Bounds, Benefits, and Bad Air:
Net Benefits of Pollution Alerts

Air Quality Alerts' Benefits

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Abstract: Though air-quality alert systems (AQAS) cover more than 1.7 billion people worldwide, there has been little analysis of the net benefits of these systems. This paper presents a theoretical framework for deriving lower bounds on the net benefits of an AQAS and applies it to a South Korean system currently covering over 51 million people. Estimating a regression discontinuity design, we find that an alert issuance reduced youth respiratory expenditures by 30% and adult cardiovascular expenditures by 23%. The overall system reduced externalised health expenditures by 28.6 million dollars during 2016−2017, with a minimum benefit-cost ratio of 7.1:1. Including dynamic impacts of alerts increases the minimum benefits (benefit-cost ratio) to 36.7 million dollars (9.2:1). Our findings imply that the AQAS generates significant net benefits and suggests that manipulation of air quality data, which has been observed in other contexts, may negatively impact social net benefits.

Keywords: air quality, welfare, avoidance behaviour, health spending

Classification: I12,I18,Q53

1 Introduction

Air quality alert systems, which notify individuals of unhealthy pollution levels, are widespread throughout the world and cover over 1.7 billion people. For example, the United States (US) Environmental Protection Agency (EPA) manages the Air Quality Alert Program in the New England Area, and California air quality districts each run their own alert systems. In the United Kingdom, the Department for Environment, Food, and Rural Affairs issues pollution alerts. In Beijing, environmental authorities enacted a four-tier warning policy in 2013, expanded nationwide in 2014,

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and the South Korean government in 2015 launched a new air quality alert system (AQAS) as well (see Appendix Table A1 for additional examples). These programmes encourage the public to wear particulate-filtering masks, stay indoors, and reduce strenuous activities to mitigate health damages from air pollution.

Despite their increasing popularity, there has been little empirical analysis of the net benefits of these programmes. In this work, we estimate a lower bound on the gross benefits of the South Korean AQAS. To do so we develop a theoretical model that formalises some simple intuition: first, providing salient information to individuals makes them weakly better off; and second, reductions in externalities represent gross benefits. We then exploit the structure of the South Korean AQAS, which publishes alerts when PM2.5 levels exceed specific thresholds, to estimate a regression discontinuity (RD) design using pollution measurements as the running variable and health expenditures as the outcome. We combine the lower bound on the health-related benefits of the AQAS with an upper bound on operating costs, yielding lower bounds on net benefits and benefit-cost ratios. Importantly, our estimates are net of the individual costs of avoidance behaviour.

Our analysis contributes directly to two strands of literature. The first strand focuses on the health impacts of air-quality alerts. In seminal work, Neidell (2009) exploited high-frequency time-series variation to demonstrate that minors with asthma benefited from ozone alerts via a decrease in Los Angeles inpatient hospital admissions due to breathing difficulties. Janke (2014) generalised these results to England and further established an effect on emergency department (ED) admissions for minors. Chen *et al.* (2018) found marginally significant reductions in asthma-related ED visits in Toronto due to alerts.[1] Mullins and Bharadwaj (2015) and Aguilar-Gomez (2020) demonstrated that when combined with driving restrictions, environmental warnings reduced elderly mortality in Chile and ED visits in Mexico City, respectively. While previous studies have examined the frequency of health-related visits or specific healthcare modalities (e.g. emergency care), this study aims to estimate the impacts of air-quality alerts on expenditures, quantified in monetary terms, across all types of healthcare institutions, including general hospitals, specialised clinics, and public health centres.

The second emerging strand examines the effects of air pollution information on welfare measures. Ito and Zhang (2020) found that the willingness to pay for air purifiers in China increased following the 2013 disclosure of air-quality information by the US Embassy in Beijing. Barwick *et al.* (2024) demonstrated that avoidance behaviours, such as

[1] A related body of research explores who responds to air quality alerts but does not estimate effects on health outcomes (Noonan, 2014; Ward and Beatty, 2016; Saberian *et al.*, 2017).

air purifier sales and the timing of credit-card purchases, changed in response to the Chinese government providing real-time air quality monitoring data to the public. Gao *et al.* (2021) exploited the disclosure of PM2.5 data in China to estimate the impact of relaxing information constraints on hedonic valuation. Our contribution to this body of literature lies in the analysis of focused warnings concerning real-time air pollution levels, rather than the general publication of air quality information.

More generally, our paper contributes to a broad literature exploring the value of information provision to the public. Previous studies found that public information provision can affect behaviour in many contexts, including restaurant hygiene (Jin and Leslie, 2003), sales taxes (Chetty *et al.*, 2009), calorie labelling (Bollinger *et al.*, 2011), restaurant quality (Anderson and Magruder, 2012), and toxic releases (Mastromonaco, 2015). Further, targeting of information may affect choices in a variety of contexts as well, from school choice to health insurance to electricity consumption (Hastings and Weinstein, 2008; Kling *et al.*, 2012; Ito, 2014; Jessoe and Rapson, 2014). It also relates to work on establishing bounds for welfare analysis in situations with limited information (Manski *et al.*, 1997; Finkelstein and Hendren, 2020; Kang and Vasserman, 2021). In this study, we present a framework for computing lower bounds on benefits in situations in which information is disclosed upon pollution exceeding a certain level. This framework can be applied to different alert mechanisms governed by legal or regulatory parameters when externalities are involved.

Finally, our paper is relevant to a series of studies investigating potential efforts to manipulate pollution information for political or economic gain. Several studies found evidence that particulate matter (PM) measurements cluster right below politically-significant thresholds, compliance with which is important for government officials' promotions (Andrews, 2008; Chen *et al.*, 2012; Ghanem and Zhang, 2014; Zou, 2021). Recent work demonstrated that reported PM concentrations increased following the automation of pollution monitoring (Greenstone *et al.*, 2022). Though our analysis does not focus on the manipulation of air pollution observations, our results demonstrate the potential losses of such information distortion.

We make several contributions to the literature. First, our work represents, to the best of our knowledge, the first benefit-cost analysis of these widespread alert systems (as opposed to general air-quality monitoring). This contribution is possible because we analyse health expenditures, rather than raw visit counts or deaths, and we apply a theoretical framework that, combined with our novel health expenditure data, allows us to estimate sharp lower bounds on benefits that are net of the costs of avoidance behaviour.[2] Second, while previous work on the benefits of alert systems exploited time-series or panel variation, we implement a more robust RD design to study health-related outcomes.[3] Third, our paper examines impacts for a broad range of ages — from children to older adults — using rich data on health expenditures. Prior studies focus on asthma visits for children (Neidell, 2009; Janke, 2014), but we find that a super-majority of health expenditure reductions occur for other age groups and other diagnostic codes. Finally, while most analyses of AQASs have occurred in developed countries with lower pollution levels, the pollution levels in our study region are more representative of developing and middle-income countries.[4] Therefore, our findings can provide valuable insights for these countries in assessing the health benefits when designing and establishing an AQAS.

2 Background and Data

South Korea is an advanced economy that nevertheless suffers from high levels of particulate pollution. According to the Organization for Economic Cooperation and Development (OECD), South Korea's level of PM2.5 (particulates less than 2.5 micrometres in diameter) is the highest among all OECD countries. The average PM2.5 concentration level recorded in 2015 was over 30 $\mu g/m^3$, while the mean of other member countries was under 15 $\mu g/m^3$ (OECD, 2018). In response, the South Korean government launched a new air quality alert system in 2015. The primary objective of the alert system is to reduce negative health effects by providing citizens with the necessary information to take precautionary measures.

Municipal governments of major cities issue the alerts. When the level of PM2.5 or PM10 exceeds a certain threshold in an alert region within a city, the local government announces a PM warning for the region (Figure 1). Citizens

[2] In much of the existing literature on air quality alerts, the avoidance behaviour is the object of interest, but its welfare impact cannot be quantified. Barwick *et al.* (2024) addresses avoidance behaviour costs by estimating two specific types of avoidance behaviour: air purifier purchases and outdoor shopping trips. These behaviours are less relevant to air quality alerts since air purifiers are durable goods, and many of the benefits of alerts accrue to minors.

[3] The one exception implementing a RDD that we are aware of is Chen *et al.* (2018), which estimates a RDD to study health outcomes. Their study appears underpowered, however, yielding a single significant t-statistic of exactly 2.0 across 12 outcomes tested, with no visual evidence of a break for any health outcome. More broadly in terms of utilising natural experimental designs, Mullins and Bharadwaj (2015) estimates the mortality impacts based on a difference-in-differences setting. For dependent variables other than health outcomes, Ito and Zhang (2020) exploits a spatial pollution discontinuity at the Huai River, but the study's effect of information disclosure is identified using a post-2013 indicator. Neidell (2009), Neidell (2010), and Liu *et al.* (2018) estimate RD designs in the context of avoidance behaviour only.

[4] The only AQAS studies in developing and middle-income countries that we are aware of are Mullins and Bharadwaj (2015), Aguilar-Gomez (2020), and Rivera (2021), all of which establish impacts of driving restrictions but cannot reject the null hypothesis that alerts in isolation have no effects.

are encouraged to curtail outdoor activities, wear face masks, drinking water, and use public transport (Table 1). Authorities disseminate public health warnings through mass media (e.g., radio, television, and online news articles), public road signs, and wireless services (text messages and mobile applications).

Given the widespread dissemination, most people are likely informed when air quality warnings are issued. To determine whether citizens are more cognizant of air quality when alerts are issued, we analyzed internet search keywords in NAVER, a search engine accounting for 75% of all web searches in South Korea. They include keywords related to air pollution ("air pollution alert", "particulate matter", and "air quality") and protection measures ("facemask", "air purifier", and "public transit"). Figure 2 indicates that searches for these keywords increased dramatically during the days on which alerts were issued.

PM alerts are triggered based on hourly monitor readings of PM2.5 and PM10. When the new alert system began, both the 24-hour average and the 2-hour minimum values served as measurement criteria for issuing the alerts. After about a year, the Ministry of Environment (MOE) settled on the 2-hour minimum value as the sole standard. Advisories were then issued when the hourly average PM2.5 (PM10) in an alert region was over 90 $\mu g/m^3$ (150 $\mu g/m^3$) for two consecutive hours (Table 1). These advisories remained in effect until the 1-hour level of PM2.5 (PM10) dropped below 50 $\mu g/m^3$ (100 $\mu g/m^3$).[5] We retrieved the alert information from the website of the Korea Environment Corporation (KECO). There were a total of 412 region-days or 2,562 district-days with alerts during 2016–2018, the coverage period of our health spending data.[6] While alerts are relatively infrequent in this context (e.g., occurring on approximately 5% of potential district-days), the PM levels that trigger alerts are not uncommon in other Asian countries. For example, PM2.5 levels in Delhi exceeded the alert threshold on approximately 80% of days during our sample (Sharma and Mauzerall, 2022), and PM2.5 levels in Beijing exceeded the threshold on the majority of days during our sample.

Our health spending data cover daily per-capita spending by district in 2016 and 2017.[7] The dataset comes from the National Health Insurance Service (NHIS) of South Korea, which covers the country's entire population (all individuals must join). Our data represent a 10% random sample of insurance subscribers in seven major cities: Seoul, Busan,

[5]When the level of PM is more extreme, governments issue a second-level warning. The PM thresholds are 180 $\mu g/m^3$ and 300 $\mu g/m^3$ for PM2.5 and PM10, respectively. Our identification strategy utilises observations around the threshold of the first-level warnings, as second-level warnings are rare. Therefore, we use terms such as advisory, alert, and warning interchangeably to indicate the first-level warning. Also note that in July 2018, the PM2.5 thresholds for issuance and cancellation were lowered to 75 $\mu g/m^3$ and 35 $\mu g/m^3$, respectively. This date lies outside of our analytic data set.

[6]Our dataset includes 73 districts across 14 alert regions.

[7]Due to privacy concerns, the dataset can be used only in selected data centres in South Korea. Furthermore, NHIS does not allow the sharing or publication of any type of processed data, except for summary statistics, figures, and regression results.

Daegu, Daejeon, Incheon, Gwangju, and Ulsan. Their combined population is 23 million, or 44% of the population of South Korea. Hence, our dataset includes about 2.3 million individuals. We requested separate spending measures by disease type (cardiovascular disease and respiratory disease) and age group (minors: 0−19; adults: 20−64; and older adults: 65 and older).[8] We focused on respiratory and cardiovascular diseases because air pollution should directly affect the respiratory system, and previous work has established high-frequency temporal associations between PM and cardiovascular morbidity and mortality (Pope and Dockery, 1999). Table 2 summarises the health spending data. While cardiovascular disease expenditures increase with age, respiratory disease expenditures decrease with age. Thus we might expect respiratory disease effects to be more pronounced for the young and cardiovascular disease effects to be more pronounced for the elderly.

Our data cover most healthcare expenditures, including outpatient care (e.g., clinics and doctors' offices), hospitals (inpatient and emergency department visits), public health centres, and most prescription medications. Three features are noteworthy. First, while the data include inpatient hospital visits (i.e., overnight stays), there is often a temporal gap of a week or more between the onset of symptoms and an inpatient admission, as an inpatient hospitalisation requires several rounds of referrals for all but the most acute cases. As the temporal unit of our data is daily, we exclude spending on inpatient stays from our analysis to reduce the noise in our dependent variable.

Second, we further exclude outpatient visits to tertiary hospitals in our primary analysis. In the South Korean healthcare system, outpatient visits to primary (clinics) and secondary facilities (hospitals and general hospitals) typically do not require referrals, but visits to tertiary general hospitals do (see Appendix Table A3 for definitions of these institutions). The referral process leads to a delay between the onset of symptoms and actual outpatient visits, and our main dependent variables accordingly exclude tertiary visit healthcare costs. Nevertheless, we test the robustness of our results to this exclusion by running regressions that include tertiary outpatient visits. The results are qualitatively similar to the main results, with coefficients and standard errors of generally comparable magnitudes.

Third, our health expenditure data consist of the sum of private co-payments and public coverage. Therefore, our regression coefficients can be interpreted as changes in total health expenditures in the covered categories. It is worth noting that the out-of-pocket payment ratio ranges from 10% to 50%, implying substantial coverage by the social health care system (see Appendix Table A4 for details on insurance coverage by treatment location). Overall, the

[8] Cardiovascular diseases are those in the "I" category according to the International Classification of Diseases, 10th revision (ICD-10). These include diseases like angina (chest pain) and many diseases that have shortness of breath and palpitations as symptoms. Respiratory diseases are those in the "J" category according to ICD-10.

South Korean healthcare system subsidised 70% of the total healthcare spending on outpatient visits. We thus interpret 70% of the spending to represent external costs from the patient's perspective.

Despite the richness of our data set, it does not include information on the number of visits, which would enable us to analyze whether health benefits accrue at the extensive or intensive margins. Instead, we obtained and analyzed separate data sets on the number of outpatient visits due to asthma and rhinitis, covering the entire population registered in the health insurance system. Another question is whether alerts are helpful for patients on the margin of a serious health emergency or death. To explore this question we secured emergency spending data based on the same sample as the main health expenditure data set and mortality data covering the entire South Korean population.

The final primary data set in our analysis is a credit card transaction data set provided by one of the largest credit card companies in South Korea, whose market share is approximately 25%. We use these data to look for evidence of avoidance behaviour. The data include daily per capita number of credit card transactions of card members at the district of residence level.[9] The transaction data sets cover the same spatial boundaries and seven metropolitan cities as the healthcare expenditure data, but the coverage dates (2017−2018) differ. We test the RD validity assumptions — e.g. treatment discontinuity, control continuity, and manipulation of the running variable — separately for this different coverage period.

Our credit card transaction data include a wide range of spending categories. Examples include larger retailers (e.g. department stores, large-sized supermarkets, and electrical appliance stores) and smaller retailers (e.g. grocery stores, bakeries, butcher shops, dollar stores, convenience stores, clothing stores, cosmetics shops, and eyewear shops). Entertainment, sports, and leisure activities, such as transactions in movie theaters, art exhibitions, museums, karaoke, bowling alleys, fitness centres, swimming pools, golf courses, and indoor golf driving ranges, are also included in our credit card data set. In addition, it includes transactions in restaurants and bars. In the data extraction procedure, we requested the exclusion of selected categories — specifically, cars, furniture, and spending on educational institutions. Transactions in these categories seem unlikely to be related to daily variation in pollution information, potentially increasing noise in the dependent variable. Transactions of medical expenditures were also excluded to avoid duplication

[9]Due to privacy concerns, our consumption data sets are not accessible on an individual member basis. Instead, they are aggregated to the district-by-day level, across multiple spending categories, and then divided by the number of active card members in each district, thus providing information on the number of card consumption transactions per person. To simplify interpretation, we multiply coefficients by 1,000. Consequently, regression coefficients represent the effect on the number of transactions for every 1,000 individuals.

between the credit card data and the medical expenditure data; the national health insurance data set provides a more complete accounting of health-related spending.

In addition to the credit card transaction data aggregated across these different consumption categories, we obtained information on the number of transactions per member for three selected consumption categories: restaurants and bars, fashion outlets, and travel accommodations. The first category, restaurants and bars, represents one of the most frequently occurring daily consumption activities and accounts for a significant portion of daily card transactions (almost 30%). In contrast, spending on fashion outlets represents less frequent consumption activities. However, fashion outlet stores in South Korea are generally situated in outdoor marketplaces, and we thus expect them to be more susceptible to the impact of air quality information disclosure. Conversely, we anticipate the impact on the travel and accommodations spending category to be minimal since most of these consumption activities are booked in advance and are difficult to change last minute; in that sense it offers a placebo test. The third panel in Table 2 presents summary statistics of the number of transactions per member for the different categories.

While credit and debit cards are not the only forms of payment, in South Korea they account for a majority of transactions. According to the Bank of Korea, in 2019 credit cards accounted for 53.8% of expenditures, followed by cash (17.4%), debit cards (15.3%), bank transfers (8.0%), and mobile payments (3.8%). Credit and debit cards thus comprise approximately 70% of payments during our sample period.

3 Theoretical Framework

In seminal work, Harrington and Portney (1987) derive a workhorse model for valuing benefits of health and safety regulations. The model emphasises that benefits from pollution abatement may arise from changes in three factors: monetary cost of illness (COI), disutility of sickness, and defensive expenditures or avoidance behaviour. Reductions in COI represent a lower bound on benefits in Harrington and Portney (1987) because all three measures weakly move in the same direction.

Evaluating the benefits of information provision is more challenging, however, because changes in COI no longer represent a lower bound on benefits. New information makes avoidance behaviour more likely; increases in the costs of avoidance behaviour must therefore be weighed against decreases in COI and disutility of sickness. In some instances researchers may attempt to enumerate all significant dimensions of avoidance behaviour (Barwick *et al.*, 2024), but in many cases researchers lack data on the full set of avoidance behaviours.

To overcome this challenge we develop a parsimonious model of an individual's avoidance behaviour and incorporate externalities. The model serves to motivate our empirical analysis and help interpret our results. The intuition underlying our model is straightforward. First, providing relevant information should make individuals weakly better off. Second, to the extent that the information provision has positive externalities (i.e. reduces negative externalities), the "weak" bound on benefits that includes zero can become a "strong" bound on benefits that includes only positive values.

To formalise this intuition, consider a representative individual i choosing an activity level a. The individual gains utility from activities that involve pollution exposure and loses utility from getting sick. Her utility function is:

$$U_i(a_i, pm) = b_i(a_i) - s_i^{pvt} p_s(a_i, pm) \tag{1}$$

where $b_i(a)$ represents the benefits of activity level a, s_i^{pvt} represents the private costs (pecuniary and non-pecuniary) of getting sick, and $p_s(a, pm)$ represents the probability of getting sick given activity level a and PM level pm. Examples of pecuniary costs include out-of-pocket medical expenditures or lost wages due to illness, while examples of non-pecuniary costs include the physical discomfort of illness or the hassle of attending medical visits. One could imagine more general utility functions — e.g. pm could affect b as well — but to motivate our bounding exercise this model suffices.

To interpret our RD estimand, note that it compares days on which the PM level is just above the alert threshold ($pm \downarrow c$) to those on which it is just below the alert threshold ($pm \uparrow c$). Thus, pm itself remains approximately constant near the threshold c (confirmed in Appendix Table A5), but perceived PM, denoted as pm_i, changes.[10] Specify individual beliefs as

$$pm_i = \begin{cases} pm^{avg} & \text{if} \quad pm = pm \uparrow c \\ pm^{hi} & \text{if} \quad pm = pm \downarrow c \end{cases}$$

[10] In our actual data, the hourly PM level is distinct from the running variable, as the latter depends on the maximum 2-hour minimum PM level. For notational simplicity, we treat PM as the running variable in the theoretical model, but in Appendix A1 we show that our conclusions generalise to a model in which the running variable is a function of PM.

where pm^{avg} represents average PM conditional on being below the threshold c and pm^{hi} represents average PM conditional on being above the threshold c. For our bounding exercise we assume that $pm^{hi} \approx c$, or at least that $|pm^{hi} - c| << |pm^{avg} - c|$. This representation is a reasonable approximation of our actual PM data.[11] More generally, the approximation only needs to be sufficiently accurate that the alerts do not cause individuals to behave *less* optimally than they would absent the alert's information.[12]

Individuals maximise utility by choosing activity levels $a_i = \text{argmax}_a\, U_i(a, pm_i)$. Then

$$U_i = \begin{cases} U_i(a_i(pm^{avg}), c) & \text{if} \quad pm = pm \uparrow c \\ U_i(a_i(pm^{hi}), c) & \text{if} \quad pm = pm \downarrow c \end{cases}$$

An individual's private change in utility from PM crossing the alert threshold is

$$\Delta\, U_i = U_i(a_i(pm^{hi}), c) - U_i(a_i(pm^{avg}), c) = \tag{2}$$

$$[b_i(a_i(pm^{hi})) - b_i(a_i(pm^{avg}))] - s_i^{pvt}[p_s(a_i(pm^{hi}), c) - p_s(a_i(pm^{avg}), c)].$$

Naturally $\Delta\, U_i \geq 0$ since $pm^{hi} \approx c$ and $a_i = \text{argmax}_a\, U_i(a, pm_i)$ — i.e. more accurate PM information can only (weakly) increase the individual's utility — but accurately quantifying $\Delta\, U_i$ is challenging even with good data on s_i^{pvt}. This challenge arises because it is difficult to estimate $b_i(a_i)$ and $p_s(a_i)$, the benefits of different activities (and thus the costs of avoidance behaviours) and the probability of getting sick. In particular, a_i may be high dimensional, and researchers rarely have data on all, or even most, elements of a_i.

