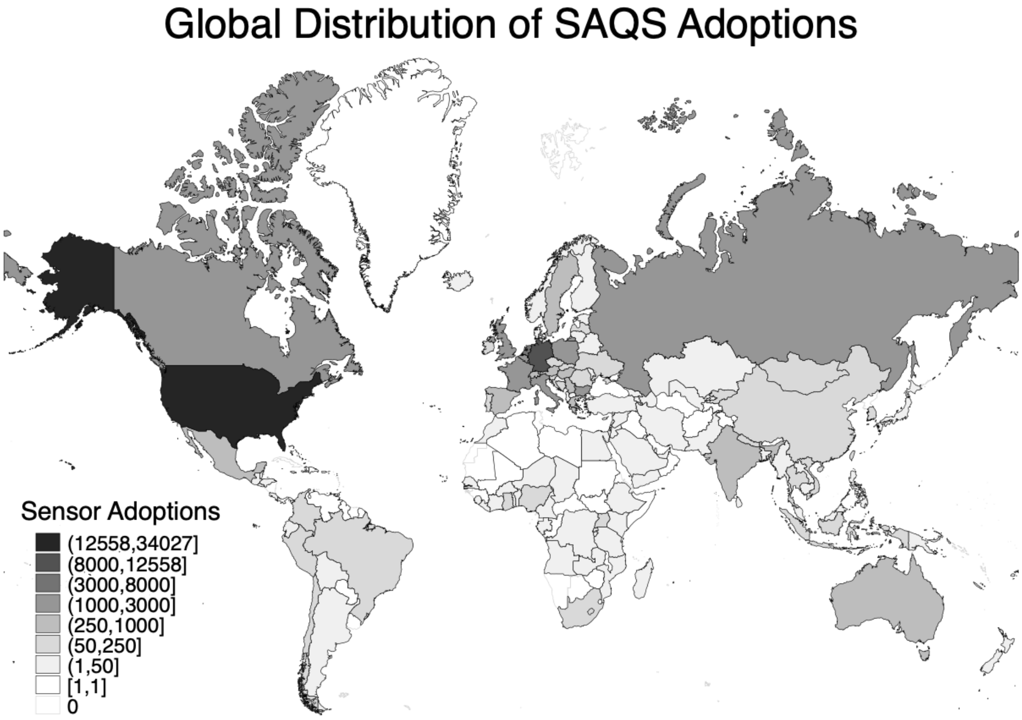

Figure 3.A.1: Cumulative stationary air quality sensor (SAQS) adoptions by country through July 2022. Included are all outdoor and indoor adoptions in the Sensor.Community and PurpleAir networks.

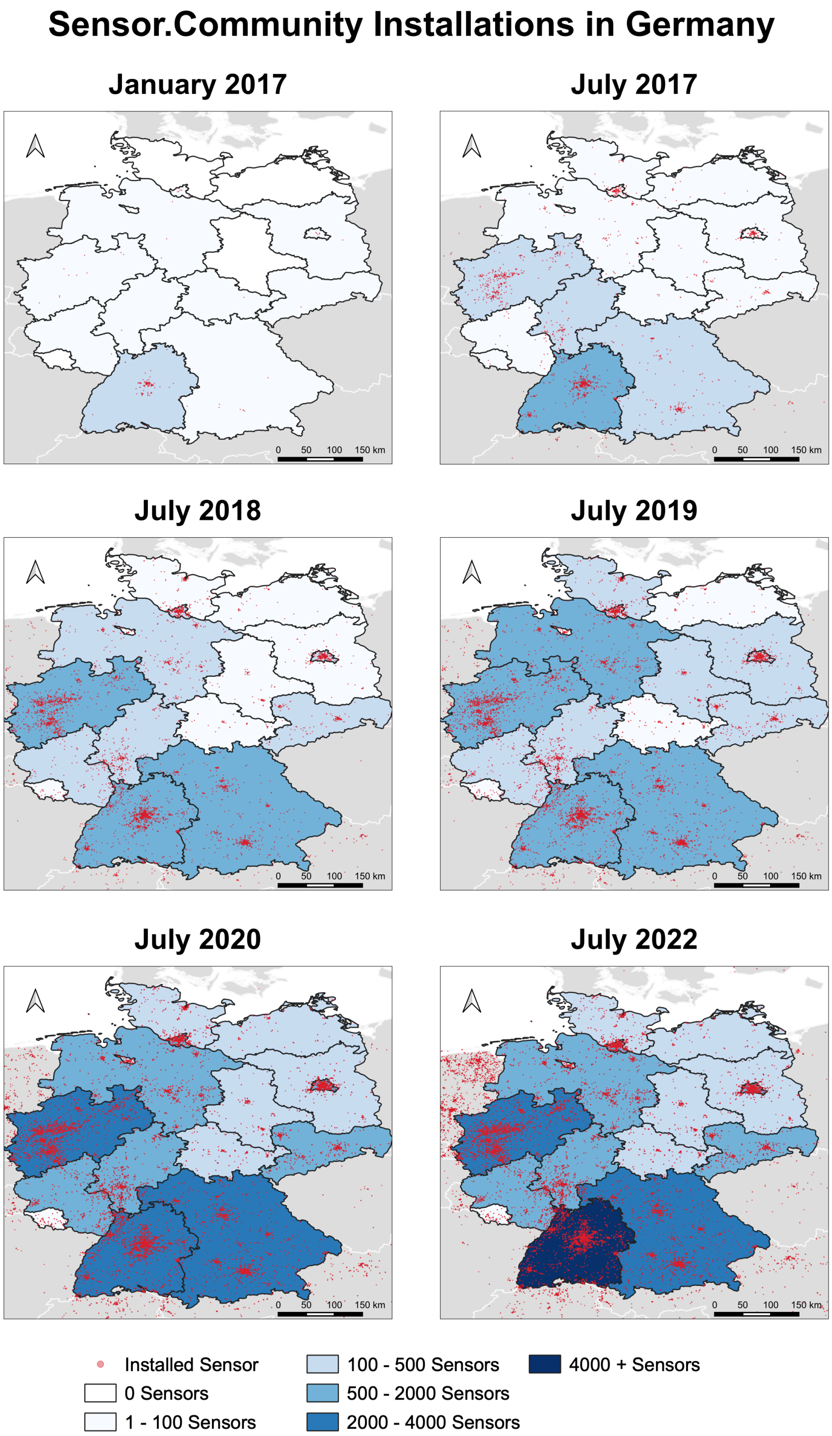

Figure 3.A.2: Sensor.Community SAQS installations in Germany. Shading corresponds to cumulative sensor installations by state.

3.B Government Pollution Monitoring

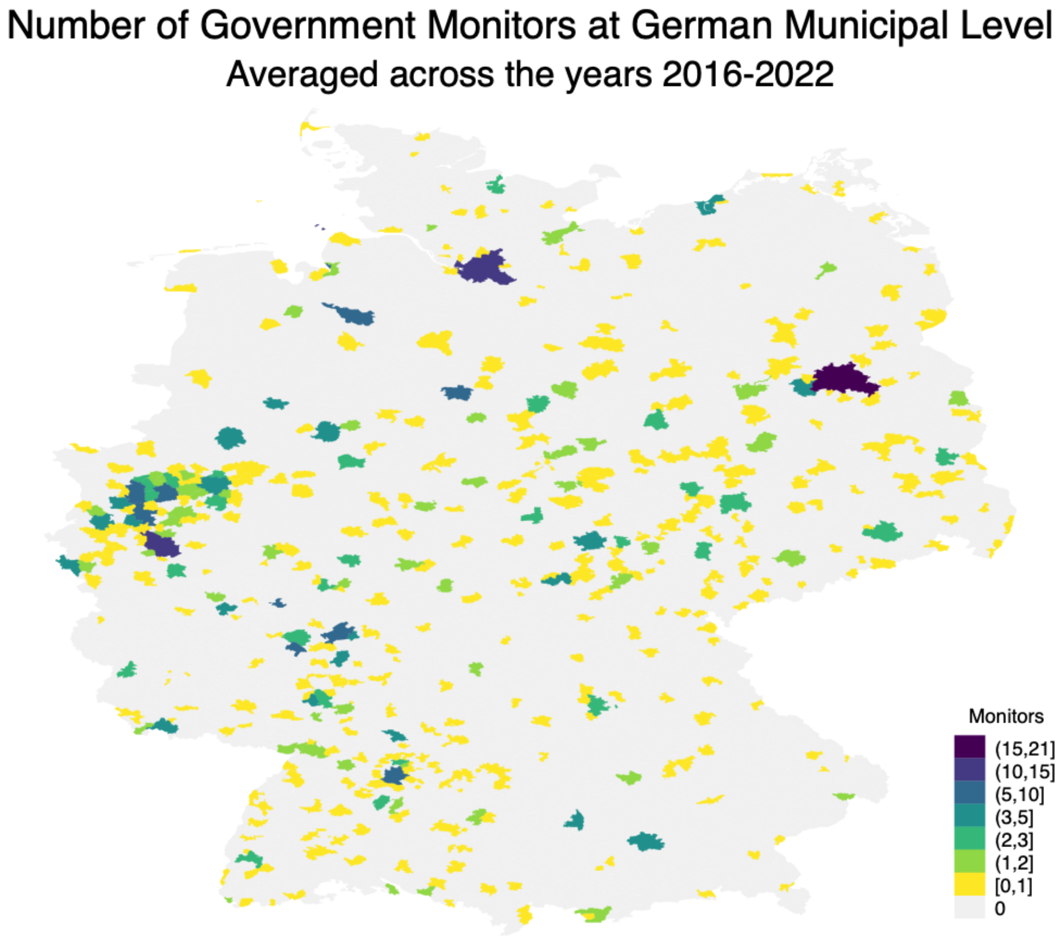

Figure 3.B.1: German municipalities with active $PM_{2.5}$, PM_{10}, or NO_2 government monitors from 2016-2022. Shading corresponds to the average number of monitors active per year.

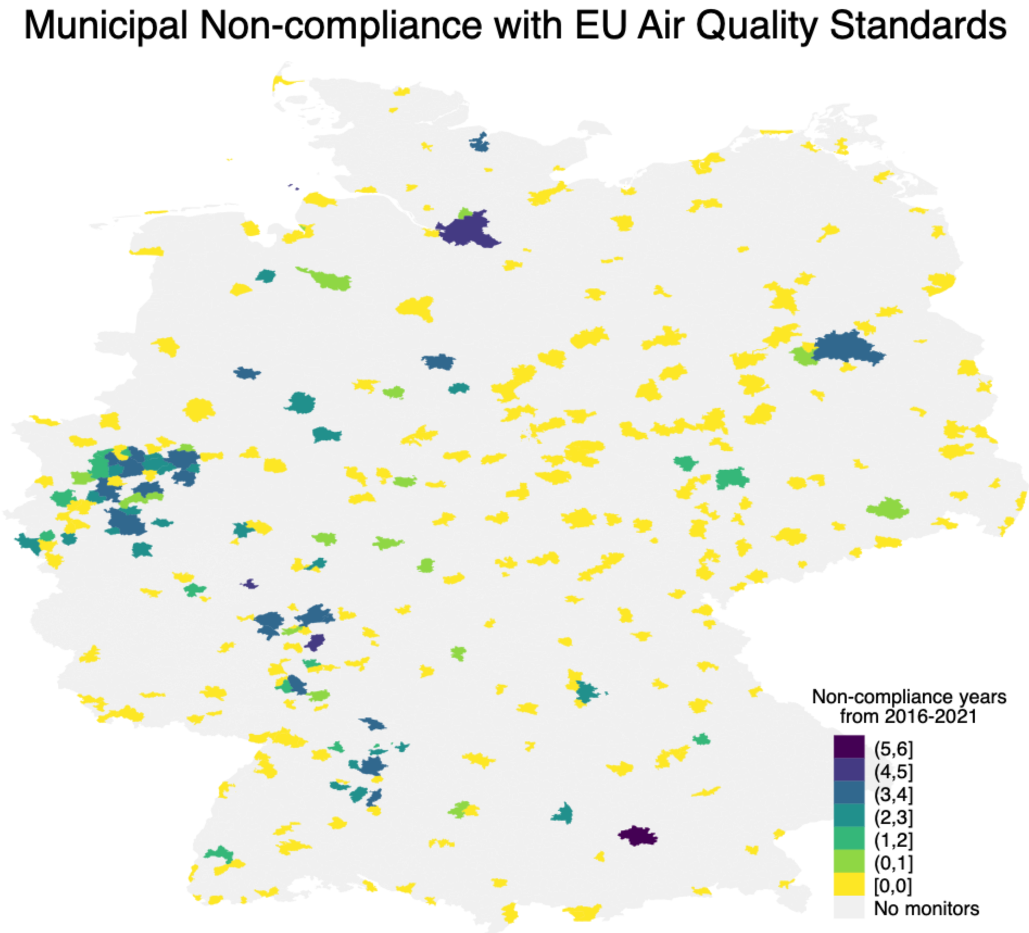

Figure 3.B.2: $PM_{2.5}$, PM_{10}, or NO_2 non-compliance years from 2016-2021.

3.C Satellite Pollution Maps

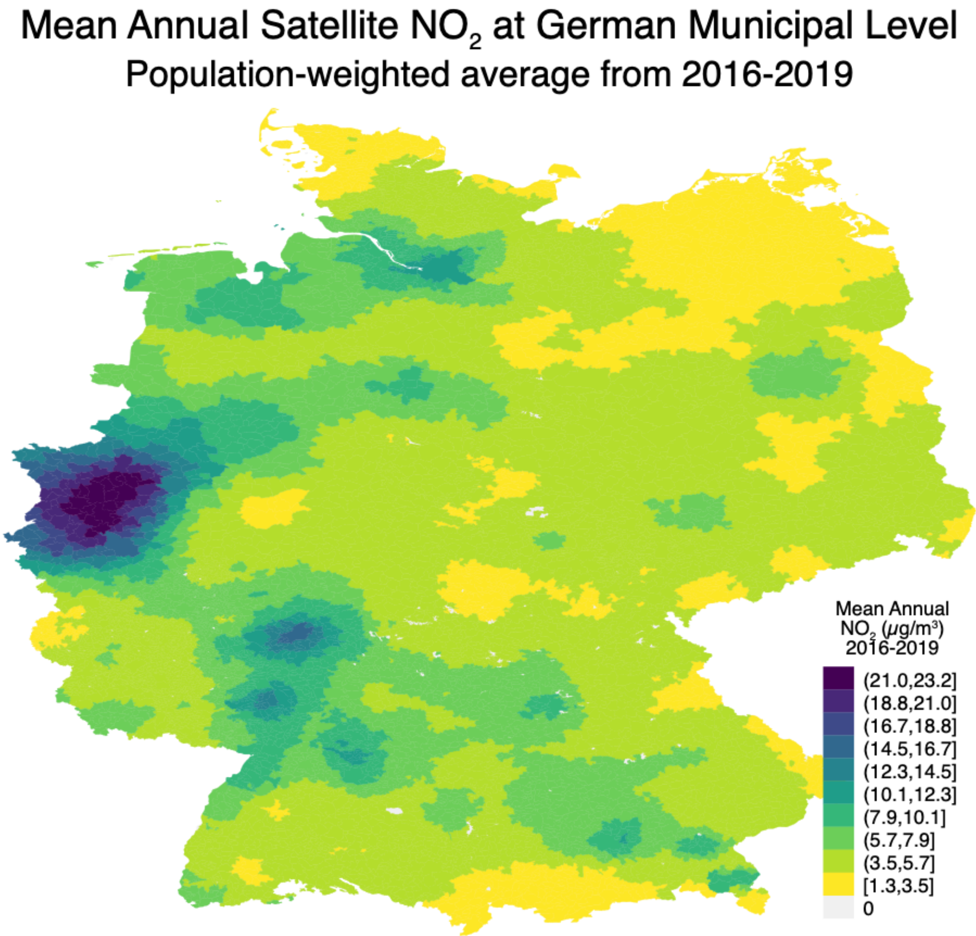

Figure 3.C.1: Mean annual satellite-monitored NO_2 concentrations at the German municipal level. Calculations based on RWI-GEO-GRID population-weighted 1-km^2 and Cooper (2022). Averages across four available years 2016-2019.

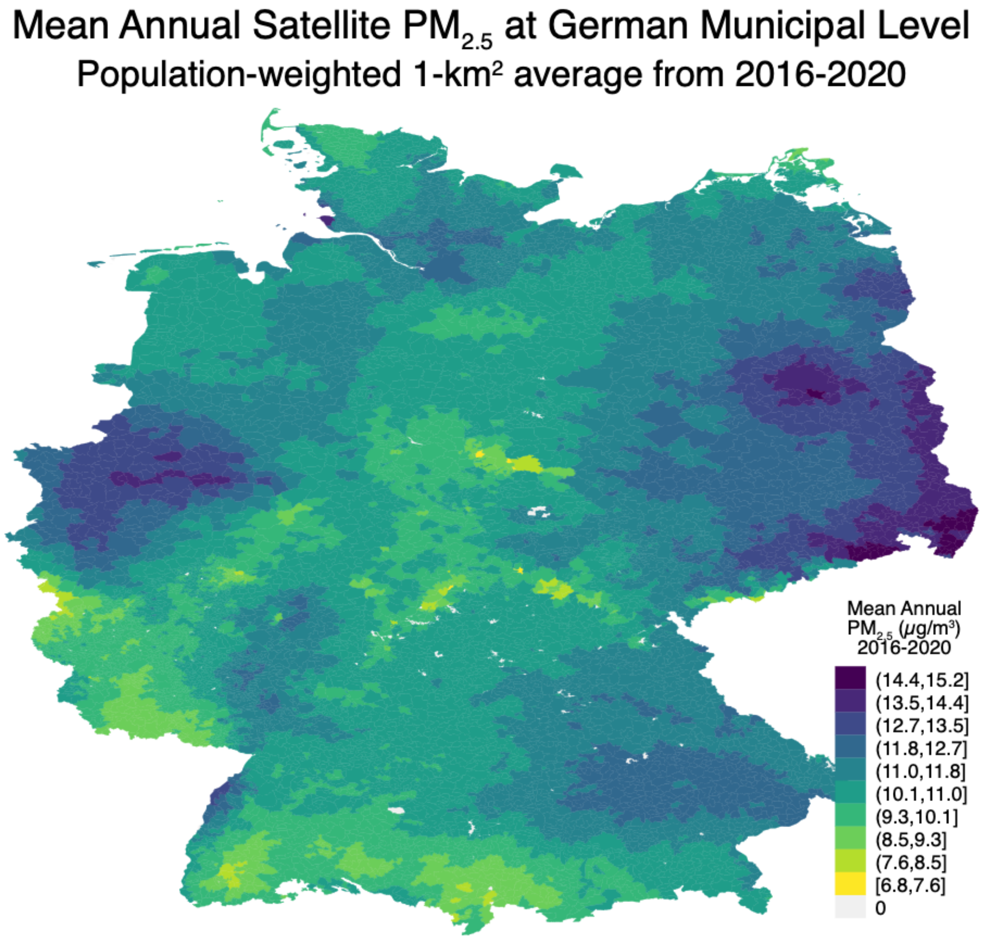

Figure 3.C.2: Mean annual satellite-monitored $PM_{2.5}$ concentrations at the German municipal level. Calculations based on RWI-GEO-GRID population-weighted 1-km^2 and van Donkelaar et al. (2021). Averages across five available years 2016-2020.

