THE EFFECTS OF RGGI ON MORTALITY OUTCOMES

by

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A thesis submitted in partial fulfillment
of the requirements for the degree

of

Master of Science

in

Applied Economics

MONTANA STATE UNIVERSITY
Bozeman, Montana

May 2024

ACKNOWLEDGEMENTS

Thank you to Ms. Tori White for her knowledge and expertise.

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ABSTRACT

Most debates around market-based solutions to reduce greenhouse gas emissions often focus on greenhouse gas emissions reductions and cost-effectiveness. The Regional Greenhouse Gas Initiative (RGGI) is a cap-and-trade program designed to curb greenhouse gas emissions, and was implemented in 2009 across nine states in the greater New England area. The World Health Organization (WHO) states that over 6.5 million people die from air pollution annually. Particulate Matter of 2.5 microns or less in diameter is a major component in greenhouse gas emissions and has a myriad of deleterious effects to human health. This paper explores whether the RGGI policy had an impact on mortality rates, using a difference-in-differences approach, and estimates reduction in Cardiovascular related mortalities for the age cohort 15-64. I estimate that there are approximately 12 fewer deaths per county effected by the RGGI policy from 2009-2019.Combined with the 45 counties affected by the policy, there are an estimated 553 fewer cardiovascular related mortalities for the 15-64 age group from 2009-2019 as compared to the counties unaffected by the policy. Robustness checks are run to verify the reliability of this finding.

INTRODUCTION

According to the World Health Organization (WHO), approximately 6.7 million people experience premature deaths annually due to air pollution (WHO, 2023). Other estimates show the death toll solely from air pollution resulting from the combustion of fossil fuels at 10.2 million annually (Vohra, et al. 2021). Recent decades have seen efforts to mitigate its quantity and adverse effects on both people and the environment. Starting in 2009, policymakers implemented the Regional Greenhouse Gas Initiative (RGGI) to reduce greenhouse gas emissions using market forces. Extensive literature exists of the RGGI's impact on actual emissions reduction and the revenue it generates for state economies, there is limited research on its broader impacts, particularly concerning health and mortality. Examining health outcomes is crucial for economists as it allows exploration into the effects on consumer decisions and productivity. In 2020, the Federal Emergency Management Agency (FEMA) valued a human life at 7.5 million dollars. If the RGGI lowers emissions and prevents premature deaths, then for each death avoided there is an additional $7.5 million added to the benefits of the RGGI policy. Investigating whether the RGGI impacts mortality rates can provide additional monetary value to the RGGI and offer valuable insights for policymakers involved with the program.

I must first determine the impact of the RGGI on greenhouse gas emissions levels. The RGGI policy, a cap-and-trade policy implemented in 2009, covers nine states primarily concentrated in the New England and Mid-Atlantic regions. During the study period after the RGGI began (2009-2019), the policy set a yearly emissions cap and auctioned off permits for greenhouse gas emissions quarterly, with generated revenue benefiting state economies. Additionally, the emissions cap decreased annually. Existing literature explores the RGGI's impact on greenhouse gas emissions, revealing a significant decrease despite some leakage in regions sharing the same electrical power grid as the RGGI region. Knowing that the RGGI effectively reduced greenhouse gas emissions at a statistically significant level, I can now proceed to investigate its effect on mortality outcomes. To accomplish this, I identify PM 2.5 as a major component of greenhouse gas emissions and a pollutant with adverse effects on human health. I split this study into two phases, the initial phase involves examining the effects of the RGGI policy on average yearly PM 2.5 values for counties within the RGGI region from 1999 to 2019. I employ an event study to detect potential changes in PM 2.5 levels between counties affected by the policy and those that are not. To address the spatial relationship between emissions from point sources and health effects near power plants, I adopt a strategy outlined in existing health literature, which involves using 20km buffers around each treated power plant. The average intersection of these buffers as a percentage of the total county area is calculated, and treatment is assigned to counties containing 15% or more of the average intersection area as a percentage of the total county area.

With the current definition of treatment, the first phase reveals potential PM 2.5 changes between the treated and control counties. The results of the first stage prompt the second stage of investigating the effects of RGGI on mortality outcomes. PM 2.5 has been demonstrated to influence cardiovascular mortalities, along with a wide variety of other causes of mortality (Bowe et al., 2019). Taking this into account, I collect data for cardiovascular and all-cause mortality for the years 1999-2019 at the county level. Additionally, I gather data on mortalities caused by external forces to check for a placebo effect. These outcomes are collected for two age cohorts, 15-64 and 65-85, to observe potential changes among age groups. The next step involves partially exploring the parallel trends assumption surrounding a difference-in-differences model using an event study. Once this is completed, I run a difference-in-differences model to investigate the potential effect of the RGGI policy on mortality rates for the treated counties compared to the control counties over the entire post period (1999-2019).

Some concerns arise regarding the missing data concerning the average annual PM 2.5 values at the county level. To address this issue, I investigate the reasons behind the missing data from the sensors, which currently stands as 80% complete. The causes of this missingness are random, primarily stemming from factors beyond the control of air quality agencies, such as alterations in monitoring network requirements. This missingness can result in either an overestimate or an underestimate of the reduction in emissions. Additionally, the absence of mean annual PM 2.5 values at the county level may vary between treatment and control groups, potentially misrepresenting the extent of reduction in PM 2.5 values associated with changes or declines in mortality rates.

The mortality data has a completion rate of 96.5%. The missing data occurs because death counts below 10 for a specific age or cause of death are unreported by the CDC (Center for Disease Control) to protect the privacy of the deceased. Over 95% of the missing data pertains to External causes of death for the 65-85 age cohort. To tackle this issue, I impute the missing data with the lowest and highest possible values it could be, which are 0 and 9, respectively. External causes of death serve as an indicator of whether the treatment effect is genuine. The imputation of different values reveals that as the number of deaths approaches the 10 count threshold, the likelihood of the RGGI policy having an effect on External mortality rates approaches to 0. This suggests that the use of External causes of mortality is a valid method for checking for a placebo effect.

Among 15-64 year olds, the remaining missing data pertains to Cardiovascular death rates. I employ the same imputation method of assigning values of 0 and 9. The exclusion of the remaining 4% of missing data results in a slight underestimation of the effect of the RGGI policy on Cardiovascular mortality rates compared to when the values are imputed as 0 or 9.

Separating a study into two parts to examine the effects of pollution on mortality is nothing new. Neither is associating a decline in mortality rates with a subsequent decline in air pollution. And while this study does not introduce any groundbreaking models or methods, as far as I know, it marks the first attempt to investigate the impact of the RGGI on mortality rates beyond infants and newborns.

The initial phase of this study reveals a decline in yearly average PM 2.5 levels at the county level, accounting for 40% of the difference in mean PM 2.5 levels between the treatment and control groups during the pre-period (1999-2009). Consequently, I observe a decrease of 2.1 deaths per 100k in Cardiovascular mortality rates among the 15-64 age group in counties affected by the policy from 2009-2019 compared to control counties, translating to approximately 553 fewer deaths due to cardiovascular-related causes across all affected counties. The consistent support for the parallel trends assumption in both stages suggests a potential impact of the RGGI on cardiovascular mortality rates for the 15-64 age cohort. Using the statistical value of a human life of $7.5 million, this equates to approximately $4.1 billion in additional benefits for the RGGI policy. To validate this finding, I conduct a robustness check by running data with treatment defined as 5% and 25% of the total county area through the same difference-in-differences and event study models, yielding promising results.

BACKGROUND

The primary focus of this paper centers around the Regional Greenhouse Gas Initiative (RGGI). Established in 2005, the RGGI initially gained the commitment of seven out of the eventual 10 member states. Its provisions were set to become effective in 2009, as highlighted by Hibbard et al. (2018). The RGGI consists of a coalition of 10 states, namely Connecticut, Delaware, Massachusetts, Maryland, Maine, New Hampshire, New Jersey, New York, Rhode Island, and Vermont. However, in the later part of 2011 Governor Chris Christie of New Jersey made the decision to withdraw the state from the RGGI program (Hibbard et al. 2018). Consequently, the number of participating states has reduced to nine for the subject states for this study. The policy only targets power plants greater than 25 megawatts.

The RGGI sets a cap on greenhouse gas emissions every year and sells allowances through a quarterly auction. An allowance corresponds to a firm being permitted to emit one short ton of greenhouse gas emissions. The price for each allowance is set where quantity demanded is equal to quantity supplied. There are allowances that are withheld if the price falls below a certain trigger price, and allowances put into the auction if the price goes above the trigger price. The allowances are allowed to be traded between any firm within the RGGI region. This trading permits power plants capable of achieving emission reductions at a lower cost and faster pace to vend their allowances to counterparts facing challenges in meeting the same emission targets (Murray and Maniloff, 2015). Each firm is required to withhold allowances corresponding to 50% of their total emissions per year. To avoid an overabundance of allowances accumulating and potentially negating the declining emissions cap, a compliance period is issued every three years where firms must surrender all available allowances (Murray & Maniloff, 2015).

Proceeds generated from the auctioning of these allowances are subsequently distributed among the member states. These states, in turn, reinvest the revenue into a spectrum of local initiatives, spanning from enhancing energy efficiency measures to providing support to financially constrained individuals struggling with electricity bill payments (Hibbard et al. 2018). It is noteworthy that the expenses of the program to producers are absorbed by consumers through their monthly bills (Hibbard et al. 2018).

A study by Hibbard, et al. (2018) demonstrates the fiscal and employment benefits of the Regional Greenhouse Gas Initiative (RGGI). Analyzing the allocation of funds from the RGGI program offers insights into its expenditure patterns, shedding light on potential reductions in energy consumption and increased investment in renewables, which can indicate meaningful decreases in emissions. The authors run a simulation model known as PROMOD (Production Cost Model). PROMOD simulates the electricity market and assesses potential policy-induced changes, comparing the current scenario against a counterfactual without the implemented policy. The simulation considers factors such as fuel costs, generation capacities, and demand patterns to accurately model market dynamics.

The PROMOD model is utilized to forecast the impacts of the Regional Greenhouse Gas Initiative (RGGI) compared to a business-as-usual scenario for the electricity market in the RGGI region. Despite substantial economic benefits, with a net gain of $4.7 billion for the RGGI states and nearly 40,000 job years (Hibbard, et al. 2018), the model indicates that power producers suffered a net revenue loss of over $1 billion dollars during the study period (2009-18). Concluding, Hibbard, et al. (2018) claim the Regional Greenhouse Gas Initiative is an overall success and demonstrates the preferability of a cap-and-trade program over a carbon-tax.