To motivate our benefit-bounding exercise, consider the public net benefits of the individual's choices:

$$W_i = U_i(a_i, pm) + E_i. \tag{3}$$

W_i, the social benefit accruing from i's choices, equals private benefit U_i plus the externalities associated with i's choices, E_i. We define externalities as any costs (or benefits) that arise from an individual's choices but are not borne by that individual. For example, if an individual consumes 50 dollars of healthcare services but pays nothing for them,

[11] For example, in our PM2.5 data, $pm^{avg} = 22.7$, $pm^{hi} = 66.7$, and $c = 57.5$. Thus pm^{hi} is much closer to c than pm^{avg} is. In this context c represents the average PM2.5 level when the running variable is close to the threshold, which differs from the running variable itself (see previous note).

[12] Of course, if individuals already have accurate information on PM levels (e.g. via real-time monitoring), then alerts will have no impact on behaviour, and our research design should estimate null effects. In that case we would correctly conclude that the alerts generate no benefits.

that represents a 50 dollar negative externality. Then

$$\Delta W_i = \Delta U_i + \Delta E_i. \tag{4}$$

Since $\Delta U_i \geq 0$, ΔE_i represents a lower bound on the social net benefits of crossing the alert threshold. By aggregating ΔE_i across individuals and alert days, we can estimate a lower bound on the gross social benefits of the alert system. Combined with data on the costs of the system, we can estimate lower bounds on benefit-cost ratios. Importantly, as discussed following Equation (2), the individual costs of avoidance behaviour — e.g. choosing to avoid exercise, keeping a vulnerable child home from school, or reducing credit card transactions at outdoor locations — net out to be weakly positive when combined with the individual benefits of this behaviour. An advantage of our framework is that we do not need to estimate any of the components appearing in Equations (1) or (2), reducing the demands on the data.

One caveat for this result is that if authorities or event organisers engage in mass avoidance behaviour by cancelling outdoor events in response to alerts, then ΔU_i need not be weakly positive, as some individuals would then be engaging in involuntary avoidance behaviour. In our specific context, this issue does not arise, as we estimate effects of "Level 1" alerts that correspond to modestly elevated pollution levels and do not trigger large-scale cancellations.[13] In other contexts, however, researchers applying this framework would need to confirm that involuntary avoidance behaviour is not enforced, or that enforcement of such behaviour does not result in negative private net benefits (i.e., on average ΔU_i is non-negative).

It also useful to note that alerts may trigger avoidance behaviour via two channels. First, they may represent an "information treatment" that imparts new information about PM levels to individuals. Second, they may have a "salience effect", reminding vulnerable individuals about the hazards of outdoor activities on high-pollution days. Our theoretical model is somewhat agnostic as to the two channels; individuals might alter behaviour because they receive more accurate information or because they are reminded about information they already have. While we cannot differentiate between the two channels in our data, we suspect the salience channel is the more relevant one, given the general availability of air quality data on smartphones during our study period. Since salience effects are by

[13]We conducted extensive searches for any instances of pollution-related event or school cancellations. The small number of examples of event cancellations that we found, such as several baseball games in 2018 and 2021, were in response to PM levels many times higher than the first-level alert threshold that we study (Yoo, 2021). Even at the second-level alert threshold, schools may only receive, at most, non-compulsory recommendations for closure (Ahn and Dabee, 2018).

nature short-run, we might expect the AQAS to have sustained benefits over time. The effects of a pure information treatment, in contrast, might diminish over time, if individuals become more adept at monitoring air quality information independent of the AQAS.

For the benefits bound to be non-trivial, the researcher must have data on meaningful externalities associated with individuals' choices. In our context, health expenditures — the majority (about 70%) of which are reimbursed by public funds — represent such an externality.[14] In other contexts in which health costs are paid by "insurers" (typically employers), covered health expenditures would also represent an externality.

4 Regression Discontinuity Design

We employ a RD design to estimate the causal impact of PM alerts on health spending. The RD focuses on the point where the running variable (RV) exceeds a threshold at which the probability of treatment changes discontinuously. The identifying assumption is that the only difference between observations right above and below the threshold is the assignment of the treatment; other factors affecting the outcome are continuous around the threshold. It then follows that we can attribute the discontinuous change of the outcome variable to the treatment assignment.

In this paper, the issuance of advisories corresponds to the treatment, with health spending as the outcome. The running variable is the daily maximum of 2-hour minimum PM values. For example, in the case of PM2.5, an advisory occurs when the PM2.5 level is over 90 $\mu g/m^3$ for two consecutive hours. Hence, when the 2-hour minimum exceeds 90 $\mu g/m^3$, the alert triggers. We calculate the daily maximum of these hourly 2-hour minimum values and code the running variable at the daily level (for a given region).

As discussed in Section 2, there are two pollutants that trigger the issuance of alerts, PM2.5 and PM10. Following Cattaneo *et al.* (2021), we calculate the daily maximum of 2-hour minimum values for PM2.5 and PM10, normalise them by their respective thresholds (90 $\mu g/m^3$ and 150 $\mu g/m^3$), and take the larger one as the assignment variable for the RD. By doing so, we construct one normalised assignment variable whose treatment threshold is zero (Table 1).[15] This running variable is similar to the minimum distance to the nearest threshold elucidated in Cattaneo *et al.* (2021).

[14] Our bounds are conservative in that they ignore the marginal cost of public funds (Dahlby, 2008). Incorporating a positive marginal cost of public funds would increase the estimated benefits. Incorporating other externalities, such as government or employer-provided sick pay, could further increase the estimated benefits. However, we do not have data on these costs.

[15] For additional details on calculating the running variable, see Appendix A2.

The running variable does not perfectly determine alert issuance for (at least) two reasons. First, municipal governments also consider weather conditions when determining whether to announce an alert. Thus alerts are not issued on some days on which the normalised running variable exceeds zero. Second, the thresholds for issuance and cancellation are different. For example, suppose that an alert was issued at 2 PM Monday, when the 2-hour minimum PM2.5 exceeded 90 $\mu g/m^3$. If the hourly PM2.5 level remains at 60 $\mu g/m^3$ until the end of Tuesday, the alert remains in effect through Tuesday, as the PM2.5 cancellation threshold is 50 $\mu g/m^3$. Nevertheless, the normalised running variable for Tuesday is -30. For these reasons, we estimate a fuzzy RD (FRD) design.[16]

After setting a bandwidth h around the threshold, we retain observations with running-variable values falling within h units of the threshold. We run the following first-stage and reduced-form regressions:

$$Alert_{it} = \gamma_1 \mathbb{1}(\widetilde{PM}_{it} \geq 0) + \gamma_2 \widetilde{PM}_{it} + \gamma_3 \widetilde{PM}_{it} \mathbb{1}(\widetilde{PM}_{it} \geq 0) + X_{1it}\theta_1 + X_{2t}\phi_1 + \delta_{1i} + u_{it} \tag{5}$$

$$Y_{it} = \beta_1 \mathbb{1}(\widetilde{PM}_{it} \geq 0) + \beta_2 \widetilde{PM}_{it} + \beta_3 \widetilde{PM}_{it} \mathbb{1}(\widetilde{PM}_{it} \geq 0) + X_{1it}\theta_2 + X_{2t}\phi_2 + \delta_{2i} + \varepsilon_{it} \tag{6}$$

Y_{it} represents health expenditures per person or the number of card transactions per person in district i on day t, $\widetilde{PM}_{it}$ is the normalised running variable (described above), and $Alert_{it}$ is an indicator variable for an air-quality alert. X_{1it} includes temperature and precipitation controls, X_{2t} includes year-month, day-of-week, and holiday fixed effects, and δ_i are district fixed effects.[17] While these controls are not strictly necessary for identification, they substantially improve the precision of our regressions (Cellini *et al.*, 2010).[18] We population weight our regressions to make the estimates more representative and further improve precision,[19] and we cluster the standard errors by running variable value or date to account for spatial correlation in alerts and health spending across districts.[20] When discussing t-statistics, to be conservative we default to whichever of the two standard errors is larger.

To estimate the FRD and recover the local average treatment effect (LATE), we divide $\hat{\beta}_1$ by $\hat{\gamma}_1$, yielding:

[16] A concern may arise regarding the discrepancy between the thresholds of issuance and cancellation. To address this concern, we also present FRD results without these cases, and these results are comparable to our baseline FRD estimates (see Appendix Table A10).

[17] In the case of credit card transactions, we added a before-holiday fixed effect that indicates whether day t is a day before a holiday since consumption increases substantially not only on holidays but also before the holidays.

[18] The adjusted R^2 in our main outcome regressions is in the range of 0.8 to 0.9 (Tables 4 and 5), implying that including the controls reduces the standard errors by a factor of 2 to 3.

[19] For the card transaction analysis we weight by the number of members.

[20] Unlike typical panel data sets, serial correlation over time has little impact in our context, because the independent variable of interest, $\gamma_1 \mathbb{1}(\widetilde{PM}_{it} \geq 0)$, exhibits very modest time-series correlation. Clustering by district (to account for time-series correlation) generates much smaller standard errors, and two-way clustering by district and date generates standard errors that are similar in size to clustering by date.

$$\hat{\tau}_{FRD} = \frac{\hat{\beta}_1}{\hat{\gamma}_1} \tag{7}$$

In practice we estimate $\hat{\tau}_{FRD}$ using two-stage least squares (2SLS). Equations (6) and (7) estimate the contemporaneous effect of an alert on health expenditures. The panel nature of our data, however, introduces additional considerations that are absent from most cross-sectional RDs. First, the asymmetry in the thresholds for issuance and cancellation ensures that many air quality alerts last for two to three consecutive days.[21] One could thus conceptualise the treatment as a single 48- to 72-hour alert. Second, the possibility of dynamic effects represents a potential violation of the stable unit treatment value assumption (SUTVA) — the treatment on day t could have spillover effects on the outcome on day $t+1$.

We address this complication in two ways. First, as a robustness check, we trim the estimation sample to exclude days following a day with an air quality alert. This estimation sample yields similar results. Second, while the contemporaneous regressions (Equations (6) and (7)) appear to generate estimates that are internally valid (based on the results referenced above), they may yield an incomplete picture of the total effect of an air quality alert. In particular, they do not capture any dynamic effects of an alert that persist beyond one day. In principle, these effects could shift the net impact in either direction. For example, if avoidance behaviour yields health benefits beyond 24 hours, the dynamic effects could increase the net impact. Alternatively, if individuals intertemporally substitute activities, resulting in higher-than-average activity on the day after an alert, then accounting for dynamic effects could decrease the net impact.

To capture dynamic effects, we estimate an alternative fuzzy RD that specifies the dependent variable as a rolling 3-day sum of health expenditures. Specifically, we estimate the reduced-form regression as:

$$Y_{it}^{+} = \beta_1 \mathbb{1}(\widetilde{PM}_{it} \geq 0) + \beta_2 \widetilde{PM}_{it} + \beta_3 \widetilde{PM}_{it} \mathbb{1}(\widetilde{PM}_{it} \geq 0) + X_{1it}\theta_2 + X_{2t}\phi_2 + \delta_{2i} + \varepsilon_{it} \tag{8}$$

Y_{it}^{+} represents health expenditures or card transactions in district i on days t, $t+1$, and $t+2$ (i.e. $Y_{it}^{+} = \sum_{s=0}^{2} Y_{it+s}$). Other variables remain as defined before, and we continue to population weight the regression. Since the treatment is effectively a multi-day alert (given the asymmetry of the activation and cancellation thresholds), we also estimate

[21] Out of the total 134 region-by-alert episodes, 45 episodes were single-day alerts, and the remaining 89 warnings were issued for two days or more in a row.

a specification in which we omit treated days whose previous dates were also treated with an air quality alert. For example, if an alert was issued on 1 January and 2 January, 2 January is omitted to avoid "double counting" the alert's impact when conducting policy simulations in Section 6.[22]

Before presenting the estimates, we note two details about the FRD regressions. First, FRD estimates may be sensitive to the polynomial degree of the running variable. For robustness, we also check results using a specification with a quadratic in the running variable.[23] Second, the FRD results may be sensitive to the choice of bandwidth, h. To find a default bandwidth for our analysis, we follow Calonico *et al.* (2014, 2015) (CCT). The CCT criteria yield optimal bias-corrected bandwidths ranging from 17 to 22 for our data set (Appendix Table A6), so we chose $h = 20$ as the default bandwidth. To demonstrate robustness, however, we report results from bandwidth choices of 16, 20, and 24.

5 Results

5.1 Contemporaneous Effects

Figure 3 plots treatment probability by the running variable using a binned scatter plot. The figure demonstrates a large discontinuity in the probability of treatment around the RD threshold.[24] Table 3 presents corresponding first-stage estimates of Equation (5) for three bandwidth choices (16, 20, and 24). Crossing the RD threshold corresponds to an approximate 60 percentage point increase in the probability of an alert. This discontinuity is robust to the choice of bandwidth and highly significant in all cases, with F-statistics between 25 and 50. There is no significant "first-stage" effect on average PM levels (Appendix Table A5), however, which is consistent with the lack of any binding alert-associated restrictions on activity or emissions. We thus interpret our RD health effects as resulting purely from avoidance behaviour rather than from reductions in ambient pollution levels.

Figure 4 plots health expenditures, converted to spending per capita in US cents, by the running variable using a binned scatter plot. The left panels present respiratory-illness expenditures, while the right panels present

[22] As an alternative strategy, we considered supplementing Equation (6) with lagged values of the treatment, similar to the "one-step" estimator in Cellini *et al.* (2010). This specification, however, would require us to trim the sample on multiple dimensions. For example, with two lagged values of treatment, we would need to trim the estimation sample based on the contemporaneous running variable and one- and two-day lags of the running variable. In practice, this would reduce our estimation sample to an impractical degree. Given that almost all treatment episodes consist of two consecutive days of alerts, we chose to instead sum the outcome over several days and regard each episode of two-day treatments as a single treatment. This strategy also avoids the need to consider different sequences of treatments (Lechner, 2009; Anderson, 2017).

[23] Following the arguments in Gelman and Imbens (2019), we do not try higher-order polynomials in the running variable.

[24] For the 2017−2018 data, Appendix Figure A3 presents an analogous plot, which also shows a clear discontinuity at the threshold.

cardiovascular-illness expenditures. From top to bottom, the panels present expenditures for minors (under age 20), adults (age 20-64), and older adults (over age 64).

The top-left panel reveals a sharp decline in respiratory expenditures for minors at the RD threshold, and a notable, though less pronounced, decline in these expenditures for adults. In contrast, there is less evidence of a decline for older adults. The top-right panel reveals no change in cardiovascular expenditures for minors, likely because cardiovascular diagnoses are rare for this age group. The middle-right and bottom-right panels, however, reveal drops in cardiovascular-illness expenditures for adults and older adults at the RD threshold.

Tables 4 and 5 present corresponding reduced-form and 2SLS estimates of Equations (6) and (7) for the preferred bandwidth of $h = 20$. Table 4 reports results for respiratory disease, while Table 5 reports results for cardiovascular disease. In each table, the top panel presents reduced-form estimates (i.e. estimates corresponding to Figure 4), and the bottom panel presents 2SLS estimates. Each column corresponds to a different age group: minors, adults, older adults, and all ages.

The tables confirm the patterns observed in Figure 4. In Table 4, an alert induces a highly significant decrease in respiratory-illness expenditures for minors ($t = -3.2$). The point estimate implies a reduction of 15 cents per capita, or approximately 30% percent of mean expenditures below the RD threshold.[25] For older age groups the change in respiratory-illness expenditures is insignificant at the RD threshold. Nevertheless, the overall reduction in respiratory-illness expenditures at the threshold is statistically significant ($t = -2.4$).

In Table 5, an alert induces significant decreases in cardiovascular-illness expenditures for adults (age 20-64) and older adults ($t = -2.9$ and $t = -2.5$ respectively). The point estimates imply reductions of 2.8 and 9.6 cents per capita respectively, or about 23% and 14% of mean expenditures below the RD threshold. For minors, there is no significant change in cardiovascular-illness expenditures at the RD threshold. The overall reduction in cardiovascular-illness expenditures at the threshold is statistically significant ($t = -3.0$), however.

Figure 5 presents an outcome discontinuity plot for credit card transaction data that is analogous to Figure 4. The top-left panel plots the number of transactions aggregated across all spending categories. The top-right, bottom-left, and bottom-right panels represent restaurant and bar, fashion store, and travel accommodation categories, respectively.

[25] When calculating percentage effects, we take the mean of the dependent variable when the running variable lies between -20 and 0.

We observe some degree of drop in transactions in the top panels. In the aggregate and in restaurants and bars, transactions decline in the first post-threshold bin and reach a local trough within the next one or two bins. This pattern is suggestive of a short-run response around the alert issuance. However, because the largest drop does not occur exactly at the cutoff and the binned scatter is noisy—and, as shown in Table **??**, the estimated discontinuity is statistically insignificant—the plot alone cannot, on its own, isolate a discrete causal jump.

On the other hand, the decrease in the number of transaction is more pronounced at the threshold in the category of fashion outlets, which is consistent with the outdoor and discretionary nature of fashion outlet consumption activities. The travel accommodations category does not display any discontinuous decrease at the threshold. This null effect is consistent with the significant cost of cancelling these consumption activities on short notice and also suggests that alert days are not unusual in an *ex ante* sense.

Table 6 reports the results of FRD regressions with the number of credit card transactions as the outcome. These results confirm the patterns visible in Figure 5. We observe significant negative impacts of air pollution alerts on credit card transactions in fashion outlets. The overall impact of an alert on the number of transactions is -5.5 per 1,000 card members (-1.6%); however, the point estimate does not reach statistical significance.[26] The effect on restaurant transactions, reported in Column (2), is -3.1 per 1,000 members (-2.8%, $t = -1.36$). An alert decreases fashion store transactions by 0.5 per 1,000 members (-11%, $t = -3.3$). Travel accommodation transactions decrease by a statistically insignificant 0.01 per 1,000 members (0.2%, $t = 0.07$).

5.2 Dynamic Effects

Air pollution alerts may have lagged effects — either because an alert lasts more than one day or because avoidance behaviour yields dividends over multiple days — further decreasing healthcare costs beyond the dates of alert issuance. It is also possible that the healthcare costs rebound due to rescheduling, attenuating the magnitude of the decreases demonstrated in Tables 4 and 5. To incorporate lagged effects into our analysis of social net benefits, we estimate FRD regressions with a rolling sum of 3-day healthcare expenditures as the dependent variable (Equation (8)).[27]

[26] For comparison, Barwick *et al.* (2024) finds that, following monitoring programmes, a doubling of air pollution reduces credit card transactions by three percentage points.

[27] In the US context, rescheduling hospital visits within a 3-day time horizon would be infeasible in many cases. However, rescheduling is much less onerous in South Korea than in the United States. First, many primary care outpatient visits in South Korea happen without reservations; 70% of outpatient visits were made without pre-scheduling, and an additional 29% of patients could receive care on the same day they wanted KIHASA (2020). Only 1% could not receive care on their desired day. Second, the average consultation time in South Korea is low. At tertiary hospitals, which have the longest consultation times, average consultation time is around 3 to 4 minutes (4.2 minutes in Lee *et al.* (2014) and 3.3 minutes in Kwon *et al.* (2017)). Third, patients typically wait about 16 minutes KIHASA (2020), even without reservations. These factors contribute to

Table 7 reports results from estimating Equation (8). The coefficient magnitudes are larger than the corresponding coefficients presented in Tables 4 and 5, by factors ranging from 2.5 to 2.8 when focusing on all age groups and the first row of each sub-panel. The larger magnitudes can be partially explained by the nature of the South Korean AQAS, where many alerts last for at least one day beyond the initial date of issuance. Indeed, 80 of 134 unique alerts were issued for more than one day, and the average number of days per alert is approximately 1.87. Nevertheless, this figure cannot fully explain the differences in the magnitudes mentioned above, which hints at potential lagged decreases in healthcare costs even after an alert expires.

To avoid double counting benefits when we conduct policy simulations, the estimation sample for the second row in each panel of Table 7 omits alert days on which the previous day was covered by an alert.[28] In general, the coefficients decrease in size; nevertheless, they are still approximately comparable in overall magnitude. Compared to the results in the first rows, the overall effects decrease by 8% to 19%. Compared to estimates of contemporaneous effects, the coefficients are 2.2 or 2.5 times larger — figures above 1.87, or the average number of days per alert.