3.D Additional Graphs

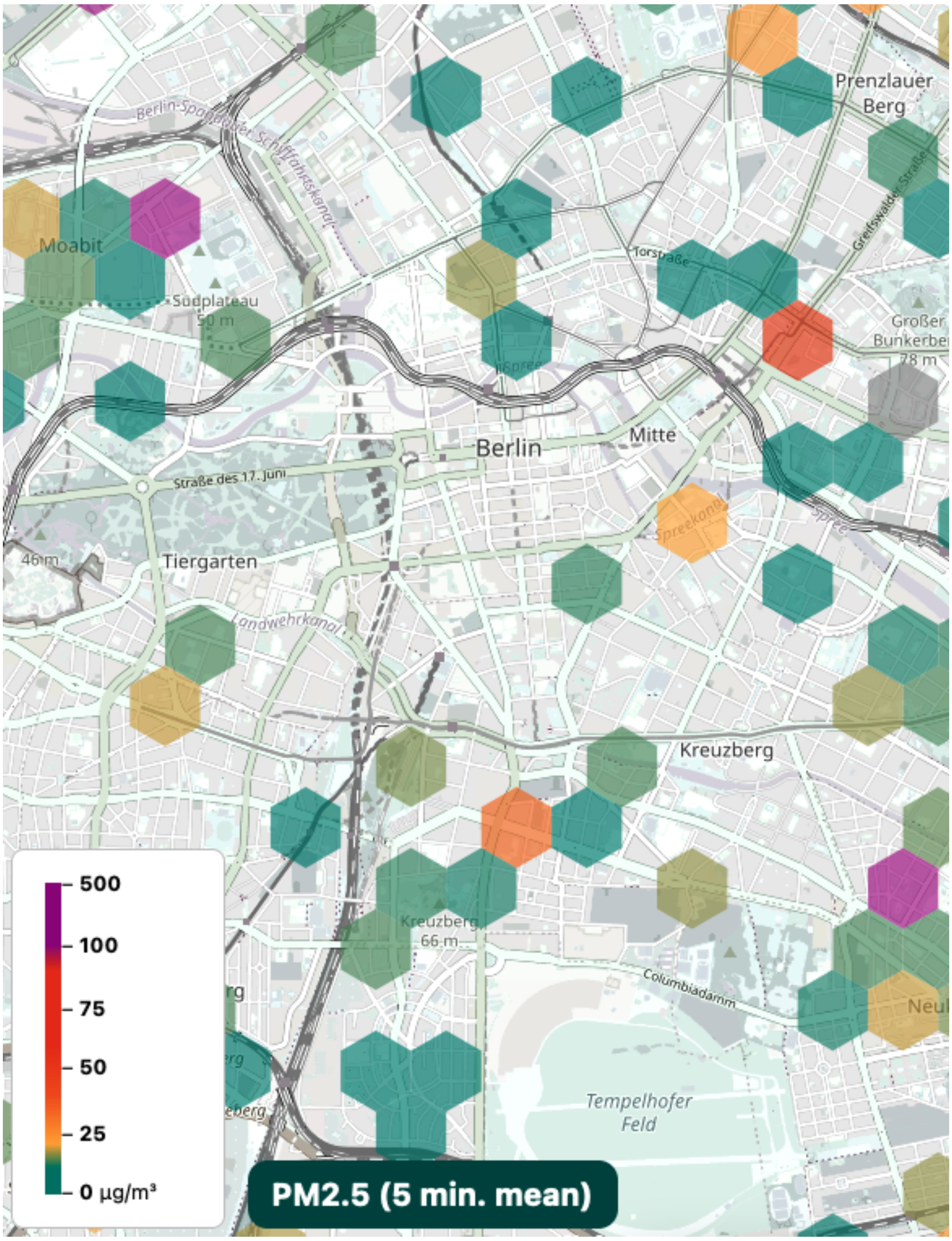

Figure 3.D.1: Sensor.Community online map.

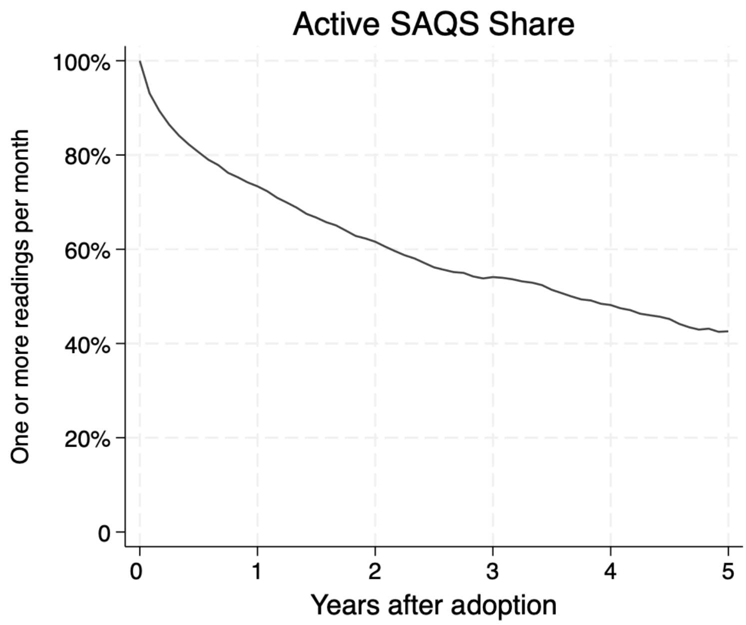

Figure 3.D.2: Sensor.Community sensor activity over sensor installation lifetime.

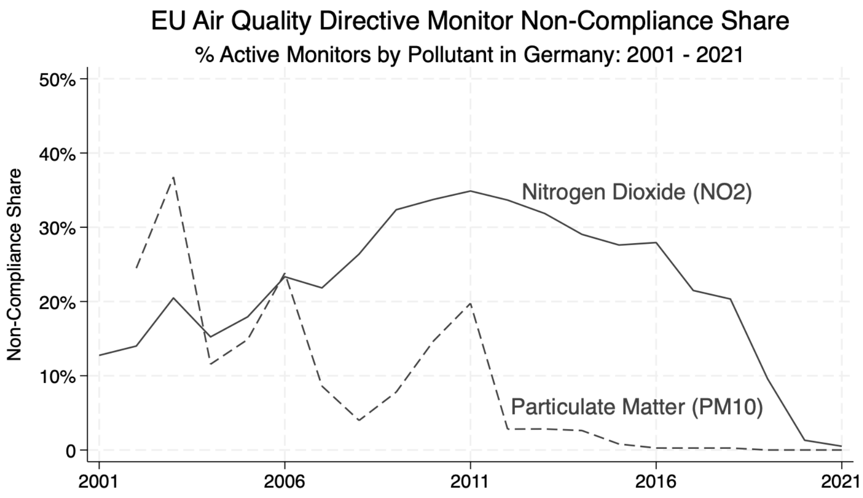

Figure 3.D.3: German air quality monitor non-compliance from 2001 to 2020

3.E Additional Regression Tables

Table 3.E.1: Regression Results: Adoption Spillovers

Distance Band	(1)	(2)	(3)	(4)	(5)
0 - 2.5km	0.002	-0.032*	-0.037**	-0.037**	-0.274***
	(0.009)	(0.013)	(0.013)	(0.014)	(0.046)
2.5 - 5km		0.031**	0.025*	0.021*	0.054**
		(0.010)	(0.011)	(0.011)	(0.018)
5 - 7.5km			0.012	0.003	0.030
			(0.009)	(0.011)	(0.025)
7.5 - 10km				0.018	0.012
				(0.012)	(0.023)
Grid FE	Yes	Yes	Yes	Yes	No
Year-quarter FE	Yes	Yes	Yes	Yes	Yes
Grid-year FE	No	No	No	No	Yes
Observations	46,662	46,662	46,662	46,662	12,588

Notes: This table reports Poisson pseudo maximum likelihood regression estimates and standard errors. In each regression, the dependent variable is quarterly grid cell SAQS adoptions per capita. Explanatory variables are spatiotemporal SAQ neighbor counts in the designated distance band installed in the previous quarter. Standard errors clustered on grid cell in columns 1-4 and on grid-year in column 5. Significance level: + = $p < 0.1$, * = $p < 0.05$, ** = $p < 0.01$, and *** = $p < 0.001$

Table 3.E.2: Regression Results: Adoption Spillovers

Distance Band	(1)	(2)	(3)	(4)	(5)
0 - 1km	-0.072	-0.104*	-0.123*	-0.139*	-0.829***
	(0.045)	(0.050)	(0.055)	(0.057)	(0.088)
1 - 2km		0.056^{+}	0.017	0.004	-0.013
		(0.033)	(0.046)	(0.042)	(0.040)
2 - 3km			0.072*	0.062	0.098*
			(0.035)	(0.046)	(0.045)
3 - 4km				-0.044	-0.011
				(0.042)	(0.038)
4 - 5km				0.080^{+}	0.073
				(0.047)	(0.047)
5 - 6km					-0.029
					(0.044)
6 - 7km					0.137*
					(0.055)
7 - 8km					-0.024
					(0.041)
8 - 9km					-0.089*
					(0.041)
9 - 10km					-0.052
					(0.051)
Grid FE	Yes	Yes	Yes	Yes	No
Year-quarter FE	Yes	Yes	Yes	Yes	Yes
Grid-year FE	No	No	No	No	Yes
Observations	46,662	46,662	46,662	46,662	12,588

Notes: This table reports Poisson pseudo maximum likelihood regression estimates and standard errors. In each regression, the dependent variable is quarterly grid cell SAQS adoptions per capita. Explanatory variables are spatiotemporal SAQ neighbor counts in the designated distance band installed in the previous quarter. Standard errors clustered on grid cell in columns 1-4 and on grid-year in column 5. Significance level: + = p < 0.1, * = p ¡< 0.05, ** = p < 0.01, and *** = p < 0.001

Table 3.E.3: Regression Results: Adoption Spillovers 2018-2022

Distance Band	(1)	(2)	(3)	(4)	(5)
0 - 1km	-0.179**	-0.190**	-0.198**	-0.224**	-0.840***
	(0.069)	(0.070)	(0.070)	(0.073)	(0.101)
1 - 2km		0.052	0.037	0.027	0.000
		(0.034)	(0.036)	(0.035)	(0.042)
2 - 3km			0.047	0.028	0.046
			(0.040)	(0.047)	(0.049)
3 - 4km				-0.057	-0.036
				(0.048)	(0.045)
4 - 5km				0.149*	0.113+
				(0.066)	(0.061)
5 - 6km					0.005
					(0.047)
6 - 7km					0.146*
					(0.066)
7 - 8km					-0.040
					(0.046)
8 - 9km					-0.132**
					(0.047)
9 - 10km					-0.049
					(0.054)
Grid FE	Yes	Yes	Yes	Yes	No
Year-quarter FE	Yes	Yes	Yes	Yes	Yes
Grid-year FE	No	No	No	No	Yes
Observations	34,903	34,903	34,903	34,903	10,428

Notes: This table reports Poisson pseudo maximum likelihood regression estimates and standard errors. In each regression, the dependent variable is quarterly grid cell SAQS adoptions per capita. Explanatory variables are spatiotemporal SAQ neighbor counts in the designated distance band installed in the previous quarter. Standard errors clustered on grid cell in columns 1-4 and on grid-year in column 5. Significance level: + = $p < 0.1$, * = $p < 0.05$, ** = $p < 0.01$, and *** = $p < 0.001$

Table 3.E.4: Regression Results: Adoption Spillovers

Distance Band	(1)	(2)	(3)	(4)	(5)
0 - 1km	-0.118**	-0.161***	-0.171***	-0.204***	-1.648***
	(0.042)	(0.044)	(0.046)	(0.048)	(0.204)
1 - 2km		0.058*	0.040	0.022	0.006
		(0.025)	(0.032)	(0.029)	(0.055)
2 - 3km			0.027	0.006	0.063
			(0.026)	(0.027)	(0.042)
3 - 4km				-0.010	-0.048
				(0.021)	(0.043)
4 - 5km				0.070**	0.139**
				(0.027)	(0.045)
5 - 6km					-0.051
					(0.041)
6 - 7km					0.090*
					(0.041)
7 - 8km					0.001
					(0.034)
8 - 9km					0.015
					(0.038)
9 - 10km					-0.005
					(0.035)
Grid FE	Yes	Yes	Yes	Yes	No
Year-quarter FE	Yes	Yes	Yes	Yes	Yes
Grid-year FE	No	No	No	No	Yes
Observations	46,662	46,662	46,662	46,662	12,588

Notes: This table reports Poisson pseudo maximum likelihood regression estimates and standard errors. In each regression, the dependent variable is quarterly grid cell SAQS adoptions per capita. Explanatory variables are spatiotemporal SAQ neighbor counts in the designated distance band installed in the previous quarter. Standard errors clustered on grid cell in columns 1-4 and on grid-year in column 5. Significance level: + = $p < 0.1$, * = $p < 0.05$, ** = $p < 0.01$, and *** = $p < 0.001$

4

Air Quality Alerts and Don't Drive Appeals: Evidence on Voluntary Pollution Mitigation Dynamics from Germany

Air Quality Alerts and Don't Drive Appeals: Evidence on Voluntary Pollution Mitigation Dynamics from Germany

Alexander Dangel and Timo Goeschl

Abstract

This paper studies temporal factors influencing the effectiveness of don't drive appeals (DDAs) which policy-makers use to encourage motorists to voluntarily reduce driving during transitory high pollution episodes. We derive and empirically validate a theoretical framework for DDAs where the desired behavioral response is sensitive to the number of consecutive DDA days and recovery time between episodes. Our analysis of daily traffic flows from automatic traffic counters in Stuttgart, Germany shows that DDAs at best reduce overall car trip demand during pollution events by an average of 1%, but treatment effects vary. Difference-in-difference event study estimates reveal that DDAs: i) lead to a 3% traffic reduction on the first three days of DDAs and taper off in effectiveness during longer episodes, ii) regain effectiveness at the tail end of DDA episodes once local authorities announce when they will be lifted, and iii) only reduce city center traffic following lengthy recovery periods between events. Our findings provide evidence that temporal factors like social norms and intertemporal substitution dynamically affect voluntary short-term pollution mitigation programs. They also confirm prior North American evidence on DDA traffic displacement and limited overall impact in a European setting.

Keywords: information-based regulation; voluntary policies; air quality alerts; timing; social norms; intertemporal substitution; prosocial behavior; transportation choice

JEL Classification: D91, Q52, Q53, R40

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4.1 Introduction

Policy-makers in urban areas commonly use air quality alerts (AQAs) to inform the public of heightened ambient air pollution levels and to appeal for short-term adaptation and mitigation. Individuals, particularly those from at-risk populations, try to reduce pollution exposure during AQAs by rescheduling commutes (Saberian et al., 2017), abstaining from strenuous outdoor activity (Fan, 2024; Ward and Beatty, 2015), forgoing leisure in outdoor recreational spaces (Janke, 2014; Graff Zivin and Neidell, 2009), and investing in protective face masks (Liu et al., 2017), but responsiveness diminishes on consecutive alert days (Graff Zivin and Neidell, 2009; Saberian et al., 2017).[1] Previous evidence on voluntary pollution mitigation during AQAs is less conclusive, and its temporal dimension remains understudied. North American programs that combine AQAs with don't drive appeals (DDAs) find that DDAs are often ineffective in reducing car use (Noonan, 2014; Sexton, 2012; Cummings and Walker, 2000), while Caplan (2023) and Tribby et al. (2013) show they may even inadvertently increase driving. Cutter and Neidell (2009) are the only ones to document an effective DDA. In this paper, we examine whether time-related DDA design choices, namely event duration and between-event recovery time, affect whether commuters voluntarily drive less during DDAs by studying a policy setting where they are implemented frequently and often for extended periods.

We begin by drawing from existing modal switching models (Cutter and Neidell, 2009; Sexton, 2012; Basso and Silva, 2014) to introduce a theoretical framework for DDAs that predicts driving reductions and incorporates dynamic social norm effects. Despite evidence of shortcomings in other contexts (Noonan, 2014; Sexton, 2012; Cummings and Walker, 2000),[2] policy-makers continue to rationalize

[1] This is called alert fatigue in the literature. Graff Zivin and Neidell (2009) and Saberian et al. (2017) study multi-day AQAs and find evidence for alert fatigue after the first AQA day.