LIT-REVIEW ON EFFECTS OF RGGI

A market-based solution for carbon emissions, such as the Regional Greenhouse Gas Initiative (RGGI), provides an opportunity to conduct a difference in differences study to examine the potential impacts of this policy on the reduction of emissions. This is the primary focus of existing literature, to determine whether the policy has resulted in a statistically significant decline in emissions since its implementation. Initially, studies primarily compared past emissions with current emissions using basic models. However, researchers have increasingly adopted a more sophisticated approach over time, considering factors such as leakage, where emissions are offset by demand in regions unaffected by the policy, and damages, such as acid rain resulting from SO2 emissions. Additionally, they have utilized advanced quasi-experimental methods.

One of the first papers to explore the impact of RGGI on emissions is Murray & Maniloff, (2015), who employ a difference-in-difference approach to compare state-year emissions within and outside the RGGI region during the compliance period. A log-log difference in differences model is utilized to estimate emissions at the state-year level, incorporating a binary variable indicating RGGI states during the compliance period (2009-2014), along with controls for coal and natural gas prices, employment levels, investments in renewable energy, and weather-induced demand fluctuations. The model accounts for an announcement effect, where plants may preemptively adopt lower-carbon technologies in anticipation of policy changes, thus mitigating the impact when regulations are enacted. To discern the effects of this announcement, emissions differences before and after program implementation is explored.

In the analysis, an additional indicator variable, representing states' announcements of intent to join RGGI is put into the model. This variable helps capture any changes in emissions occurring prior to the official implementation of the program, possibly due to anticipation of regulatory changes. By comparing emissions levels before and after the program's implementation, the authors can distinguish between the effects of the announcement and the direct impact of the program itself. Results suggest no significant statistical evidence against the continued reduction of emissions by the RGGI program. One possible explanation for the lack of impact of the announcement effect on emissions is the establishment of the annual greenhouse gas emissions cap, which is determined based on current emissions levels. This means that if a power-producing firm adopts emissions reduction technologies before the policy takes effect, the cap is adjusted accordingly to a level below the reduced emissions, effectively nullifying the anticipation effect. After controlling for factors like the natural gas boom and economic recessions, simulations indicate that emissions would have been 24% higher in the absence of the RGGI program, as Murray states, “Our simulations suggest that emissions would be 24% higher in the region if the RGGI program were not in effect, controlling for all other factors” (Murray & Maniloff, 2015, p. 8). Underscoring a significant estimated effect of emissions reduction attributable to the RGGI program.

This previous result is promising but it does not account for the potential leakage. Two papers deal with this issue (Fell & Maniloff, 2018) and (Chan & Morrow, 2019). Leakage in this sense, is where power producers on the same grid recognize the cap, and if needs cannot be met by other energy sources, then additional power is taken from states that are not under the RGGI policy, thus increasing the emissions in the non-RGGI regions.

The first study examined generator level results on capacity factors for coal and natural gas combined cycle (NGCC) plants before and during the RGGI period. To do this, the authors use a difference in difference style model with fixed effects at the plant and year levels, plus a within estimator to analyze the treatment effect associated with RGGI. Results indicate an increase in generation from the leaker regions (Pennsylvania & Ohio as they are on the same grid), primarily from cleaner natural gas emissions. While RGGI induced some leakage, it also prompted a reduction in emissions-intensive generation within the regulated region and an expansion of cleaner generation in the unregulated region, leading to an overall reduction in emissions across both areas. This pattern was further supported by electricity transmission data (Fell & Maniloff, 2018).

The second study analyzed data collected from generators across the continental United States spanning from 2002 to 2016. Utilizing emissions data for CO2, SO2, and NOX, the authors employ a log-linear difference-in-differences approach with fixed effects for facilities, months, and years to assess whether the policy resulted in significant reductions in these emissions sources (Chan & Morrow, 2019). The model also accounted for temperature variations, renewable portfolio standards policies, and fuel prices. Results indicate a statistically significant decline in CO2 emissions in RGGI states compared to non-RGGI states, with a magnitude of approximately 22%. Notably, this reduction aligns closely with findings from the Murray & Maniloff (2015) study. Furthermore, SO2 emissions exhibited a significant decline of approximately 38% in facilities within RGGI states compared to those in non-RGGI states, attributed to overall decreases in electricity generation and a shift towards less polluting firms. Notably, there was no statistically significant reduction observed in NOX emissions (Chan & Morrow, 2019).

To investigate leakage, the authors interacted an indicator variable with the RGGI treatment year to assess its impact in nearby states like Ohio and Pennsylvania. The analysis revealed that while leakage diminished the impact of RGGI to some extent, the transition to cleaner energy sources and more efficient fuel utilization (such as natural gas) in both the leaker and RGGI regions countered the effects of leakage (Chan & Morrow, 2019). Although the study did not provide a precise quantification of this counteraction, it underscored that despite leakage, the policy still resulted in statistically significant declines in emissions[1].

While having precise numerical data on the extent of emissions reduction facilitated by the policy may seem convenient, it is not necessarily indispensable for the focus of this paper. Rather than quantifying emissions reduction in terms of specific numbers or percentages, the primary objective of this study lies in examining the health impacts of the RGGI policy. The existence of substantial evidence demonstrating, to a statistically significant degree, that the RGGI policy effectively reduced emissions despite leakage, provides sufficient justification to explore its ancillary benefits. Understanding the exact magnitude of emissions reduction does not inherently provide a direct indication of the potential reduction in mortality attributable to the policy. Establishing such connections requires a comprehensive analysis of the policy's impact on mortality outcomes. Given the ample evidence suggesting both direct and indirect reductions in emissions due to the policy, this analysis can proceed. To effectively isolate the effects of the treatment, only counties within the nine RGGI states will be evaluated. This approach ensures that the effects of the treatment are not diluted by leakage. Which means, counties in Pennsylvania and Ohio will be excluded from the analysis.

[1] To see the results of the leakage and to assess quantification see table 6 in Chan&Morrow (2019).

PM 2.5 is widely recognized as one of the most hazardous components of greenhouse gas emissions. The largest source of PM 2.5 comes from combustion of fossil fuels (Vohra, et al. 2021). Since the RGGI policy reduces greenhouse gas emissions, it should therefore create some health benefits by reducing PM 2.5 present in greenhouse gas emissions. Research has elucidated the health effects associated with PM 2.5 exposure, and a brief overview on the effects of PM 2.5 on health can give context to the mortality outcomes that I use in this paper. PM 2.5 is not a singular compound but rather a complex mixture of particles, including small liquid droplets, desiccated solid fragments, or combinations thereof. The term "PM 2.5" refers to particles with a diameter of 2.5 microns or less, meaning PM 2.5 is a component if PM 10. The health impacts of PM 2.5 are diverse and far-reaching, affecting individuals across all age groups from neonates to seniors. These impacts are particularly pronounced in relation to the respiratory and circulatory systems.

With respect to neonatal impacts, Lee and Park (2019) use a state and time fixed effects to examine the impact of RGGI on infant mortality, building on previous research establishing a positive association between CO2 emissions and infant mortality. Their analysis revealed a decrease of 0.43 per 1000 Infant Mortality Rate (IMR) between RGGI states and non-RGGI states, as well as a decrease of 0.61 per 1000 for Neonatal Mortality Rates (NMR) between RGGI states and non-RGGI states. Intriguingly, these reductions were found to be statistically significant only for male newborns, with no significant effects observed for females. This gender disparity is attributed to the larger airways in female infants during the first year of development, potentially reducing their susceptibility to harmful emissions (Lee & Park, 2019).

Seniors represent another demographic vulnerable to PM 2.5 exposure. Qian et al. (2017) conducted a study spanning from 2000 to 2012, involving an open cohort of all enrolled Medicare recipients. By utilizing annual averages of PM 2.5, the researchers estimated the risk of death associated with a 10 microgram per cubic meter increase in PM 2.5, in conjunction with 10 parts per billion of ozone (O3). Employing a Cox[2] proportional hazards model controlling for demographic characteristics and area-level covariates such as weather, and topography. The model's main dependent variables are expressed as an absolute risk in mortality, for long term exposure to PM 2.5 and O3. they found that exceeding these pollutant thresholds led to a 7.3% increase in All-Cause mortality. When the current EPA standards of exposure to 12 micrograms per cubic meter of PM 2.5 and 50 ppb of O3 were exceeded by the same threshold, All-Cause mortality increased to 13.6%. While the risk of death was higher for men and blacks, the study revealed statistically significant adverse health effects for the entire Medicare population when exposed to PM 2.5 and O3 concentrations below current EPA standards. These findings suggest that even modest decreases in PM 2.5 levels below current EPA standards for good air quality could yield beneficial health outcomes (Qian, et al 2017). The authors highlight the increase in vulnerability of the elderly population to the effects of PM 2.5.

Pollution ranks as the fourth leading cause of death in the US, with the release of PM 2.5 and other pollutants exacerbating climate change, thereby increasing the frequency and severity of wildfires and heatwaves. These environmental shifts, in turn, escalate the demand for power, perpetuating the cycle of emissions (Rajagopalan & Landigran, 2021). The study by Rajagopalan & Landigran (2021) present an overview of the effects of heightened exposure to PM 2.5.

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[2] For more information on what this model entails, see pg. 2515 in Qian, et al.(2017).

Increased exposure to PM 2.5 triggers inflammation and oxidative stress, straining the body and disrupting its essential functions for maintaining wellness. Moreover, long-term exposure to PM 2.5 is associated with arterial plaque buildup and left ventricular hypertrophy, increasing susceptibility to cardiovascular diseases. This chronic stress contributes to the development of various conditions, including hypertension, thrombosis, pulmonary edema, heart failure, stroke and type 2 diabetes, ultimately leading to mortality (Rajagopalan & Landigran, 2021).

Bowe, et al. (2019), demonstrate that the spectrum of health effects influenced by PM 2.5 is consistently growing. They employ ensemble modeling, which integrates predictions from various modeling techniques and datasets, to examine the pathways linking PM 2.5 exposure to mortality in a cohort of 4.5 million veterans spanning 2006-2016. The study controls for factors including race, location, and socioeconomic status. The authors identify nine established causes of death linked to PM 2.5 exposure, and introduce three new ones: chronic kidney disease, dementia, and hypertension, supplementing the existing list which includes cardiovascular disease, cerebrovascular disease, chronic obstructive pulmonary disease, type 2 diabetes, lung cancer, and pneumonia (Bowe, et al., 2019). They conclude that the impact of PM 2.5 on health is broadening, noting that nearly all deaths in the cohort were associated with ambient PM 2.5 levels below EPA standards. This suggests the potential need for stricter standards to mitigate PM 2.5-related mortality.