Lagged effects are also possible with credit card transactions, since consumers may intertemporally substitute their consumption activities. Table 8 presents results from estimating Equation (8) using credit card transactions. Although the coefficients in the first row generally do not achieve statistical significance, the fashion outlet category presents a marginally significant coefficient ($t = 1.85$), approximately 1.5 times larger than the contemporaneous effects estimate. This figure, lower than the average number of days per alert (1.87), suggests a small degree of intertemporal substitution, which would potentially reduce the net impact of air pollution alerts on consumption activities.[29]. Analogous to the results of healthcare expenditure analysis, the estimation of coefficients while eliminating alert days that immediately follow another alert day yields comparable point estimates, confirming that the findings are not attributed to potential double counting.

the flexibility of the South Korean healthcare system, allowing patients to visit clinics with only 24 to 48 hours notice. Nevertheless, we ran FRD regressions extending the time window to test for longer dynamic impacts. Specifically, we utilised a weekly aggregated RV and a 7-day rolling sum of the dependent variables. We presents the results in the next subsection.

[28]Consider an alert that lasts for two consecutive days. If both days from the alert are within the analysis bandwidth (defined by PM levels), the 3-day impacts of both alert days would be counted separately even though there is a two-day overlap between the two 3-day sums of healthcare spending. By dropping alert days that immediately follow an alert day, we filter out any secondary sets of overlapping 3-day sums for that particular alert.

[29]In the context of our model, intertemporal substitution in consumption activities reduces the sensitivity of $b(a)$ to a, if a represents activity for a single day. Alternatively, if consumption activities feature perfect intertemporal substitution over a several-day period, then a could represent activity summed over several days.

5.3 Robustness

We first test the robustness of our results by incorporating health expenditures from outpatient visits to tertiary hospitals. The medical referral process, which is required when visiting a tertiary hospital, generates a gap between the onset of symptoms and actual outpatient visits. We excluded those visits in our main analysis to clearly identify the immediate impacts of the AQAS, and we do not expect their inclusion to qualitatively change our main results since these expenditures comprise less than 15% of the total outpatient expenditures. Appendix Tables A7 and A8 confirm that our estimates are robust to the inclusion of tertiary outpatient visits. The coefficients and standard errors are of similar magnitudes to the analogous estimates in Tables 4 and 5.

Table 9 estimates a variety of alternative specifications to demonstrate the robustness of our results to specification choices. The most important modelling choice in most RD studies is the bandwidth for the local linear regressions. The first three rows in each table present reduced-form estimates utilising bandwidths of 16, 20 (our baseline specification), and 24.[30] In the top panel of Table 9, the magnitude and significance of respiratory-illness effects for minors and all age groups remain stable across all bandwidths. In the bottom panel, cardiovascular-illness effects for both adult groups and all ages are statistically significant for the smaller bandwidth, in line with the main results, but they become insignificant for the larger bandwidth. We note, however, that the motivation for choosing a larger bandwidth is to trade off increased bias for a smaller standard error; in this context the standard error actually rises with the bandwidth, likely due to the increased mean-squared error associated with higher pollution levels (see Figure 4), suggesting little gain from using a larger bandwidth. Appendix Table A9 reports analogous results for the dynamic effects specification; for both disease categories, the pooled age-group results remain statistically significant at all three bandwidths (16, 20, and 24).

Another concern in our context is the timing of advisories. In some instances, alerts may be triggered early in the morning but cancelled by 9 am; in others, they may not be triggered until the evening. In either case, we would not expect the alerts to have meaningful effects on behaviour. The last row in both panels of Table 9 filters out days with alerts cancelled before 9 am or triggered after 7 pm. As expected, the coefficients become slightly larger in magnitude and remain statistically significant.

[30] 2SLS estimates are identical to the reduced-form estimates after rescaling coefficients and standard errors by 1.6, as the first-stage estimates are insensitive to bandwidth choice.

In Appendix A3, we report a wide range of alternative specifications and robustness checks for the main results. Briefly, we find no visual evidence of manipulation of the running variable near the RD threshold (Appendix Figures A1 and A2) or a discontinuity in the control variables (Appendix Figures A4, A5, A6, and A7).[31] Removing alert days on which the air quality warning was issued too late or too early yields similar results, as does accounting for the asymmetry in thresholds for alert issuance and cancellation (Appendix Table A10).[32] We demonstrate that controlling for a quadratic of the running variable, a quadratic of the temperature variable, or the air quality variables (PM10, PM2.5, PM10 and PM2.5, and the Air Quality Index (AQI)) does not change our main conclusions (Appendix Tables A11 and A12). Our findings are also robust to using alternative sets of time fixed effects (Appendix Table A11) or clustering at different levels (Appendix Table A13). To test for spatial spillovers of alerts, we estimate the FRD regressions with the largest running variable observed in an adjacent alert region; Appendix Table A14 finds no effect of an adjacent region's alert on the focal region's health expenditures. Additionally, we perform falsification tests by changing the RD threshold by 20, 30, or 50 units from the true threshold. Appendix Table A14 demonstrates no statistically significant results at these placebo thresholds. Finally, pre-trend analyses (Appendix Table A18) help us rule out the possibility that people may anticipate air pollution alerts due to the availability of air quality forecasts.

To test whether our estimates are affected by the possibility of rescheduling healthcare visits beyond a 3-day time horizon, we perform an FRD analysis with weekly aggregated data. We also extend our analysis to a 7-day rolling sum of healthcare expenditures. Appendix Table A15 shows that the results with weekly-level data are comparable to our main results, and that the health impacts from the extended 7-day analysis are larger than those from the baseline 3-day analysis.

We also investigate whether the decrease in health expenditures is due to improvements in cardiovascular and respiratory health rather than a general reduction in healthcare visits. To discriminate between these two mechanisms, we analyse day-of-alert health expenditure changes for digestive illnesses,[33] which we hypothesise will be less affected

[31] We also conducted the statistical manipulation test based on a local polynomial density estimation technique proposed by Cattaneo *et al.* (2018) and do not reject the hypothesis that the density of the score does not change discontinuously at the cut-off point. For 2016−2017, the p-values are 0.509, 0.554, and 0.690 for the bandwidths 16, 20, and 24, respectively. For 2017−2018, the corresponding p-values are 0.248, 0.257, and 0.206, respectively.

[32] Given that adopting protective measures often requires preparation (e.g. rescheduling commutes), it is likely that these measures are adopted more readily when alerts are issued early in the morning compared to later in the day. To examine whether the timing of alert issuance affects health outcomes, we created binned scatter plots for each healthcare expenditure outcome based on four time windows: before 10 AM, 11 AM to 2 PM, 3 PM to 6 PM, and after 7 PM (Appendix Figures A9 and A10). The results for the time windows after 7 PM are noisy, while the earlier windows show clearer patterns. For respiratory symptoms, there is a distinct discontinuous jump around the threshold and a sequential continuous response, most evident in the earliest time window. This suggests that earlier alerts lead to a higher adoption of protective measures, thereby reducing health impacts from air pollution.

[33] Category K in the ICD-10.

by air pollution.[34] Appendix Figure A8 shows RD scatter plots for respiratory, cardiovascular, and digestive diseases. There is no discontinuity in digestive-illness spending for minors and older adults around the RD threshold, and the corresponding results in Appendix Table A16 show statistically insignificant coefficients. For adults, we observe a modest discontinuity with a RD coefficient at the margin of significance ($t = -2.1$), but the absolute magnitude (1.37) is much smaller than for respiratory (3.78) or cardiovascular (2.83) illnesses. The proportionate effects are also smaller: around 15% of mean spending compared to 26% and 23% for respiratory and cardiovascular spending, respectively. These findings suggest that reduced healthcare visits are not the primary mechanism driving our results.

We conduct robustness checks for the credit card transaction results as well. The first three rows of Table 10 present coefficients and standard errors with different bandwidths. The fashion outlet results rema in significant for all bandwidths, showing comparable coefficients, though they decrease in magnitude for the largest bandwidth. The fourth row presents results using the data that filter out days with advisories issued too late or cancelled too early. The estimates remain similar in magnitude and significance to those in Table 6. The fifth row uses only data from before the change in the threshold for PM2.5 advisories at the end of June 2018. In this case, the travel sector's magnitude increases compared to the baseline result with bandwidth 20, but it does not attain statistical significance. The remaining estimates are similar to those in Table 6.

5.4 Other Health Outcomes

To determine whether alerts affect more or less severe health outcomes, we compare the reductions in morbidity spending with changes in other health outcomes, specifically mortality rates, per capita emergency expenditures, and the number of hospital visits. Appendix Table A17 presents the mortality results. Unlike the findings for morbidity spending per person, the FRD coefficients for mortality are statistically insignificant, with some point estimates being positive.[35] These null results suggest that individuals facing severe health conditions may engage in avoidance behaviour even without alerts. However, we must also consider the possibility that our mortality analyses are underpowered. For example, the standard error on the respiratory mortality coefficient across all age groups is 32% of the respective mean, and the standard error on the cardiovascular mortality coefficient across all age groups is 24% of the

[34] Changes on the day of the alert provide the strongest test of the hypothesis that our effects represent a mechanical reduction in healthcare visits due to avoidance behaviour; changes over longer time horizons may be attenuated by rescheduling opportunities.

[35] When we filter out the running variable within a bandwidth of 20, cardiovascular and respiratory mortality for "minors" has only 4 and 1 non-zero district-by-day observations, respectively. Thus, we do not report and discuss the impact of the alerts on minors' mortality.

respective mean. Thus, even if the mortality effect coefficients were the same proportional size as the spending effect coefficients, we would be unable to reject the null hypothesis.

We also analysed morbidity spending per person for emergency visits using FRD regressions. The results, shown in Appendix Table A20, reveal no significant effects on emergency visits. This suggests that individuals more likely to visit the emergency department for respiratory and cardiovascular diseases are more vulnerable and thus may be more aware of pollution levels. Nevertheless, we must again consider the possibility that our analyses for these outcomes are under-powered. For example, the respiratory all-age emergency spending standard error is 42% of the respective mean, and the cardiovascular all-age emergency spending standard error is 46% of the respective mean.

Overall, the mortality and emergency spending results complement our morbidity spending findings, highlighting the types of health outcomes that may benefit from an AQAS. Specifically, actively disclosing information on poor air quality appears to have a more consistent impact on less severe illnesses than on emergent or fatal cases.

Additionally, we examined the impacts of the AQAS on the number of hospital visits. Although our main datasets do not include visit counts, we obtained data on outpatient visits related to asthma and rhinitis. The results indicate a decrease in the number of visits, but this decrease is not statistically significant (Appendix Table A19). This may suggest that health benefits accrue at the intensive margin rather than the extensive margin; that is, patients who continued to visit outpatient facilities reduced their expenditures per visit, rather than forgoing the visit altogether.

6 Discussion and Economic Impacts

We highlight several of our empirical findings. First, our results provide new insights on responses across broader disease categories. Previous studies on air pollution alerts have primarily analysed respiratory symptoms — Neidell (2009) found effects on asthma-related hospitalisations, and Janke (2014) found effects on respiratory emergency admissions.[36] Our findings suggest that effects are not limited to respiratory diagnoses, as cardiovascular disease spending of adults also decreased significantly. At the same time, our credit-card spending results corroborate previous findings that alerts can induce avoidance behaviour for certain activities.

Second, our results yield novel findings of alerts' effects for adults. Neidell (2009) and Janke (2014) found statistically significant impacts of air quality warnings on health and avoidance behaviour (e.g. zoo attendance) for the

[36] Chen *et al.* (2018) also find weak evidence of an effect on asthma-related visits.

youngest ages.[37] In contrast, our results reveal that the benefits of air quality warnings may not be restricted to minors. While minors' health spending was reduced by 12.6 million USD, prime-age and older adults also demonstrated significant health expenditure decreases due to alerts, amounting to 20.4 million and 8 million USD, respectively. Furthermore, the reductions in credit-card transactions correspond to activities undertaken primarily by adults. Our results imply that alerts can motivate all age groups to take appropriate avoidance measures, reducing the negative impacts of air pollution.

Our results represent effects of avoidance behaviour, but there are two mechanisms that could underlie these effects. First, avoidance behaviour could improve health, reducing expenditures. Second, avoidance behaviour could directly reduce healthcare visits if individuals specifically avoid leaving their residences for medical appointments. In theory both mechanisms generate positive benefits via reductions in expenditures, but rescheduling of visits or longer-term health complications could attenuate the benefits if the second mechanism drives the results.

In practice, the evidence suggests that the first mechanism seems likely to be the primary one. First, we do find evidence of general avoidance behaviour in the credit-card transaction data — individuals appear to avoid outdoor shopping experiences. This implies that they are not solely avoiding healthcare visits. Second, the multi-day dynamic effects on health expenditures are considerably larger than the contemporaneous effects. For example, if we study only alert episodes with a single alert day, we find 3-day dynamic effects that are 2.8 times larger than contemporaneous effects for youth respiratory expenditures and 6.0 times larger for elderly cardiovascular expenditures. If postponed healthcare visits were the primary mechanism, we would expect the 3-day dynamic effects to be weakly smaller than the contemporaneous effects, as both visit rescheduling and complications from avoided visits would attenuate the first-day effects. In addition, we find that the impacts of alerts on healthcare expenditures still remain statistically significant over a 7-day time horizon, indicating that our findings on health benefits are unlikely to be attenuated by possible rescheduling behaviour (Appendix Table A15).

To calculate total benefits from decreased healthcare expenditures, we first tabulate the number of people exposed to the alerts in the seven major South Korean cities. As the FRD estimates measure the reduction in health spending per capita, we multiply the FRD estimates by the population affected by the alerts. Based on the coefficients of spending in

[37] Mullins and Bharadwaj (2015) and Aguilar-Gomez (2020) both find effects of pollution warnings on elderly health outcomes. In their contexts, however, the warnings also reduce pollution levels, suggesting a direct effect of pollution on health. Our study estimates the pure effect of alerts on health expenditures, as measured PM does not change at the RD threshold. Furthermore, our analytic framework would not apply to policies which reduce emissions, as these policies entail additional implicit net costs.

the both disease categories across all ages, which sum to 9.1 cents per capita, the total reduction in health expenditures during 2016−2017 in seven major cities was approximately 41 million USD. This estimate considers only the alerts' contemporaneous effects. Incorporating dynamic effects into the calculation increases the reduction by about one third, for a total reduction in health expenditures of approximately 52 million USD.[38]

Combined with our theoretical framework, the empirical findings presented above can provide a lower bound on the gross benefits from the AQAS. Specifically, our framework implies that the reduction in *public* healthcare expenditures represents a lower bound on the gross benefits of the AQAS. Our health expenditure data contain the sum of private copayments and public coverage, with an approximate ratio of 7:3. Thus, approximately 70% of the FRD coefficients represents a reduction in public expenditures. Applying this share to the total expenditure reductions computed above yields lower bounds on gross benefits that amount to 18.4 million USD (respiratory) and 10.2 million USD (cardiovascular) respectively. Figure 6 presents lower bounds on gross benefits by age group, with non-elderly adults realising the largest gross benefits. Benefits using estimates from the dynamic specification (Table 7) are somewhat larger than those using estimates from the contemporaneous specification (Tables 4 and 5) (24.5 million USD for respiratory and 12.2 million USD for cardiovascular), as the former captures a larger change in health expenditures than the latter.

To quantify AQAS net benefits and benefit-cost ratios, we collected reports on environmental expenditures from the websites of the seven major cities' municipal governments. The cost of managing the alert system in 2017 was estimated at 2 million USD. To be conservative we include a wide range of expenditures in this estimate, from the price of sending alerts via text messages to the maintenance cost of the air pollution monitors.[39] As we could not obtain complete expenditure information for 2016, we assume that costs would be similar to 2017, yielding a total cost of 4 million USD in 2016−2017.[40] The cost is considerably lower than the total health benefit calculated above, 28.6 million USD, yielding an approximate benefit:cost ratio of 7.1:1 and a net benefit of 24.6 million USD for 2016−2017.

[38] For this calculation we must assume that the benefits of alerts do not decrease as the pollution level rises above the RD threshold, as our RD estimates are local to the RD threshold. This assumption seems plausible, however, as health damages must weakly increase with pollution, so the benefits of avoidance behaviour likely increase as well.

[39] Appendix Tables A21 and A22 list the expenditure items and the sum of these expenditures by metropolitan city, respectively. See Appendix A2 for additional details.

[40] Government expenditures may vary year-to-year. We collected similar information for 2018 and found that total 2018 expenditures were approximately 2.8 million USD.

Incorporating the dynamic effects further increases the net benefits and the benefit:cost ratio to 32.7 million USD and 9.2:1, respectively. [41]

One potential cost omitted from our analyses is the possible reduction in economic activity that arises from alert-induced avoidance behaviour. To quantify this potential cost, we estimated a version of Table 6 that uses raw credit card spending as the outcome (Appendix Table A23). The point estimates in this table, while not statistically different from zero, imply that an alert reduces daily spending by 25.7 cents per card member.[42]

Taken at face value, the $0.257 figure is a transfer. To translate it into a welfare measure, we must consider the consumer and producer surpluses associated with purchases. Consumer surplus we may ignore because individuals already account for the loss in consumer surplus when considering the costs of avoidance behaviour (i.e. it is contained in the ΔU_i term in our model, which we conservatively bound at zero in Section 3). Producer surplus depends on gross profit margins in the South Korean restaurant, retail, and entertainment industries.

We could not find any authoritative figures on these margins. However, we reviewed the annual reports of two large, publicly-listed South Korean firms operating in the restaurant, food and food services, retail, logistics, and entertainment industries. These firms reported gross profit margins of 29% to 34% in recent periods, so we assume that one-third of spending represents producer surplus.[43] Finally, as discussed in Section 5.2, we must make assumptions about intertemporal substitution; purchases not made today will presumably result in some additional spending at a later date. We refer to these future induced purchases as "rebound effects." Absent detailed data we consider rebound effects on contemporaneous spending reductions of 25%, 50%, and 75%.

Combining these figures we estimate the per capita welfare effect of an alert via reduced spending to be as follows when assuming a 50% rebound effect: $\$0.257 \cdot 0.33 \cdot 0.5 = \$0.042$. The analogous figures for a 75% or 25% spending rebound are $0.021 and $0.064 respectively. Notably, the largest of these figures is equivalent to the lower bound on

[41] Utilising the 2018 expenditures as the reference cost leads to the total costs of 5.6 million USD. The corresponding net benefit is about 23 million USD, with a benefit:cost ratio of approximately 5.1:1. When considering dynamic effects, the total net benefit is 31.1 million USD, and the associated benefit:cost ratio is 6.6:1.

[42] Two factors working in opposite directions could make the 25.7 cent figure an over or underestimate of the effect of an alert on average contemporaneous daily spending. First, card members may make purchases not captured in our data. For example, they may have cards from multiple firms, some of which are not captured in our data, or they may use cash. This factor causes us to underestimate the per capita effect. Second, card members likely spend more than the average South Korean individual; in particular, children are not card members and have low average spending. This factor causes us to overestimate the per capita effect. Absent detailed data on either factor, we assume that they roughly offset each other.

[43] The first firm is the Born Korea Company, which owns a range of restaurant chains operating primarily in South Korea. It reported a gross profit margin of 34% in the latest 12 months (Yahoo! Finance, 2025). The second firm is the CJ Group, a South Korean conglomerate that generates approximately 85% of its revenue from food and food services (restaurants and cafes), retail, logistics, and entertainment (e.g. movie theaters). These are representative of the types of establishments that we might expect credit card spend at, and the firm reported a gross profit margin of 29% (CJ Corporation Investor Relations, 2024).

per capita benefits from reduced healthcare expenditures described earlier ($\$0.091 \cdot 0.7 = \$0.064$), and the smallest is only one-third of the lower bound on per capita benefits. Overall, the analysis suggests that concerns about the economic impacts of alerts on retail activity, such as those described in Min (2019), are not a sufficient reason to halt the alert programme.

We also explore the potential gains from expanding the alert system's covered range. For this analysis we consider two scenarios. In the first scenario, we assume that advisories are issued on *all* days on which the running variable exceeds zero (i.e. the fuzzy RD becomes a sharp RD). In the second scenario, we assume that the government tightens the advisory criterion for PM2.5 from 90 $\mu g/m^3$ to 75 $\mu g/m^3$ during our analysis period (2016−2017). This corresponds to an actual policy the government enacted starting July 2018. In both scenarios we assume that the magnitudes of the alerts' effects remain similar to our FRD estimates.[44,45]

We find that expanding the alert system's coverage could yield significant benefits. Figure 7 presents net benefits for the baseline scenario and the two alternative scenarios. Issuing alerts whenever the RD threshold is exceeded would have reduced health expenditures by an additional 5.7 million USD (Scenario A) during our sample period, bringing the total reduction to 42.4 million USD (right panel). Lowering the threshold for alert issuance from 90 $\mu g/m^3$ to 75 $\mu g/m^3$ (for PM2.5) would have reduced total health expenditures by 76.5 million USD (Scenario B) during our sample period (right panel), or a 109% increase from the baseline policy. Notably, this corresponds to the current alert criteria, implemented in July 2018. In all scenarios the benefits greatly exceed the costs, indicated by the solid or dashed horizontal lines for comparison.

Two limitations are worth noting. First, we cannot measure the potential psychological impacts of operating the alert system. For example, issuance of alerts may cause increased anxiety and stress amongst vulnerable populations that would not exist if they remained ignorant. On the other hand, the system's existence might relieve stress by resolving uncertainty regarding daily air quality or relieving vulnerable individuals of the daily hassle of checking air quality forecasts themselves. Incorporating these possibilities could increase or decrease net benefits. Second, even

[44] Concerns may arise regarding "alert fatigue" in these simulations, as the number of days with alert issuance necessarily increases. It is worth noting, however, that the number of treated days remains modest even in those scenarios. The rates of alert district-days are 2.67%, 2.81%, and 4.57% in the baseline scenario, Scenario A, and Scenario B, respectively. We thus assume that alert fatigue does not become a serious concern in these simulations.