[2] These findings correspond with first-order expectations under the assumption of self-interested, utility-maximizing agents. Motorists, who pollute the air and thereby impose a negative externality on others, optimize their private well-being (including private health costs) when deciding how much to drive but do not factor in the social cost of their choices. In aggregate, this leads to a socially-inefficient pollution surplus. Policy-makers attempt to solve this collective action problem using moral levers (i.e. DDAs) or congestion management policies (i.e. transit fare subsidies, congestion pricing, vehicle bans, etc.) to make driving relatively more costly and

the use of moral appeals (Ito et al., 2018; Ferraro et al., 2011; Cutter and Neidell, 2009; Reiss and White, 2008) for voluntary driving reductions, for example, as part of Action Day programs in US cities[3] and similar policies in major urban areas around the world.[4] Our first contribution in this paper is to model this thinking.

We then test this model empirically in Stuttgart, Germany, a European metropolitan setting seemingly well-suited for a program targeting voluntary driving reductions due to its abundant transit alternatives[5] and widespread environmental preferences.[6] Local authorities in Stuttgart, Germany raised a particulate matter AQA (*Feinstaubalarm*) to inform the public of high ambient air pollution levels during multi-day periods with limited atmospheric interchange capacity from January 2016 to April 2020[7]. When Stuttgart's AQA is active, authorities also temporarily reduced public transit fares and widely broadcast DDAs encouraging motorists to stop driving cars and to switch to riding public transit, cycling, walking, working from home, or otherwise abstaining from driving. Our ordinary least squares (OLS) regression analysis leverages daily traffic flows from 56 automatic traffic counters (ATCs) located within and just beyond the Stuttgart administrative border to measure the impact of DDAs on aggregate car trip demand. Our preferred estimation framework studies multi-day, dynamic DDA effects using a difference-in-difference (DiD) event study design that compares traffic levels in Stuttgart with traffic levels in the neighboring metropolitan city of Munich using

shift individual driving choices towards the socially-optimal level. However, we would not expect self-interested, utility-maximizing agents to be swayed by an appeal for collective benefits at a private cost, beyond its direct effect on private well-being.

[3]See https://www.airnow.gov/aqi/action-days/ for a list of Action Day programs in the US.

[4]For example, see program descriptions for Korea (https://airkorea.or.kr/eng/O3Alert?pMENU˙NO=162) or Île de France, France (https://www.airparif.fr/en/index.php/procedure-dinformation-et-dalerte).

[5]Stuttgart has an extensive public transportation network consisting of seventeen regional train lines, seven suburban train lines, nineteen light-rail lines, and 390 bus lines.

[6]A coalition led by the Green party has governed the state of Baden-Württemberg since 2011, Germany's first Green party state Minister-President was elected in Baden-Württemberg in 2011 and reelected in 2016 and 2021, and a Green party politician has held office as Stuttgart's Mayor since 2013.

[7]The German Weather Agency (Deutscher Wetterdienst, *DWD*) defines days with a limited interchange capacity as days with low rainfall, low wind speed, nighttime ground inversions, and low daytime atmospheric mixing layers. In these conditions, particulate matter pollution can easily accumulate to higher levels. The program targeted collective environmental benefits from emissions reductions related to to driving reductions. See Background for more details.

data from an additional twenty ATCs located there.[8]

Our empirical results make three additional contributions to the literature. First, we study the overall impact of DDAs on voluntary driving reductions outside the United States for the first time, and thereby provide evidence about whether previous results transfer to other policy settings. Our analysis of Stuttgart traffic data shows that vehicle flows in the city decrease at best between 0.5% and 0.7% on days when authorities implement DDAs. Previous evidence on DDA effectiveness finds that DDAs can be moderately effective (up to a 3% reduction, Cutter and Neidell, 2009), statistically ineffective (Noonan, 2014; Sexton, 2012; Henry and Gordon, 2003; Cummings and Walker, 2000), or even counter-productive (Tribby et al., 2013) in temporarily abating driving in the United States. Unlike most previously studied programs in North America, our empirical setting has an abundance of transit alternatives and widespread environmental preferences, suggesting that a DDA has high impact potential. However, estimated DDA impacts in our metropolitan European setting appear no more effective than previously studied DDA programs in the North American context.

Second, we highlight temporal heterogeneity in DDA effectiveness. We show that DDAs in our setting lead to traffic reductions up to 3% on the first three DDA days after activation, but that effectiveness wanes during prolonged DDAs. Unlike previous studies that are limited to analyzing second day alert fatigue (Graff Zivin and Neidell, 2009; Sexton, 2012; Saberian et al., 2017), our empirical setting enables us to evaluate alert effectiveness over a much longer treatment period. We find evidence suggesting that DDAs are, in general, most effective when they are soon to be lifted and that traffic may rebound on the second day after DDAs end, which both point to intertemporal substitution factoring into decisions about when to adhere to appeals for voluntary pollution mitigation. In general, our results on prolonged DDA treatment exposure may be particular valuable in settings with more persistent pollution episodes than previously studied North American settings.

[8] Munich is a similarly sized metropolitan city in southern Germany (metropolitan region population: 6.2 million in Munich vs. 5.3 million in Stuttgart in 2023) with highly correlated traffic, pollution, and meteorological conditions that, like Stuttgart, failed to comply with EU air quality regulations during the DDA policy implementation period (2016-2020) but did not implement an AQA program or a DDA policy.

Third, we provide novel evidence about the sensitivity of DDA effectiveness to the recovery period between DDA events. In our theoretical framework, we hypothesize that DDAs are less effective after short recovery periods. Our empirical results confirm this prediction and show that DDAs implemented with at least a nine day recovery period reduce traffic by 5% at the city center. To the best of our knowledge, this paper is the first to test the importance of this temporal dimension for DDAs empirically and provides guidance for policy-makers deciding how to incorporate frequency considerations into the design of AQAs, DDAs, and other voluntary mitigation policies.

The remainder of this paper is structured in the following manner. The next section provides background information about Stuttgart's AQA program and its accompanying DDA. In section 4.3, we formalize a theoretical framework for DDAs. Section 4.4 describes the data we use for our empirical analysis, while section 4.5 explains our estimation strategy for identifying DDA impacts. Section 4.6 discusses our results and section 4.7 concludes.

4.2 Background

4.2.1 Stuttgart's Air Quality Alert Program

On January 1, 2016, Stuttgart city officials introduced its AQA program as part of a multi-policy air quality plan targeting compliance with EU air quality standards.[9] During the PM season,[10] the AQA program notified residents in the greater Stuttgart metropolitan region of upcoming and ongoing poor air quality episodes

[9]Under EU Air Quality Directive 2008/50/EC, daily average ambient PM_{10} concentrations are not to exceed 50 $\mu\ g/m^3$ more than 35 times per calendar year. From 2004 through 2017, daily ambient PM_{10} concentrations at the Neckartor air quality monitor in central Stuttgart annually exceeded this legal threshold. The city government, under the auspices of the state government, implemented an air quality improvement plan which included establishing a low emissions zone and corresponding bans on high polluting vehicles, upgrading public transit and bicycle infrastructure, investing in cleaner public transit fleets, expanding park-and-ride parking lots, lowering speed limits on busy streets, banning wood burning stoves during AQAs, reducing public transit fees, increasing street cleaning, and incentivizing employers to recruit employees to purchase monthly public transit tickets.

[10]Stuttgart authorities can call an AQA during the particulate matter (PM) season from October 15th to April 15th, when PM levels are typically highest.

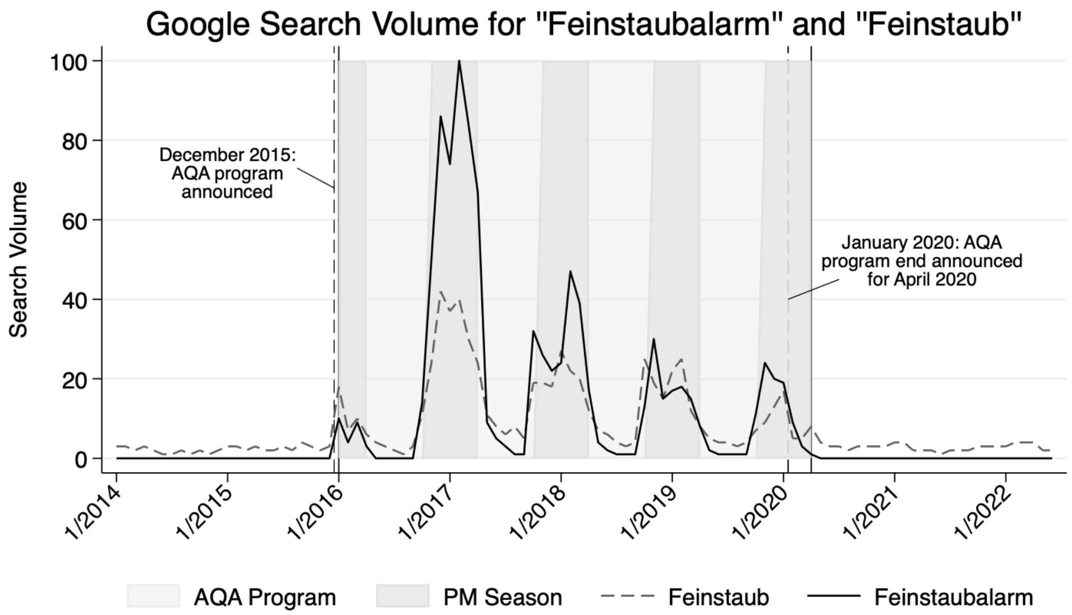

Figure 4.2.1: Google Trends search interest for particulate matter alert ("*Feinstaubalarm*") and particulate matter ("*Feinstaub*", PM) in Baden-Württemberg from January 2014 through May 2022. From January 2016 to April 2020, the AQA program was active annually during the PM season from October 15 to April 15. Search volume is relative to maximum search volume (=100) in February 2017.

via electronic road signs, radio, television, social media, and newspapers. The AQA program's DDA encouraged motorists not to drive and instead to use less-polluting transportation. In contrast to health-oriented air quality alert programs in other cities, local authorities did not explicitly warn Stuttgart residents about the negative health effects of air pollution exposure; the AQA program focused on the collective environmental benefits or so-called "quality-of-life improvements" that could result from a widespread temporary switch away from cars.[11] In early 2020, local authorities announced plans to abandon the AQA program after April of that year, citing its success in reducing air pollution in the city.[12]

[11]Residents may certainly have been aware of air pollution exposure's negative health impacts *ex ante*, may have become informed of them through AQA-adjacent media programming, or may have inferred them from the nature and language of the AQA program.

[12]*Stuttgarter Zeitung*. 2020. Bessere Luft in Stuttgart: Feinstaubalarm wird im April abgeschafft. January 17, 2020.

The city of Stuttgart has approximately 630,000 residents, and based on commuting statistics from the German Federal Employment Agency and the Baden-Württemberg State Statistical Office, we estimate that roughly 382,000 commuters (73% of individuals employed in the city) travel by car or motorcycle in the city of Stuttgart on a given workday, compared to 66,000 (13%) who take public transit and 75,000 who walk or bike (14%).[13] In two telephone surveys conducted by the city government in early 2016, 90-92% of respondents (n_1=1,008, n_2=1,004) reported having heard about the AQA program and 15-25% of respondents stated that they reduced their car use on DDA days.[14] The survey results and online search query data (figure 4.2.1) confirm that AQA messaging arrives in the general population. However, survey responses were self-reported and were collected when the AQA program was new. Surveyors neither elicited nor observed the actual extent of driving reductions, and social-desirability bias presumably leads individuals to over-report driving reductions, so these findings must be interpreted cautiously.

4.2.2 Don't Drive Appeal Conditions and Timing

Stuttgart authorities decide whether to call an AQA and broadcast a DDA using a decision tree based on six binary atmospheric conditions. On each day during the PM season, the German Weather Agency (DWD) takes stock of the following conditions:[15]

- Condition 1 (primary): Whether the daily mean PM_{10} concentration at Neckartor monitoring station is over 30 μg/m^3 and no rainfall is forecast until 12am of the first forecast day.[16]

[13] Hence, for each percentage point change in daily car commuters on DDA days, we estimate that about 4,000 car commuters switch their mode of transit or work from home. We anticipate that these are low ballpark estimates for the daily number of vehicles on Stuttgart roads, as our calculations do not include non-employed motorists (e.g. retirees, students, unemployed people, etc.), nor do estimates include other reasons for driving into the city (e.g. leisure or business travel, through traffic, etc.).

[14] See *Befragung zum Thema Feinstaubalarm in Stuttgart und Umgebung* (Omnitrend, 2016b) and *Befragung zum Thema Feinstaubalarm in Stuttgart und Umgebung im Zeitraum 26.2.2016 bis 28.2.2016* (Omnitrend, 2016a)

[15] See *Schadstoffrelevante Kriterien des Deutschen Wetterdienstes* (DWD, 2020)

[16] Snowfall and sleet are treated as rainless.

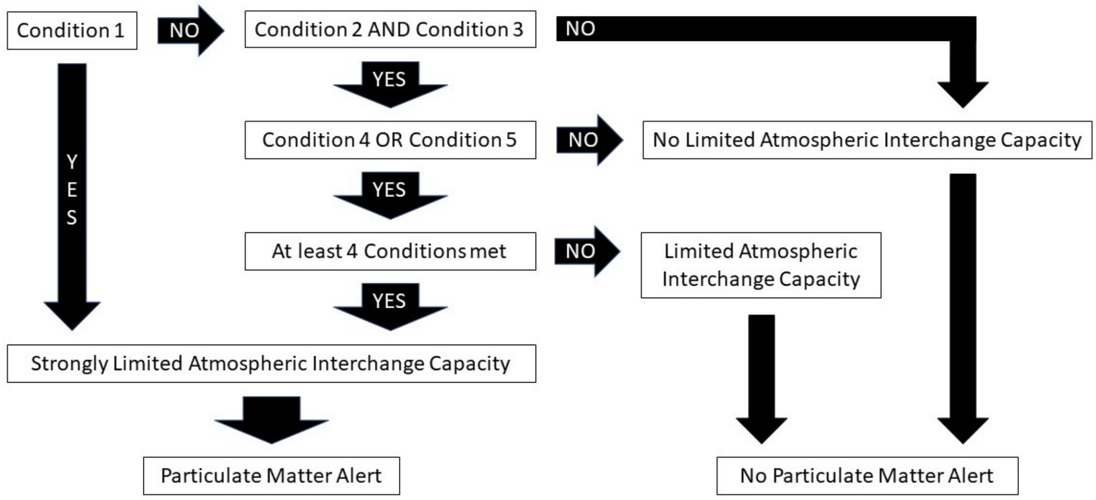

Figure 4.2.2: DWD Decision Tree for calling and ending an AQA. The "Particulate Matter Alert" outcome leads authorities to broadcast a Don't Drive Appeal (DDA). Adapted from information from DWD.

- Condition 2: Whether no rainfall is forecast for both the bridge day[17] and the first forecast day.
- Condition 3: Whether wind blows with an average wind speed over 3 km per hour from 180°-330°.
- Condition 4: Whether there is a nighttime ground inversion.[18]
- Condition 5: Whether there is a low daytime mixing layer.[19]
- Condition 6: Whether average wind speed is below 3 km per hour.

According to the outcome of each binary condition and the corresponding decision rules in figure 4.2.2, DWD classifies the atmospheric interchange capacity as either "not limited," "limited" or "strongly limited" with only the latter leading to an

[17] There is a one day pause between the day an AQA event is announced and the day the DDA is activated.

[18] Nighttime ground inversion is defined as an air layer within which temperature increases with altitude. Such an inversion traps particulate matter in the Stuttgart valley.

[19] The mixing layer height indicates the interchange capacity of the low lying air masses. The lower the mixing layer height, the smaller is the interchange capacity. The criterion is fulfilled if the mixing layer height is lower than 500 meters during the day.

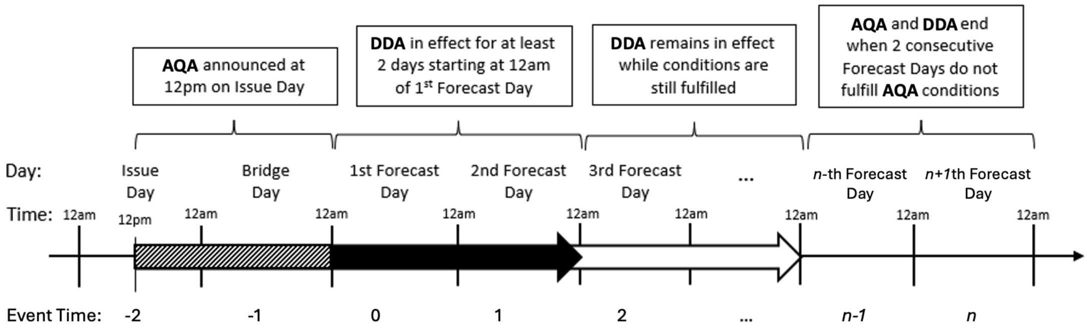

Figure 4.2.3: Air quality alert (AQA) and don't drive appeal (DDA) timing. Information from the City of Stuttgart. In our analysis, DDA activation corresponds with event time period 0.

AQA. There are two paths to an AQA. First, as the primary condition, fulfilling condition 1 is sufficient for activation. Second, if condition 1 is not satisfied, then conditions 2 and 3, either condition 4 or 5, and at least four criteria overall must be fulfilled for the city to call an AQA. In the latter path, the 30 μg/m^3 threshold from condition 1 is no longer relevant for activation.

If local authorities decide to call an AQA, they begin notifying the public in the early afternoon of the issue day of high air pollution levels and about a forthcoming DDA that will be activated 36 hours later (see figure 4.2.3, event time: -2). A bridge day (event time: -1), when the public continues to be informed about the upcoming AQA but the DDA has not gone into effect, follows the issue day. The DDA comes into effect after the bridge day at 0:00 am of the first forecast day (event time: 0). The DDA continues for at least a second day (event time: 1) and remains in effect until the DWD forecasts two consecutive days where the atmospheric interchange capacity is not "strongly limited." Local authorities announce the end of the AQA and DDA two days before DDA messaging subsides.