Considering the broad spectrum of health effects resulting from PM 2.5 emissions, it becomes evident that certain demographic groups and causes of mortality could be significantly affected by any reduction in PM 2.5 exposure. The wide array of health effects linked to PM 2.5 and previous research utilizing All-Cause mortality present an opportunity to investigate it as a potential outcome that can be influenced by a decrease in PM 2.5 resulting from emission reductions. In addition to All-Cause mortality, there is an association between PM 2.5 levels and cardiovascular-related forms of mortality, so it is prudent to include this as an outcome when examining the potential effects of the Regional Greenhouse Gas Initiative on mortality.

SAMPLE FORMATION

This project focuses on modeling the impact of the Regional Greenhouse Gas Initiative (RGGI) policy on mortality outcomes across different age groups and type, including All-Cause, Cardiovascular, and Accidental (such as automobile accidents and other external causes of injury). Despite leakage of carbon emissions from neighboring states like Pennsylvania and Ohio, the RGGI successfully reduced carbon emissions by approximately 19-24%, as documented by (Chan & Morrow, 2019) and (Murray & Maniloff, 2018). I select PM2.5 as the pollutant for historical data collection to form this sample due to its prevalence in carbon emissions and its potential to cause adverse health effects, as detailed in the health effects section.

The decision to exclusively monitor PM 2.5, rather than additional components, aligns with the primary focus of the RGGI policy on reducing carbon emissions, as PM 2.5 is a crucial element in carbon emissions. To address concerns regarding sulfur dioxide emissions from fossil fuel combustion primarily causing pulmonary problems, data collection for mortality statistics excluded categories related to pulmonary health. Furthermore, the majority of sulfur dioxide emissions were found to originate from sources over 200km away, as indicated by the CAMx study conducted by (Wagstrom & Pandis, 2011), while PM 2.5, conversely, is mostly regional and from local sources (Wagstrom & Pandis, 2011), (Isakov, et al. 2012).

Monitoring sulfur dioxide emissions would require impractically large monitoring areas. Although the RGGI policy did not significantly impact nitrous oxide emissions, as indicated by Chan & Morrow, (2019), this may be due to existing measures aimed at reducing such emissions. The consistent decline in emissions observed over the study period suggests that the RGGI's influence on nitrous oxide emissions was minimal. Meaning, tracking nitrous oxide emissions would yield little insight into the effectiveness of the RGGI policy. It's worth noting that nitrous oxide primarily affects pulmonary health (CDC, 2024); hence, excluding pulmonary-related mortality categories allows for a more accurate assessment of the RGGI's impact on carbon emissions.

The decision to utilize historical AQI data for PM 2.5 for the available regions is different than another common method which is air dispersion modeling. This policy affects the emissions from power plants, which are point source emitting sources. The preferred method of analyzing the dispersion of emissions is the Gaussian puff model (El-Harbawi, 2013), and the most widely used and EPA-approved Gaussian puff model is AERMOD. However, beyond the technical and specialization in atmospheric science required to accurately calibrate and utilize the model, it is primarily used on a relatively small scale, while this policy impacts a much larger area of 157 counties in nine states over 10 years. Thus, to conduct a study using an air dispersion model would require vast amounts of data, training, and cost that is beyond the scope of this paper. For more information about the basics, and limitations of air dispersion modeling can be found in the appendix labeled Air Dispersion Lit. Review.

Designation Of Treatment

The designation of treatment and control groups to explore the effects of this policy is based on observed PM 2.5 values. These values were taken from the EPA's Air Quality System API at the county and year level for the years of 1999-2019[3]. From here, I collect all the data for the given time period and merged it. Of the 157 counties in the region publicly available, data only

[3] Data was gathered for the pollution code 88101 and 88502, the two designations for PM 2.5 by the EPA. https://aqs.epa.gov/aqsweb/documents/data_api.html#annual.

existed for 75 total counties. I was advised by a physical scientist at the EPA[4] to use the 24-hour mean measurements certified by the 2012 24-hour standard that were over 75% complete for analysis. For counties with multiple sensors, I was advised to take the average of all of them to represent the average concentration for the county in question.

After compiling the concentration data for the respective years, I review the health literature to determine a buffer zone indicative of observable adverse health effects within the surrounding population. This approach sought to refine the proximal threshold for discerning between treatment and control groups with greater precision in a causal context. Specifically, its aim was to pinpoint the spatial correlation between pollutants stemming from greenhouse gas emissions and associated health impacts within a defined proximity of a power plant. As previously mentioned, pollutants can disperse over considerable distances (Wagstrom & Pandis, 2011), but this does not necessarily imply that minimal exposure to pollutants over vast distances directly influences the measured mortality outcomes mentioned earlier. A study investigating the association between proximity to coal-fired power plants and the prevalence of neurobehavioral disorders among children in Louisville, Kentucky, revealed 24 statistically significant hot spots of diagnosed anxiety disorders and ADHD within 10 miles of two coal-fired power plants. The study, encompassing 235 children aged 6-14 recruited between fall 2015 and March 2020, identified a causal relationship between these hot spots and weekly PM10 concentrations (Zhang et al., 2022). The results indicate a distance decay effect, wherein the presence and significance of these hot

[4] The physical scientist was a Ms. Black at the Air Quality Analysis group in North Carolina.

spots diminish as the distance from the power plants increases. This finding aligns with the study's initial hypothesis and is consistent with established trends in the field and existing literature.[5]

A similar study explores the association of proximity powerplants and adverse health effects in newborn infants regarding low birth weight, preterm delivery, and very preterm delivery. The researchers tracked 423,715 single child births in Florida between 2004 and 2005 as well as all active power plants and PM 2.5 emissions. Exposure to PM 2.5 for the women living near the powerplants was tracked noting that coal and solid waste plants were the highest producers of PM 2.5 compared to the other measured types (nuclear & natural gas). It was stated, "We also created 20 km buffers around each birth and determined the total number of power plants within this buffer (Table 4). The association between adverse birth outcomes and total number of power plants within 20 km was determined. Compared with pregnant women who lived with no power plants within a 20 km radius, women living near ≥2 power plants had a 7% increased odds of term LBW (OR = 1.07, 95% CI: 1.01, 1.12), 12% increased odds of PTD (OR = 1.12, 95% CI: 1.09, 1.15), and 17% increased odds of VPTD (OR = 1.17, 95% CI: 1.09, 1.25). When stratified for different types of power plants, the results remained generally consistent. Coal was strongly associated with all adverse birth outcomes" (Ha et al., 2015, p. 220). This indicates that adverse health effects on newborns are more pronounced within a 20 km radius of a power plant compared to beyond this distance. While the authors examined the proximity of births to power plants, a similar analysis focusing on the proximity of power plants to births within 20 km would likely yield comparable results. This, in conjunction with the distance decay effect, provides evidence that creating 20 km buffers around all power plants in the sample can effectively capture the impact of emissions on

[5] For a more comprehensive lit review exploring different distance decay thresholds for certain pollutants see (Zhou & Levy, 2007).

mortality. Such an approach facilitates accurate assignment of treatment and control statuses, particularly if there is any overlap between these buffers and control counties.

I utilize the spatial locations of the powerplants effected by the RGGI policy and proceeded to map 20 km around each plant, facilitating the calculation of intersections with the originating county and potential surrounding control counties. To quantitatively assess the spatial relationship between point source emissions and affected areas, I compute the intersection area of these buffers a percentage of total county area. In counties hosting multiple power plants, the intersection areas of each plant were aggregated and averaged to gauge the level of impact on the county. Similarly, for counties without power plants but with multiple intersections, the same procedure was applied.

After computing the average intersection area as a percentage of the total county area, I identified 47 treated counties and 28 control counties. Among these, 11 control counties experienced spillovers from the 20km buffers of treated power plants. Treated counties showed intersection areas ranging from 6% to 97% of the total county area, with a mean of 37% and a median of 28%. Conversely, the 11 spillover counties had intersections ranging from 0.3% to 25%, with a mean of 9% and a median of 8%. This observation raised concerns as some treated counties exhibited less average intersection area than control counties, which seemed illogical given their designation. To address this issue, I determined a threshold value for the average intersection area as a percentage of total county area (i.e., exposure) that minimally affected the overall number of counties in both treated and control groups. This threshold was set at 15%, serving as a proxy for treatment beyond mere presence of a power plant affected by the RGGI policy. Consequently, treatment was defined as counties with 15% or more of average buffer intersection area as a percentage of total county area.

After completing this process, there were 45 treatment counties and 30 control counties. Increasing the number of counties in the control group would help reduce the variability of standard errors. However, neighboring counties in Pennsylvania and Ohio were deemed unrepresentative due to documented emissions leakage from the grid in these regions (Maniloff & Fell, 2018). Keeping this in mind, I consider alternative counties from states not in the Regional Greenhouse Gas Initiative (RGGI). Therefore, all adjacent counties in West Virginia and Virginia bordering Maryland, without power plants exceeding 25 MW, were eligible for classification as controls under the RGGI policy. This narrowed the selection to a total of 12 counties that could potentially be added to the control group, provided there was available PM 2.5 Air Quality Index (AQI) data for them. Out of the 12 selected counties, data was only available for two counties. These counties were then verified to be located outside the 20 km buffer zone, ensuring no intersections greater than 15% with power plants in adjacent counties. As a result, the sample included two additional counties: Berkeley County, WV, and Loudoun County, VA. With the additional counties added to the control group the total number of counties in my sample is 77, with 45 treated counties and 32 control counties. A map depicting the counties, with the darker grey being control counties and the lighter blue counties being treated counties, with the blue circles depicting the locations of the powerplants with a 20km buffer around them.