[45] A related concern is that non-advisory days on which the running variable exceeds zero ("non-compliant" days) systematically differ from advisory days on which the running variable exceeds zero ("compliant" days). Indeed, as shown in Table 1, weather conditions influence alert issuance. We thus compared precipitation and temperature across non-compliant and compliant days falling within the running variable interval of [0, 40]. Average precipitation and temperature on non-compliant days were 0.65 mm and 3.121 C, respectively, with standard deviations of 5.12 mm and 2.24 C. Average precipitation and temperature on compliant days were 1.215 mm and 7.618 C, respectively, with standard deviations of 6.12 mm and 3.79 C. We assume that these modest meteorological differences would not yield large differences in the alerts' impacts.

within South Korea our estimates apply to alert systems operated within urbanised areas containing approximately half the country's population. Since the system did not operate in most rural areas, we cannot directly analyse its impacts in those areas. Furthermore, we are hesitant to project its effects to those areas as they have different densities and types of healthcare facilities. Nevertheless, we believe the results remain policy-relevant as both pollution and alert systems tend to be concentrated in urbanised areas.

7 Conclusion

Combining our RD estimates with a theoretical framework, we find lower bounds on the benefits of the South Korean air pollution alert system that greatly exceed the costs of operating the system. Given the insignificant changes in average PM levels at the RD threshold, we interpret our results as a "pure" effect of avoidance behaviour, rather than a combined effect of avoidance behaviour and reduced ambient pollution levels. Our results thus stand in contrast to those from some recent work, which found effects only in contexts in which alerts were combined with policies to reduce ambient pollution levels (Mullins and Bharadwaj, 2015; Aguilar-Gomez, 2020). Our theoretical framework is likely to prove applicable in other settings in which individuals endogenously respond to information provision but there are substantial externalities. For example, one might bound the benefits of restaurant hygiene grade cards using the reduction in insurance-covered hospitalisation costs (Jin and Leslie, 2003) or the benefits of electricity usage information using the reduction in environmental damages (Jessoe and Rapson, 2014).

Our study also highlights that manipulation of air pollution information for economic and political gains may be costly. If pollution alerts are not issued due to manipulation, the public may not engage in welfare-enhancing avoidance behaviours. Despite the alerts' benefits, governments may have incentives to distort air pollution information if they worry about temporary economic declines from decreased outdoor activities (Min, 2019). Furthermore, local government officials may have incentives to manipulate air pollution levels for more favourable evaluations (Andrews, 2008; Chen *et al.*, 2012; Ghanem and Zhang, 2014; Zou, 2021). Our findings imply that these distortions can reduce public health and generate additional healthcare expenditures.

A primary limitation of our study is that the results apply specifically to South Korean metropolitan areas. While these areas are economically important in themselves, with a combined population of over 23 million, our estimates may not generalise to other countries. Nevertheless, there are good reasons to believe that our main qualitative finding

— an AQAS can generate meaningful net gains — applies to other contexts. First, previous studies have found significant reductions in some types of healthcare utilisation due to air-quality alerts. For example, Neidell (2009) found that alert-induced avoidance behaviour decreased Los Angeles asthma hospitalisations between 12 and 60 percent, and Janke (2014) found that even alerts for "moderate" pollution levels reduced asthma admissions by 8 percent in England. Furthermore, South Korean healthcare prices are remarkably low by developed-country standards. For example, relative to South Korea, 2016 per capita health expenditures (PPP-adjusted) were 60 percent higher in the United Kingdom, 77 percent higher in France, 113 percent higher in Sweden, and 270 percent higher in the United States (Lorenzoni and Koechlin, 2017). Thus, even if avoidance behaviour or high pollution levels are less prevalent in other countries than in South Korea, overall impacts on health expenditures may still be of similar magnitude.

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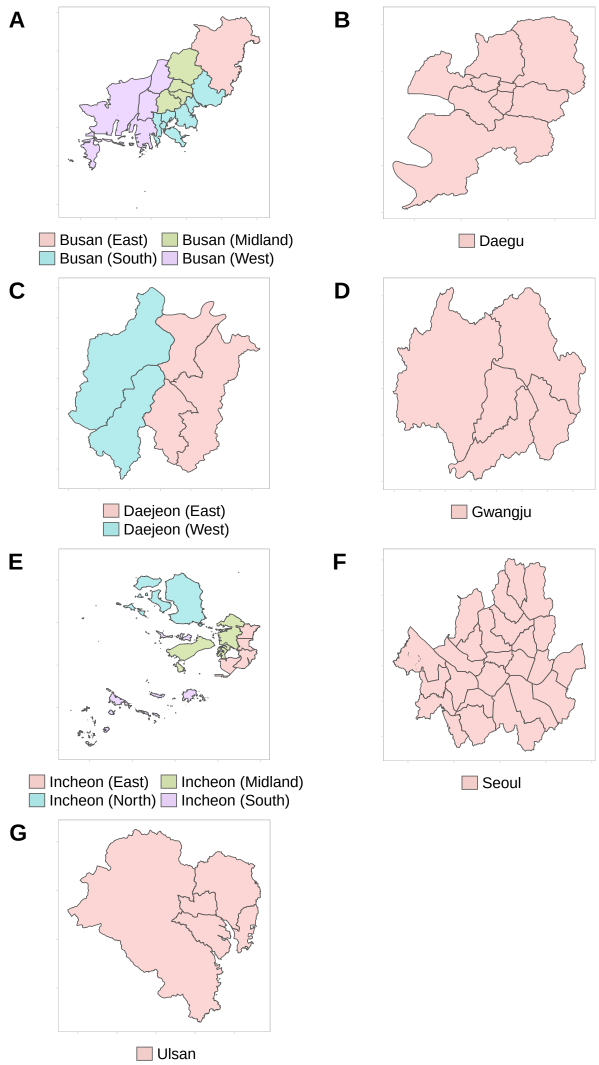


Figure 1. *Alert Clusters for Seven Major Cities in South Korea*

Notes: Panels A through G depict the alert clusters in Busan, Daejeon, Daegu, Gwangju, Incheon, Seoul, and Ulsan respectively. Different colors (in a given city) represent different alert clusters. Border lines separate districts in each major city area.

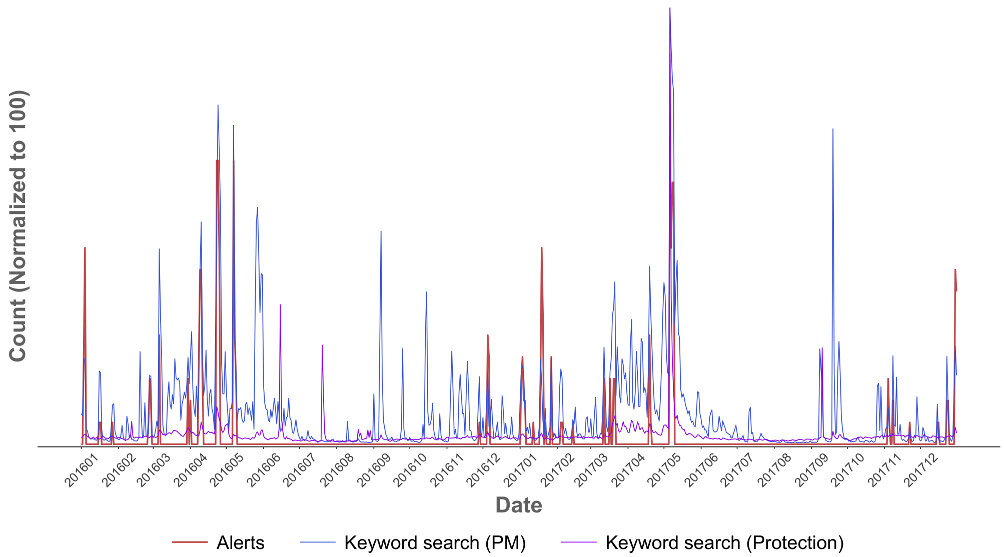
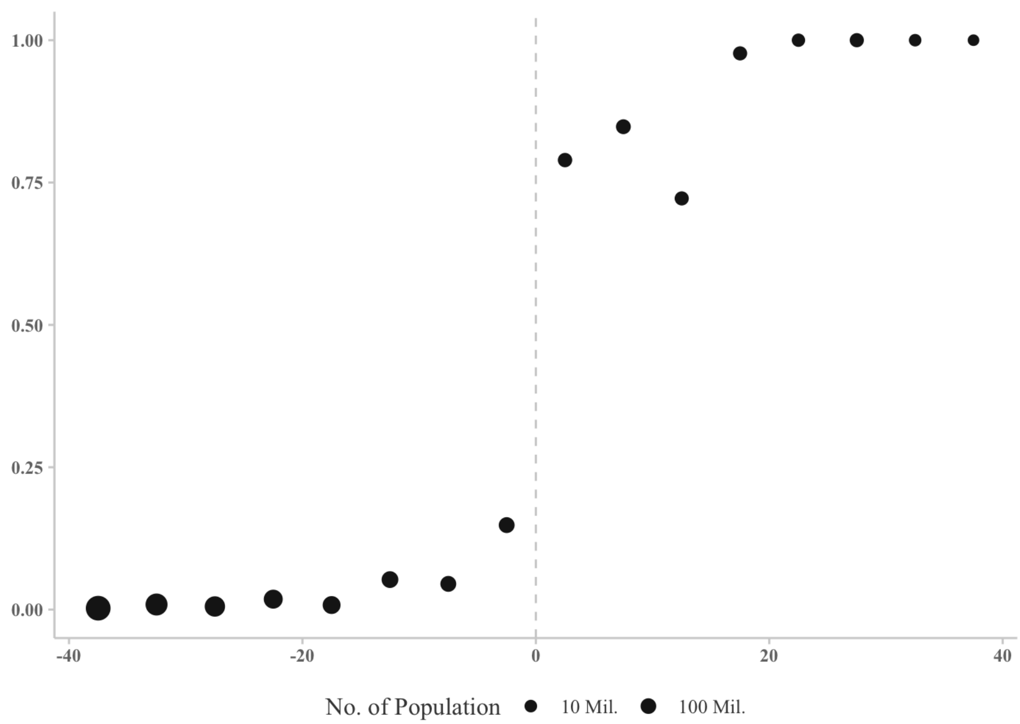


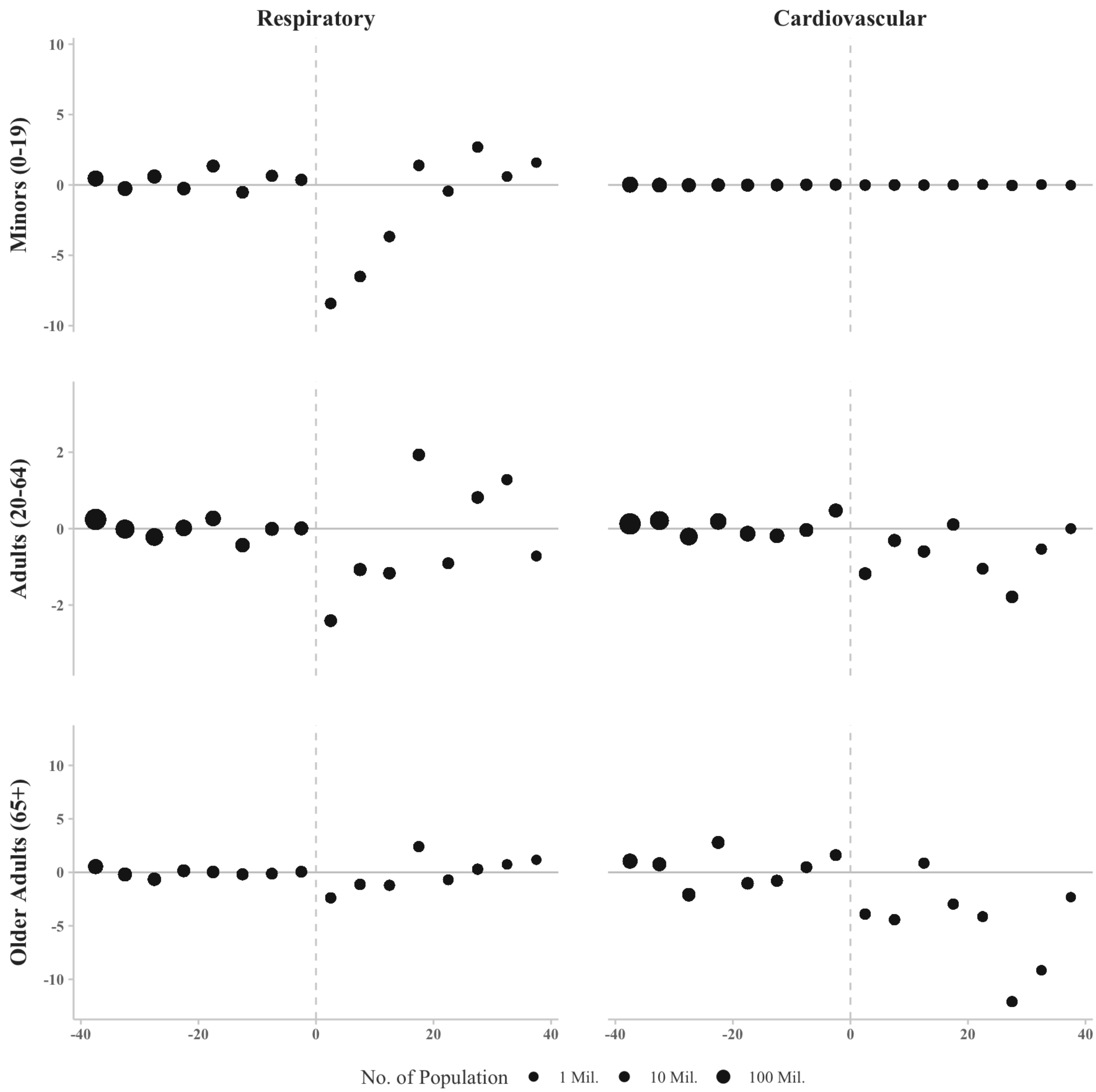
Figure 2. *Alert counts and keyword search results related to air quality information*

Notes: (1) The red series represents the daily count of air quality alerts, (2) the blue series represents searches for "air pollution alert", "particulate matter", or "air quality", and (3) the purple series represents searches for "facemask", "air purifier", or "public transit" on NAVER. The maximum value of daily keyword search counts is set to 100.



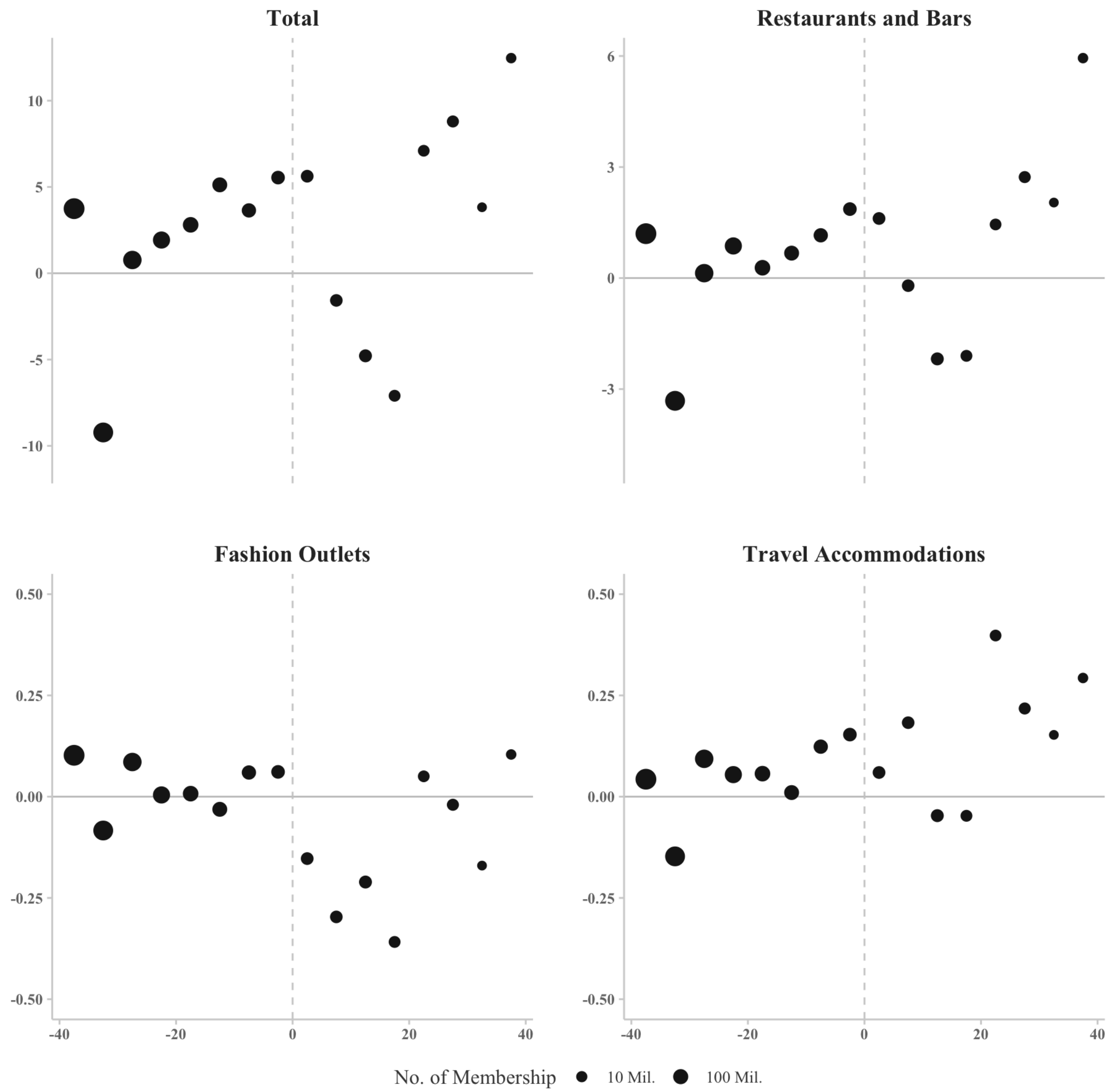
Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the average probability of a particulate matter advisory. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM).

Figure 3. *Treatment Discontinuity*



Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the per capita level of residualised health expenditures, in US cents (11.5 KRW = 0.01 USD). The residualisation was performed with respect to day-of-week, year-by-month, holiday, and district fixed effects. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM). The period of the analysis is 2016−2017.

Figure 4. *Outcome Discontinuity - Health Spending*



Notes: Each point represents the number-of-membership-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the number of credit card transactions per 1,000 members. The residualisation was performed with respect to day-of-week, year-by-month, holiday, one-day-before-holiday, and district fixed effects. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM). The period of the analysis is 2017−2018.

Figure 5. *Outcome Discontinuity - Credit Card Transactions*

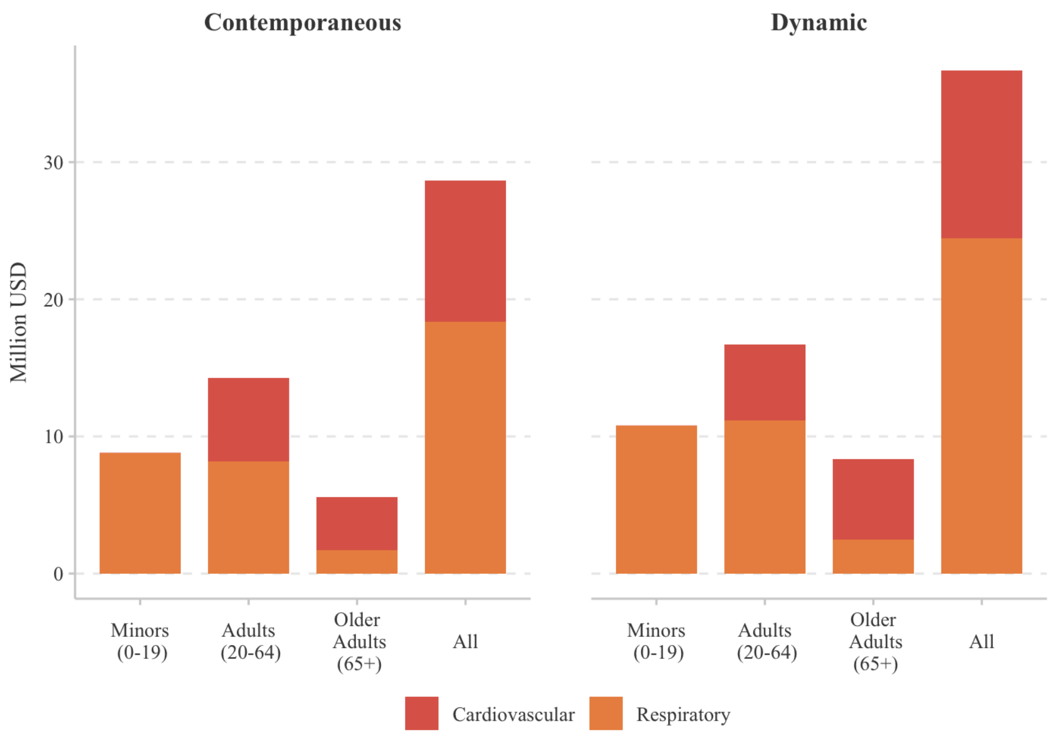


Figure 6. *Potential Health Benefits by Age Group*

Notes: The left (right) panel plots lower bounds on gross benefits by age groups using estimates from Tables 4 and 5 (Table 7). To bound gross benefits we scale the table coefficients by the average share of health expenditures that are publicly covered (70%).