Importantly, AQA and DDA designation is based on weather forecasts, not *actual* weather conditions on a given day. If authorities call an AQA, unanticipated meteorological changes between the issue day and any subsequent day may improve atmospheric interchange capacity to the extent that some AQA conditions may no longer be fulfilled on that day. On these days, DDA messaging continues to be broadcast although the atmospheric conditions are not necessarily fulfilled.

By similar logic, actual meteorological conditions may worsen the atmospheric interchange capacity to the extent that, on a given non-DDA day, a DDA should have been broadcast, even though it was not. At the margin, local authorities can exercise limited discretion when deciding whether to initiate an AQA event and broadcast the DDA, specifically in cases when thresholds are just barely met (e.g. a small amount of rainfall may not be deemed sufficient to clear particulates from the air).

4.3 Theoretical considerations

Stuttgart's policy-makers employ a DDA in the ostensible belief, publicly expressed, that a morally framed request directed at car owners, combined with a public transit subsidy, will reduce driving. To see whether this belief can be rationalized, we develop a plausible mental model that formalizes this thinking. This simple theoretical framework is informed by existing models of modal switching for the *Spare The Air* (STA) program in the San Francisco, USA Bay Area (Cutter and Neidell, 2009; Sexton, 2012) and urban congestion management policies in London, UK and Santiago, Chile (Basso and Silva, 2014). To adapt the framework for the case at hand, we explicitly downplay the individual health aspects at the heart of the Bay Area's STA program, which – unlike for the case of Utah's "yellow alert days" (Caplan, 2023) – are not part of Stuttgart's DDA. We instead emphasize its moral appeal considerations.

The literature identifies injunctive and descriptive norms as the main pathways through which a moral appeal can change the behavioral calculus of which action to choose (Bicchieri, 2005). Injunctive norms define how an individual ought to act. They constitute abstract moral absolutes, that is behavioral benchmarks independent of other people's behavior. Descriptive norms, on the other hand, reflect how most other people act. They are observable behavioral patterns in the population. In both cases, the literature has argued, individuals receive emotional rewards or losses from themselves and others as a function of adherence to or deviation from the norm. The associated feelings of righteousness and approval and of shame and guilt enter the utility function and can thus affect decision-making (Battigalli and Dufwenberg, 2007; Zafar, 2011).

Policy-makers are unlikely to be unaware of the subtle distinction between injunctive and descriptive norms. Yet, their mental model of DDAs may well capture the idea of injunctive norms by postulating that a DDA makes people attach positive feelings to deciding not to drive.[20] Descriptive norms could be captured by attaching to driving a negative feeling whose strength depends on the effectiveness of the appeal on others: Guilt and shame are strongest if the individual driver finds himself the only driver on the road, particularly if watched by non-drivers. They do not arise when traffic density during the DDA event is the same (or even higher) than before (Zafar, 2011). Considerations of positive and negative feelings triggered by adhering and deviating from norms would provide policy-makers with a behaviorally informed model of how car owners respond to the introduction of a DDA. They can also be extended to the question of how effective a DDA is likely to be over time. Policy-makers' intuition that the impact of DDAs wears off over a multi-day DDA event and needs time to recover between DDA events accords with well-established findings in psychology. Experimental tests of the theory of "ego depletion of self control" (Baumeister et al., 2000) consistently show that the emotional costs of not complying with norms that require a change from previous behavior decrease over time (Dang, 2018) and require a 'recovery period' between norm activation events (Tice et al., 2007). Considerations of both a static and dynamic nature are therefore likely to populate policy-makers' mental models of how a DDA affects driving.

4.3.1 Static model

To give some analytical heft to policy-makers' reasoning, we assume in line with the static congestion model of Basso and Silva (2014) that at any given point in time t, each individual i with access to a car and wishing to travel decides between driving (D) and not driving (ND) to reach their destination.[21] Driving is associated with utility (time arguments suppressed)

$$U_i^D = V_i^D - \tau_i t^D(1 + Q^D) - p^D - \mathbb{1}_A E_i \max\left\{(\overline{Q}^D - Q^D); 0\right\} \tag{4.1}$$

[20] Equivalently, it could be introduced as a negative feeling attached to driving. Analytically, it leads to the same results.

[21] These model formulations purposefully neglect the extensive margin of deciding not to travel.

while not driving is associated with utility

$$U_i^{ND} = V_i^{ND} - \tau_i t^{ND} - p^{ND}(1 - \mathbb{1}_A \delta) + \mathbb{1}_A G \tag{4.2}$$

with $\mathbb{1}_A$ an indicator variable that is one if an appeal has been issued and zero otherwise.

Expressions (4.1) and (4.2) capture that in the absence of a DDA ($\mathbb{1}_A = 0$), the respective utilities are a function of the intrinsic value that individual i associates with driving D and not driving ND, V_i^D and V_i^{ND}, the expenses of driving and not driving at market prices, p^D and p^{ND}, and the mode-independent[22] opportunity cost of time τ_i multiplied by the mode-specific travel time, t^D and t^{ND}. As in other models, total driving time is approximated as linear in car traffic density, measured by the aggregate demand for driving Q^D, along the entire itinerary, $t^D(1 + Q^D)$.[23] The driving-related air quality impacts that play a central role in the health-messaging models by Cutter and Neidell (2009) and Sexton (2012) are neglected in our representation of the policy-makers' mental model of moral appeals.

When a DDA is issued ($\mathbb{1}_A = 1$), three additional factors in expressions (4.1) and (4.2) are activated. First, in (4.2), the policy-maker reduces the cost of public transit through a discount δ, reducing non-driving expenses to $p^{ND}(1-\delta)$. Second, also in (4.2), the policy-maker conveys through the appeal an injunctive norm that foregoing the use of car is the 'right thing to do'. The affective benefits of not driving are captured by a warm glow parameter G associated with norm compliance. Third, in (4.1), the DDA conveys a descriptive norm about driving: The greater the reduction in traffic densities during the DDA event relative to before, the greater the emotional cost to someone still driving. To approximate this effect, a simple linear formulation captures the emotional costs associated with violating the descriptive norm by driving as $E_i \max\left\{\overline{Q}^D - Q^D; 0\right\}$, with $\overline{Q}^D$ denoting aggregate demand for driving outside a DDA event. For traffic densities

[22]Empirical evidence points to mode dependence: Time spent in one's own car has a lower opportunity cost than time spent in public transit. We abstract from this detail here.

[23]Total travel time is t^D when no other car is on the road ($Q^D = 0$) and increases in proportion to use by drivers. The linear approximation overestimates the effect of density on travel time for low levels of density and vice versa for high levels. This will lead to a slight overestimation of the effect of a DDA close to road capacity.

Q^D at or above pre-DDA levels, the emotional cost of driving is zero; for densities below, it is $E_i(\overline{Q}^D - Q^D)$. To bound the possible magnitude of the emotional cost, we assume for simplicity that $E < \tau t^D$, i.e. the marginal driver contributes more to road congestion than to relieving emotional cost.

As in Basso and Silva (2014), equilibrium traffic is the aggregate outcome of individuals deciding to drive if $U_i^D - U_i^{ND} > 0$. Across individuals, this leads to aggregate demand for driving of

$$Q^D = \sum_i \mathbb{1}_i^D, \tag{4.3}$$

with $\mathbb{1}_i^D$ and indicator variable that is one if for individual i, $U_i^D - U_i^{ND} > 0$.

As a result of the congestibility of the road network, there is a demand equilibrium outside DDA events with a simple closed-form solution under the assumption of identical agents of the type

$$\overline{Q}^D = \frac{1}{\tau t^D} \left\{ \Delta V - \Delta p - \tau \Delta t \right\} \tag{4.4}$$

with $\Delta V = V^D - V^{ND}$ denoting the difference in intrinsic values, $\Delta p = p^D - p^{ND}$ the difference in expenses, and $\Delta t = t^D - t^{ND}$ the difference in travel time between driving and not driving. Equilibrium traffic density increases in the intrinsic value differential and decreases in the price and travel time differential between driving and not driving. It is scaled down by the effective cost of time of driving τt^D on account of the congestion externality that every driver imposes on all other drivers in the road network.

A few steps of simple algebraic manipulation also yield the equilibrium traffic density during a DDA as

$$Q^D = \overline{Q}^D - \frac{G + p^{ND}\delta}{\tau t^D - E} \tag{4.5}$$

This leads to our first hypothesis.

Hypothesis 1 *A DDA reduces equilibrium traffic: Static equilibrium traffic density is always lower in the presence of a DDA compared to its absence. The reduction in equilibrium traffic depends positively on the level of material incentives for* *modal switch and on the warm glow of norm-compliant behavior.*

Hypothesis 1 predicts that the first-order impact of a DDA is to reduce traffic. This means that the policy maker achieves the intended policy impact of the DDA in equilibrium. The reduction increases in the warm glow of the appeal, G, and in the public transit discount, δ. Their effect size is scaled by the effective cost of driving time, τt^D, net of the emotional cost of driving when others do not, E. The static congestion model highlights the presence of an instrument for inducing a switch from driving that policy-makers in the city of Stuttgart did not consider: Increasing travel time t^D through speed restrictions.

4.3.2 Dynamic considerations

On most days of the year, potential drivers take their driving decision against the background of no DDA, consistent with a predicted density $\overline{Q}^D$. The announcement of a DDA, its implementation for an uncertain length of time, the announcement of its removal, and the removal represent four transitions during which traffic density is driven by additional dynamic factors. At least two factors are at play that shape changes in traffic densities during transitional periods, intertemporal substitutability and "ego depletion".

The literature commonly assumes that households aim to realize an individually optimal pattern of driving and non-driving that is determined by finite intertemporal substitutability between driving today and driving tomorrow (Cutter and Neidell, 2009; Rivera, 2021; Caplan, 2023).[24] Deviations from this pattern are

[24] Theory does not provide a complete characterization of individual optimal dynamic demand behavior for a congestible setting in which a third party (in this case the policy-maker) changes the cost structure of consumers in a stochastic way. There is a literature on optimal dynamic behavior in settings such as air travel in which parties on the supply side, such as airlines, have committed to supplying a certain capacity at a certain point in time, but have not committed to a price path up to that time (Deneckere and Peck, 2012; Board and Skrzypacz, 2016; Dilme and Li, 2019). Another related literature examines labor-leisure choices in stochastic decision environments (Camerer et al., 1997; Hoffmann and Rud, 2024). While related, neither of these approaches accurately captures the specifics of a modal transport choice of a private household facing the probabilistic imposition and lifting of a DDA. In the appendix, Caplan (2023) provides a possible theoretical model based on the behavior of myopic individuals. Such models make somewhat different predictions than those based on dynamically optimizing individuals (e.g. Dilme and Li, 2019).

costly in welfare terms, yet reflect optimal adjustments to a DDA shock. Depending on whether the DDA shock is in the form of a DDA being announced to be coming into force or to be lifted, this adjustment can have two effects. In the first case, there is a potential anticipation effect: Some household now bring forward to the bridge day driving activities that would otherwise has happened on a day that now falls into the DDA episode. This gives rise to Hypothesis 2.

Hypothesis 2 *There is an anticipation effect of announcing a DDA such that traffic is higher on bridge days. Households' planning horizons allow a share of driving activities to be shifted from a future DDA day to the bridge day so as to benefit from a higher net utility of driving on a non-DDA day.*

We expect the anticipation effect on traffic to be positive, but limited since the DDA announcement only allows for a single bridge day before the DDA comes into force.

In the second case, the announcement of the DDA being lifted, there is a potential postponement effect: Some households that would have driven on a DDA day now shift driving activities backwards. This allows them to benefit from the higher intrinsic utility of driving on a non-DDA day tomorrow rather than driving on a DDA day today. The presence of a postponement effect affects traffic volumes both on the day ending the DDA and on the first non-DDA day.

Hypothesis 3 *There is a postponement effect of lifting a DDA such that traffic is lower on the n-th day of a DDA if that day precedes the lifting of the DDA – and higher on a non-DDA day if that day is the first day following a DDA. Households' planning horizons allow a share of driving activities to be shifted from a current DDA day to the following non-DDA day so as to benefit from a higher net utility of driving on a non-DDA day.*

Hypotheses 2 and 3 summarize our predictions of change in traffic during the transition from a non-DDA to a DDA phase and vice versa, driven by intertemporal substitution once the uncertainty of whether a transition will take place has been resolved. Hypothesis 4 completes the analysis with a focus on the dynamics during the DDA.

For the period during which the DDA is in force and its lifting has not been announced yet, expression 4.5 provides a simple characterization. This characterization suggests a constant level of traffic unless there are changes in the emotional cost of non-compliance with the DDA norm (E).[25] Such changes are consistent with empirical evidence that supports theories of "ego depletion". This depletion process leads to an emotional cost of driving (E) that is highest on the first day of a multi-day DDA event and declines over the duration of the DDA, leading to an increase in traffic.[26]

Hypothesis 4 *There is an ego depletion effect of a continuing DDA such that traffic increases during a multi-day DDA towards non-DDA levels until the lifting of the DDA is announced.*

A corollary of Hypothesis 4 is that – since ego depletion requires a recovery time – drivers are predicted to be less responsive to a DDA after shorter recovery periods between DDA events.

Together, hypothesese 1 through 4 emphasize three aspects. One is that policy-makers can rationalize their belief in the effectiveness of DDAs: Invoking the norm-setting effects of DDAs in a behaviorally informed model provides a causal mechanism for affecting the choice whether to drive or not. The second is that the predicted equilibrium car traffic density under a DDA is below non-DDA levels.

The third aspect is that the dynamic patterns of driving choices within and between multi-day DDA events make specific empirical predictions: Following the announcement of a DDA, traffic volumes first increase on the bridge day due to an anticipation effect before dropping on the first DDA day. Traffic then recovers through ego depletion, before the postponement effect induces a drop on the last DDA day and a surge of traffic on the first non-DDA day. The reduction in traffic due to the DDA is expected to be negatively affected when DDA events are spaced closely together.

[25] Additional factors could be changes either in the policy variable δ or in the psychological variables of warm glow (G). For the first, there is no corresponding data in our empirical context. For the second, we are not aware of established theories of the dynamics of warm glow.

[26] This can be seen in expression 4.5 by differentiating traffic Q^D with respect to (falling) emotional cost $-E$: $-\frac{dQ^D}{dE} = \frac{G+p^{ND}\delta}{(\tau t^D - E)^2} > 0$.

While the framework is good at capturing the moral appeal considerations of policy-makers, it probably does injustice to their understanding of the complexity of driving decisions. For example, it neglects issues of expectations and learning that are likely to be particularly important during early phases of the DDA program as car owners closely observe traffic densities. It also neglects health-related aspects of driving decisions (Cutter and Neidell, 2009; Sexton, 2012) and the congestibility of public transit (Basso and Silva, 2014). These complexities can be expected to impact on the success of DDAs – and to be part of the ex-ante assessment undertaken by policy-makers in a more or less systematic fashion.

4.4 Data

4.4.1 Traffic Data

We obtain hourly vehicle traffic counts for the five PM seasons from January 2016 through December 2019 for 46 automatic traffic counters (ATCs) operated by the City of Stuttgart's Integrated Traffic Control Center (*Integrierte Verkehrsleitzentral*, IVLZ) and for 30 ATCs from the Federal Highway Research Institute (*Bundesanstalt für Strassenwesen*, BaSt) located in Stuttgart and Munich. Although we also acquire data from January 2020 through April 2020, we exclude this from our analysis due to the unprecedented effect of COVID-19 lockdowns on mobility and the city's announcement in January 2020 that the DDA program would conclude after the 2019-2020 PM season.

In our dataset, daily counter-level traffic flows are only recorded as the sum of twenty-four hourly counts if data are available for all 24 hours of a day, otherwise they are recorded as missing. Of 55,708 possible counter-day observations spanning 76 counters and 733 particulate matter season days, we ultimately observe 39,403 vehicles per counter-day observations (70.7% of all possible counter-days). Although the share of missing data is considerable for some ATCs, we do not believe that there is a systematic pattern of missing data that would affect our empirical analysis.

On average, 18,238 vehicles pass each Stuttgart ATC each day, with traffic increasing moderately (+6.5%) over the course of the work week before dropping

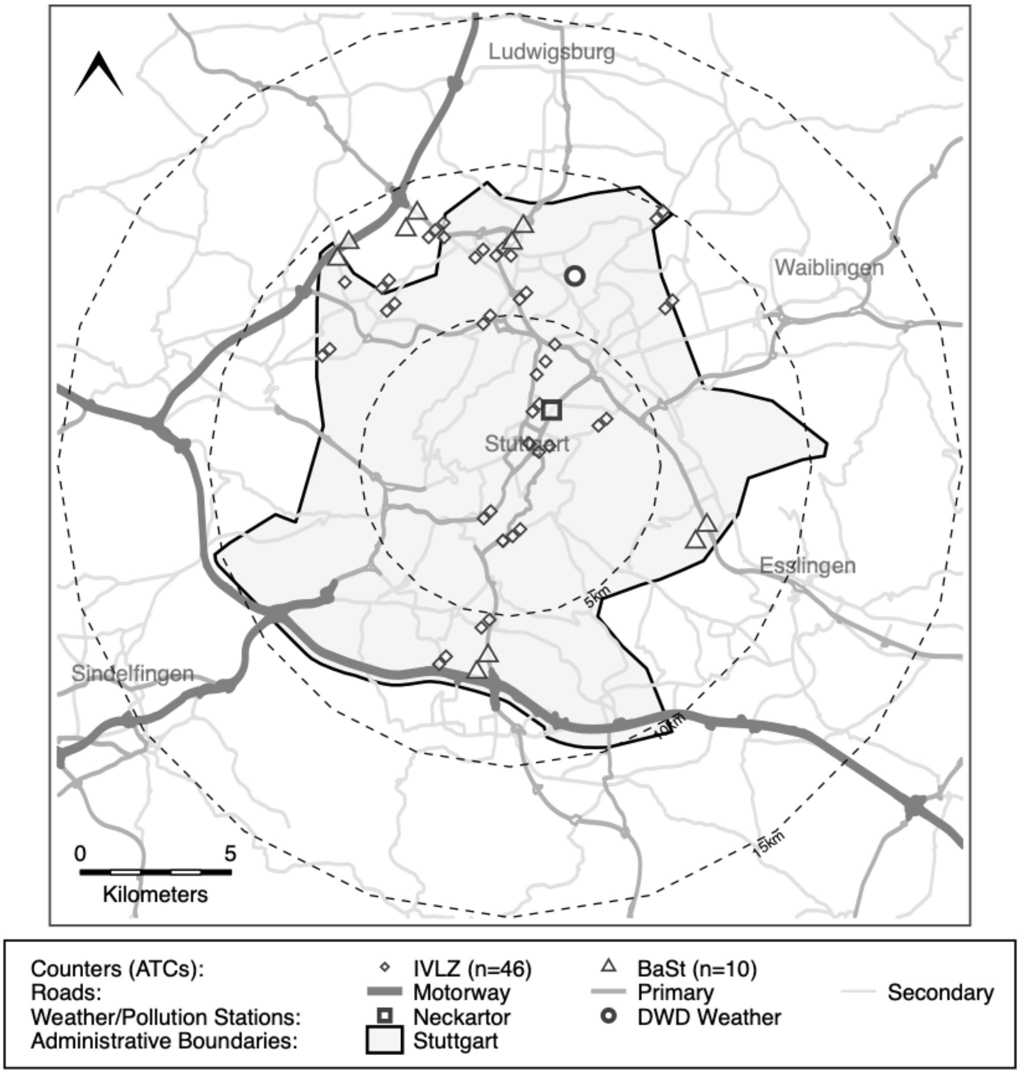

Figure 4.4.1: Stuttgart with automatic traffic counters (ATCs) by data source, road network, Schnarrenberg DWD weather station, and Neckartor AQA trigger monitor. Map generated with data from OpenStreetMap, IVLZ, BaSt, BKG, LUBW, and DWD.