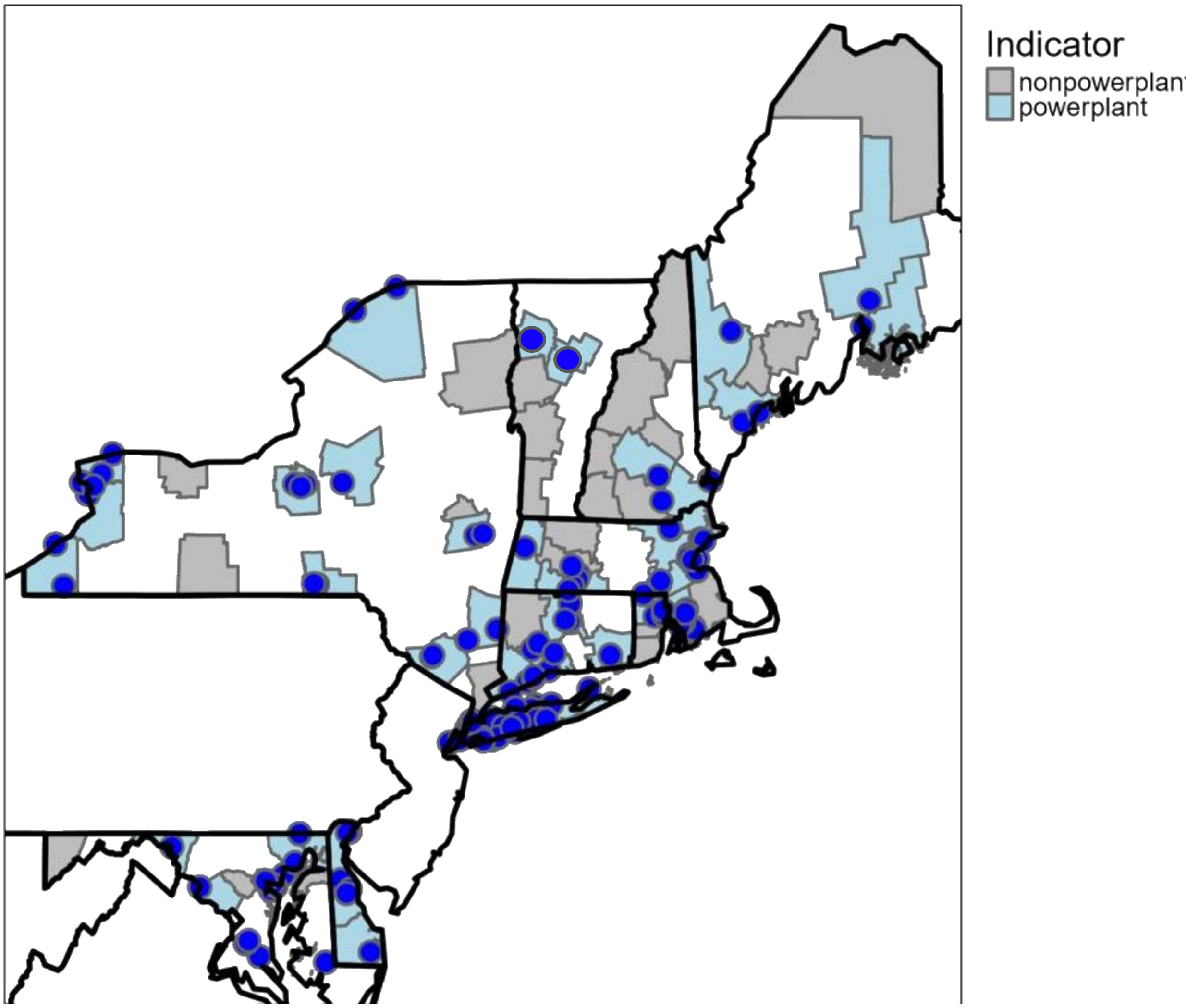

Figure 1. Map Depicting Treatment and Control Counties With 20km Buffers (map not to scale).

Sample Strengths and Weaknesses

Utilizing observed PM2.5 data allows for a direct and precise comparison to identify causal trends in mortality resulting from the Regional Greenhouse Gas Initiative (RGGI) policy. Unlike previous studies focusing on smaller areas and shorter time frames, this study examines a broader area and longer time span, aiding in identifying gradual mortality trends due to emissions exposure. By establishing buffers based on documented health outcomes and accurately identifying the distance decay effect, this study provides a method to assess exposure at the county level. The method I employ for classifying treatment also identifies the potential spillover of greenhouse gas emissions from treated plants into control counties, thereby attempting to address the reality that pollution does not remain confined within county boundaries. Using different age group cohorts allows my analysis to examine the effects of the RGGI policy on different age groups in the population, potentially identifying effects that are more pronounced in one age group than another.

The RGGI region, spanning from 1999 to 2019, encompassed 9 states and 157 counties. However, this sample consists of only 77 counties, of which 75 are from the RGGI region. This sample size is notably smaller than the entirety of the RGGI region, potentially limiting the study’s ability to fully capture the impact of the RGGI policy. Conducting a broad-scale analysis may provide insights into overarching trends; however, it may overlook more localized instances where the impact of the policy is more pronounced. Consequently, localized effects might be overshadowed in the broader comparison, potentially leading to an incomplete understanding of the policy's impact at a localized level.

DATA AND SAMPLE STATISTICS

Mortality data at the county and year levels was collected from the CDC WONDER database spanning from 1999 to 2019, encompassing age ranges 15-64 and 65-85. I calculated the crude rate per 100k by taking the total number of observed mortalities and dividing it by the total county population for a given year. Before merging, the datasets lacked age and type of death identifier variables, which were subsequently added to facilitate integration.[6]

All-cause mortality data encompassed all types of fatalities available from the CDC WONDER database, excluding pulmonary and circulatory causes. Suppressed and unreliable values were retained; a suppressed value denotes reported deaths under 10, withheld for privacy reasons, evidently the inclusion of these suppressed values were simply registered as not available or missing. Cardiovascular death rates were extracted from the same source for identical age ranges across 77 counties. Cardiovascular deaths included all ICD-10 codes falling under the circulatory category in the CDC WONDER database[7], with suppressed values included. The data regarding mortalities for transportation accidents and other external forces, labeled as "External," were collected from the WONDER database (ICD-10: V01-V99, W00-W59), containing mortality counts for the specified causes. Like other datasets, suppressed values were retained in the dataset and denoted as "NA" or not available.

The missing death rates, comprising only 3.5% of all mortality data, mostly affected the External 65-85 age group, accounting for 96% of missing data. As age increases, individuals are less likely to be involved in mortality causes such as automobile accidents or exposure to electrical

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[6] All cleaning, gathering, merging of data and regression running was done using R.
[7] Website for health data: https://wonder.cdc.gov/controller/datarequest/D76

current. This disparity in missing data is likely due to the fact that this age group has a lower involvement in such types of mortalities. Additionally, the biggest risk factor to mortality in this age group is likely age itself, especially the closer individuals get to 85. The RGGI policy theoretically should not affect External mortality, thus missing data might skew estimates. Tables 14 and 16 in the additional figures depict results of a difference in differences model. Compared to table 8, which displays results for the average treatment effect of the treated at the 15% treatment level, all results are negative. Imputing missing entries as 0 yields more negative coefficients. Imputing values of 0 and 9, the lowest and highest possible, respectively, was chosen. The increased negativity and statistical significance (95%) of coefficients when missing values are imputed as 0 suggest a larger treatment effect due to the RGGI policy. However, this reasoning is flawed as the event study (figure 17) shows potential violation of parallel trends in the pre-period. Additionally, coefficients for data imputed as 9 are statistically significant (95%) and closer to 0, suggesting diminishing treatment effects as values approach 10, and reinforcing the notion that RGGI should not affect External mortalities.

In the Cardiovascular mortality for the 15-64 age group, there were only 14 missing total values. PM 2.5 definitively impacts Cardiovascular mortality, as stated in health literature. The RGGI policy likely affects this type of mortality. Inclusion of values imputed as 0 or 9 should result in an underestimation compared to the 15% treatment level. Tables 13 and 15 display these results, showing only slightly more negative results than the 15% treatment level: 0.4 when imputed as 0, and 0.3 when imputed as 14, respectively.

The RGGI program's member powerplants' locations is sourced from the EPA's CAMPD (Clean Air Markets Program Data). Facility-level locations and operating status from the relevant time period (1999-2019) determined the locations of the powerplants depicted in the map of Figure 1. Approximately 1% of the data involved powerplants either ceasing operations or new plants being commissioned during this period; I removed this operation change status from the analysis to ensure consistency for powerplants affected by the policy throughout the entire duration. The powerplant data was then merged with health data by county name. Since several counties shared the same name, merging proved challenging. To address this, I use the full FIPS code including the state, and use this for both datasets, along with the year, to facilitate proper merging. An indicator variable was created to denote counties containing powerplants. I earmark the locations of powerplants within counties, as well as control counties, for mapping and buffer zone creation. Then I separate counties with multiple powerplants from the main dataset, excluding associated years, and subsequently merge the data containing all control counties.

The annual PM 2.5 data observed was 80% complete, with some monitors discontinued partway through the time period or newly installed, resulting in missing data. Rather than drop the missing or incomplete data, I kept it as part of the dataset and the missing values were registered as NA. When generating graphs, the unavailable values were not counted or treated as 0; instead, the graphs were constructed using the available data. Further details on the data collection process are provided in the Sample Formation section. The trends in average PM 2.5 concentrations between treatment and control counties are illustrated in Figure 2 below. A noticeable downward trend is observed before the treatment date and continues after the treatment date as well. Mean concentrations and standard deviations for both treatment and control groups are presented in Table 1. As anticipated, the concentrations in treated counties are higher than those in control counties, with statistically significant difference in means.

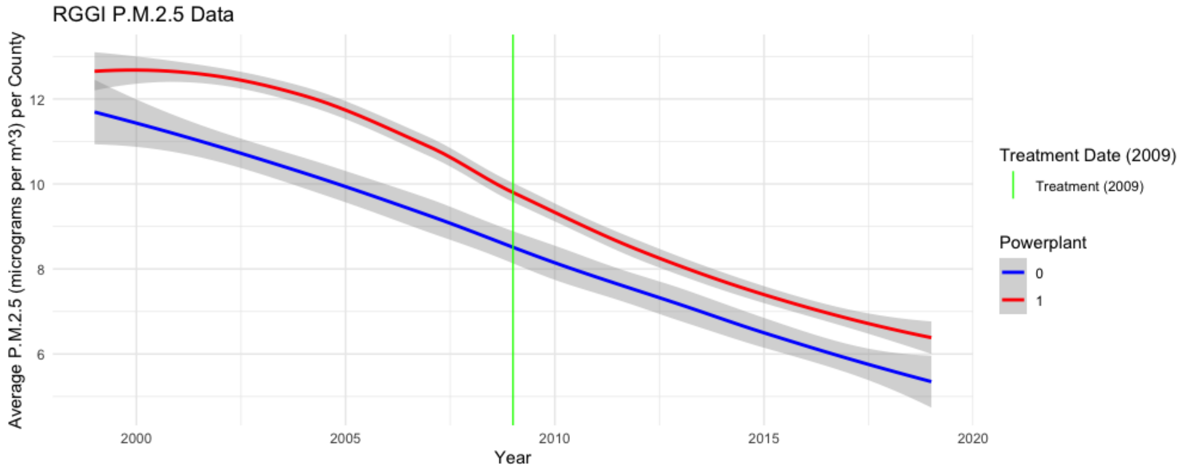

Figure 2. Notes: 0 denotes control counties, 1 denotes treatment counties. The grey shades around the line represent the 95% confidence interval.