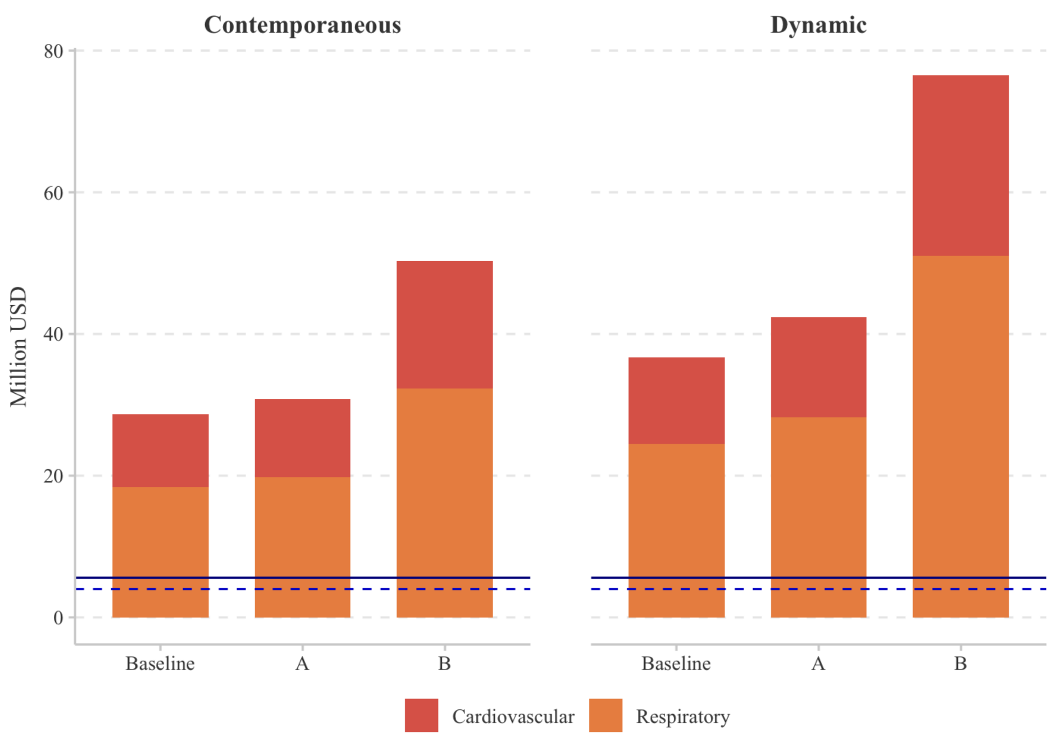


Figure 7. *Cost-Benefit Comparison and Potential Health Benefits*

Notes: The left (right) panel plots lower bounds on gross benefits by scenario using estimates from Tables 4 and 5 (Table 7). To bound gross benefits we scale the table coefficients by the average share of health expenditures that are publicly covered (70%). "Baseline" represents the policy during the sample period, "Scenario A" represents a policy that triggers an alert whenever the running variable crosses the relevant threshold, and "Scenario B" represents a policy that lowers the PM2.5 threshold to the value adopted in July 2018. The solid (dashed) line represents the total system maintenance cost for the analytic period (2016−2017) using 2018 (2017 for the dashed line) as the reference year for costs.

Table 1. *Thresholds and Guidelines of South Korean PM Advisories*

	PM Advisory Thresholds	
	PM10	PM2.5
Issuance	Over 150 $\mu g/m^3$ for 2 hours	Over 90 (75)† $\mu g/m^3$ for 2 hours
Cancellation	Under 100 $\mu g/m^3$ for 1 hour	Under 50 (35)† $\mu g/m^3$ for 1 hour

Target Groups	**Guidelines**
General Population	· Stay indoors and reduce outdoor activities · Wear hygiene masks when you go outside · Reduce emissions (e.g. use public transportation) · Avoid roadways and construction sites
Children & Teenagers	· Reduce or forbid outdoor classes. · Replace outdoor activities with indoor activities. · Enhance the hygiene management of dining facilities
The Elderly	· Enhance the hygiene management of dining facilities

†The numbers in the parentheses indicate the thresholds that were changed after June 28, 2018.
Source: AirKorea, Korea Environment Cooperation
https://www.airkorea.or.kr/, accessed on Sep 30, 2021

Table 2. *Summary Statistics*

		Full Sample				Bandwidth = 40			
Respiratory Expenditures		Mean	SD	Min	Max	Mean	SD	Min	Max
Minors	(0−19)	46.0	23.2	0	201.8	50.1	24.9	0	171.6
Adults	(20−64)	13.8	7.8	0	59.6	15.2	8.3	0	50.6
Older Adults	(≥65)	21.3	14.0	0	239.8	23.3	14.8	0	147.2
Cardiovascular Expenditures									
Minors	(0−19)	0.1	1.7	0	316.5	0.1	3.1	0	316.5
Adults	(20−64)	12.2	7.5	0	107.6	12.0	7.4	0	107.6
Older Adults	(≥65)	65.1	42.7	0	268.9	64.3	42.1	0	248.5
Credit Card Transactions									
Total		346.8	80.7	0.02	562.6	341.8	82.8	0.02	554.1
Restaurants & Bars		110.6	30.1	0	199.4	109.0	30.2	0	197.8
Fashion Outlets		4.9	1.5	0	11.8	4.8	1.5	0	10.7
Travel		6.1	2.0	0	16.0	6.0	2.0	0	15.4
Treatment & Covariates									
Alert		0.03	0.2	0	1	0.1	0.3	0	1
PM10	($\mu g/m^3$)	44.5	22.0	2.8	253.9	68.4	18.8	19.3	170.3
PM25	($\mu g/m^3$)	25.1	12.9	1.6	109.5	41.8	13.4	6.6	109.5
Precipitation	(mm)	3.1	12.2	0	313.8	1.1	4.5	0	67
Temperature	(°C)	13.8	9.9	-16.4	32.5	10.7	8.3	-9.3	32

Notes: The number of district-day observations is 53,363 (73 districts, 761 days, 2016−2017) and 10,547 for the full sample and the sample based on a bandwidth of 40, respectively. The corresponding numbers for the credit card transaction data are 53,290 (73 districts, 760 days, 2017−2018) and 11,324, respectively. Morbidity spending variables are presented in US cents per capita (11.5 KRW = 0.01 USD). Credit card transaction is the number of transactions per 1,000 users. The summary statistics of the treatment variable and other covariates for 2017−2018 are presented in Appendix Table A2.

Table 3. *First-stage Regression Results*

	Bandwidth		
	16	20	24
$\mathbb{1}(RV >= 0)$	0.636 (0.091) [0.122]	0.622 (0.091) [0.114]	0.618 (0.092) [0.110]
Adjusted R^2	0.708	0.720	0.728
F-statistics	46.029	65.351	89.686
N	1,857	2,530	3,380

Notes: This table reports three first-stage estimates of the change in advisory likelihood when the running variable crosses the RD threshold. These estimates are based on coefficients from three separate local-linear regressions, one for each reported bandwidth. The dependent variable in all regressions is an advisory indicator, and the independent variable of interest is an indicator for the running variable being above the RD threshold. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample).

Table 4. *RD Results for Respiratory Diseases*

	Age Group			
	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Reduced form				
$\mathbb{1}(RV >= 0)$	−9.412	−2.346	−2.661	−3.624
	(2.326)	(1.391)	(1.674)	(1.437)
	[2.930]	[1.379]	[1.860]	[1.528]
RV	−0.074	−0.041	0.002	−0.038
	(0.143)	(0.043)	(0.062)	(0.056)
	[0.140]	[0.045]	[0.069]	[0.057]
RV $\cdot \mathbb{1}(RV >= 0)$	0.645	0.214	0.167	0.270
	(0.167)	(0.113)	(0.129)	(0.110)
	[0.225]	[0.078]	[0.108]	[0.088]
2SLS				
$\mathbb{1}(RV >= 0)$	−15.03	−3.777	−4.303	−5.829
	(4.709)	(2.195)	(2.753)	(2.397)
	[5.674]	[2.127]	[2.804]	[2.495]
RV	0.039	−0.013	0.035	−0.006
	(0.204)	(0.062)	(0.084)	(0.084)
	[0.170]	[0.055]	[0.083]	[0.071]
RV $\cdot \mathbb{1}(RV >= 0)$	0.596	0.204	0.156	0.254
	(0.243)	(0.112)	(0.127)	(0.120)
	[0.305]	[0.078]	[0.106]	[0.105]
Adjusted R^2	0.836	0.866	0.797	0.893
N	2,530			

Notes: This table reports results from four reduced-form local-linear regressions (top panel) and four 2SLS local-linear regressions (bottom panel). The dependent variable in all regressions is respiratory disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest in the reduced-form (2SLS) regressions is an indicator for the running variable being above the RD threshold (advisory indicator). All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample). The bandwidth is set to 20 in all regressions.

Table 5. *RD Results for Cardiovascular Diseases*

	Age Group			
	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Reduced form				
$\mathbb{1}(RV >= 0)$	−0.025	−1.757	−5.962	−2.026
	(0.021)	(0.513)	(2.070)	(0.573)
	[0.022]	[0.481]	[2.250]	[0.622]
RV	0.003	0.056	0.234	0.066
	(0.001)	(0.023)	(0.102)	(0.024)
	[0.001]	[0.020]	[0.113]	[0.026]
RV $\cdot \mathbb{1}(RV >= 0)$	−0.003	0.012	−0.083	0.006
	(0.002)	(0.035)	(0.158)	(0.038)
	[0.002]	[0.037]	[0.213]	[0.049]
2SLS				
$\mathbb{1}(RV >= 0)$	−0.040	−2.828	−9.641	−3.259
	(0.032)	(0.976)	(3.849)	(1.086)
	[0.035]	[0.767]	[3.677]	[1.008]
RV	0.003	0.077	0.308	0.090
	(0.001)	(0.029)	(0.104)	(0.030)
	[0.001]	[0.023]	[0.124]	[0.029]
RV $\cdot \mathbb{1}(RV >= 0)$	−0.003	0.005	−0.109	−0.003
	(0.002)	(0.042)	(0.152)	(0.039)
	[0.002]	[0.045]	[0.228]	[0.055]
Adjusted R^2	0.029	0.794	0.847	0.869
N	2,530			

Notes: This table reports results from four reduced-form local-linear regressions (top panel) and four 2SLS local-linear regressions (bottom panel). The dependent variable in all regressions is cardiovascular disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest in the reduced-form (2SLS) regressions is an indicator for the running variable being above the RD threshold (advisory indicator). All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample). The bandwidth is set to 20 in all regressions.

Table 6. *RD Results for Credit Card Transactions*

	Spending Category			
Reduced form	All	Restaurant	Fashion	Travel
$\mathbb{1}(RV >= 0)$	−4.062	−2.290	−0.391	−0.008
	(3.693)	(1.614)	(0.112)	(0.097)
	[3.927]	[1.913]	[0.116]	[0.119]
RV	0.444	0.204	0.020	0.012
	(0.181)	(0.079)	(0.005)	(0.005)
	[0.238]	[0.103]	[0.006]	[0.006]
RV $\cdot \mathbb{1}(RV >= 0)$	−1.239	−0.463	−0.024	−0.026
	(0.422)	(0.196)	(0.011)	(0.010)
	[0.431]	[0.201]	[0.011]	[0.009]
2SLS				
$\mathbb{1}(RV >= 0)$	−5.504	−3.103	−0.530	−0.011
	(5.159)	(2.277)	(0.159)	(0.132)
	[5.532]	[2.728]	[0.159]	[0.161]
RV	0.473	0.220	0.023	0.012
	(0.211)	(0.090)	(0.006)	(0.005)
	[0.249]	[0.106]	[0.007]	[0.006]
RV $\cdot \mathbb{1}(RV >= 0)$	−1.280	−0.486	−0.028	−0.026
	(0.418)	(0.194)	(0.011)	(0.010)
	[0.411]	[0.190]	[0.011]	[0.009]
Adjusted R^2	0.914	0.915	0.821	0.847
N	2,905			

Notes: This table reports results from four reduced-form local-linear regressions (top panel) and four 2SLS local-linear regressions (bottom panel). The dependent variable in all regressions is the number of credit card transactions per 1,000 users for the relevant item group, and the independent variable of interest in the reduced-form (2SLS) regressions is an indicator for the running variable being above the RD threshold (advisory indicator). All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month, day-of-week, holiday, and 1-day-before holiday fixed effects. The level of observation is the district by day, and observations are credit-card-user weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample). The bandwidth is set to 20 in all regressions.

Table 7. *Dynamic Impacts (Health Spending)*

	Age Group			
Respiratory Illness	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Dependent Variable				
3-day Spending	−38.220 (11.551) [13.769]	−10.703 (3.952) [4.661]	−12.977 (4.951) [6.624]	−16.125 (4.777) [5.935]
3-day Spending without Later Alert Days	−38.616 (12.422) [16.370]	−7.122 (3.258) [3.262]	−8.202 (4.427) [4.651]	−13.005 (4.473) [5.381]
Cardiovascular Illness				
Dependent Variable				
3-day Spending	−0.064 (0.086) [0.075]	−5.279 (1.907) [1.993]	−30.388 (12.229) [12.987]	−8.042 (2.657) [3.057]
3-day Spending without Later Alert Days	0.023 (0.100) [0.094]	−4.486 (1.706) [2.040]	−36.044 (11.875) [13.975]	−7.414 (2.515) [3.303]

Notes: This table reports results from 16 2SLS local-linear regressions. The dependent variable in all regressions is three-day respiratory or cardiovascular disease expenditures (from day t to day $t+2$) for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample). The bandwidth is set to 20 in all regressions. N = 2,530 in the first row of each panel. The last row of each panel drops from the sample advisory district-days on which the previous district-day experienced an advisory, which results in N = 2,228.

Table 8. *Dynamic Impacts (Credit Card Transactions)*

Credit Card Transactions	Spending Category			
	All	Restaurant	Fashion	Travel
Dependent Variable				
3-day Transactions	−16.395 (18.188) [21.106]	−2.149 (8.334) [9.501]	−0.784 (0.423) [0.483]	−0.294 (0.318) [0.404]
3-day Transactions without Later Alert Days	−17.988 (20.059) [22.766]	−2.538 (9.192) [10.234]	−0.775 (0.435) [0.510]	−0.247 (0.324) [0.447]

Notes: This table reports results from eight 2SLS local-linear regressions. The dependent variable in all regressions is the number of three-day credit card transactions per 1,000 users (from day t to day $t+2$) for the relevant item group, and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month, day-of-week, holiday, and 1-day-before holiday fixed effects. The level of observation is district by day, and observations are weighted by the population. Parentheses (square brackets) contain standard errors clustered by the running variable (day of the sample). The bandwidth is set to 20 in all regressions. N = 2,905 in the first row of each panel. The last row of each panel drops from the sample advisory district-days on which the previous district-day experienced an advisory, which results in N = 2,701.

Table 9. *Robustness Checks (Health Spending)*

	Age Group			
Respiratory Illness	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Sample Modification				
Bandwidth: 16	−12.504	−4.357	−4.468	−5.851
	(4.256)	(2.293)	(2.626)	(2.492)
	[4.779]	[2.071]	[2.752]	[2.391]
Bandwidth: 20	−15.033	−3.777	−4.303	−5.829
	(4.709)	(2.195)	(2.753)	(2.397)
	[5.674]	[2.127]	[2.804]	[2.495]
Bandwidth: 24	−14.049	−2.876	−2.393	−4.867
	(4.521)	(2.008)	(2.918)	(2.254)
	[5.666]	[2.009]	[2.857]	[2.387]
Without Late/Early Advisories	−15.867	−3.558	−4.586	−5.815
	(4.861)	(2.228)	(2.592)	(2.426)
	[5.937]	[2.261]	[2.833]	[2.615]
Cardiovascular Illness				
Sample Modification				
Bandwidth: 16	−0.042	−2.781	−7.426	−2.994
	(0.039)	(0.889)	(2.897)	(0.900)
	[0.035]	[0.675]	[3.328]	[0.864]
Bandwidth: 20	−0.040	−2.828	−9.641	−3.259
	(0.032)	(0.976)	(3.849)	(1.086)
	[0.035]	[0.767]	[3.677]	[1.008]
Bandwidth: 24	−0.019	−1.654	−4.469	−1.870
	(0.030)	(1.016)	(4.320)	(1.219)
	[0.035]	[0.838]	[3.850]	[1.087]
Without Late/Early Advisories	−0.035	−2.643	−8.913	−3.032
	(0.034)	(0.862)	(3.558)	(0.890)
	[0.036]	[0.697]	[3.214]	[0.843]

Notes: This table reports results from 32 2SLS local-linear regressions. The dependent variable in all regressions is respiratory or cardiovascular disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample). The bandwidth is set to 20 unless otherwise noted. For bandwidths of 16, 20, and 24, N = 1,857, 2,530, and 3,380 respectively. The last row in each panel drops from the sample advisory district-days on which the advisory was cancelled before 9 am or triggered after 7 pm, which results in N = 2,443.

Table 10. *Robustness Checks (Credit Card Transactions)*

Credit Card Transactions	Spending Category			
	All	Restaurant	Fashion	Travel
Sample Modification				
Bandwidth: 16	−9.014	−4.783	−0.548	−0.060
	(5.930)	(2.990)	(0.180)	(0.158)
	[7.442]	[3.663]	[0.196]	[0.202]
Bandwidth: 20	−5.504	−3.103	−0.530	−0.011
	(5.159)	(2.277)	(0.159)	(0.132)
	[5.532]	[2.728]	[0.159]	[0.161]
Bandwidth: 24	−1.799	−1.671	−0.362	0.032
	(6.583)	(2.486)	(0.167)	(0.168)
	[5.176]	[2.469]	[0.160]	[0.164]
Without Late/Early Advisories	−4.764	−2.795	−0.529	0.007
	(5.620)	(2.720)	(0.162)	(0.142)
	[6.258]	[3.074]	[0.165]	[0.178]
Before Threshold Changes	−6.550	−3.777	−0.654	−0.131
	(6.325)	(2.913)	(0.147)	(0.145)
	[7.067]	[3.318]	[0.174]	[0.194]

Notes: This table reports results from 20 2SLS local-linear regressions. The dependent variable in all regressions is the number of credit card transactions per 1,000 users for the relevant item group, and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month, day-of-week, holiday, and 1-day-before holiday fixed effects. The level of observation is the district by day, and observations are credit-card-user weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample). The bandwidth is set to 20 unless otherwise noted. For bandwidths of 16, 20, and 24, N = 2,277, 2,905, and 3,997 respectively. The last row in each panel drops from the sample advisory district-days on which the advisory was cancelled before 9 am or triggered after 7 pm, which results in N = 2,701.

Appendix
For Online Publication

A1 Generalisation of Theoretical Framework

For ease of exposition we present a model in which average daily PM is the running variable, but in our application the running variable is the maximum 2-hour minimum PM level (over one day). Here we show that our model's conclusions hold if the running variable is an arbitrary function of daily PM.

Suppose that individuals base their behaviour on daily mean PM, $\overline{pm}$, while the running variable is a different function of daily pm, $f(pm)$. The RD estimand then compares days on which $f(pm)$ is just above the alert threshold ($f(pm) \downarrow c$) to those on which it is just below the alert threshold ($f(pm) \uparrow c$).

Specify individual beliefs as

$$\overline{pm}_i = \begin{cases} pm^{avg} & \text{if} \quad f(pm) = f(pm) \uparrow c \\ pm^{hi} & \text{if} \quad f(pm) = f(pm) \downarrow c \end{cases}$$

where pm^{avg} represents average daily mean PM conditional on $f(pm)$ being below the threshold c and pm^{hi} represents average daily mean PM conditional on $f(pm)$ being above the threshold c. Let pm^c be average daily mean PM conditional on $f(pm) = c$. For our bounding exercise we assume that $pm^{hi} \approx pm^c$, or at least that $|pm^{hi} - pm^c| << |pm^{avg} - pm^c|$. This representation is a reasonable approximation of our actual PM data. For example, in our PM2.5 data, $pm^{avg} = 22.7$, $pm^{hi} = 66.7$, and $pm^c = 57.5$. Thus pm^{hi} is much closer to pm^c than pm^{avg} is. More generally, the approximation only needs to be sufficiently accurate that the alerts do not cause individuals to behave *less* optimally than they would absent the alert's information.

Individuals maximise utility by choosing activity levels $a_i = \text{argmax}_a\, U_i(a, \overline{pm}_i)$. Then

$$U_i = \begin{cases} U_i(a_i(pm^{avg}), pm^c) & \text{if} \quad f(pm) = f(pm) \uparrow c \\ U_i(a_i(pm^{hi}), pm^c) & \text{if} \quad f(pm) = f(pm) \downarrow c \end{cases}$$

An individual's private change in utility from PM crossing the alert threshold is

$$\begin{aligned} \Delta U_i &= U_i(a_i(pm^{hi}), pm^c) - U_i(a_i(pm^{avg}), pm^c) = \\ &[b_i(a_i(pm^{hi})) - b_i(a_i(pm^{avg}))] - s_i^{pvt}[p_s(a_i(pm^{hi}), pm^c) - p_s(a_i(pm^{avg}), pm^c)]. \end{aligned}$$

Naturally $\Delta U_i \geq 0$ since $pm^{hi} \approx pm^c$ and $a_i = \text{argmax}_a\, U_i(a, pm_i)$ — i.e. more accurate PM information can only (weakly) increase the individual's utility — but accurately quantifying ΔU_i is challenging even with good data on s_i^{pvt}. This challenge arises because it is difficult to estimate $b_i(a_i)$, the benefits of different activities (and thus the costs of avoidance behaviours); a_i may be high dimensional, and researchers rarely have data on all, or even most, elements of a_i.