Table 4.4.1: Summary Statistics: Vehicles per Day by Counter Group and Day-of-the-Week

	Mon	Tue	Wed	Thu	Fri	Sat	Sun	Overall
Stuttgart, <10km, n=56								
Mean	18,889.0	19,232	19,743.7	20,142.8	20,119.7	16,156.6	13,274.3	18,238.0
Std. Dev.	14,879.6	15,229	15,469.0	15,709.0	15,862.4	12,840.2	11,191.5	14,745.2
Obs.	3,992.0	3,982	3,949.0	3,954.0	3,953.0	3,975.0	3,868.0	27,673.0
Min	37.0	56	51.0	75.0	47.0	59.0	17.0	17.0
Max	66,072.0	67,423	69,957.0	75,021.0	76,671.0	60,536.0	58,609.0	76,671.0
Munich, <10km, n=20								
Mean	45,283.2	45,652	47,134.7	48,382.0	48,223.6	37,981.4	32,709.4	43,609.8
Std. Dev.	15,477.2	15,632	15,891.2	16,041.7	16,657.0	12,730.1	11,739.7	15,964.0
Obs.	1,676.0	1,680	1,664.0	1,664.0	1,682.0	1,682.0	1,682.0	11,730.0
Min	9,720.0	3,620	5,367.0	5,924.0	6,819.0	5,574.0	4,044.0	3,620.0
Max	77,810.0	81,062	82,570.0	88,091.0	87,469.0	87,787.0	61,615.0	88,091.0

off on Saturdays (-14.5%) and more considerably on Sundays (-30%) relative to Mondays. Public and school holidays also have considerably lower traffic levels (-18.4%) compared to non-holidays. Traffic flows are also subject to daily shocks (e.g. accidents, congestion), weekly and monthly variation (e.g. short-term construction sites, traffic re-routing), seasonality, and long-term shifts in road usage (e.g. vehicle bans, road closures, new road infrastructure, transit alternatives, macroeconomic shocks).

Figure 4.4.1 maps Stuttgart ATCs and categorizes them into "center" counters located within five kilometers of Stuttgart's administrative centroid (n=19) and "periphery" counters located five kilometers to ten kilometers from the centroid (n=37). The inner five kilometer radius proxies for the AQA's target region in the city center, which is located at the middle of a basin and contains the Neckartor alert trigger PM monitor. The periphery counter group from five kilometers to ten kilometers contains many of the closest park-and-ride locations to the city center, where, on DDA days, car commuters can take subsidized public transit for the final leg of their commute to reach the city center. Periphery traffic flows are considerably higher (20,002 vehicles per counter-day) than city center traffic flows (14,465 vehicles per counter-day).

The properties of our traffic data limit the scope of our analysis in three ways. First, we observe aggregate traffic counts per ATC and cannot identify individual intensive and extensive driving margins. That is, we cannot decipher between a relatively small set of automobiles on the road being driven more intensively (i.e. high daily vehicle kilometers traveled per car) and a proportionally larger set of automobiles being driven relatively less intensively (i.e. fewer daily vehicle kilometers traveled per car). Second, we are not able to observe individual-level modal switching. That means we can only assess the DDA's impact on driving reductions but not on the AQA's other recommended behaviors like using public transit or cycling.[27] Third, our data set consists of traffic flows for a small subset of all streets in Stuttgart, but we do not believe this is a relevant limitation to our data. The 56 Stuttgart ATCs we use in our analysis are distributed across 28 sites, which we believe are representative of overall traffic conditions in Greater Stuttgart as they are dispersed across diverse road types, along key traffic arteries, and in different cardinal directions from the city center.

4.4.2 Weather, Pollution, and DDA Status

We follow the existing AQA literature to control for daily weather factors which may influence driving and AQA activation such as temperature, precipitation by type, and wind speed. We retrieve weather data for the Schnarrenberg weather station from DWD Open Data (see location in figure 4.4.1). Local authorities use atmospheric data from this weather station to evaluate the AQA conditions, so we believe it is most relevant when controlling for program determinants. Furthermore, this weather station is located centrally in Stuttgart, so we assume that weather conditions there are the best available measure of meteorological factors that might influence commuters.[28] Air pollution data come from the Baden-Württemberg State Institute for the Environment, Survey and Nature Conservation

[27]We have inquired at the city of Stuttgart and its public transportation partners about alternative transit records. The city nor its public transportation partners maintain turnstiles at public transit stations that would deliver daily public transit statistics. Available overall monthly ticket sales do not have the temporal or spatial resolution necessary for our analysis. The city does track daily cycling counts at two automatic bicycle counters over the time period of interest, and this data could be exploited in a future extension of our analysis.

[28]We could merge weather data from the Stuttgart airport weather station at the city's southern periphery to ATCs located close to it, but we believe weather conditions at the Schnarrenberg weather station in Stuttgart are most indicative of commuters' expectations about the city center. Moreover, we don't expect that accounting for differences in local weather variation over such short distances (i.e. less than fifteen kilometers) would significantly affect our results.

Table 4.4.2: Summary Statistics: Covariates by City and Stuttgart DDA Status

	Stuttgart			Munich			Difference	
	(1) DDA Mean	(2) No DDA Mean	(3) t-Test (2)-(1)	(4) DDA Mean	(5) No DDA Mean	(6) t-Test (5)-(4)	(7) t-Test (1)-(4)	(8) t-Test (2)-(5)
Temperature ($°C$)	4.38	6.20	1.83***	3.47	5.58	2.11***	0.91	0.62*
Rainfall (mm)	0.18	1.92	1.74***	0.29	2.41	2.13***	-0.11	-0.50*
Snowfall (mm)	0.00	0.00	0.00	0.01	0.04	0.03	-0.01	-0.04
Sleet (mm)	0.04	0.33	0.29***	0.06	0.79	0.73***	-0.02	-0.46***
Rel. Humidity (%)	73.93	77.47	3.54***	75.67	76.33	0.66	-1.74	1.14
Sunshine Hours	5.13	2.34	-2.79***	4.90	2.43	-2.47***	0.23	-0.09
Wind (km/h)	2.57	3.37	0.80***	2.33	3.43	1.09***	0.24**	-0.06
PM10 ($\mu g/m^3$)	37.74	19.46	-18.28***	31.24	18.10	-13.14***	6.50***	1.36
Holiday (=1)	0.06	0.28	0.22***	0.06	0.28	0.22***	0.00	0.00
Days	250	483	733	250	483	733	500	966

Notes: Columns 1 and 4 report mean covariates in Stuttgart and Munich, respectively, on days when a don't drive appeal (DDA) has been called in Stuttgart. Columns 2 and 5 report mean covariates in Stuttgart and Munich, respectively, on days when a DDA has not been called in Stuttgart. Columns 3 and 6 report the results of two-sample t-tests comparing differences in means between DDA days and non-DDA days for each city. Columns 7 and 8 report the results of two-sample t-tests comparing differences in means between Stuttgart and Munich covariates on DDA days and non-DDA days, respectively. Significance level: * = p <0.05, ** = p <0.01, and *** = p <0.001

(*Landesanstalt für Umwelt Baden-Württemberg*, LUBW), which monitors PM_{10} concentrations in the city center (see location in figure 4.4.1). We perfectly observe DDA status and manually input it from an official Stuttgart website as a binary variable that equals one on days when a DDA is called and zero otherwise (figure 4.4.2).

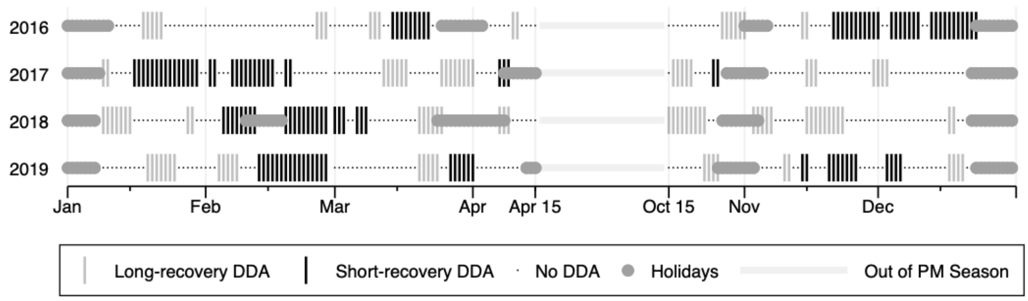

Figure 4.4.2: DDA days split by median recovery time (9 days) from January 2016 through December 2019.

In comparison to other DDAs and AQAs studied in the literature, Stuttgart's DDA is implemented very frequently and for long durations.[29] Over 733 possible AQA days from January 2016 through December 2019, Stuttgart authorities broadcast a DDA on 250 days (34%) in 44 multi-day DDA events with an average duration of 5.7 days. Table 4.4.2 shows that DDA days are, on average, colder, less windy, less humid, sunnier, and more polluted in Stuttgart than non-DDA days, which is in line with the AQA design. DDA days also experience less non-snow precipitation (i.e. rain or sleet) and fewer heavy non-snow precipitation events. DDA days are typically preceded by days with similar weather and pollution levels, while the same holds for non-DDA days. Authorities are also less likely to call DDAs on public and school holidays, possibly due to lower expected traffic levels on these days. Figure 4.4.2 shows that few DDAs fall on public or school holidays (14 DDA days during 149 holidays, 9.4%) compared to non-holidays (236 DDA days during 584 non-holidays, 40.4%). For this reason, we believe that local authorities may

[29]For example, in Cutter and Neidell (2009) about 4.5% of days in San Francisco, USA are treated with an *Spare the Air* alert, in Saberian et al. (2017) about 1.3% of days in Sydney, Australia experience an ozone alert day, and in Tribby et al. (2013) about 16% of PM season days have either a yellow or red AQA. In Sexton (2012), the likelihood of two consecutive *Spare the Air* days is just 0.6%.

systematically treat holidays differently than non-holidays, so we remove public and school holidays from parts of our analysis. Table 4.A.1 shows that authorities often announce DDAs on weekends and at the beginning of the week, leading a large share of DDAs to start on Mondays, Tuesdays, and Wednesdays. DDAs most frequently end on Saturdays (33%, 12 of 36 possible days). Overall, there is a fairly uniform distribution of DDA days across the working week with weekends being treated with DDAs less often than weekdays.

4.5 Empirical Framework

4.5.1 OLS Estimation

We begin by estimating the impact of Stuttgart's DDA on traffic levels using an ordinary least squares (OLS) regression model described by the following equation:

$$log(y_{i,t}) = \beta_1 DDA_t + \delta_1 M_t + \gamma_i + \phi_t + \epsilon_{i,t}, \tag{4.6}$$

where $y_{i,t}$ is the number of vehicles passing counter i on date t, and β_1 estimates the DDA effect as the percent difference in daily traffic counts between DDA days and non-DDA days.[30] The variable of interest, DDA_t, is a binary variable that takes on a value of one on DDA days and zero otherwise. We include weather controls (M_t) to account for same-day weather conditions in Stuttgart.[31] Counter-level fixed effects (γ_i) account for counter-specific traffic levels and year-month time fixed effects (ϕ_t) flexibly capture trends and temporal discontinuities that might influence overall car use from month-to-month (e.g. construction, varying public transit prices, vehicle bans, new transit infrastructure, etc.). We also include day-of-the-week dummies to account for weekly traffic cycles and holiday dummies to capture changes in traffic levels during holidays and vacation periods.

Our estimation equation tests the null hypothesis that the DDA effect is equal

[30] Log-scaling the outcome variable leads the coefficient of interest to approximately estimate a percentage change rather than an absolute change in levels.

[31] We follow the literature on air quality alerts and transportation choice in including precipitation, temperature, wind speed as control variables. In addition to absolute precipitation by type, we also include squared terms for rainfall (mm^2), snowfall (mm^2), and sleet (mm^2).

to zero ($H_0 : \beta_1 = 0$), or, in other words, that traffic flows in Stuttgart do not differ significantly on days when a DDA is broadcast. If, as intended, car use decreases on DDA days, the DDA coefficient must be negative ($\beta_1 < 0$) and differ significantly from zero. In our setting, traffic counts are correlated over time[32] and across ATCs.[33] In the regression model defined by equation (4.6), we employ heteoscedasticity-robust Huber-White standard errors and adjust for serial correlation and cross-sectional dependence by clustering standard errors at the counter level. As we explain in section 4.5.2, numerous factors could plausibly bias these OLS point estimates, so we caution against interpreting regression results from equation 4.6 as causal estimates. We describe these identification challenges in the following subsection and turn to a difference-in-difference (DiD) estimation strategy in section 4.5.3.

4.5.2 Identification Challenges

The non-random assignment of DDA treatment from day to day in Stuttgart presumably biases our OLS estimates for several reasons. First, DDAs are broadcast based on a set of multi-day atmospheric and pollution determinants, meaning that DDA days are a non-random selection of days that are colder, less windy, less humid, sunnier, and more polluted than non-DDA days as shown in table 4.4.2. Unlike previously-studied AQAs (Cutter and Neidell, 2009; Noonan, 2014), Stuttgart DDA treatment is also not determined by a single contemporaneous atmospheric parameter (e.g. a pollution threshold value which may be imperceptible at the margin). Furthermore, treatment conditions must be satisfied for a prolonged period to activate, and, when activated, treatment remains in effect for at least two days independent of how treatment conditions actually develop.[34] Meteoro-

[32] We would like to test for serial correlation but the gaps in our dataset and the unbalanced nature of our panel prevent us from successfully running common STATA commands like xtserial, xtqptest, and xtistest. It is unclear how to appropriately test for this given our dataset.

[33] We implement a CD-test for cross-sectional dependance (Pesaran, 2020) in the outcome variable, $y_{i,t}$, and reject the null hypothesis of cross-sectional independence (p-value < 0.001).

[34] The AQA program design complicates identification via a standard regression discontinuity design as in Cutter and Neidell (2009) or Noonan (2014). For DDA treatment to activate, multiple atmospheric thresholds must be fulfilled simultaneously and multiple pathways exist (see section 4.2). Hence, there is no single cut-off point we could exploit as a policy disconti-

logical treatment determinants presumably also directly influence transportation demand and could thereby confound DDA effect estimates. In particular, persistent weather conditions, which are endogenous to the treatment protocol, may correlate with car trip demand and modal switching. For example, some motorists may be more likely to naturally choose transit alternatives such as public transit or cycling during prolonged dry, sunny weather, while such conditions also increase the likelihood that a DDA is called, potentially biasing our DDA effect estimates downward. Selecting a meaningful control group of untreated multi-day events with similar weather patterns would best isolate the DDA effect, but the small number of in-sample control days during the policy implementation period limits the statistical power of such an approach. Instead, we control for these factors in our OLS regressions by including same-day covariates, adding first-day and second-day lagged meteorological variables, and flexibly controlling for trends over time with monthly and weekly fixed effects.

Air pollution may also confound our OLS estimates through similar channels. For example, even in the absence of a DDA program, high pollution levels may induce some motorists to naturally avoid pollution and change car trip demand. It is not clear ex ante which strategies individuals in Stuttgart might employ to reduce pollution exposure and how this affects car trip demand, but causal DDA treatment estimates would need to disentangle behavioral responses to high pollution from those due to an active DDA. We note that a considerable share of untreated days have high pollution levels (due to the no rainfall conditions in the treatment protocol) and use controls for contemporaneous pollution levels and their lags in our OLS regressions to account for the net effect of pollution avoidance strategies on car trip demand independent of DDA treatment status.[35]

Another source of bias arises if local authorities' expectations about car trip

nuity. Other feasible identification strategies in our setting include: i) synthetic control with never-treated German cities, ii) difference-in-difference comparison with pre-program (i.e. pre-2016) traffic in Stuttgart during multi-day periods that fulfilled the meteorological conditions, or iii) exploiting alert designation errors (e.g. false positives, false negatives).