In this study, I utilize PM 2.5 data that is only 80% complete, with missingness primarily split 60/40 between control and treatment groups. As informed by the same physical scientist, missing data from sites predominantly stem from factors such as changes in funding, loss of lease, monitoring network requirements, and weather, which appear relatively random and non-deliberate. As a result, there may be an overestimate or underestimate of the first-stage estimates. For instance, if missing data in the control group originate from urban areas, it could inflate the average yearly PM 2.5 levels at the county level, resulting in an underestimation of the treatment effect due to a smaller observed difference compared to the true difference with complete data. Conversely, missing data from predominantly rural areas in the control group could lead to an overestimation of the treatment effect, as the observed difference in mean yearly PM 2.5 concentrations at the county level would be much larger than the true difference if all data were present.

The populations in the treatment and control groups exhibited significant disparities. Treated counties had an average population of approximately 585,757, whereas untreated counties averaged approximately 241,155 residents. There were no missing population values. I obtained the population data from the CDC WONDER database; it reflects annual fluctuations at the county level across various age brackets. Population figures are influenced by current-year deaths and estimated migration, leading to inherent variability. To normalize the distribution of the variable across sub-categories, I use a logarithmic transformation. This transformation aimed to achieve a more uniform distribution and improve linearity between the dependent and independent variable. Fig.4 compares the population variable before and after transformation. Comparative analyses of the log-transformed population between treatment and control counties can also be seen in Fig. 4.

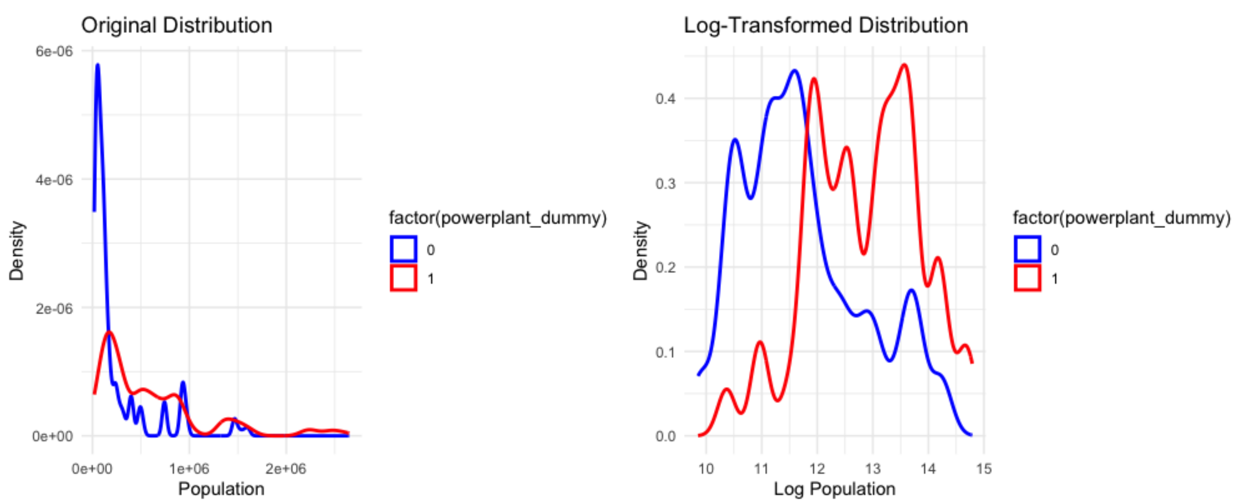

Figure 3. Note: 1 signifies treatment counties, 0 signifies control counties.

The temperature variable captures the average temperature per county per year. This dataset was compiled from NOAA by The Washington Post and made available on GitHub[8]. The dataset's creation involved utilizing NOAA's gridded climate divisional database alongside daily temperature and precipitation datasets. It tracks the average mean temperature trends in each of

8 WP weather data: https://github.com/washingtonpost/data-2C-beyond-the-limit-usa

the lower 48 states and counties from 1895 to 2019. Data corresponding to the respective FIPS codes for treated and control counties was extracted for the years 1999 to 2019. Table 1 presents the mean temperatures between treatment and control counties. Notably, treatment counties exhibit an average temperature over 4°F warmer than control counties, possibly attributed to the generally more northerly location of control counties.

As presented in Table 1, the disparities between the mean treatment and mean control mortality rates per 100k variable exhibit statistical significance at the 95% confidence level for all subcategories and age-ranges, with the exception of the Cardio category lacking age range specification, and the All-Cause (15-64) category. The dataset comprises 4,032 total observations for the control group and 5,670 total for the treatment group. When I run regressions using different types of mortality and age groups, the number of observations for each regression is lower. Given this disparity in the number of observations, I use a Welch t-test to ascertain the significance of differences in means. Interestingly, the control group manifests higher age-adjusted death rates compared to the treatment group across all overarching categories when age brackets are not considered. However, upon disaggregating by age groups, the control group only demonstrates higher means within the External 65-85 group and the All Cause 65-85 age group. The statistical disparity in means across all general categories underscores fundamental differences in population demographics, temperature, and average PM 2.5 exposure between the control and treatment groups.

	Control	Treatment	Diff
temp	47.1	51.4	-4.3**
	(4.30)	(4.30)	(0.1)
PM 2.5	8.3	9.8	1.5**
	(2.80)	(2.80)	-0.8
Log_Population	11.7	12.8	-1.1**
	(0.01)	(0.01)	(0.02)
Rate_per_100k:			
All Cause	263.70	237.80	25.9**
	(3.90)	(2.60)	(4.7)
All Cause (15-64)	141.9	143.4	-1.6
	(1.20)	(1.20)	(1.7)
All Cause (65-85)	385.60	332.2	53.3**
	(3.90)	(341.10)	(4.8)
Cardio	155.9	147.8	8.2**
	(3.40)	(2.60)	(4.2)
Cardio (15-64)	44.6	45.8	-1.2*
	(0.50)	(0.50)	(.96)
Cardio (65-85)	265.00	249.7	15.3**
	(2.80)	(2.00)	(3.5)
External	21.3	17.7	3.6**
	(0.30)	(0.20)	(.4)
External (15-64)	24.1	22.1	2.1**
	(0.50)	(0.30)	(0.6)
External (65-85)	18.2	13.3	4.9**
	(0.40)	(0.20)	(0.5)
Total # of Observations	4,032	5,670	9,702

Table 1: Summary Statistics. Notes: standard deviation in parenthesis below mean. Welch t-test used for different sample sizes. *** denotes 1% significance, ** denotes 5% significance, *denotes 10% significance. Total number of observations for entire sample denoted in final row.

ECONOMETRIC MODELS

To examine the effect between the RGGI policy and its impact on mortality, I will use a difference in differences model, and an event study model. The selected models aim to address potential complexities inherent in analyzing the causal relationship between this policy and mortality, while controlling for population and temperature. The assignment of treatment and control counties was based on the overlap of powerplant buffers, aiming to account for the spatial relationship of exposure to individuals residing in proximity to point source emissions. As stated before, treatment is defined as counties with average buffer intersection areas accounting for 15% or more of total county area. The model used for the event studies is listed below:

$$rate\_per\_100k_{it} = \beta_0 + \beta_1 log\ \_pop_{it} + \beta_2 temp\_f_{it} + \sum_{h=-10}^{10} \tau_h * 1[K_{it} = h] + \tau_{-1} * 1[K_{it} = 1] + Treat_i + Post_t + \theta_i + \mu_t + \varepsilon_{it}$$

Here, in the above model, the outcome is rate per 100k, it is subset to different age groups and categories before each run of the regression. This outcome variable represents the mortality rate per 100k, at the county and yearly level. The θ_i and μ_t are the county and year fixed effects, to account for time and county effects that remain constant across those units, like weath. One of the control variables in this regression is $\beta_1 temp_{it}$. The temperature variable is in Fahrenheit, and it is included to account for the increased exposure of PM 2.5 that comes with lower temperatures, with lower temperatures there is a higher concertation of PM 2.5 which can increase exposure and higher exposure may lead to higher mortality (Cichowicz et al., 2017),(Wylie,2021). The log transformation variable is included to account for the changes in mortality rates due to population, and the log transformation reduces skewness in the distribution of the variable and improves linearity between the outcome and independent variables. The K_{it} variable is defined as the event time where $K_{it} = t - E_i$ where E_i is the event date. In this case the event date is 2009, and to distinguish it from treated and control groups it is interacted with a powerplant dummy that takes on the values 2009 for treated units and 0 for untreated units. The range of the sum is meant to distinguish the number of lags and leads. In this case, -10:10 is the length of the study in event time and T_h is the estimate of the treatment effect per year distinguishing the difference in treatment between the treatment and control groups for each year in the sample (1999-2019).In other words, this T_h term shows the difference between the outcome variable for the treatment and control groups for the years in the study. In the pre period the coefficients represent the trend of the outcome between the treatment and control groups. In the post period the coefficients represent the average effect of the treatment for each subsequent year after the treatment began relative to when the treatment came into effect. It is customary to exclude one year before the start of treatment to account for anticipation of the policy, and it is excluded to account for the effect this may have on the treatment in the post period, the variable τ_{-1} is the indicator variable that will remove the year just before RGGI started in 2009. The final two variables $Treat_i \& Post_t$ are binary indicator variables identifying treatment county i, and t for the period after RGGI begins in 2009.The final term in the modelε_{it}, is simply the error term. The model for the difference in differences design is below:

$$rate\_per\_100k_{it} = \beta_0 + \beta_1 log\_pop_{it} + \beta_2 temp\_f_{it} + \beta_3(Post_t * Treat_i) + Treat_i + Post_t + \theta_i + \mu_t + \varepsilon_{it}$$

The variables defined here are identical to the variables described above for the event study model. The key difference here with this model is that it calculates the aggregate effect of the treatment. This is captured in the variable $\beta_3(Post_t * Treat_i)$ which represents the difference in mortality rate per 100k between the treatment group as compared to the control group in the post period (2009-2019). For example if the value of the coefficient was -2, this would mean there are two less deaths per 100k of people in the treatment group compared to the control group in the post period. This coefficient is the main coefficient of interest in this model as it represents the difference in differences due to the policy.