A2 Additional Data Notes

Procedure for Calculating Running Variable: To be consistent with the particulate pollution alert system, we first calculate the 2-hour-minimum of particulate matter ($PM_{dh}^{2h\ min}$) in hour h on day d. Next, we obtain a daily maximum of 2-hour-minimum PM measures in each hour and subtract the alert thresholds (c), where $c = 150$ in case of PM10 and $c = 90$ (or 75 since 27 March 2018) in case of PM2.5. These transformations generate the daily running variables (rv_d). Last, we utilise the running variables rounded to the nearest integer. The process may be summarised as follows:

1. $$PM_{dh}^{2h\ min} = \min_h \{PM_{d(h-1)}, PM_{dh}\} \quad (d : date,\ h : hour)$$
2. $$PM_d^{2h\ min\ max} = \max_d \{PM_{dh}^{2h\ min}\}$$
3. $$rv_d = PM_d^{2h\ min\ max} - c$$
4. Rounding rv_d to the nearest integer

As expected, our running variable has a tight correlation with PM10 and PM2.5, respectively. It is thus reasonable to believe the running variable captures the relationship between particulate pollution and health expenditures in the main regression. Nevertheless, we also control for different combinations of air quality variables as a robustness check (Appendix Table A12).

Cost Evaluation of Air Pollution Alert System: To determine alert system costs, we first searched for reports about environmental expenditures from the seven major cities in our study. We identified a category entitled "Air Pollution Management System - Public Management Cost." Among the items listed under this category, we selected the ones related to the air pollution alert system (Appendix Table A21). We considered not only directly related items, such as the cost of issuing alerts via SMS, but also broadly related items, such as the management cost of air pollution monitors. Appendix Table A22 presents the total costs of the alert system for each city.

A3 Additional Robustness Checks

Manipulation Test: In other contexts researchers have found evidence that pollution measurements are manipulated to remain below certain thresholds. While the officials charged with determining alerts in our context face no obvious incentives to manipulate PM measurements, we nevertheless check to see whether there is any "missing" density of the running variable above the RD threshold. Appendix Figures A1 and A2 plot the distribution of the daily and hourly running variables, respectively. In both graphs, we see no unusual decrease in the density of observations above the threshold. In addition, we test for discontinuities in the density of the running variable at the RD threshold using the procedure in Cattaneo *et al.* (2018). We fail to reject the null hypothesis of continuity in the density of the running variable (2016 to 2017) for bandwidths of 16, 20, and 24 units ($p = 0.43,\ 0.49,$ and 0.6 respectively) and the running variable (2017 to 2018) for bandwidths of 16, 20, and 24 units ($p = 0.46,\ 0.14,$ and 0.1 respectively).

Control Continuity: Our main control variables are weather variables. We test the continuity of the two weather variables in our model, temperature and precipitation, at the RD threshold. Appendix Figure A4 shows the average temperature and precipitation per district and day-of-the-sample plotted against the running variable, which lies in the interval $[-40, 40]$. There is no visual evidence of a discontinuity in either weather variable at the RD threshold.

Asymmetry in the Thresholds for Alert Issuance and Cancellation: Following the issuance of an alert, the running variable generally needs to drop substantially below the RD threshold before the alert gets cancelled. To test the sensitivity of our results to this asymmetry in the issuance and cancellation of alerts, we consider two sample restrictions. First, we trim the sample to exclude alert days following the first day of an air quality alert and report parameter estimates obtained from the main specification. Alternatively, we drop out alert days on which the running variable is below zero. The results of these exercises, reported in Appendix Table A10, suggest that our estimates are not sensitive to the asymmetry in the issuance and cancellation thresholds.

No Treatment During Nighttime Hours: It seems unlikely that most individuals modify their behaviour in response to an air quality alert issued during nighttime hours. As a robustness check, we estimate FRD regressions when dropping district-days on which alerts were issued between 6 AM and 9 PM or between 8 AM and 8 PM. The results, reported in Appendix Table A10, are of similar magnitude to the estimates from our base model.

Alternative RD Specifications: Appendix Table A11 reports estimates from alternative RD specifications that control for a quadratic of the running variable. The health expenditure effects are of similar magnitude and statistical significance as our main results. We also check robustness to specifications in which we control for a quadratic in temperature or in which we control for year and month fixed effects instead of year-by-month fixed effects. Both sets of results are qualitatively similar to our main estimates.

Addition of Different Air Quality Variables We interpret our results as representing benefits of avoidance behaviour. If the alerts had a direct effect on pollution levels, however, then our results would represent a combination of the benefits of avoidance behaviour and lower ambient pollution levels. Appendix Table A5 demonstrates that there is no discontinuous change in ambient PM levels at the RD threshold. As an extra check we estimate specifications, reported in Appendix Table A12 that control for PM10, PM2.5, both PM measures, or the AQI. The addition of these air-quality controls has virtually no impact on our RD estimates.

Different Standard Error Clusters: The panel nature of our data is atypical for a RD design. We thus consider the impacts of different clustering choices for the standard errors. Appendix Table A13 reports the standard errors clustered at different combinations of spatial and time units. All coefficients that were significant in Tables 4 and 5 remain significant across all clustering choices.

Spillover Effects: Our RD estimates could be attenuated if alerts affect health spending in adjacent regions. To address this concern, we estimate the impact of PM alerts in the nearest alert region to region r on the outcome variables in region r. Appendix Table A14 demonstrates that the RD estimates are not statistically significant across all three age groups, suggesting an absence of spillover effects of the alerts to other regions.

Dynamic Effects for Longer Time Horizons: To address the potential avoidance behaviour of rescheduling hospital visits beyond the 3-day alert period, we perform several robustness checks. First, we estimate the FRD design with data aggregated to the weekly level.[46] Appendix Table A15 presents the results of this analysis, confirming our main RD findings: alerts significantly impact respiratory spending for the young and cardiovascular spending for the elderly. We also extend our analysis to estimate 7-day dynamic effects (Appendix Table A15) after filtering out district and

[46]In the weekly aggregation procedure, we take the maximum value of running variable to obtain a clear treatment discontinuity. For time-varying covariates (e.g. PM2.5 concentration), we calculate their means at the weekly level. District and year-by-month fixed effects are included in the regression model to be consistent with our baseline daily RD specification.

time (e.g. day-of-week) fixed effects to reduce noise. We find that the health impacts from the extended 7-day results are larger than those of the 3-day ones, implying that the observed health spending benefits are not solely due to patients skipping hospital visits during alerts.

Pre-Trend Tests: We consider the possibility that individuals may anticipate air pollution alerts due to the availability of air quality forecasts and known alert thresholds. Such anticipation could lead to behavioural changes before an alert is officially issued or even in its absence, potentially attenuating our estimated effects. To address this concern, we analyze the impact of air pollution alerts on health expenditures one or two days prior to their issuance.[47] Appendix Table A18 reveals that alerts do not have statistically significant effects on health expenditures across all age groups in the days preceding their issuance, indicating that preemptive avoidance behaviour is unlikely.

Falsification Tests: As the final robustness check, we estimate effects at alternative "placebo" RD thresholds. Specifically, we construct alternative running variables by subtracting 20, 30, or 50 units from PM10 and PM2.5 concentration. All of the estimates for alternative running variables are statistically insignificant (Appendix Table A14), as would be expected if our research design is valid.

[47]For example, the second-stage RD specification for the impact of alerts ($Alert_{it}$) on the one-day-ahead outcomes ($Y_{i,t-1}$) is as follows:

$$Y_{i,t-1} = \beta_1 \mathbb{1}(\widetilde{PM}_{it} \geq 0) + \beta_2 \widetilde{PM}_{it} + \beta_3 \widetilde{PM}_{it} \mathbb{1}(\widetilde{PM}_{it} \geq 0) + X_{1i,t-1}\theta_2 + X_{2,t-1}\phi_2 + \delta_{2i} + \varepsilon_{i,t-1}$$

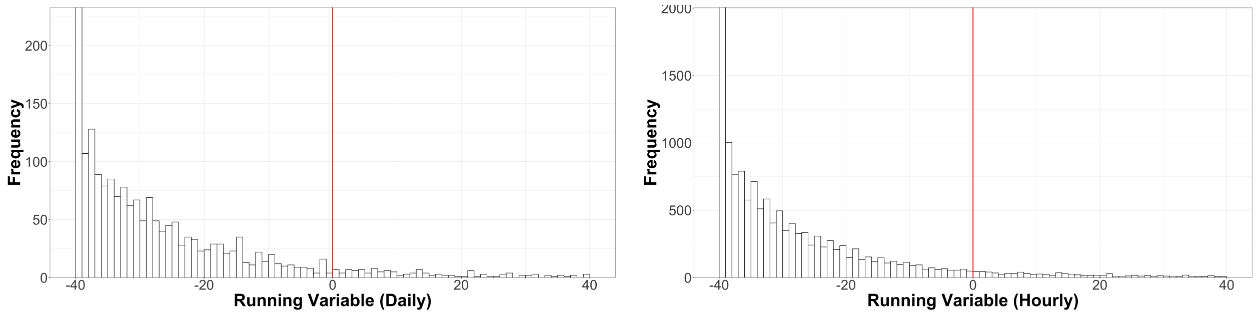


Figure A1. *Histogram of running variable (2016–2017)*

Notes: The red line denotes the RD threshold of zero.

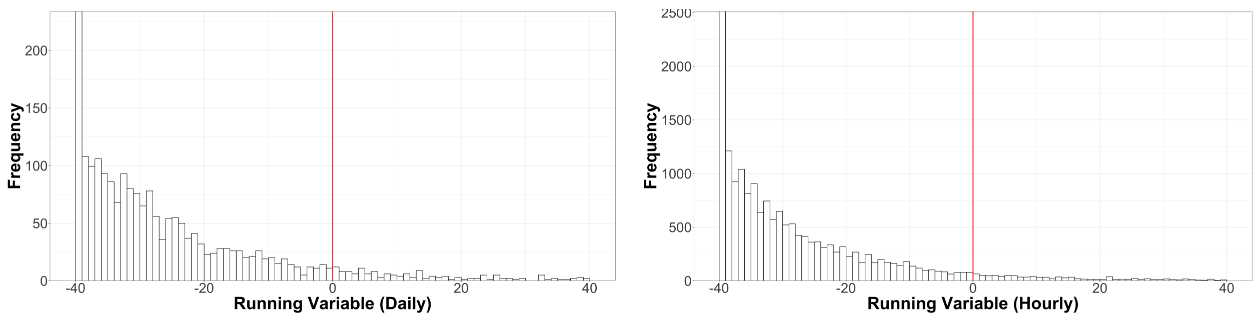
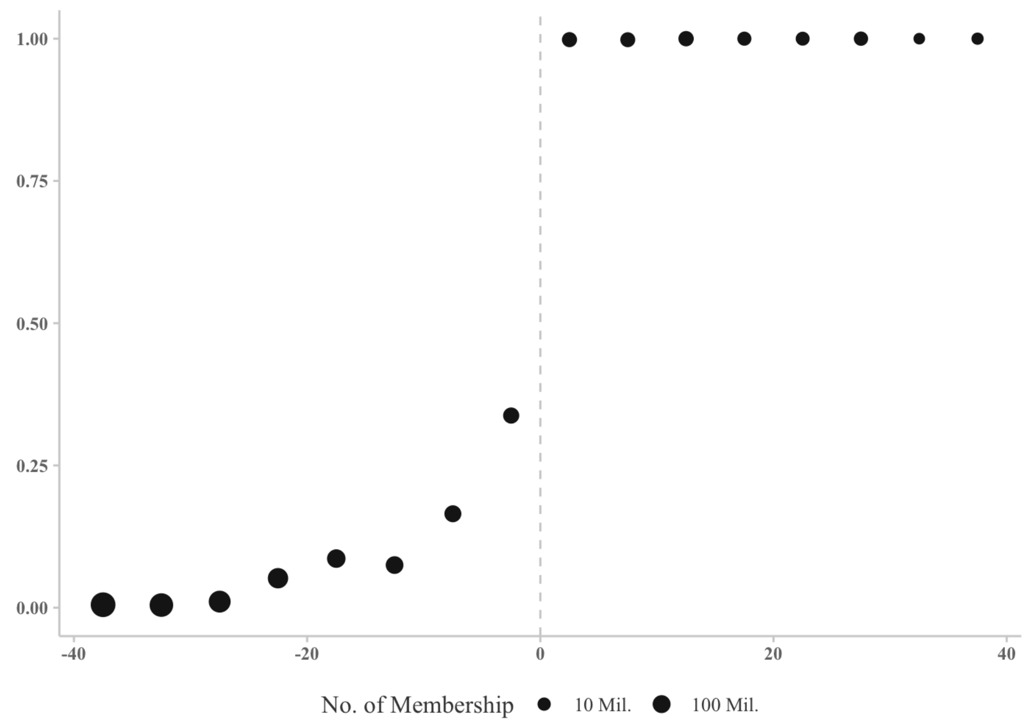


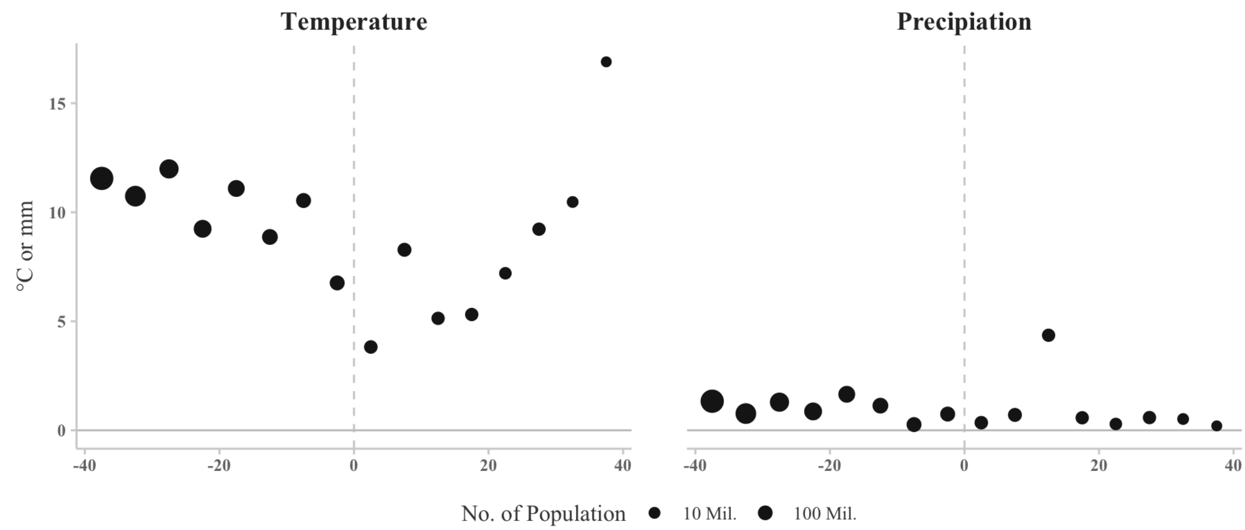
Figure A2. *Histogram of running variable (2017–2018)*

Notes: The red line denotes the RD threshold of zero.



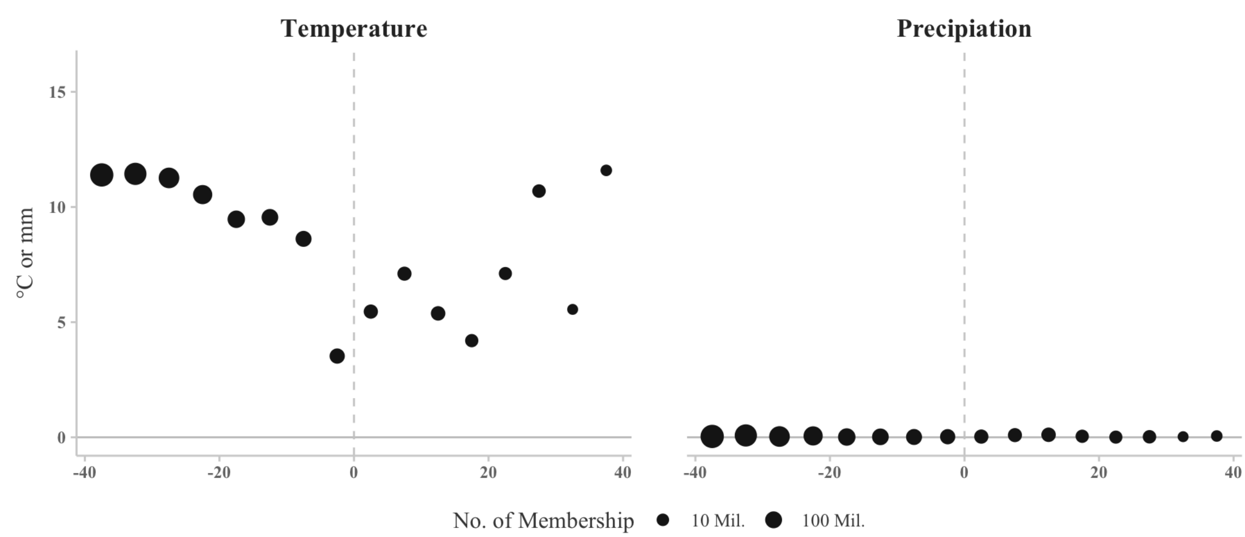
Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the average probability of a particulate matter advisory. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM).

Figure A3. *Treatment Discontinuity (2017−2018)*



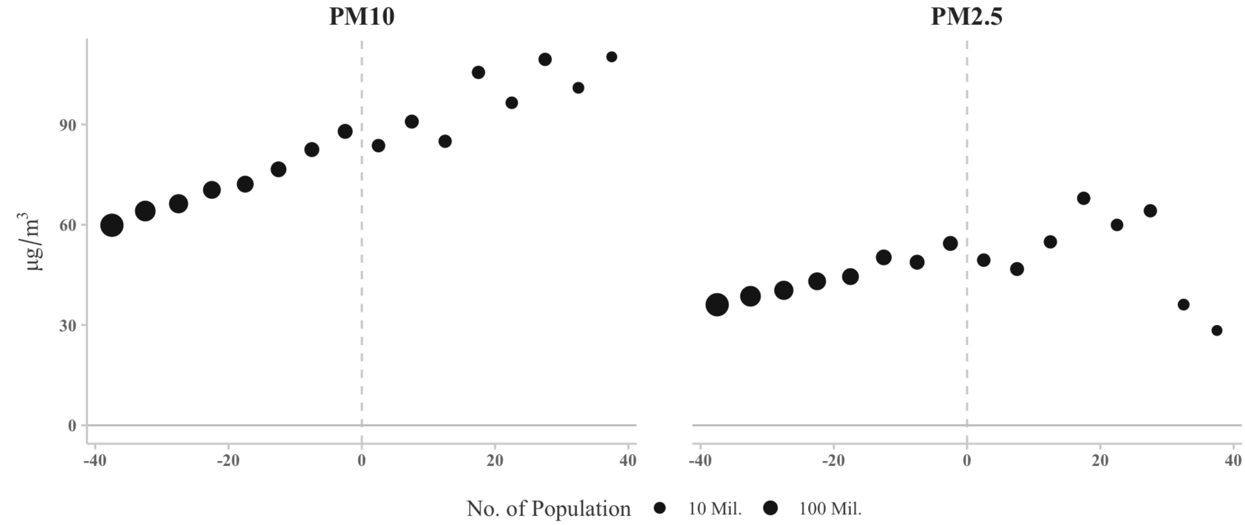
Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the average temperature or rainfall. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM).

Figure A4. *Continuity of weather control variables (2016−2017)*



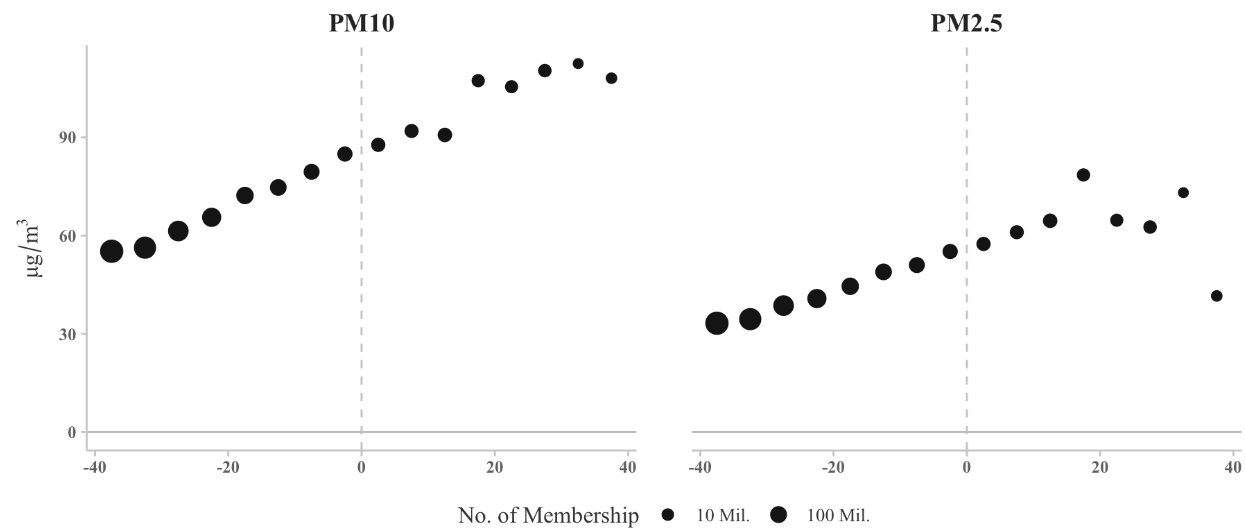
Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the average temperature or rainfall. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM).