[35]Pollution avoidance strategies can plausibly affect overall car trip demand. For example, if a significant share of individuals respond to high pollution levels by staying at home indoors and abstaining from travel, car trip demand would fall. Alternatively, if a substantial number of commuters drive more in cars to protect themselves (with filtered, recirculated air) rather than walking and using public transit outdoors or drive out of the city to avoid pollution, car trip demand would increase.

demand affect their decision whether to call a DDA or not. Local authorities have some discretion when evaluating AQA conditions and could decide not to call an alert if they don't see it as worthwhile, even though the alert conditions are technically satisfied. For example, in section 4.4 we demonstrate that local authorities are less likely to broadcast a DDA during school or public holidays presumably because they already anticipate low traffic levels on these days. If authorities systematically under-assign DDAs on days with lower traffic levels, and we do not account for this, our OLS estimates would be biased upward. But, we cannot observe all of the factors leading authorities to diverge from the AQA design rules or policy-makers' traffic expectations, so we prefer OLS specifications that remove periods with traffic outliers such as holidays and weekends.

Reverse causality between the outcome of interest, car trip demand, and DDA treatment status could also plausibly threaten the internal validity of our OLS estimates. A larger or smaller number of cars driving in Stuttgart could, by increasing or decreasing total vehicle emissions, cause PM_{10} levels to rise or fall relative to the DDA's 30 $\mu g/m^3$ primary sub-condition threshold and switch the DDA on or off. However, we note that car trip demand, and thereby its subsequent effect on pollution, has no influence over the necessary second sub-condition of primary DDA condition 1, namely whether rainfall is anticipated or not, nor over the remaining five atmospheric conditions which can activate DDA treatment independent of PM_{10} pollution levels. Considering this embedded exogeneity in the DDA treatment protocol, previous findings of moderate to negligible impacts of DDAs on driving in other settings, and looking ahead to the magnitude of our estimates presented in section 4.6, we believe it is improbable that marginal changes to car trip demand cause treatment status to change.

Finally, the announcement of an upcoming DDA may change motorists' choices until the DDA actually takes effect (i.e. on or preceding the issue or bridge day) or after the end of a DDA (i.e. on the first or second recovery day). We cannot observe whether individual motorists take additional trips on issue and bridge days or on the first recovery days after a DDA to avoid taking trips during the DDA, but such a scenario would bias our overall DDA effect estimates downward. We account for these anticipatory and posttreatment effects by removing issue, bridge, the first two recovery days from our sample in some OLS specifications and inspecting for parallel time trends in the following section.

4.5.3 Difference-in-Difference Estimation

To recover the causal effect of DDAs on car trip demand, we rely on a difference-in-difference (DiD) approach that uses ATCs in the metropolitan city of Munich in the neighboring state of Bavaria as a never-treated control group for Stuttgart ATCs. Compared to OLS estimation, the main advantage of this approach is that we can account for day-to-day variation in car trip demand driven by unobservable factors common to Munich and Stuttgart. We believe that Munich is an appropriate comparison for Stuttgart in our setting because it has a similarly sized metropolitan population, pollution routinely exceeds annual EU air quality limits, and it has a similarly dense public transit network, but it never implemented a DDA program. Munich is located over 160 kilometers from Stuttgart, minimizing the likelihood of treatment spillovers from Stuttgart in violation of the stable unit treatment variable assumption (SUTVA). We compare traffic at ATCs within ten kilometers from the Stuttgart geographic centroid with a control group of never-treated ATCs located within ten kilometers of the Munich geographic centroid. This enables us to shed light on treatment effect heterogeneity over DDA event time and recovery duration.[36]

A key assumption for successfully identifying causal DiD effects is that Munich is a meaningful treatment counterfactual for Stuttgart or, in other words, that traffic trends in Stuttgart develop in parallel with Munich would the DDA policy not be implemented (Angrist and Pischke, 2009). To test this, we begin by visually inspecting figure 4.5.1, which plot trends in mean logged unconditional traffic levels in the two cities on the same calendar days averaged over Stuttgart DDA event time. In the pre-DDA window in figure 4.5.1, which spans from -6+ days to -1 day before DDA activation, mean traffic levels in both Stuttgart and Munich trend downward at a similar rate from five days before AQA activation (event time: -5) through the bridge day (event time: -1).

[36] Previous research on AQAs highlighted some important heterogeneities in alert effectiveness. For example, Tribby et al. (2013) find evidence of spatial displacement effects where traffic increases at Salt Lake City, USA's periphery on alert days and Saberian et al. (2017) and Graff Zivin and Neidell (2009) find evidence of alert fatigue on the second day of ozone alerts.

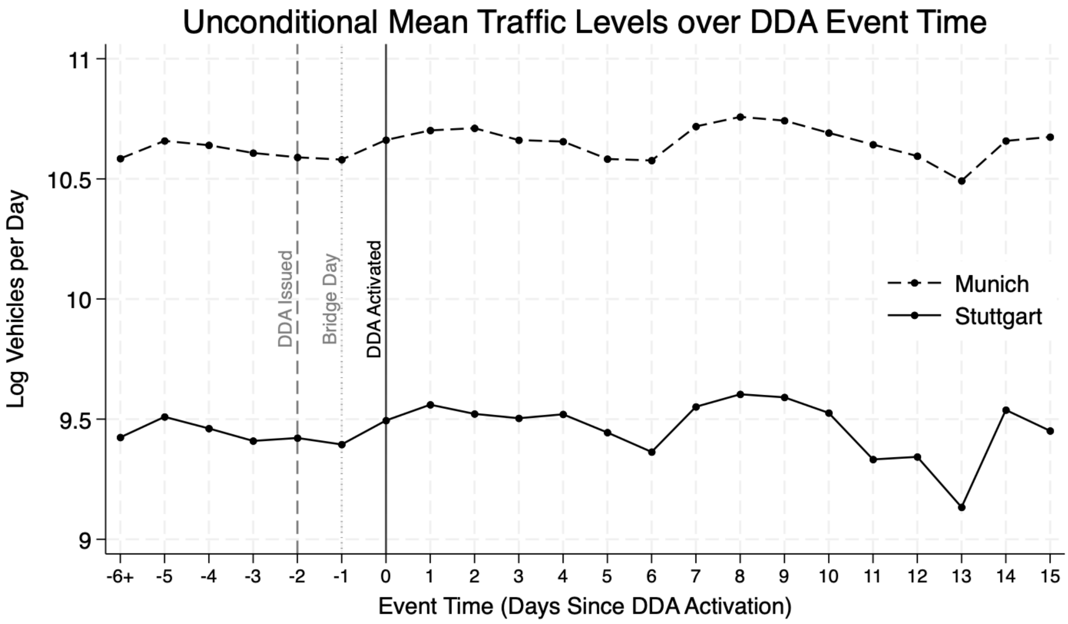

Figure 4.5.1: This figure plots mean traffic levels across all counters in Stuttgart and Munich within ten kilometers of each city's centroid over don't drive appeal (DDA) event time. The pretreatment period includes up to ten days before the activation day. The treatment period includes all DDA days including those when the end of the DDA event has already been announced.

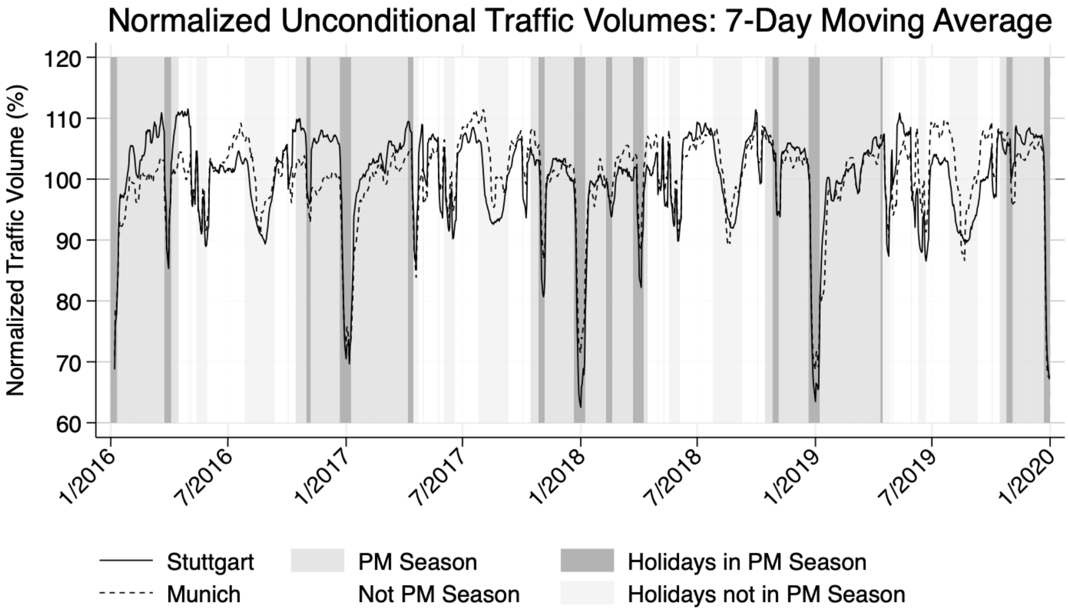

Figure 4.5.2: Seven-day moving average of normalized traffic volumes.

In building our argument for common trends between Stuttgart and Munich, we next refer to figure 4.5.2, which depicts normalized traffic trends in each city.[37] Average traffic patterns in Munich trace Stuttgart traffic patterns very well over time as we believe they capture previously unexplained daily and week-to-week variation in economic activity in Stuttgart. There are strong seasonal trends but most striking are symmetrical drop-offs in and reversions to mean traffic levels during and after holiday periods. While controls for holiday periods and monthly temporal fixed effects would flexibly capture some of this variation and seasonality, there is a concern that dramatic changes in traffic levels, as depicted in figure 4.5.2, might be absorbed into DDA effect estimates if fixed effect are temporally coarse. Calendar date fixed effects in counter-level DiD specifications will resolve this by capturing the day-to-day variation in traffic common to both cities. For the majority of the program period traffic trends in figure 4.5.2 are closely linked, but there is a noticeable spread in our measure of Stuttgart and Munich traffic

[37]We normalize by calculating in percentage terms how much each day each ATC deviates from its own mean traffic level and then averaging this across all ATCs in each city.

levels in the 2016 PM seasons. Counter level fixed effects will help to absorb these differences.

Furthermore, we argue that Munich ATCs are a suitable control group for Stuttgart ATCs because Munich commuters are exposed to very similar same-day meteorological and pollution conditions. Table 4.4.2 in section 4.4 includes summary statistics for these covariates and two-sample t-tests for differences between the two cities. Column 7 shows that there are no statistically detectable differences in temperature, rainfall, snowfall, sleet, relative humidity, or sunshine hours between the two cities on days when a DDA is called in Stuttgart. Munich is, however, somewhat less windy and exposed to about 6.5 fewer $\mu g/m^3$ of PM_{10}, on average during DDAs. Figure 4.5.3 sheds further light on trends in PM_{10} pollution over DDA event time. Pollution develops in parallel in both cities in the pre-treatment period (event time: -6+ to -1), but on the day that the DDA takes effect, PM_{10} levels in Stuttgart jump nearly 9 $\mu g/m^3$ higher than in Munich. Presumably, this is linked to Stuttgart's geographic position in a bowl-shaped valley that better traps pollution than Munich's morphology. However, mean pollution levels remarkably evolve in the same fashion in Stuttgart and Munich over event time. Ultimately, differences in pollution levels don't appear to be substantial, and controlling for same-days differences should address concerns about comparability.

To build our DiD models, we rely on a standard two-way fixed effects (TWFE) specification as a baseline:[38]

$$log(y_{i,t}) = \beta_1 DDA_t \times Treated_i + \gamma_i + \phi_t + \eta \mathbf{X}_{i,t} + \epsilon_{i,t}, \tag{4.7}$$

where DDA_t is a dummy variable that designates whether a DDA has been activated on that day, $Treated_i$ is a dummy variable for whether the ATC belongs to

[38]The recent TWFE literature has identified shortcomings to TWFE estimators in treatment settings that extend beyond the classic two period and two group setup (Roth et al., 2023). Our setting has the canonical two main groups but many pretreatment and posttreatment periods, repeated treatments of varying duration, and plausible heterogeneous treatment effects between treated units (i.e. between Stuttgart ATCs). However, there are no variations in treatment timing between units, which mitigates the concern that our point estimates may be contaminated by effects from other time periods (Sun and Abraham, 2021). Moreover, our control group of Munich ATCs remains never-treated throughout. Because there is currently no estimator tailored to our empirical setting, we carefully select a control group, demonstrate parallel trends, and check for anticipation effects.

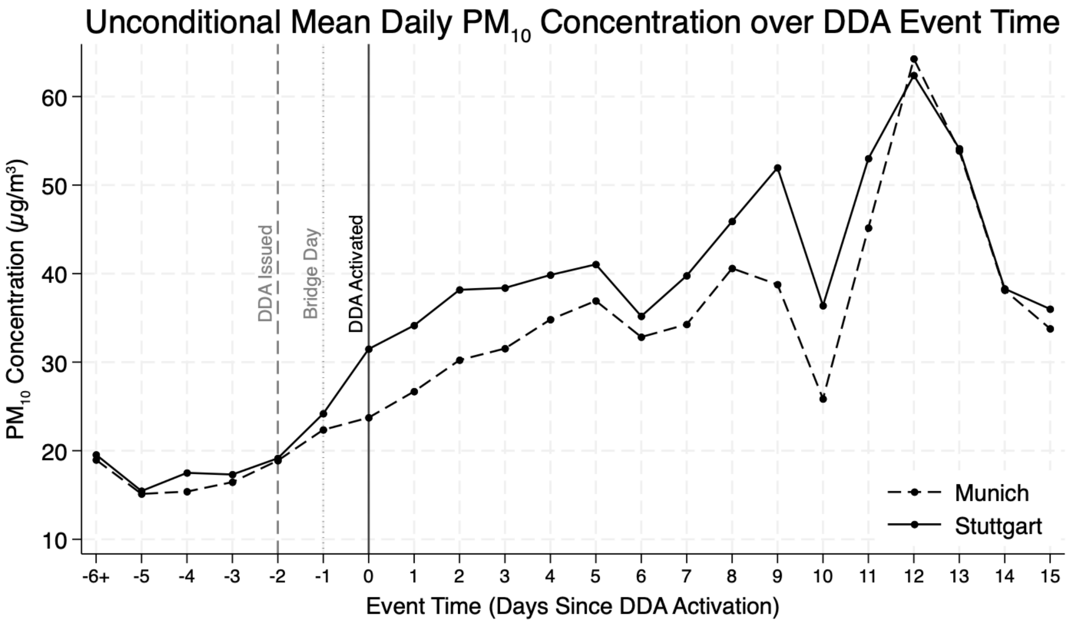

Figure 4.5.3: This figure plots mean daily PM_{10} pollution concentrations across all pollution monitors in Stuttgart and Munich over don't drive appeal (DDA) event time. The pretreatment period includes up to ten days before the activation day. The treatment period includes all DDA days including those when the end of the DDA event has already been announced.

the treatment group (e.g. within ten kilometers of the Stuttgart city centroid), γ_i are ATC fixed effects, ϕ_t date fixed effects, and $\mathbf{X}_{i,t}$ a vector of city-specific weather and pollution controls and lags. β_1 estimates the average percentage difference in traffic levels between treated and untreated ATCs.

In a subsequent specification, we spatially disaggregate our DDA effect estimates by interacting the $DDA_t \times Treated_i$ with counter-specific dummy variables (ATC_i) to estimate counter-specific DDA effects. We then create binary variables for DDA event time and between event recovery time and interact these with $Treated_i$ to examine temporal heterogeneity in DDA effectiveness.

Dynamic Treatment Effect Estimation

We create D_j, a set of dummy variables corresponding to event time days over the duration of each DDA event ranging from ten days before the activation day through the last DDA day. Calendar days that are not within these windows are removed from this part of the analysis. We group pretreatment days more than six days before the issue day (day -6+, j = -6) and posttreatment days on the fifth or later day of a DDA (day 5+, j = 5).[39] Accordingly, we replace the DDA_t variable in equation 4.7 with D_j and test for DDA effect dynamics over event time by estimating the following regression equation:

$$log(y_{i,t}) = \sum_{j \in -6,\ldots,0,\ldots,5} \beta_j D_{i,t-j} \times Treated_i + \gamma_i + \phi_t + \eta \mathbf{X}_{i,t} + \epsilon_{i,t}, \tag{4.8}$$

where each β_j corresponds to the percent change in traffic between treated and untreated ATCs on event time day j. Equation 4.8 include a vector of city-specific weather and pollution covariates (X_t) to control for differences in same-day weather conditions.

Dynamic Posttreatment Effect Estimation

We similarly employ a set of D_k dummy variables to test for posttreatment effect dynamics when the DDA is terminated. This model is described by:

[39] For our regressions, we consider the day before the issue day ($j = -3$) as the baseline.

$$log(y_{i,t}) = \sum_{j \in -5,...,0,....,2} \beta_k D_{i,t-k} \times Treated_i + \gamma_i + \phi_t + \eta \mathbf{X}_{i,t} + \epsilon_{i,t}, \tag{4.9}$$

and replaces D_j in equation 4.8 with D_k, which is relative to the first posttreatment day without an alert. We consider an event time window from five or more days before day the DDA is lifted (day -5+, k=-5) including only treated days to two or more days after the DDA is no longer in effect (day 2, k=2) and no new DDA has been announced. Two transition days (day -2 and day -1, k=-2 and k=-1, respectively) when the DDA is still in effect but it has been announced that it will be lifted precede the first post-DDA day (day 0, k=0).[40]

Recovery Time Effect Estimation

As depicted in figure 4.4.2, we split DDA events into those with less than a median recovery time since the preceding DDA (short-recovery: < 9 days) and DDA events with a greater than median recovery time (long-recovery: ≥ 9 days). For each DDA event, we construct a window around each DDA activation day that spans up to eight days before the activation day until the last DDA day.[41] We then generate a binary variable, $Recovery_l$, corresponding to whether each calendar date is part of a short recovery DDA window ($Recovery_l = 0$) or a long recovery DDA window ($Recovery_l = 1$). Days that are not in a short or long DDA window are omitted from the analysis. In a triple-difference specification, we fully interact the temporal DDA treatment term (DDA_t), the Stuttgart vs. Munich treatment group term ($Treated_i$), and the recovery split term ($Recovery_l$) as described by the following equation:

[40]The day before the issue day ($j = -3$) is the baseline in our regressions.