A difference in differences model earns its name from its method of estimating the effect of a treatment or policy. It achieves this by calculating the difference between the mean outcome for the treated group in the post period and that in the pre period. Similarly, it calculates the difference for the control group between the post and pre periods. This initial step constitutes the first "difference." Subsequently, to determine the estimated treatment effect, we calculate the difference between the mean outcome for the treatment group and the mean outcome for the control group, after completing the first difference. Hence completing the difference in differences. Difference in differences comes with some assumptions in order for the results to be considered valid. The first is the parallel trends assumption, the second is the no anticipation assumption, and the third is a representative sample.

The first assumption, no parallel trends, simply states that the average outcome for the treated and untreated groups would have changed in parallel in the absence of treatment. The outcomes can be at different levels prior to treatment, but they should trend similarly in the time leading up to treatment. This is where the event study equation comes in; I use it to partially test parallel trends. Each coefficient in the event study represents the difference between the treatment and control group outcomes for that year leading up to the enactment of the policy or after the policy. Ideally, in the pre-period, I would observe coefficients that are not statistically different from zero, indicating a rather flat trend and no change in outcomes between the treatment and control groups over time. Therefore, if there is a trend in the post-period, it can be attributed to the enactment of the policy, as the assumption is that in the absence of treatment, the two groups being compared would trend the same. If this assumption does not hold, attributing any changes in the post-period to the policy becomes difficult.

For example, the outcome variable of interest in my study is the mortality rate per 100k. If there were a lack of funding or some regulation that closed several hospitals in treated counties, particularly in more populated areas. Which results in people suffering from critical conditions having to travel farther to receive treatment, then this could cause mortality rates in the treatment group to deviate from the trend of the control group. If this occurs in the pre-period, it would be challenging to attribute any differences in mortality rates in the post-period to the Regional Greenhouse Gas Initiative (RGGI).

The second assumption, no anticipation, simply means that the knowledge of the start of a treatment has no causal effect before the treatment is enacted. An example of this would be in the context of the RGGI. If powerplant owners were aware that the policy would be implemented years in advance, they might adopt greenhouse gas emission-reducing technologies before the RGGI begins to lower the financial burden of the program. If this were to occur, emissions would start to decline before the policy began, potentially leading to a reduced impact of the RGGI on emissions, or an overestimate of the RGGI's impact on mortality rates because emissions had already been declining for years before the policy began.

The third assumption entails a representative sample, indicating that the randomly selected sample originates from a larger super population and accurately represents it. In this context, the super population refers to the population of US counties. The counties included in this sample are randomly selected from the broader pool of US counties. This random selection process is necessitated by the lack of prior knowledge regarding which counties would possess yearly average PM 2.5 data at the county level. Since only counties meeting this criterion are utilized in this study, random selection is upheld. Furthermore, the remainder of the third assumption presupposes that if the sample is divided into groups based on treatment status before and after the treatment commences, and the distribution of this sample adheres to the parallel trends assumption, then the treatment effect tends towards a normal distribution as n approaches infinity. Therefore, if these conditions hold true, alongside the first two assumptions, the treatment effect can be estimated using OLS methods (Roth, 2023).

Another concern, not a specific assumption attributed to the difference-in-differences model, is the absence of spillover effects. Spillover effects imply that the treatment under study doesn't directly or indirectly influence the control group's outcome. This poses a significant consideration for my research question. Since pollution disregards county borders, a policy aimed at reducing pollution may extend its effects beyond the county borders where emissions originate.

This is why I established the treatment based on 20 km buffers; as the distance from the pollutant source increases, it becomes less likely to directly impact individuals. However, some counties in the control group still have overlapping areas, albeit less than 15% of the total county area, which could experience spillover effects from the treatment. I believe the treatment definition I use will underestimate the effect of the RGGI on mortality rates.

The comparison I've set now includes some counties with minimal overlap in the control group, which, when compared to a treatment group with more overlap, will likely result in an underestimation as compared to making the comparison to a group with absolutely no spillovers. While this may be an underestimate, there is no straightforward way to compare a treatment and control group that accounts for all potential spillovers. I use the 20 km buffers to consider the spatial relationship between power plant emissions and health effects from published literature, it's possible that the true buffer size to account for all health effects caused by proximity to power plants may be large enough that no counties in the sample escape spillover effects. The treatment definition I'm using is designed to minimize this exposure while still maintaining the composition of the treatment and control groups. The RGGI policy only affects power plants that are 25 MW or greater. There are several power plants producing greenhouse gas emissions harmful to human health located in control counties unaffected by the RGGI policy, and emissions from these plants may spill over into treated counties as well. Therefore, with the knowledge that the RGGI policy lowered emissions, the use of 20 km buffers from health literature, and the designation of a treatment that doesn’t contain counties with more exposure in the treated group than in the control group, an assessment of the effects of the RGGI policy on mortality rates can be made. While the threat of spillover effects is minimized as much as possible without dramatically changing the composition of the treatment and control groups, the average treatment effect of the policy on the treated will likely be underestimated due to the aforementioned reasons. Comparing without any spillover effects whatsoever for a policy designed to reduce greenhouse gas emissions with a representative sample may be exceedingly difficult. To be thorough, I conduct an additional analysis using a dataset containing control counties with no overlap as a percentage of county area through the specified difference-in-differences model. With the removal of all counties with overlap, the number of control counties reduces from 32 to 18. I only run this no spillover check for cardiovascular mortality rates, where I expect the largest effect of the RGGI policy to be observed. The results of this regression analysis are in Table 17. Compared to the 15% treatment level, results with no spillovers show a decrease of approximately 1.5 Cardiovascular deaths for the 15-64 age group when compared to control counties.

In recent years, new research has highlighted certain issues with estimations generated by OLS in studies that use the difference-in-differences design. Borusyak et al. (2023) argue that OLS tends to compare all groups among themselves, as long as there is some variation within treatment status. This inclination to identify variation to minimize MSE might introduce bias due to improper comparison between treatment and control groups. To address this concern, Borusyak et al. (2023) propose a method that constrains how the difference-in-differences model is approximated. To address these concerns, I employ the model proposed by Borusyak et al. (2023) as an additional check to compare against the OLS estimates. The results of this method will be presented in tables 18-20.

To briefly summarize Borusyak et al.'s (2023) methodology, I will describe the steps they use to estimate their coefficients. The first step involves estimating the fixed effects, both time and unit, for just the untreated observations via linear regression. The second step entails subtracting the values of the fixed effects for each treated unit from the treated observations to obtain a counterfactual group of observations that represents what would happen in the absence of the treatment. The third step involves subtracting the counterfactual values from the observed treated values to obtain the estimated treatment effect. The fourth step, to obtain the average treatment effect for the treated in the post period, is to sum the observed individual treatment effects with clearly defined weights. For example, if the number of individual treatment effects is 1,000 for period j, then each weight would be 1/1000. The coefficients that result from this imputation method are interpreted the same as when OLS is used to calculate them. It is worth noting that the results between these two methods are quite similar. For instance, in the Cardiovascular mortality 15-64 age cohort, after controlling for temperature and population at the yearly and county level, the estimate produced by OLS is identical to the estimate produced by Borusyak et al. (2023). Again, the main models used in this paper will be using OLS.[9]

[9] For more information on the specifics see Borusyak, et al.(2023).

RESULTS

As previously discussed, PM 2.5 exerts significant impacts on the circulatory system, leading to oxidative stress and inflammation, which in turn increase the risk of heart disease and heart failure, among other health concerns. Therefore, implementing policies aimed at reducing carbon emissions and subsequent PM 2.5 levels could prove beneficial, particularly for vulnerable populations, such as individuals with circulatory diseases. The downward trend in the PM 2.5 data in Figure 2 does not seem to show any significant increase or change in the difference between mean concentrations of yearly PM 2.5 at the county level over time. To demonstrate a difference in the PM 2.5 levels between the treatment and control groups I perform an event study depicting the difference in mean PM 2.5 levels at the county level between the treatment and control counties per year. This is shown in Figure 4 below. Figure 4 reveals fluctuations in PM 2.5 levels across the pre- and post-periods. While most of the pre-period exhibits differences around or near 0, there is one year with a notable difference above 0. In the post-period, the yearly differences display a slight dip, occasionally dipping below 0 or slightly overlapping with 0 in the upper bound of the confidence interval. This variability complicates the assessment of a statistically significant decline in PM 2.5 levels. Although the event study aids in trend identification, estimating coefficients for each year reduces the total number of observations, thereby diminishing the statistical power to reject the null hypothesis that each $\beta\_i=0$. Which means fewer observations lead to wider confidence intervals.

To identify the average treatment effect of the treated I ran the data through the previously specified difference in differences model with average yearly PM 2.5 values at the county and year level as the outcome. The result showed a -0.7 decline in yearly average PM 2.5 values at the county level for the treated counties as compared to the control counties over the entire post period, holding all else equal. This value -0.7, seems small, but when the difference in PM 2.5 means for the treatment and control groups in the pre-period is 1.75, the resulting -0.7 decline represents 40% of the difference in means in the pre-period. The results of this regression can be seen in figure 21. The evident change allows the examination of mortality outcomes with the given treatment group in the sample as ideally any significant changes in mortality may be attributable to this change in PM 2.5 levels between treatment and control groups as a result of the policy.