Figure A5. *Continuity of weather control variables (2017−2018)*



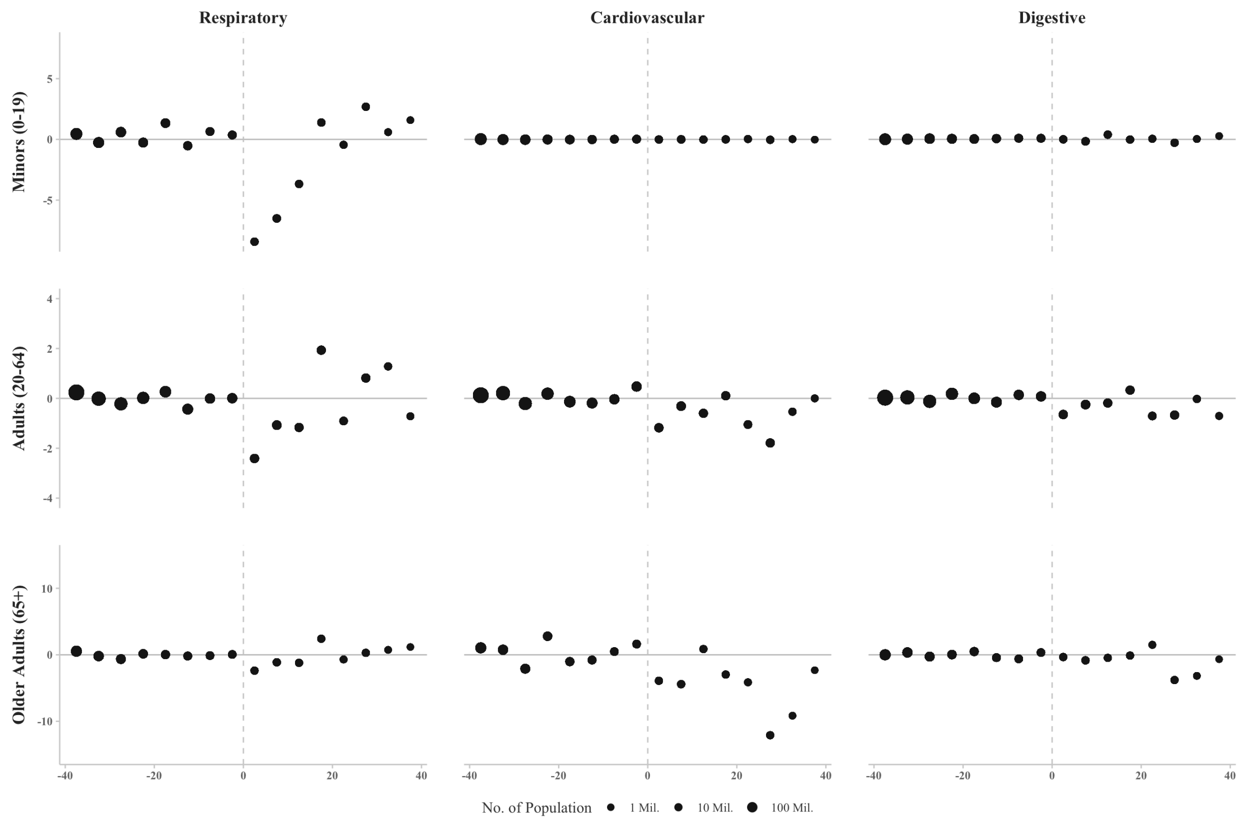
Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the weighted average level of daily PM10 or PM2.5 The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM).

Figure A6. *Continuity of PM variables (2016−2017)*



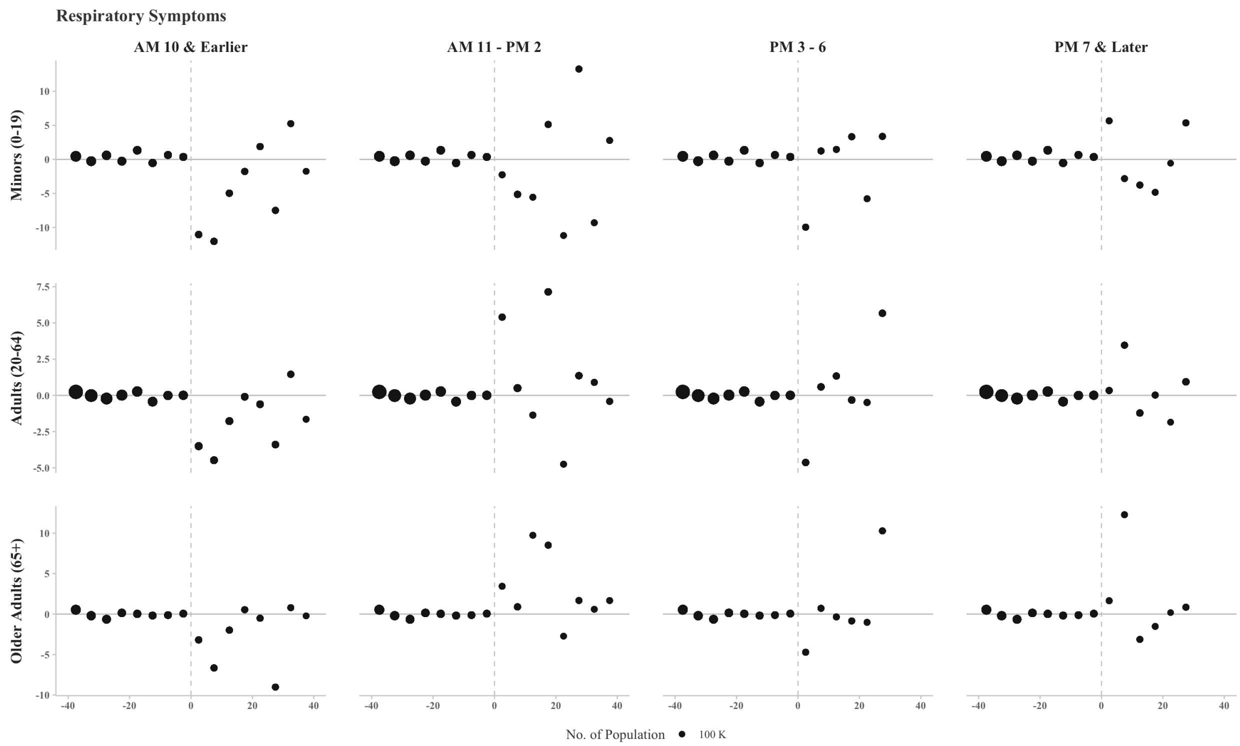
Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the weighted average level of daily PM10 or PM2.5. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM).

Figure A7. *Continuity of PM variables (2017−2018)*



Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the per capita level of residualised health expenditures, in US cents (11.5 KRW = 0.01 USD). The residualisation was performed with respect to day-of-week, year-by-month, holiday, and district fixed effects. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM). The period of the analysis is 2016−2017. The points plotted in the support of RV < 0 are maintained the same across different time groups. The points plotted in the support of RV $>= 0$ are different across columns according to the varying issuance time window.

Figure A8. *Outcome Discontinuity - Health Spending, All Diseases*



Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the per capita level of residualised health expenditures, in US cents (11.5 KRW = 0.01 USD). The residualisation was performed with respect to day-of-week, year-by-month, holiday, and district fixed effects. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM). The period of the analysis is 2016−2017. The points plotted in the support of RV <0 are maintained the same across different time groups. The points plotted in the support of RV $>=0$ are different across columns according to the varying issuance time window.

Figure A9. *Outcome Discontinuity by Issuance Time - Respiratory Illnesses*

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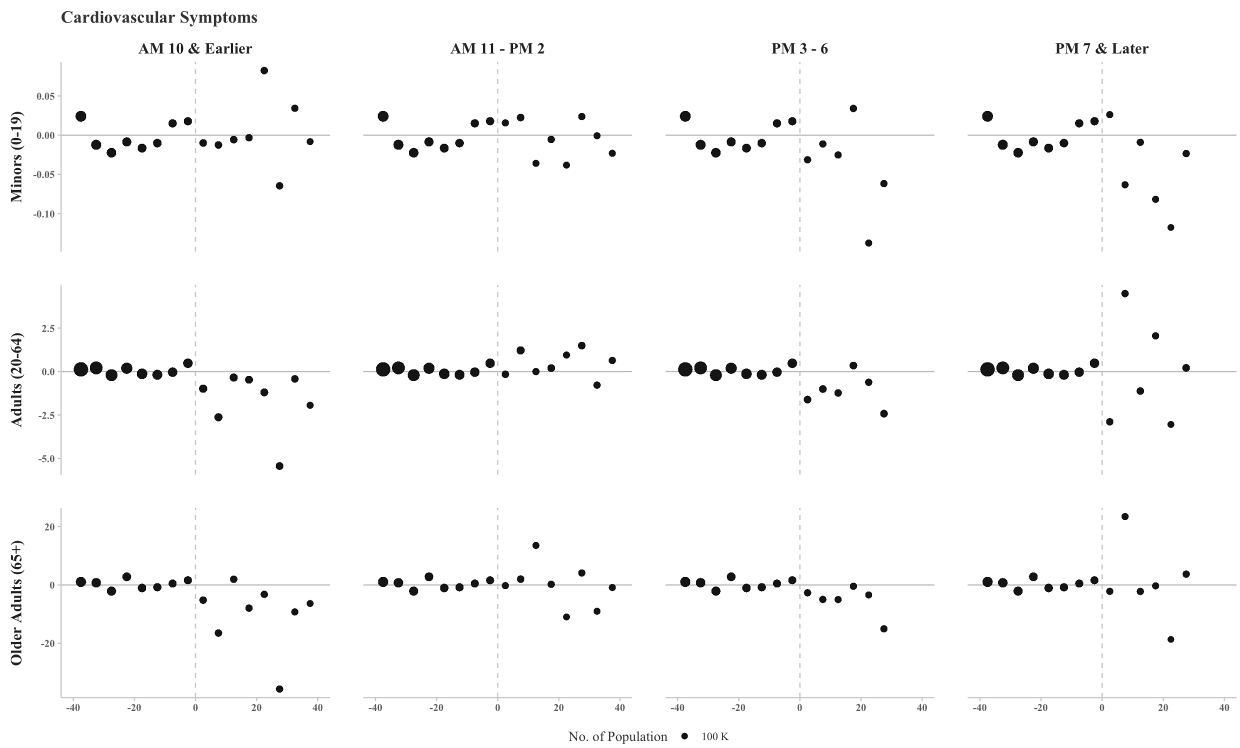

Notes: Each point represents the population-weighted average of observations in a given bin, the width of which is five. The *y*-axis indicates the per capita level of residualised health expenditures, in US cents (11.5 KRW = 0.01 USD). The residualisation was performed with respect to day-of-week, year-by-month, holiday, and district fixed effects. The *x*-axis indicates the value of the running variable (a threshold-normalised function of PM). The period of the analysis is 2016−2017.

Figure A10. *Outcome Discontinuity by Issuance Time - Cardiovascular Illnesses*

Table A1. *Examples of Alert Systems in the World*

Alert Policies	Country	Regions Covered	Estimated Population Covered (mil.)	Related Links
4-tier Alert System	China	All	1,387.2	LINK
Particulate Matter Alerts	South Korea	All	51.7	LINK
EnviroFlash	US	Many	193.8	LINK
Public Weather Alerts	Canada	Ontario	14.6	LINK
Air quality alerts	Australia	New South Wales	8.2	LINK
Sistema de Monitoreo Atmosférico	Mexico	Mexico City	8.8	LINK
Haze Alerts	Singapore	All	5.6	LINK
Pollution Alerts	UK	All	66.7	LINK

Notes: This list includes examples of prominent air-quality alert systems worldwide, but it is not comprehensive. The Chinese 4-tier Alert system coverage was calculated by summing the populations of the second-tier administrative units in which air pollution monitors are installed. EnviroFlash coverage was calculated based on the population in counties where the US EPA manages air pollution monitors. All links were checked on 20 September 2021.

Table A2. *Summary Statistics*

Treatment & Covariates		Full Sample Mean	SD	Min	Max	Bandwidth = 40 Mean	SD	Min	Max
Alert		0.04	0.2	0	1	0.1	0.3	0	1
PM10	($\mu g/m^3$)	42.0	22.3	3.0	253.9	65.7	20.2	14.9	160.1
PM2.5	($\mu g/m^3$)	23.6	13.9	1.1	123.9	41.0	14.7	6.6	123.9
Temperature	(°C)	13.5	10.2	-17.3	34.4	10.5	8.6	-9.6	34.4
Precipitation	(mm)	0.1	0.5	0	13.1	0.05	0.2	0	3.8

Notes: The number of district-day observations is 53,286 (73 districts, 760 days, 2017−2018) and 11,197 for the full sample and the sample based on a bandwidth of 40, respectively.

Table A3. *Types of Medical Institutions in South Korea*

Levels of Institutions	Types of Institutions	Description
Tertiary	Tertiary General Hospitals	· The Minister of Health and Welfare may designate a general hospital providing highly specialised medical services for treating serious diseases as a tertiary hospital among general hospitals.
Secondary	General Hospitals	· A general hospital shall have at least 100 beds.
	Hospitals	· Hospitals shall have at least 30 beds or beds for long-term care.
Primary	Public Health Centers	· Publicly owned regional healthcare institutions
	Clinic	· A medical institution in which a doctor, dentist, or oriental medical doctor provides medical services primarily to outpatients

Sources: Korea Law Translation Center, Korea Legislation Research Institute
https://elaw.klri.re.kr/kor_service/lawView.do?hseq=53532&lang=ENG, accessed on Sep 15, 2021

Notes: Source link provides additional details on the definitions of each institution type.

Table A4. *Out-of-pocket Payments for Outpatient Visits in the South Korean Healthcare System*

Type of Institutions	Cases	Out-of-pocket Payments
Tertiary Hospitals	Normal Patients	· 100% of consultation fee + 40% of remaining medical expenses · (Pregnant Women) 40% of total medical expenses · (Age $<$ 1) 20% of total medical expenses
General Hospitals	Urban Areas	· 50% of total medical expenses · (Pregnant Women) 40% of total medical expenses · (Age $<$ 1) 20% of total medical expenses
	Rural Areas	· 45% of total medical expenses · (Pregnant Women) 40% of total medical expenses · (Age $<$ 1) 20% of total medical expenses
Hospitals	Urban Areas	· 40% of total medical expenses · (Pregnant Women) 20% of total medical expenses · (Age $<$ 1) 10% of total medical expenses
	Rural Areas	· 35% of total medical expenses · (Pregnant Women) 40% of total medical expenses · (Age $<$ 1) 20% of total medical expenses
Clinics	Age $\geq$ 65	· ₩1,500 when total medical expenses $\leq$ ₩15,000 · 10% when total medical expenses $>$ ₩15,000 & $\leq$ ₩20,000 · 20% when total medical expenses $>$ ₩20,000 & $\leq$ ₩25,000 · 30% when total medical expenses $>$ ₩25,000
	Age $<$ 65	· 30% of total medical expenses · (Pregnant Women) 10% of total medical expenses · (Age $<$ 1) 5% of total medical expenses
Public Health Centers	Age $\geq$ 6	· 30% when total medical expenses $>$ ₩12,000 · ₩500−₩2,200 depending on cases when total medical expenses $\leq$ ₩12,000
	Age $<$ 6	· 21% when total medical expenses $>$ ₩12,000 · ₩500−₩2,200 depending on cases when total medical expenses $\leq$ ₩12,000

Sources: Health Insurance Review and Assessment Service (HIRA) of South Korea

https://www.hira.or.kr/dummy.do?pgmid=HIRAA030056020110 (in Korean), accessed on Sep 10, 2021

Notes: ₩ indicates Korean Won (KRW). 10,000 KRW is equivalent to approximately 8.7 USD. While this table shows general out-of-pocket payment ratios, it does not describe every specific case that could result in different ratios of out-of-pocket payments. For more details, refer to source link above.

Table A5. *FRD Results using Particulate Matter as Dependent Variables*

Dependent Variable	Bandwidths 16	20	24
PM10	−3.815 (5.866)	−6.287 (5.003)	−4.205 (4.980)
PM2.5	−3.689 (5.133)	−3.261 (4.572)	1.459 (5.226)

Notes: This table reports results from six 2SLS local-linear regressions. The dependent variable in all regressions is PM10 or PM2.5, measured in cents $\mu g/m^3$, and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses contain standard errors clustered by the day of sample. The bandwidth varies across columns as noted.

Table A6. *Optimal Bandwidth*

Dependent Variable	Optimal Bandwidth
Resp. (All)	18.321
Resp. (Adults)	19.293
Resp. (Older Adults)	20.526
Resp. (Minors)	17.135
Cardio. (All)	21.084
Cardio. (Adults)	21.354
Cardio. (Older Adults)	20.611
Cardio. (Minors)	17.726
Credit (All)	19.149
Credit (Restaurant)	18.218
Credit (Fashion)	23.349
Credit (Travel)	30.697
Resp. (All, 3-Day Rolling Sum)	16.730
Resp. (Adults, 3-Day Rolling Sum)	17.749
Resp. (Older Adults, 3-Day Rolling Sum)	19.683
Resp. (Minors, 3-Day Rolling Sum)	16.411
Cardio. (All, 3-Day Rolling Sum)	23.888
Cardio. (Adults, 3-Day Rolling Sum)	23.849
Cardio. (Older Adults, 3-Day Rolling Sum)	22.243
Cardio. (Minors, 3-Day Rolling Sum)	23.652
Credit (All, 3-Day Rolling Sum)	20.264
Credit (Restaurant, 3-Day Rolling Sum)	20.208
Credit (Fashion, 3-Day Rolling Sum	19.266
Credit (Travel, 3-Day Rolling Sum)	24.096

Notes: This table reports "optimal" bandwidths for different dependent variables, computed using methods from Calonico *et al.* (2014, 2015).

Table A7. *RD Results for Respiratory Diseases, With Visits to Tertiary General Hospitals*

	Age Groups			
Reduced form	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
$\mathbb{1}(RV >= 0)$	−8.804	−2.260	−2.532	−3.446
	(2.344)	(1.385)	(1.655)	(1.426)
	[2.887]	[1.381]	[1.866]	[1.505]
RV	−0.086	−0.045	0.011	−0.044
	(0.145)	(0.044)	(0.064)	(0.057)
	[0.142]	[0.045]	[0.072]	[0.058]
RV $\cdot \mathbb{1}(RV >= 0)$	0.627	0.218	0.173	0.270
	(0.166)	(0.112)	(0.131)	(0.109)
	[0.227]	[0.079]	[0.114]	[0.088]
2SLS				
$\mathbb{1}(RV >= 0)$	−14.060	−3.639	−4.095	−5.543
	(4.662)	(2.184)	(2.696)	(2.367)
	[5.484]	[2.134]	[2.829]	[2.450]
RV	0.020	−0.018	0.021	−0.003
	(0.204)	(0.063)	(0.084)	(0.084)
	[0.170]	[0.055]	[0.086]	[0.071]
RV $\cdot \mathbb{1}(RV >= 0)$	0.581	0.208	0.162	0.254
	(0.237)	(0.110)	(0.127)	(0.118)
	[0.297]	[0.079]	[0.110]	[0.104]
Adjusted R^2	0.839	0.866	0.796	0.897
N		2,530		

Notes: This table reports results from four reduced-form local-linear regressions (top panel) and four 2SLS local-linear regressions (bottom panel). The dependent variable in all regressions is respiratory disease expenditures for the relevant age group, *including visits to tertiary general hospitals* and measured in cents per capita (11.5 KRW = 0.01 USD). The independent variable of interest in the reduced-form (2SLS) regressions is an indicator for the running variable being above the RD threshold (advisory indicator). All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample). The bandwidth is set to 20 in all regressions.

Table A8. *RD Results for Cardiovascular Diseases, With Visits to Tertiary General Hospitals*

	Age Groups			
Reduced form	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
$\mathbb{1}(RV >= 0)$	−0.096	−1.877	−5.992	−2.133
	(0.054)	(0.527)	(2.133)	(0.618)
	[0.062]	[0.509]	[2.421]	[0.681]
RV	0.005	0.059	0.216	0.065
	(0.003)	(0.025)	(0.100)	(0.025)
	[0.003]	[0.021]	[0.118]	[0.028]
RV $\cdot \mathbb{1}(RV >= 0)$	0.003	0.012	−0.032	0.014
	(0.007)	(0.038)	(0.155)	(0.042)
	[0.008]	[0.039]	[0.214]	[0.052]
2SLS				
$\mathbb{1}(RV >= 0)$	−0.153	−3.022	−9.691	−3.431
	(0.095)	(1.024)	(3.930)	(1.168)
	[0.113]	[0.789]	[3.786]	[1.073]
RV	0.006	0.082	0.291	0.091
	(0.003)	(0.032)	(0.102)	(0.033)
	[0.004]	[0.025]	[0.129]	[0.033]
RV $\cdot \mathbb{1}(RV >= 0)$	0.003	0.003	−0.058	0.005
	(0.007)	(0.044)	(0.147)	(0.041)
	[0.007]	[0.045]	[0.227]	[0.057]
Adjusted R^2	0.022	0.799	0.854	0.874
N		2,530		

Notes: This table reports results from four reduced-form local-linear regressions (top panel) and four 2SLS local-linear regressions (bottom panel). The dependent variable in all regressions is cardiovascular disease expenditures for the relevant age group, *including visits to tertiary general hospitals* and measured in cents per capita (11.5 KRW = 0.01 USD). The independent variable of interest in the reduced-form (2SLS) regressions is an indicator for the running variable being above the RD threshold (advisory indicator). All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample). The bandwidth is set to 20 in all regressions.