[41]Preceding DDAs take precedence, so the pretreatment period is smaller for DDAs with shorter recovery periods. Ultimately, DDA event windows include for long recovery DDAs a total of 277 calendar days and for short recovery DDAs 226 calendar days.

$$
\begin{aligned}
log(y_{i,t}) = \beta_1 DDA_t + \beta_2 Treated_i + \beta_3 Recovery_l + \beta_4 Recovery_1 \times Treated_i \\
+\beta_5 Recovery_1 \times DDA_t + \beta_6 Treated_i \times DDA_t \\
+\beta_7 Recovery_1 \times Treated_i \times DDA_t + \gamma_i + \phi_t + \eta \mathbf{X}_{i,t} + \epsilon_{i,t}.
\end{aligned}
\tag{4.10}
$$

All remaining regressors from equation 4.7 are also included. To test for spatial heterogeneity in recovery effects, we also separately estimate equation 4.10 for i) city center ATCs within five kilometers from the Stuttgart centroid and ii) periphery ATCs located between five kilometers and ten kilometers from the Stuttgart centroid.[42]

4.6 Results

4.6.1 Overall DDA Effect

Our regression results show that the overall DDA effect is of small to negligible magnitude across a variety of model specifications. Table 4.6.1 displays DDA effect point estimates for different specifications of three different regression models: 1) OLS with year-month and counter fixed effects, 2) OLS with a full interaction of year-month and counter fixed effects, and 3) a TWFE DiD specification with calendar date and counter fixed effects that uses Munich ATCs as a never-treated control group. For each model, we display results for the full sample of 733 days and then restrict the sample to 331 normal, non-holiday weekdays excluding DDA termination transition days.

Across all models, DDAs affect Stuttgart traffic levels by -0.7% to +1.5% and some specifications estimate a statistically significant overall effect at odds with DDA goals. In column 4, our most rigorous specification within OLS estimates that the DDA leads to a traffic decreases by about 0.7% on DDA days, or about 128 fewer vehicles per counter-day. This estimate is statistically significant at the 5% significance level. Our main causal estimate is a DiD model that uses Munich

[42] In both regressions, we consider all Munich ATCs as the control group because only two Munich ATCs are within five kilometers of the city center.

Table 4.6.1: OLS and DiD Regression Results: Overall DDA Effect on Traffic

	(1) OLS b/se	(2) OLS b/se	(3) OLS b/se	(4) OLS b/se	(5) DiD b/se	(6) DiD b/se
DDA	0.0123***	-0.0047*	0.0076***	-0.0068**	0.0155*	0.0018
	(0.0027)	(0.0027)	(0.0022)	(0.0026)	(0.0082)	(0.0065)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Lags	Yes	Yes	Yes	Yes	Yes	Yes
Restricted Sample	No	Yes	No	Yes	No	Yes
Time FE	YM	YM	YMxATC	YMxATC	Date	Date
Unit FE	ATC	ATC	ATC	ATC	ATC	ATC
# ATCs	56	56	56	56	76	76
Observations	26,103	11,878	26,094	11,868	37,171	16,906

Note: Dependent variable is logged vehicles per counter-day. Controls include contemporaneous weather and pollution variables. Lags are two days of lagged weather and pollution covariates. Regressions either include all PM season days or non-holiday weekdays excluding DDA transition days. Unit fixed effects are always at the automatic traffic counter level. Time fixed effects are either year-month ("YM"), year-month by ATC ("YMxATC"), or calendar date. Standard errors clustered on counter in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

ATCs as an untreated control group, includes date and counter fixed effects, and excludes weekends, holidays, and DDA termination transition days. Unlike our preferred specification within OLS, the coefficient of interest corresponds to a statistically insignificant of negligible magnitude (0.2% increase).

In no specification do we estimate a statistically significant overall traffic reduction stronger than 0.7%. The moderate DDA effect in our main DiD specification and the mixed effects across all specifications suggest that the DDA has a modest to neglible effect on traffic and that it does not substantially reduce overall traffic levels in Stuttgart. In the next steps of our analysis, we extend our DiD approach to test whether these aggregate DDA effect estimates mask differences in DDA effectiveness across geographic space and time.

4.6.2 Spatially Disaggregated DDA Effects

We spatially disaggregate our main DiD specification by including counter-specific interaction terms regression equation 4.7 to estimate the daily DDA effect at each ATC location. To explore spatial heterogeneity in DDA effectiveness, we group the 56 counters relative to distance from the Stuttgart center into: i) 19 center loca-

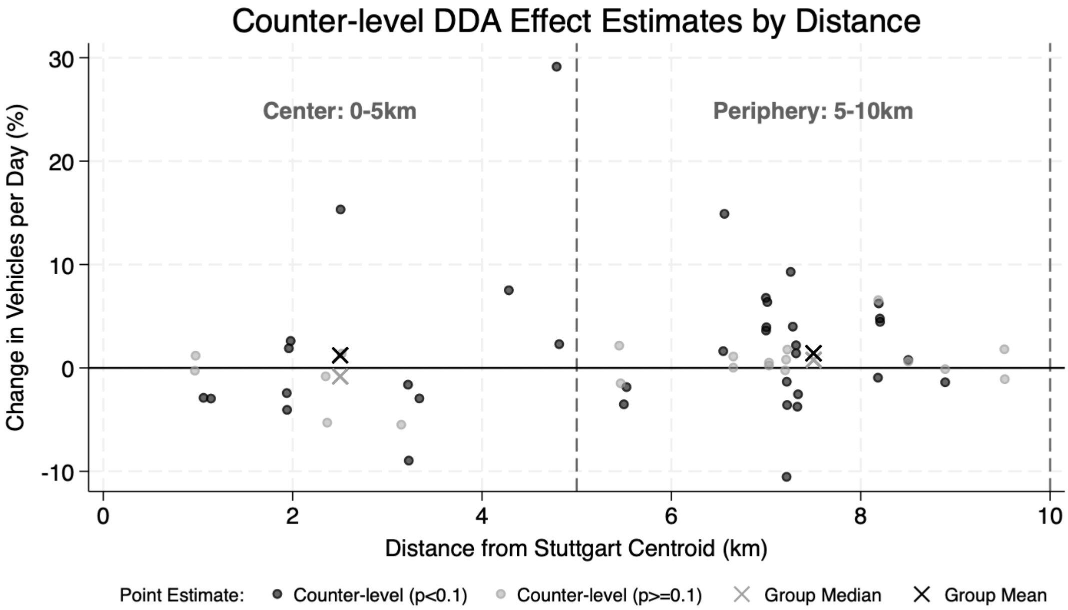

Figure 4.6.1: The figure plots counter-level don't drive appeal (DDA) effect point estimates from our main DiD regression model. Stuttgart counters are split into two groups relative to their distance from the city center: i) center counters located within five kilometers and ii) periphery counters located between five kilometers and ten kilometers. For each group, we plot the group mean and median DDA effect point estimate at the group's distance midpoint.

tions within five kilometers and ii) 37 periphery sites between five kilometers and ten kilometers. Figure 4.6.1 plots individual counter-level DDA effect estimates and group means and median point estimates. With four exceptions, counter-level estimates range from -10% to +10% across all three counter groups, suggesting that local traffic may indeed change substantially on DDA days. Our counter-level estimates show traffic differs statistically at a majority of counters on DDA days. Group mean effects are positive and very similar, but considering that the median center group point estimate is below zero, the DDA may be marginally more effective at reducing city center traffic than traffic at the city's periphery. Moreover, two disproportionately large positive outliers appear to drive the center group mean up. At distances closest to the city center, point estimates seem to reliably be smaller and more often negative. Between five kilometers and ten kilometers, a larger share of ATCs see traffic increases on DDA days, reflected in the positive group median in the periphery counter group.

Figure 4.C.1 in appendix 4.C maps the coefficients plotted in figure 4.6.1 from our main DiD specification. Again, visual inspection suggests that ATCs located closest to the city center are most likely to experience statistically significant traffic reductions on DDA days, while positive DDA effect estimates in the center counter group are primarily located on major roads further from the immediate center. In the periphery counter group, a large share of ATCs are located at the city's northern periphery and suggest that traffic uniformly increases along the northern periphery, while similar results appear to hold at the southern periphery. While our analysis lacks sufficient data on traffic flows at the southwestern and northeastern periphery, we do not have any reason to believe that trends there would differ significantly from the periphery effects we observe at other locations.

These spatially disaggregated results indicate that the DDA policy may heterogeneously affect traffic with respect to distance to the city center. While our analysis does not enable us to pinpoint a mechanism underlying this effect, we believe two factors may play an important role. First, commuters and policy-makers may see reducing traffic levels at the city center, where the AQA trigger monitor is located, as the DDA program's ultimate goal and consider traffic reductions at other locations within the city's administrative boundary as secondary. Second, car commuters who want to adhere to the DDA may respond to this spatial differentiation by minimizing their commuting time with alternative transportation modes. For example, they may drive their cars up to the city's periphery or even slightly within the official boundary, park their vehicles there, take advantage of public transit subsidies, and switch to public transit for the remainder of their commute.

4.6.3 DDA Effect Dynamics

Treatment Effect Dynamics

A plausible mental model of the effects of a DDA on driving decisions hypothesizes possible dynamic effects (see section 4.3). To explore these, we interact DDA event time terms with the DDA treatment variable in equation (4.6) to estimate daily DDA effects over DDA event duration. Figure 4.6.2 displays DDA effect point estimates for each day of a DDA. Once the DDA takes effect, we find that overall Stuttgart traffic levels drop by 2% on the first DDA day (event time: 0) and by 3% on the second and third DDA day (event time: 1 to 2) relative to the counterfactual. We can statistically rule out that the DDA has no impact for each of the first three DDA days at the 95% significance level. On the fourth DDA day and beyond (event time: 3+), the DDA effect drops in magnitude and only the fifth DDA day (event time: 4) remains statistically different from zero. DDA effect estimates after the third DDA day are all still in line with the DDA program's intended reduction of traffic volumes. The point estimate for the sixth day and beyond (event time: 5+) is relatively large in magnitude and has larger confidence intervals. Larger variation in program effectiveness during extended DDAs might be driving this, but our small sample of days at the tail end of longer DDAs prevents us from analyzing this more carefully.

We also note that pretreatment coefficients (days j=-6 to j=-1) do not differ significantly from zero, suggesting that average traffic levels in Stuttgart and Munich develop in parallel on days before DDAs are called. Importantly, our point estimates do not provide any evidence of anticipatory effects, even on issue or bridge days when motorists have been informed of the upcoming DDA but have yet not been asked to reduce driving. These results are largely in line with hypothesis 1 of our theoretical framework and demonstrate that DDAs are most effective

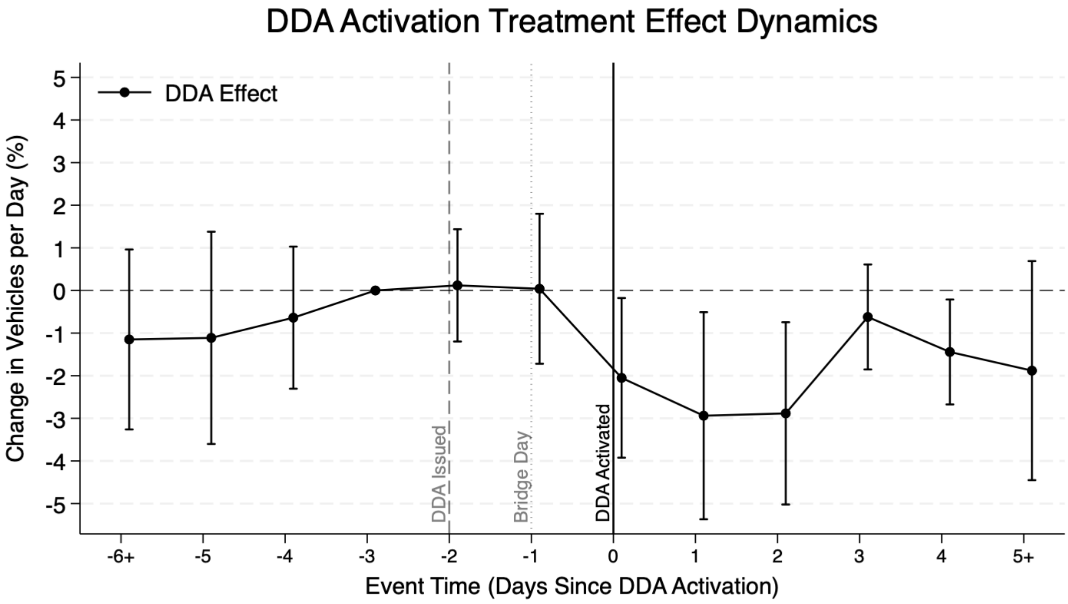

Figure 4.6.2: Dependent variable is logged vehicles per counter-day. Regressions include counter and calendar date fixed effects and local weather and pollution controls. Only days with normal weekday traffic are included. Don't drive appeal (DDA) days when the DDA event end date has already been announced and first two days following the event end date are excluded. Treated group includes 56 counters within ten kilometers from Stuttgart city centroid. Control group includes 20 counters located within ten kilometers from Munich city centroid. Standard errors are clustered on counter and 95% confidence intervals are depicted.

immediately after being activated.

Posttreatment Effect Dynamics

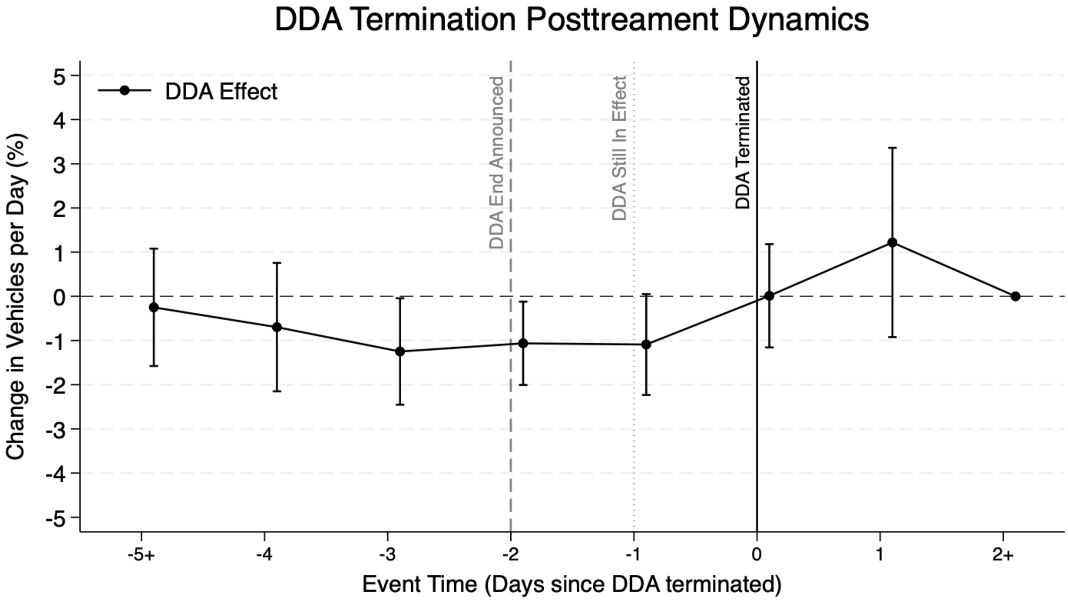

Figure 4.6.3: Dependent variable is logged vehicles per counter-day. Regressions include counter and date fixed effects and local weather and pollution controls. Treated group includes 19 center counters within five kilometers of city center and 37 periphery counters five to ten kilometers from Stuttgart city centroid. Control group includes 20 counters located within ten kilometers of the Munich city centroid. Standard errors are clustered on counter and 95% confidence intervals are depicted.

We also examine how DDA effect estimates change when DDA events are terminated. Figure 4.6.3 shows posttreatment effect estimates. [43] Starting on the third day before the DDA ends, traffic levels drop by about 1% relative to a baseline of

[43]In this post-DDA analysis, we estimate a DDA effect during the treated period (k=-5+ to k=0) of between -0.25% and -1.25%. While the signs on the DDA treatment effect estimates align with the direction of our results from figure 4.6.2, the magnitude of our estimates differs because we look at treatment days relative to the end of the DDA rather than the beginning. In other words, we count backward from the event time day that the DDA is terminated. In the posttreatment analysis a DDA treatment day three days before the DDA is lifted (k=-3) could have originally been on one of many different DDA days relative to the DDA activation day depending on how long the DDA lasted. For example, it could have been on first day of a three day DDA or on the seventh day of a ten day DDA.

two or more days after the DDA is lifted. This effect remains statistically significant for the last two DDA days, including the day when authorities have already announced a DDA end date (always two days before the DDA is actually lifted). Once the DDA is no longer in effect and DDA messaging subsides, traffic levels return to baseline levels and the DDA effect does not differ statistically from zero. These results affirm our prediction from hypothesis 2, which postulates that DDAs are effective when commuters can expect when the DDA will be lifted. On the first posttreatment day, we can rule out changes in traffic above 1% in magnitude at the 95% significance level, while the higher point estimate and wider confidence interval on the second posttreatment day shows that traffic may rebound after the DDA is lifted, providing some suggestive evidence in favor of hypothesis 4.