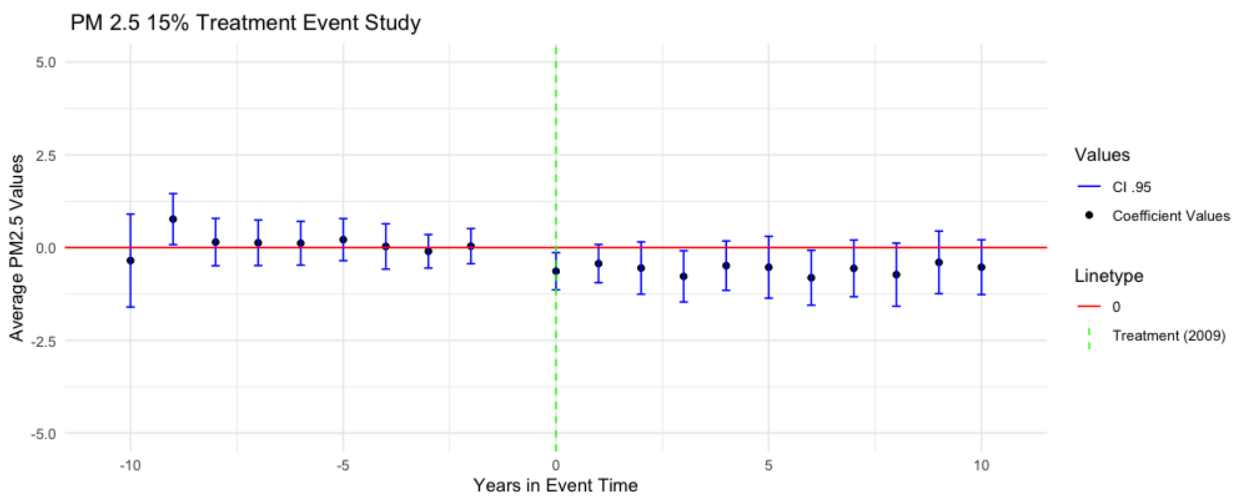

Figure 4. Notes: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

After examining the All-Cause mortality outcome across all age groups, I observe a significant difference between treated and control counties when not disaggregated by age category. Additionally, when analyzing the All-Cause outcome by age groups, both the 15-64 and 65-85 age categories exhibit statistically significant results at the 95% confidence level. Table 2 demonstrates the relatively minor impact of including temperature and log population on the estimates. An interpretation of a coefficient on the 15-64 group would be as follows: there is a reduction of 10 All-Cause deaths of 15-64 year old's per 100k of population in the treated group compared to the control group over the entire post period, holding all else equal. The interpretation on other coefficients is similar. I urge the reader to take these results with caution as the event studies for the age groups paint a different story. See Figure 5 on the following pages.

	OLS 15% Level								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All Cause	All Cause	All Cause	All Cause (15-64)	All Cause (15-64)	All Cause (15-64)	All Cause (65-85)	All Cause (65-85)	All Cause (65-85)
Treat x Post	-17.2**	-16.8**	-16.3**	-9.9**	-9.9**	-9.7**	-24.4**	-23.7**	-22.9**
	(4.8)	(4.8)	(4.6)	(3.7)	(3.6)	(3.7)	(7.6)	(7.6)	(7.2)
Observations	3233	3233	3233	1617	1617	1617	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 2: Rate Per 100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

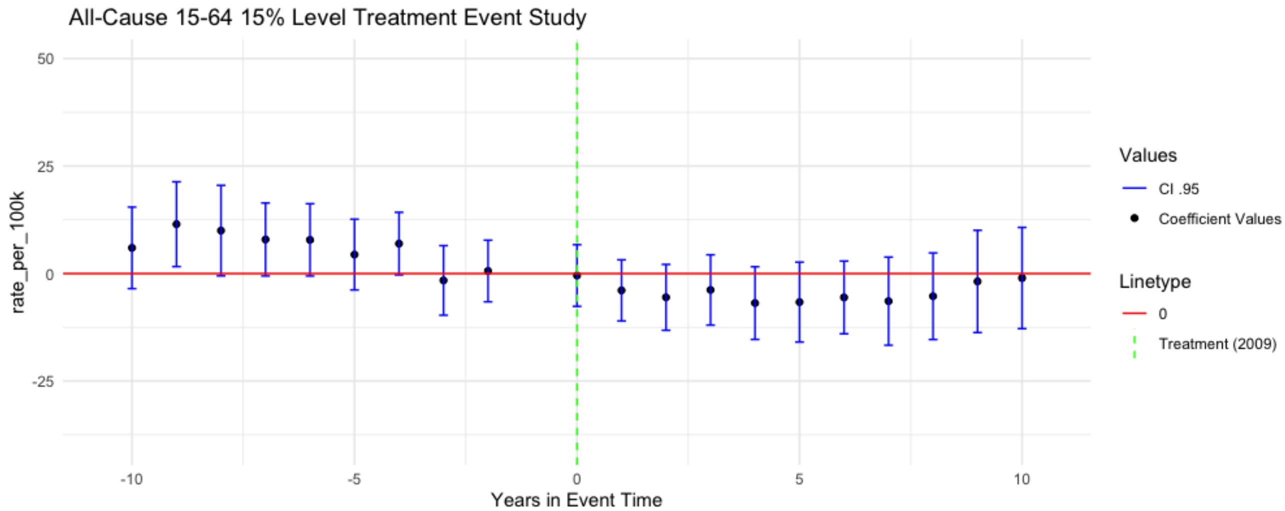

Figure 5. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

Figure 5 represents the event study for All-Cause mortality per 100k for the 15-64 age group at the treatment defined as average intersection area being 15% or more of total county area. There is no real clear parallel trends in the pre-period. While some of the coefficients contain zero in the confidence intervals there are a few outliers indicating distinct differences between the treatment and control groups in the pre-period, making the results seen in the static model be skeptical. The results of the event study for All-Cause ages 65-85 have the same underlying issues as the 15-64 age group. See figure12.

The initial examination of the Cardio mortality category yields unexpected findings contrary to previous literature on the effects of PM 2.5 on the circulatory system. Notably, no significant difference exists until the outcome is examined by age cohort. Table 3 reveals the 15-64 cohort as the most robust group, with minimal change upon inclusion of control variables in subsequent models. Conversely, the 65-85 group, which I thought would be more vulnerable to PM 2.5 impacts, exhibits significance only when both log population and temperature are controlled for, but only at the 90% confidence level. Specifically, the coefficient value shifts from

-7.7 per 100k in the model without log population control to -8.2 per 100k when log population is controlled for. For the 15-64 age group this can be interpreted as 2.1 less Cardio deaths of 15-64 year old per 100k of population for the treated counties as compared to the control counties over the entire post period. While 2/100,000 seems small, it helps to put it into context. The mean number of cardiovascular deaths per 100k for the treated group over the entire study period is 45.8. Given, for example, that possibly 10 of these deaths per 100k are directly a result of PM 2.5 from greenhouse gas emissions, then a reduction of 20% is significant. Numerically, this equates to an estimated reduction of 553 deaths attributed to circulatory diseases during the post-period from 2009 to 2019. A look at figure 6 will give details for the event study regarding 15-64 year old Cardiovascular mortality rates.

	OLS 15% Level								
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Cardio	Cardio	Cardio	Cardio (15-64)	Cardio (15-64)	Cardio (15-64)	Cardio (65-85)	Cardio (65-85)	Cardio (65-85)
Treat x Post	-4.1	-4.1	-4.3	-2.3*	-2.1**	-2.1**	-7.7	-7.7	-8.2*
	(2.7)	(2.8)	(2.7)	(1.2)	(1.0)	(1.0)	(5.2)	(5.2)	(5.0)
Observations	3220	3220	3220	1603	1603	1603	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 3: Rate_per_100k. Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

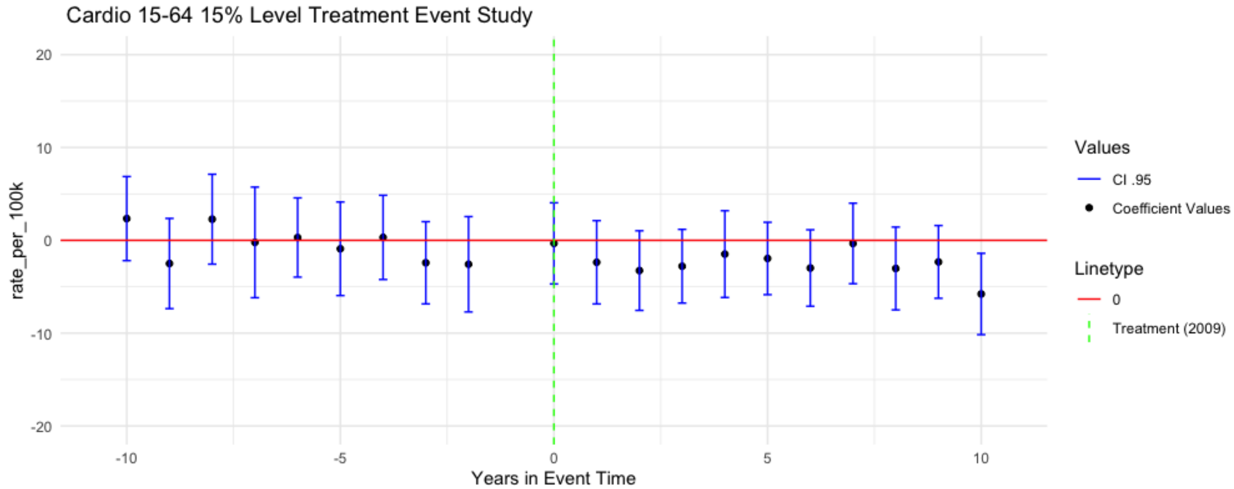

Figure 6. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

Figure 6 presents the event study for Cardiovascular mortality rates per 100k for 15-64 year olds, with treatment defined as an average intersection area of 15% or more of total county area. Unlike the All-Cause event study for the same age group, there is a slightly insignificant pre-trend, indicating more approximate parallel trends between the treatment and control groups. In the post period, there appears to be some undulation with an eventual downward trend in the final year. When considered as an aggregate, there is a significant decline for the treated counties compared to the control counties in the post period. However, when examining each year individually, no obvious trend is evident. This may be because of the reduced statistical power that arises from a lower number of observations that occurs from looking at each year individually. The trend in the post period is initially quite small, possibly only becoming noticeable in the last year or so of the study. Indicating a potential delay in the appearance of any significant effects on the 15-64 age cohort for Cardiovascular deaths as a result of RGGI.

The External Mortality outcome serves as a sort of placebo measure within the analysis. The insignificance of results across age groups implies that the model effectively captures the targeted variation attributable to the RGGI policy. I hypothesized that reductions in greenhouse gas emissions and PM 2.5, resulting from the policy, would likely have minimal impact on fatalities related to transportation and other accidental outdoor incidents. As evidenced in Table 4, no coefficient values reach significance at the 95% level, affirming this hypothesis. Both age cohorts exhibit little change with the inclusion of log population and temperature variables. Nevertheless, the results suggest that the treatment definition, based on a 15% average overlap as a percentage of total county area, effectively avoids capturing the effects of the RGGI policy where they would not be expected. This indicates the robustness of the treatment definition, aside from any potential violations in difference-in-differences assumptions.[10]

[10] All additional event study figures are presented in the appendix titled Additional Figures.