Table A9. *Analysis of Dynamic Effects with Different Bandwidths*

	Age Groups			
	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Respiratory Illness				
Sample Modification				
Bandwidth: 16	−32.087 (10.967)	−10.266 (4.058)	−11.953 (5.892)	−14.688 (5.065)
Bandwidth: 20	−38.220 (13.769)	−10.703 (4.662)	−12.977 (6.624)	−16.125 (5.935)
Bandwidth: 24	−32.998 (13.702)	−7.851 (4.715)	−8.139 (6.878)	−12.693 (5.882)
Cardiovascular Illness				
Sample Modification				
Bandwidth: 16	−0.146 (0.089)	−4.796 (1.682)	−25.911 (12.085)	−7.034 (2.533)
Bandwidth: 20	−0.064 (0.075)	−5.279 (1.993)	−30.388 (12.987)	−8.042 (3.057)
Bandwidth: 24	0.007 (0.076)	−3.785 (2.059)	−26.687 (12.366)	−6.718 (3.081)
N	Bandwidth 16: 1,857 Bandwidth 20: 2,530 Bandwidth 24: 3,380			

Notes: This table reports results from 24 2SLS local-linear regressions with varying bandwidths (16, 20, or 24). The dependent variable in all regressions is three-day respiratory or cardiovascular disease expenditures (from day t to day $t+2$) for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses contain standard errors clustered by day of sample.

Table A10. *Robustness Check - Exclusion of Later Alert Days and Removal of Early/Late Alerts*

	Age Groups			
Respiratory Illness	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Sample Modification				
Without Later Alert Days	−13.622	−1.563	−1.229	−3.579
	(6.692)	(1.290)	(1.396)	(2.006)
Without Later Alert Days of RV < 0	−13.213	−3.412	−3.812	−5.219
	(4.678)	(1.855)	(2.432)	(2.174)
Without 9PM−6AM	−16.274	−4.093	−4.687	−6.320
	(6.248)	(2.268)	(2.996)	(2.675)
Without 8PM−8AM	−17.759	−4.428	−5.113	−6.856
	(6.908)	(2.451)	(3.281)	(2.918)
Cardiovascular Illness				
Sample Modification				
Without Later Alert Days	−0.025	−2.056	−8.425	−2.237
	(0.045)	(0.746)	(3.484)	(0.896)
Without Later Alert Days of RV<0	−0.036	−2.387	−8.204	−2.723
	(0.030)	(0.619)	(2.897)	(0.815)
Without 9PM−6AM	−0.043	−3.064	−10.502	−3.533
	(0.038)	(0.834)	(4.012)	(1.091)
Without 8PM−8AM	−0.047	−3.315	−11.457	−3.833
	(0.041)	(0.924)	(4.495)	(1.218)

Notes: This table reports results from 32 2SLS local-linear regressions. The dependent variable in all regressions is respiratory or cardiovascular disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses contain standard errors clustered by day of sample. The bandwidth is set to 20 in all regressions. In each panel, the first excludes alert days following the first day of an air quality alert, and the second row excludes alert days on which the running variable falls below zero. The third row excludes days on which the alert was cancelled before 6 am or triggered after 9 pm, while the fourth row excludes days on which the alert was cancelled before 8 am or triggered after 8 pm.

Table A11. *FRD Coefficients with Different Specifications*

	Age Groups			
Respiratory Illness	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Specification Modification				
Quadratic RV	−18.220 (11.167)	−7.586 (3.221)	−7.718 (4.404)	−9.607 (4.337)
Quadratic Temperature	−15.611 (5.710)	−4.064 (2.105)	−4.569 (2.808)	−6.167 (2.499)
Alternative Time FE	−14.502 (5.698)	−2.982 (2.152)	−3.274 (2.858)	−5.045 (2.495)
1-Day-Before Holiday FE	−13.414 (5.771)	−1.881 (1.410)	−1.765 (1.862)	−3.907 (1.958)
Cardiovascular Illness				
Specification Modification				
Quadratic RV	−0.028 (0.074)	−4.611 (1.487)	−14.329 (6.994)	−5.138 (1.714)
Quadratic Temperature	−0.039 (0.035)	−2.702 (0.776)	−9.022 (3.607)	−3.126 (1.020)
Alternative Time FE	−0.041 (0.034)	−2.650 (0.760)	−9.286 (3.561)	−3.128 (0.983)
1-Day-Before Holiday FE	−0.040 (0.038)	−2.697 (0.888)	−11.887 (4.558)	−3.342 (1.182)

Notes: This table reports results from 32 2SLS local-linear regressions. The dependent variable in all regressions is respiratory or cardiovascular disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The first, second, and third rows in each panel add controls for a quadratic in the running variable, a quadratic in temperature, and year and month fixed effects (instead of year-by-month fixed effects) respectively. The level of observation is the district by day, and observations are population weighted. Parentheses contain standard errors clustered by the day of sample. The bandwidth is set to 20 in all regressions.

Table A12. *Robustness Check - Addition of Air Pollution Covariates*

	Age Groups			
Respiratory Illness	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Added Covariates				
PM10	−14.665	−3.294	−3.860	−5.379
	(5.249)	(1.912)	(2.507)	(2.253)
PM2.5	−14.232	−3.558	−4.033	−5.486
	(5.117)	(1.970)	(2.536)	(2.274)
PM10 & PM2.5	−14.531	−3.296	−3.849	−5.353
	(5.100)	(1.902)	(2.491)	(2.230)
AQI	−14.320	−3.471	−3.870	−5.431
	(5.217)	(1.986)	(2.626)	(2.319)
Cardiovascular Illness				
Added Covariates				
PM10	−0.036	−2.654	−8.957	−3.076
	(0.037)	(0.733)	(3.537)	(0.948)
PM2.5	−0.045	−2.790	−9.510	−3.174
	(0.035)	(0.733)	(3.529)	(0.945)
PM10 & PM2.5	−0.037	−2.663	−8.995	−3.076
	(0.036)	(0.731)	(3.545)	(0.942)
AQI	−0.034	−2.792	−9.195	−3.163
	(0.036)	(0.729)	(3.436)	(0.936)

Notes: This table reports results from 32 2SLS local-linear regressions. The dependent variable in all regressions is respiratory or cardiovascular disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The first, second, third, and fourth rows in each panel add controls for PM10, PM2.5, both PM10 and PM2.5, and the AQI, respectively (averaged across the day). The level of observation is the district by day, and observations are population weighted. Parentheses contain standard errors clustered by day of sample. The bandwidth is set to 20 in all regressions.

Table A13. *Robustness Check - Standard Errors with Different Clusters*

	Age Groups			
Respiratory Illness	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
FRD Coefficient	−15.033	−3.777	−4.303	−5.829
Clustering Level				
Province by Day of Week	(5.345)	(1.568)	(1.953)	(1.938)
Province by Day of Sample	(6.114)	(2.140)	(2.621)	(2.524)
Region by Day of Week	(5.217)	(1.497)	(1.929)	(1.880)
Region by Day of Sample	(6.081)	(2.110)	(2.592)	(2.490)
District by Day of Week	(1.828)	(0.645)	(1.171)	(0.751)
District	(2.037)	(0.869)	(0.990)	(0.994)
Cardiovascular Illness				
FRD Coefficient	−0.040	−2.828	−9.641	−3.259
Clustering Level				
Province by Day of Week	(0.042)	(0.954)	(4.199)	(1.253)
Province by Day of Sample	(0.038)	(0.834)	(3.742)	(1.033)
Region by Day of Week	(0.042)	(0.923)	(3.966)	(1.209)
Region by Day of Sample	(0.038)	(0.813)	(3.607)	(1.001)
District by Day of Week	(0.039)	(0.462)	(2.645)	(0.569)
District	(0.035)	(0.508)	(2.292)	(0.530)

Notes: This table reports different standard errors for eight 2SLS local-linear regressions. The dependent variable in all regressions is respiratory or cardiovascular disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest is an advisory indicator. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. In each panel parentheses contain standard errors clustered by province by day-of-week (second row), province by day-of-sample (third row), region by day-of-week (fourth row), region by day-of-sample (fifth row), district by day-of-week (sixth row), and district (seventh row). The bandwidth is set to 20 in all regressions.

Table A14. *Robustness Check - Spillover Effect and Falsification Tests*

	Age Groups			
Respiratory Illness	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Specification Modification				
Spillover	−0.029 (4.658)	0.217 (1.180)	0.453 (1.778)	0.147 (1.743)
Falsification (−20)	0.381 (1.528)	0.296 (0.478)	0.832 (0.792)	0.334 (0.634)
Falsification (−30)	1.422 (1.023)	0.036 (0.333)	−0.185 (0.520)	0.285 (0.430)
Falsification (−50)	−0.664 (0.715)	−0.335 (0.232)	−0.389 (0.358)	−0.401 (0.310)
Cardiovascular Illness				
Specification Modification				
Spillover	0.029 (0.023)	0.295 (0.809)	−1.054 (4.547)	0.074 (1.067)
Falsification (−20)	0.007 (0.025)	−0.062 (0.340)	−0.458 (1.951)	−0.037 (0.461)
Falsification (−30)	−0.046 (0.017)	−0.072 (0.257)	−0.925 (1.655)	−0.207 (0.380)
Falsification (−50)	−0.014 (0.009)	0.102 (0.131)	1.221 (0.822)	0.196 (0.193)

Notes: This table reports results from 32 2SLS local-linear regressions. The dependent variable in all regressions is respiratory or cardiovascular disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), and the independent variable of interest is an advisory indicator, which is shifted either geographically or generated by shifting the running variable by a constant. In each panel, the first row shifts the advisory indicator to correspond to an advisory in the nearest alert region to region *i*. The second, third, and fourth rows use an advisory indicator that is generated after shifting the running variable downwards by 20, 30, or 50 units respectively. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses contain standard errors clustered by day of sample. The bandwidth is set to 20 in all regressions.

Table A15. *Robustness Check - Longer Window for the Dynamic Impacts*

	Age Groups			
	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Respiratory Illness				
Dependent Variable				
Weekly Spending	−8.546 (3.707)	−1.630 (0.706)	−2.426 (1.148)	−2.965 (1.197)
3-day Sum of Residuals	−36.454 (18.472)	−6.989 (2.995)	−6.898 (4.155)	−12.848 (5.528)
7-day Sum of Residuals	−50.180 (27.677)	−6.187 (3.991)	−4.310 (6.312)	−15.668 (7.483)
Cardiovascular Illness				
Dependent Variable				
Weekly Spending	−0.021 (0.015)	−1.440 (0.901)	−13.218 (6.864)	−2.765 (1.564)
3-day Sum of Residuals	0.006 (0.095)	−5.037 (1.975)	−28.712 (13.754)	−7.689 (3.100)
7-day Sum of Residuals	−0.032 (0.115)	−6.566 (2.475)	−43.320 (19.894)	−10.930 (4.217)

Notes: This table reports results from 24 2SLS local-linear regressions. The dependent variables in all regressions are respiratory or cardiovascular disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD), but with different aggregation methods. In each panel, the first row uses the morbidity spending aggregated by week. Accordingly, the covariates are calculated at the weekly level and standard errors are clustered by week-of-the-sample. For example, the running variable is the maximum of the daily running variables within a week. The second line of results is based on the 3-day rolling summation of residualised morbidity spending. The residualisation method followed the same process used in Figure 4. The third row in each panel demonstrate the results analogous to the second row, the only difference being the length of summation window. The level of observation is the district by week or the district by day for the first and the second/third rows, respectively. Observations are population weighted. Parentheses contain standard errors clustered by day of sample. The bandwidth is set to 20 in all regressions.

Table A16. *Robustness Check - Digestive Illness*

	Age Groups			
	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Dependent Variable				
Expenditure on Digestive Illness	−0.123 (0.190)	−1.374 (0.649)	−0.701 (1.148)	−1.086 (0.573)

Notes: This table reports results from 4 2SLS local-linear regressions. The dependent variables in all regressions are digestive disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD). To be consistent with the baseline regression for respiratory and cardiovascular spending, all regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses contain standard errors clustered by day of sample. The bandwidth is set to 20 in all regressions.

Table A17. *Mortality Impacts*

	Age Groups			
	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Respiratory Illness				
Dependent Variable				
1-day Mortality	—	−0.011 (0.015)	−0.159 (0.355)	−0.010 (0.048)
3-day Sum	—	−0.064 (0.037)	0.531 (0.619)	0.082 (0.090)
3-day Sum of Residuals	—	−0.068 (0.037)	0.388 (0.591)	0.058 (0.085)
7-day Sum of Residuals	—	−0.072 (0.053)	0.739 (1.130)	0.013 (0.150)
Cardiovascular Illness				
Dependent Variable				
1-day Mortality	—	−0.019 (0.050)	0.364 (0.473)	0.022 (0.078)
3-day Sum	—	0.046 (0.065)	−0.024 (0.705)	0.039 (0.120)
3-day Sum of Residuals	—	0.041 (0.065)	−0.117 (0.702)	0.021 (0.119)
7-day Sum of Residuals	—	0.014 (0.083)	0.363 (1.100)	0.076 (0.173)

Notes: This table reports results from 32 2SLS local-linear regressions. The dependent variables in all regressions are mortality caused by respiratory or cardiovascular disease for the relevant age group, measured in deaths per 100,000, but with different aggregation methods. We drop out mortality results for young (minors) due to very few non-zero observations. In each panel, the first and second row uses the one-day and three-day (from day t to day $t+2$) mortality, respectively. The third and fourth lines of results are based on the three-day and seven-day rolling summation of residualised mortality, respectively. The residualisation method followed the same process used in Figure 4. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. Parentheses contain standard errors clustered by day of sample. The bandwidth is set to 20 in all regressions.

Table A18. *Robustness Check - Pre-trends*

	Age Groups			
Respiratory Illness	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Dependent Variable				
One-Day-Ahead Spending	−8.709 (7.367)	−0.528 (1.587)	0.806 (2.233)	−1.889 (2.547)
Two-Days-Ahead Spending	−1.373 (5.235)	1.169 (1.135)	2.440 (1.446)	0.850 (1.629)
Cardiovascular Illness				
Dependent Variable				
One-Day-Ahead Spending	0.045 (0.044)	0.400 (1.244)	2.326 (5.831)	0.393 (1.463)
Two-Days-Ahead Spending	−0.068 (0.119)	0.152 (0.932)	0.432 (4.550)	−0.298 (1.068)

Notes: This table reports results from 16 2SLS local-linear regressions. The dependent variables in all regressions are respiratory or cardiovascular disease expenditures for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD). In each panel, the first row uses the one-day-ahead morbidity spending ($Y_{i,t-1}$) so each cell shows the parameter estimates for β_1 in Eqn. (47). The second line of results is the impact of alerts on the two-day-ahead outcomes. Observations are population weighted. Parentheses contain standard errors clustered by day of sample. The bandwidth is set to 20 in all regressions.

Table A19. *Robustness Check - Hospital Visits*

	Age Groups			
Outpatient Visits	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Dependent Variable				
Asthma	−0.757 (0.323)	−0.098 (0.046)	−0.245 (0.110)	−0.248 (0.089)
Rhinitis	−3.646 (1.700)	−0.955 (0.533)	−1.428 (0.746)	−1.504 (0.654)

Notes: This table reports results from 8 2SLS local-linear regressions. The dependent variable in all regressions is hospital visits related to asthma (first row) or rhinitis (second row) for the relevant age group, measured in visits per 1,000. All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. The bandwidth is set to 20 in all regressions.

Table A20. *Robustness Check - Expenditure on Emergency Department*

	Age Groups			
Emergency Expenditure	Minors (0-19)	Adults (20-64)	Older Adults (65+)	All
Dependent Variable				
Respiratory Illness	0.131 (0.146)	−0.062 (0.055)	1.012 (0.611)	0.108 (0.099)
Cardiovascular Illness	0.128 (0.101)	−0.238 (0.243)	0.243 (1.265)	−0.151 (0.254)

Notes: This table reports results from 8 2SLS local-linear regressions. The dependent variable in all regressions is expenditure on emergency departments related to respiratory or cardiovascular disease for the relevant age group, measured in cents per capita (11.5 KRW = 0.01 USD). All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month and day-of-week fixed effects. The level of observation is the district by day, and observations are population weighted. The bandwidth is set to 20 in all regressions.

Table A21. *List of budget items related to alert system*

City	Item
Gwangju	Air Pollution Monitor - Electricity Cost
Gwangju	Line Rental for Air Quality Warning System
Gwangju	Air Pollution Monitor Management Cost (General)
Gwangju	Air Pollution Monitor - Automatic Control System
Gwangju	Air Pollution Monitor Alert System
Daejeon	Advertisement - Electronic Board Fee
Daejeon	Line Rental for Air Pollution Management System
Daejeon	Air Pollution Management System - Maintenance Cost
Daejeon	Air Pollution Management System - Establishment Cost
Daejeon	Air Pollution Monitor - Installation Cost
Daegu	Line Rental for Air Quality Warning System
Daegu	Air Pollution Alert System - Maintenance Cost
Busan	Air Pollution Monitor - Maintenance Cost
Busan	Air Pollution Monitor Data Collecting Machine - Maintenance Cost
Busan	Air Pollution Monitor - Battery Change
Busan	Indoor Air Quality Monitor - Maintenance Cost
Busan	Indoor Air Quality Monitor Data Collecting Machine - Maintenance Cost
Busan	Air Pollution Alert System - Maintenance Cost
Busan	Sampling & Analysis Equipment - Maintenance Cost
Busan	Air Pollution Monitor - Maintenance Cost
Busan	Indoor Air Quality Monitor - Maintenance Cost
Busan	Air Pollution Monitor - Electronic Board Maintenance Cost
Seoul	Air Pollution Information System - Office Management Cost
Seoul	Air Pollution Information System - Maintenance Cost
Seoul	Air Pollution Information System - SMS Service
Seoul	Air Quality Modeling System Construction
Seoul	Air Quality Evaluation Program Development
Seoul	Air Quality Real-Time Information Provision System Construction
Seoul	Air Pollution Information System - IT Development
Seoul	Ambient Air Quality Situation Room Construction
Seoul	Ambient Air Quality Information Service Infrastructure
Seoul	Air Quality Information System - Supervision
Seoul	Air Pollution Information System - IT Solution & Planning Development
Ulsan	Advertisement - Electronic Board Fee
Ulsan	Air Pollution Monitor - Electricity Cost
Ulsan	Line Rental for Air Pollution Monitor
Ulsan	Ambient Air Quality Situation Room - Maintenance Cost
Ulsan	Air Pollution Monitor - Outsourcing Maintenance Cost
Ulsan	Air Pollution Monitor Equipment - Maintenance Cost
Ulsan	Air Pollution Monitor - Maintenance Cost
Ulsan	Environmental Measurement Equipment Inspection Cost
Ulsan	Air Pollution Monitor - Battery Change
Incheon	Air Pollution Monitoring - Public Cost
Incheon	Air Pollution Monitoring - Equipment Maintenance Cost
Incheon	Environmental Measurement Equipment Inspection Cost
Incheon	Air Pollution Monitor - Movement Cost
Incheon	Air Pollution Monitor - Outsourcing Maintenance Cost
Incheon	Environment Automatic Monitoring System - Outsourcing Maintenance Cost
Incheon	Air Pollution Monitor Network Establishment

Notes: Each row reports an item related to the alert system in each city.

Table A22. *Costs of the air pollution alert system management*

City	Cost [USD, 2017]	Cost [USD, 2018]
Gwangju	190,751	192,317
Daejeon	151,530	190,887
Daegu	293,925	309,012
Busan	375,257	471,538
Seoul	187,879	634,157
Ulsan	329,588	464,695
Incheon	485,627	571,334
Toal	2,014,558	2,833,940

Notes: Each row reports the system management cost of air quality alerts in the seven major cities in the sample.

Table A23. *RD Results for Credit Card Spending*

	Spending Category			
	All	Restaurant	Fashion	Travel
Reduced form				
$\mathbb{1}(RV >= 0)$	−18.977	−9.177	−2.191	0.676
	(13.820)	(3.096)	(0.608)	(0.799)
	[12.537]	[2.895]	[0.645]	[0.851]
RV	0.247	0.470	0.095	−0.034
	(1.018)	(0.144)	(0.028)	(0.044)
	[0.861]	[0.166]	[0.035]	[0.050]
RV $\cdot \mathbb{1}(RV >= 0)$	−1.778	−0.536	−0.103	−0.079
	(1.273)	(0.353)	(0.061)	(0.079)
	[1.423]	[0.354]	[0.067]	[0.085]
2SLS				
$\mathbb{1}(RV >= 0)$	−25.713	−12.434	−2.969	0.916
	(20.331)	(4.837)	(0.963)	(1.044)
	[17.333]	[4.430]	[0.874]	[1.123]
RV	0.381	0.535	0.110	−0.038
	(1.202)	(0.212)	(0.041)	(0.045)
	[0.906]	[0.186]	[0.034]	[0.054]
RV $\cdot \mathbb{1}(RV >= 0)$	−1.970	−0.629	−0.125	−0.072
	(1.405)	(0.373)	(0.067)	(0.076)
	[1.357]	[0.338]	[0.065]	[0.085]
Adjusted R^2	0.844	0.922	0.784	0.820
N	2,905			

Notes: This table reports results from four reduced-form local-linear regressions (top panel) and four 2SLS local-linear regressions (bottom panel). The dependent variable in all regressions is credit card expenditures per user for the relevant item group, measured in cents (11.5 KRW = 0.01 USD), and the independent variable of interest in the reduced-form (2SLS) regressions is an indicator for the running variable being above the RD threshold (advisory indicator). All regressions control for the running variable, an interaction between the running variable and the indicator for the running variable being above the RD threshold, temperature, precipitation, and year-by-month, day-of-week, and 1-day-before holiday fixed effects. The level of observation is the district by day, and observations are credit-card-user weighted. Parentheses (square brackets) contain standard errors clustered by the running variable (day of sample). The bandwidth is set to 20 in all regressions.