Recovery Time Effects

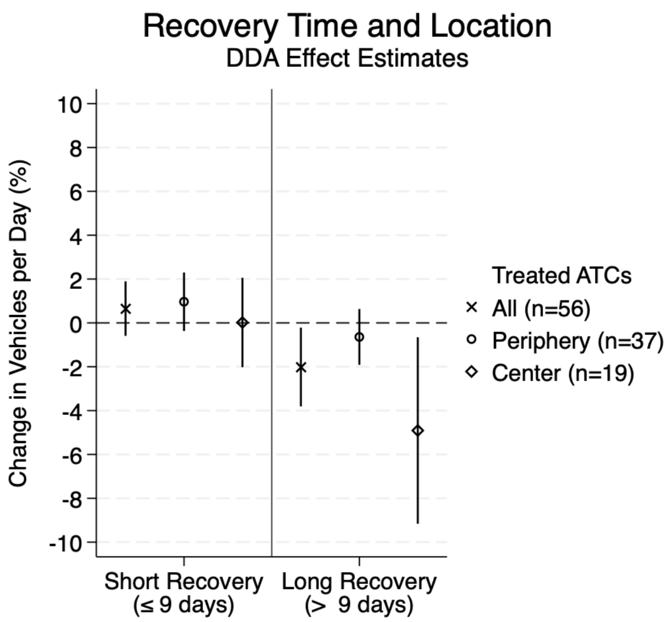

Figure 4.6.4: This figure plots don't drive appeal (DDA) effect point estimates by recovery time since the previous DDA event and ATC location. DDA event windows include a total of 277 calendar days for long recovery DDAs and 226 calendar days for short recovery days. Table 4.B.3 in appendix 4.B displays corresponding DiD regression estimates.

Finally, figure 4.6.4 plots point estimates highlighting differences in DDA effectiveness by between-event recovery time and counter group location.[44] Looking at all ATCs, DDAs reduce traffic in Stuttgart by a statistically significant 2% after a long recovery period of at least nine days, but do not meaningfully impact traffic levels after a short recovery DDA. In column 1 of table 4.B.3 in appendix 4.B, the point estimate on the linear combination of short-recovery DDA terms corresponds to a 0.6% increase in traffic during short-recovery DDAs. We then separately estimate equation 4.10 for city center and periphery counters. Of the four disaggregated estimates, the DDA effect is only statistically significant for Stuttgart ATCs at the city center after a longer than median recovery time. During DDAs following a shorter than median recovery period, the estimated DDA effect is between +1% and +0.01% and statistically insignificant at the 95% level for ATCs at both the city periphery and center respectively, providing empirical evidence for hypothesis 3 that an insufficiently long recovery period may hamper overall DDA effectiveness. While the DDA effect remains statistically insignificant and of negligible magnitude (-0.6%) for periphery counters during DDAs following longer recovery periods, the DDA effect increases to over a 5% reduction at the city center following a long recovery period and is statistically significant at the 95% level.

These dynamic patterns discussed in this section capture our main theoretical hypotheses from section 4.3. In figure 4.6.2, we show that the DDA leads to traffic reductions that are strongest at the beginning of DDA events and subside over the DDA's duration, as hypothesized social norm effects and other dynamic factors kick in. We show in figure 4.6.4 and highlight in our spatially disaggregated analysis that the DDA is most effective in reducing traffic at the city center, in particular following longer between-event recovery periods, confirming our hypothesis that DDA responsiveness is sensitive to recovery time. Finally, our results in figure 4.6.3 point to suggestive evidence that individuals trade-off voluntary pollution reductions during the treatment period for pollution increases (i.e. additional car trips) in the posttreatment period.

[44]Table 4.B.3 in appendix 4.B shows regression estimates.

4.7 Conclusion

Taken together, our theoretical framework and the results of our empirical test suggest that policy-makers should carefully consider dynamic factors when designing DDA programs and other policies appealing for voluntary pollution mitigation. Our results show that, on average, DDAs decrease traffic on DDA days by at most approximately 1% in line with the program's overall objective. Back-of-the-envelope calculations equate this DDA effect with a net decrease of about 2,598 motorists in Stuttgart on DDA days.[45] To put this into perspective, this is less than the difference between traffic levels in Stuttgart on an average Monday versus an average Tuesday.[46] We highlight several important caveats to this result. First, the DDA is most effective immediately after it is activated, and we show that it leads to average reduction of about 3% on the first three DDA days. Second, the DDA is only effective on aggregate following a lengthy between-event recovery period at the city's center. We find that the prediction that DDAs reduce driving on DDA days can be rationalized by appealing to a behaviorally informed model of car owners, and this passes an empirical test.

Our estimated overall DDA effect of a 1% traffic reduction on DDA days is situated between no effect results from other DDA studies (Noonan, 2014; Sexton, 2012; Cummings and Walker, 2000) and Cutter and Neidell (2009)'s finding of 2%-3% traffic reductions on *Spare the Air* days in San Francisco, USA. Our result contrasts with the finding that Salt Lake City, USA's particulate matter alert inadvertently increases traffic in the city by 3%-4% (Tribby et al., 2013). On aggregate, we believe these modest traffic reductions are not substantial enough to meaningfully impact pollution levels in the policy's target area. However, an analysis of DDA pollution impacts is beyond the scope of this paper. Our analysis also emphasizes that heterogeneity in spatial and temporal effectiveness may be obscured by overall DDA effect estimates.

These findings may generally caution policy-makers interested in combining AQAs with DDAs. This policy bundle has demonstrated mixed effectiveness for

[45] This assumes 382,000 motorists on an average work day and a 0.7% decrease in traffic.

[46] Monday counter-day traffic averages 18,889 vehicles per day and Tuesday 19,232. The difference in mean levels is 1.8%.

achieving driving reductions in other settings, and our study does not provide resounding evidence that this type of policy is persistently effective even when implemented in an ideal setting that has widespread environmental preferences and a dense public transit network. Our analysis is limited to analyzing the impacts of the DDA policy on traffic flows and does not evaluate it relative to other presumably important policy-making factors such as its implementation cost, its impact on residents' perceived quality of life, or its effect on actual pollution exposure. For example, we find suggestive evidence in our analysis that DDAs may displace some traffic to the city periphery. It is not clear in Stuttgart's case whether modest traffic decreases at the city center and traffic increases at the periphery effectively reduce air pollution exposure in the target population. However, policy-makers might value traffic and emissions reductions at the city center, where population density is often highest, more than moderate increases at the periphery. Pollution monitoring systems which provide more spatially resolved pollution measurements would enable a more holistic consideration of spatially-heterogeneous DDA pollution impacts.

The external validity of our results may be limited by Stuttgart's self-selection into the program. Stuttgart authorities designed and implemented the DDA program in response to the city's prolonged non-compliance with EU air quality standards with the ostensible belief that the DDA would lead its motorists to voluntary drive less to reduce pollution peaks.[47] In an ideal experimental setting, we would compare Stuttgart to a city that fully mimics Stuttgart, but authorities in the comparison city do not broadcast a DDA for reasons unrelated to its impact. Given the empirical nature of our analysis, such a perfect counterfactual does not exist – we don't know why Munich never chose to implement a DDA program, but it is likely not random. So, we urge caution when considering whether observed effects will transfer to different cities.

Finally, several other factors which we cannot observe could plausibly affect our estimates. First, we cannot account for same-day traffic shocks at the counter level. While we do not have information about traffic events which may heterogeneously affect car trip demand across counters (e.g. traffic jams, accidents, large

[47]To our knowledge, Stuttgart is the only city in Germany to have implemented a large-scale program appealing for voluntary driving reductions to temporarily reduce pollution.

events, etc.), barring remarkable changes in traffic conditions on DDA days or considerable spatial displacement effects, we think it is unlikely that same-day traffic shocks differ systematically on treatment days or would otherwise significantly bias our DDA effect estimates. Furthermore, we expect temporary traffic displacement to average out across nearby counters. Second, our analysis is limited to analyzing aggregate traffic impacts and provides little evidence of the individual level mechanisms underlying policy effectiveness. We are unable to observe individual motorists' driving decisions, expectations about DDA effectiveness, or their DDA information exposure (e.g. salience of DDA messaging, consumption of AQA-adjacent programming, etc.). For example, motorists may make their transportation choices based on some combination of weather forecasts, congestion expectations, and beliefs about the DDA, all of which we do not observe. Future research examining individual level responses to DDAs with individual level commuting data and individual level DDA information exposure could provide important insights into which population subgroups are responsive to appeals for voluntary pollution mitigation. Such analyses might also be able to shed light on socioeconomic dimensions of DDA effectiveness and, with an eye to an equitable mobility transition, inform policy-makers about effective targeting approaches and the distributional impacts of their policies.

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Appendix - Chapter 4

4.A Additional Tables

Table 4.A.1: Day Type by Day-of-the-Week

Day Type	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday	Sunday	Total
No DDA								
Issue Day	8	5	2	4	6	10	7	42
Bridge Day	7	8	5	2	4	6	11	43
Recovery Day #								
1	4	7	7	1	3	12	2	36
2	2	4	7	7	1	3	11	35
3	8	2	4	7	7	1	3	32
4+	38	43	42	42	46	44	40	295
Total	67	69	67	63	67	76	74	483
DDA								
DDA Day #								
1	12	7	8	5	2	4	6	44
2	6	12	7	8	5	2	4	44
3	4	2	9	7	7	3	2	34
4	0	3	2	9	6	5	3	28
5	2	0	3	2	9	4	4	24
6	3	2	0	2	2	6	4	19
7	3	2	1	0	2	1	6	15
8	6	2	1	1	0	1	0	11
9	0	5	2	1	1	0	0	9
10	0	0	3	2	1	0	0	6
11	0	0	0	3	1	1	0	5
12	0	0	0	0	2	0	1	3
13	1	0	0	0	0	2	0	3
14	0	1	0	0	0	0	1	2
15	1	0	1	0	0	0	0	2
16	0	0	0	1	0	0	0	1
Total	38	36	37	41	38	29	31	250

Table 4.A.2: Post-DDA Days by Day-of-the-Week

Post-DDA Day #	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday	Sunday	Total
-4+	18	15	16	16	12	11	12	100
-3	3	9	4	3	4	4	1	28
-2	1	4	11	4	5	5	4	34
Transition Day 1	7	1	5	13	4	5	9	44
Transition Day 2	9	7	1	5	13	4	5	44
1	4	7	7	1	3	12	2	36
2	2	4	7	7	1	3	11	35
3	8	2	4	7	7	1	3	32
4+	38	43	42	42	46	44	40	295
Total	90	92	97	98	95	89	87	648

4.B Additional Regression Results

Table 4.B.1: OLS and DiD Regression Results: Overall DDA Effect (No Lags)

	(1) OLS b/se	(2) OLS b/se	(3) OLS b/se	(4) OLS b/se	(5) DiD b/se	(6) DiD b/se
DDA	0.0073***	-0.0101***	0.0023	-0.0105***	0.0155*	-0.0042
	(0.0024)	(0.0036)	(0.0027)	(0.0031)	(0.0084)	(0.0058)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Lags	No	No	No	No	No	No
All Days	Yes	No	Yes	No	Yes	No
Unit FE	YM	YM	YMxATC	YMxATC	Date	Date
Time FE	ATC	ATC	ATC	ATC	ATC	ATC
# ATCs	56	56	56	56	76	76
Observations	26,779	12,239	26,770	12,217	38,131	17,405

Note: Dependent variable is logged vehicles per counter-day. Controls include contemporaneous weather and pollution variables. Lagged weather and pollution covariates are not included. Regressions either include all PM season days or non-holiday weekdays excluding DDA transition days. Unit fixed effects are always at the automatic traffic counter level. Time fixed effects are either year-month ("YM"), year-month by ATC ("YMxATC"), or calendar date. Standard errors clustered on counter in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

Table 4.B.2: OLS and DiD Regression Results: Overall DDA Effect (No Lags, No Controls)

	(1) OLS b/se	(2) OLS b/se	(3) OLS b/se	(4) OLS b/se	(5) DiD b/se	(6) DiD b/se
DDA	0.0721***	0.0073*	0.0679***	0.0048**	0.0118	-0.0033
	(0.0042)	(0.0041)	(0.0035)	(0.0023)	(0.0084)	(0.0062)
Controls	No	No	No	No	No	No
Lags	No	No	No	No	No	No
All Days	Yes	No	Yes	No	Yes	No
Unit FE	YM	YM	YMxATC	YMxATC	Date	Date
Time FE	ATC	ATC	ATC	ATC	ATC	ATC
# ATCs	56	56	56	56	76	76
Observations	27,673	12,683	27,664	12,661	39,403	18,021

Note: Dependent variable is logged vehicles per counter-day. Contemporaneous and lagged weather and pollution covariates are not included. Regressions either include all PM season days or non-holiday weekdays excluding DDA transition days. Unit fixed effects are always at the automatic traffic counter level. Time fixed effects are either year-month ("YM"), year-month by ATC ("YMxATC"), or calendar date. Standard errors clustered on counter in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

Table 4.B.3: DiD Regression Results: Recovery Time and Location DDA Effect

	(1) All DiD	(2) Center DiD	(3) Periphery DiD
Main coefficients			
DDA=1 × Treated=1	0.0065	0.0054	0.0083
	(0.0063)	(0.0131)	(0.0065)
Recovery=1 × Treated=1	0.0021	-0.0047	0.0059
	(0.0075)	(0.0119)	(0.0091)
DDA=1 × Recovery=1 × Treated=1	-0.0288**	-0.0518*	-0.0184*
	(0.0110)	(0.0280)	(0.0098)
Sum of coefficients			
Short Recovery DDA	0.0065	0.0054	0.0083
	(0.0063)	(0.0131)	(0.0065)
Long Recovery DDA	-0.0201**	-0.0511**	-0.0042
	(0.0092)	(0.0228)	(0.0064)
Controls	Yes	Yes	Yes
Lags	Yes	Yes	Yes
Days	503	503	503
DDA Days	227	227	227
Short-Window DDA Days	148	148	148
Long-Window DDA Days	79	79	79
Unit FE	Date	Date	Date
Time FE	ATC	ATC	ATC
# ATCs	75	39	56
Observations	26,832	13,978	20,836

Note: Dependent variable is logged vehicles per counter-day. Controls include contemporaneous weather and pollution variables. Lags are two days of lagged weather and pollution covariates. Regressions include PM season days within the specified window of each DDA activation day. Unit fixed effects are always at the automatic traffic counter level. Time fixed effects are calendar date. Standard errors clustered on counter in parentheses. *=p<0.1, **=p<0.05, ***=p<0.01.

4.C Additional Graphics

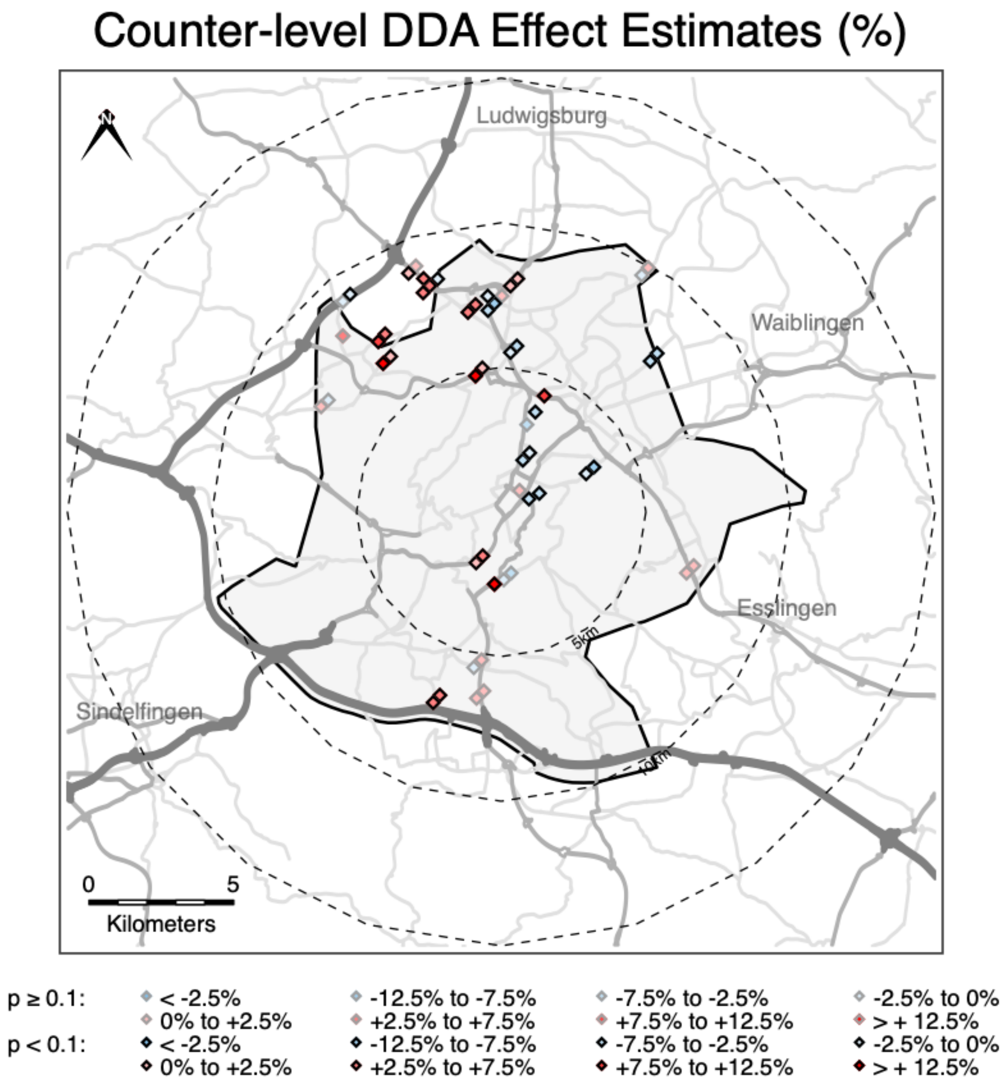

Figure 4.C.1: This figure maps counter-level don't drive appeal (DDA) effect point estimates at ATC locations from our main DiD regression model. Marker color corresponds to estimated effect sizes. Counter-level estimates that are statistically significant have a black marker border, while statistically insignificant markers have a grey border. Map includes Stuttgart administrative boundaries, main roads, and buffer zones in five kilometer intervals around the Stuttgart city centroid as in figure 4.4.1.

List of Figures

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