	OLS 15% Level								
	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)
	External	External	External	External (15-64)	External (15-64)	External (15-64)	External (65-85)	External (65-85)	External (65-85)
Treat x Post	0.02	0.04	0.08	0.7	0.7	0.8	-1.0	-0.9	-0.9
	(0.02)	(0.9)	(0.9)	(1.5)	(1.5)	(1.4)	(0.8)	(0.8)	(0.8)
Observations	2907	2907	2907	1617	1617	1617	1290	1290	1290
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 4: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

Robustness Checks

To assess the ability of the econometric models and designation of treatment to capture the intended effects of the RGGI policy, I use two datasets with different levels of treatment and run them through the same models shown in the Econometric Models section. The two levels of treatment are 5% and 25% are compared to the results obtained at the 15% level. This means treatment is defined as an average intersection area of 5% and 25% of total county area in an attempt to model PM 2.5 exposure. With the 5% treatment group the number of treatment counties is 50 and the number of control counties is 27. This is not that different compared to the 15% treatment group which has 45 treated counties and 32 control counties.

The event study results of average yearly PM 2.5 concentrations at the county level for the 5% and 15% treatment groups exhibit striking similarities. The only discernible distinctions emerge in the pre period, where the 5% treated group exhibits coefficients marginally closer to 0. Notably, both groups display the same relative downward shift in the post-period. Figure 7 illustrates the event study for the 5% treatment group, while Figure 8 depicts the event study for the 15% treatment group.

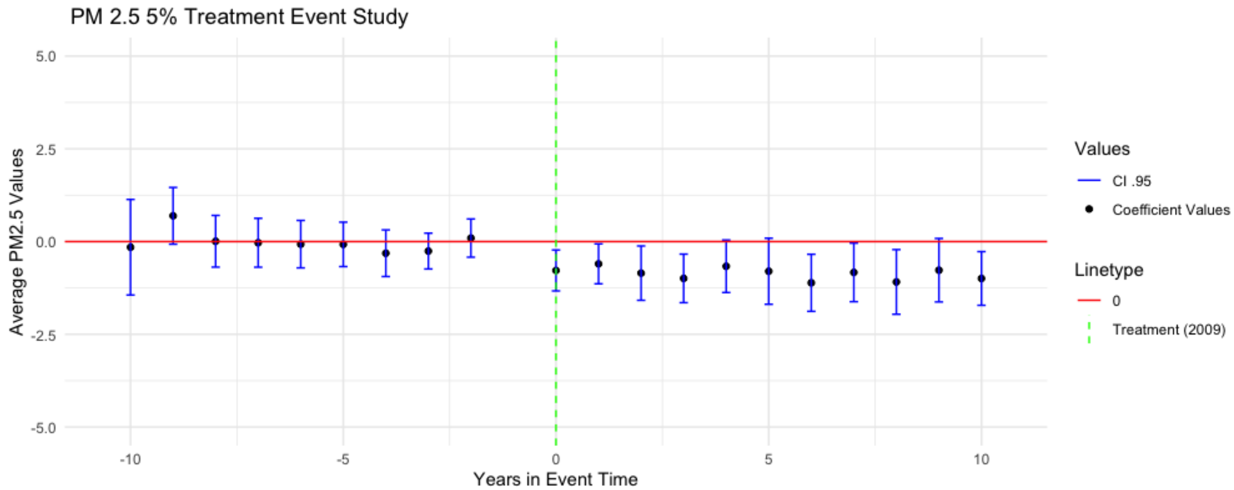

Figure 7. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

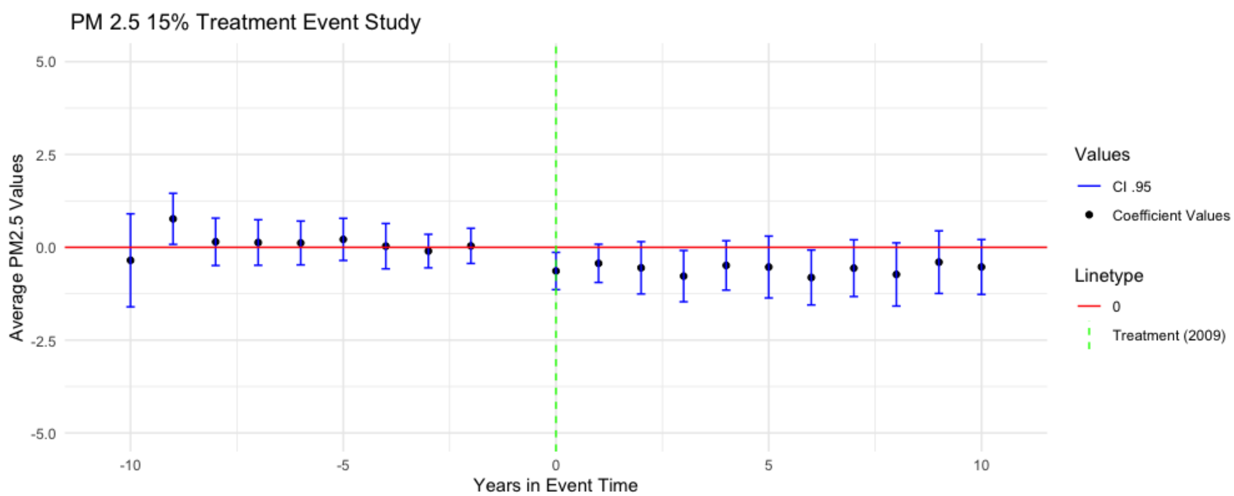

Figure 8. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

The similarity in the static difference in differences model between the 5% and 15% groups can be seen as well. Tables 5 and 6 demonstrate the results surrounding the Cardiovascular mortality rates. Both levels of treatment yield similar findings for the 15-64 age group in significance level and magnitude. With a decline of 2.1 Cardio deaths for the 15-64 age cohort at the 15% treatment level, vs a decline of 2.4 Cardio deaths for the 15-64 age cohort at the 5% treatment level. The only noticeable difference occurs for the 65-85 age cohort, for the 15% treatment level it is significant at the 90% level, whereas for the 5% treatment level it is not significant. Table 5 shows the results for Cardiovascular mortality rates for the 15% treatment level and table 6 shows the results for Cardiovascular mortality rates for the 5% treatment level.

The results appear to be different for treatment defined as average intersection area being 25% of total county area vs 15%. There is a clear difference in the event studies depicting average yearly PM 2.5 levels at the county level between the two treatment levels. The 25% treatment levels shows a noticeable uptick in the pre-period indicating a potential violation of the parallel trends assumption whereas the 15% treatment level seems to have a more uniform trend in the pre-period around zero, and adhere more closely to the parallel trends assumption. The event study for the PM 2.5 at the 25% level can be seen in figure 9, and the event study for the PM 2.5 at the 15% level can be seen in figure 10.

	OLS 15% Level								
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Cardio	Cardio	Cardio	Cardio (15-64)	Cardio (15-64)	Cardio (15-64)	Cardio (65-85)	Cardio (65-85)	Cardio (65-85)
Treat x Post	-4.1	-4.1	-4.3	-2.3*	-2.1**	-2.1**	-7.7	-7.7	-8.2*
	(2.7)	(2.8)	(2.7)	(1.2)	(1.0)	(1.0)	(5.2)	(5.2)	(5.0)
Observations	3220	3220	3220	1603	1603	1603	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 5: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

	OLS 5% Level								
	(37)	(38)	(39)	(40)	(41)	(42)	(43)	(48)	(50)
	Cardio	Cardio	Cardio	Cardio (15-64)	Cardio (15-64)	Cardio (15-64)	Cardio (65-85)	Cardio (65-85)	Cardio (65-85)
Treat x Post	-4.4	-4.4	-4.6*	-2.7**	-2.6**	-2.4**	-6.1	-6.1	-6.8
	(2.7)	(2.7)	(2.7)	(1.2)	(1.2)	(1.1)	(5.4)	(5.4)	(5.1)
Observations	3220	3220	3220	1603	1603	1603	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 6: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

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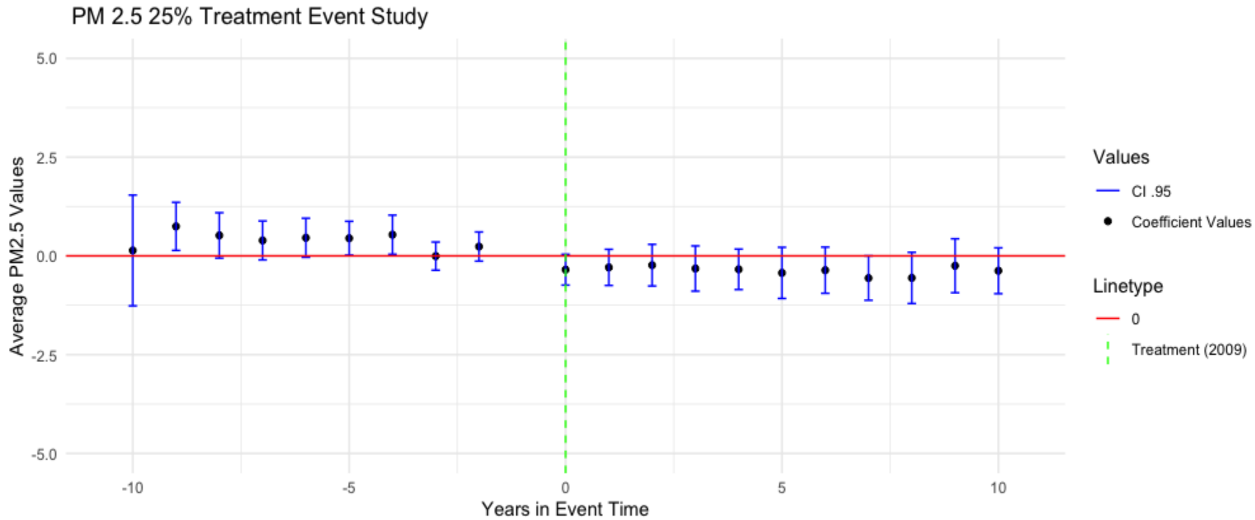

Figure 9. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

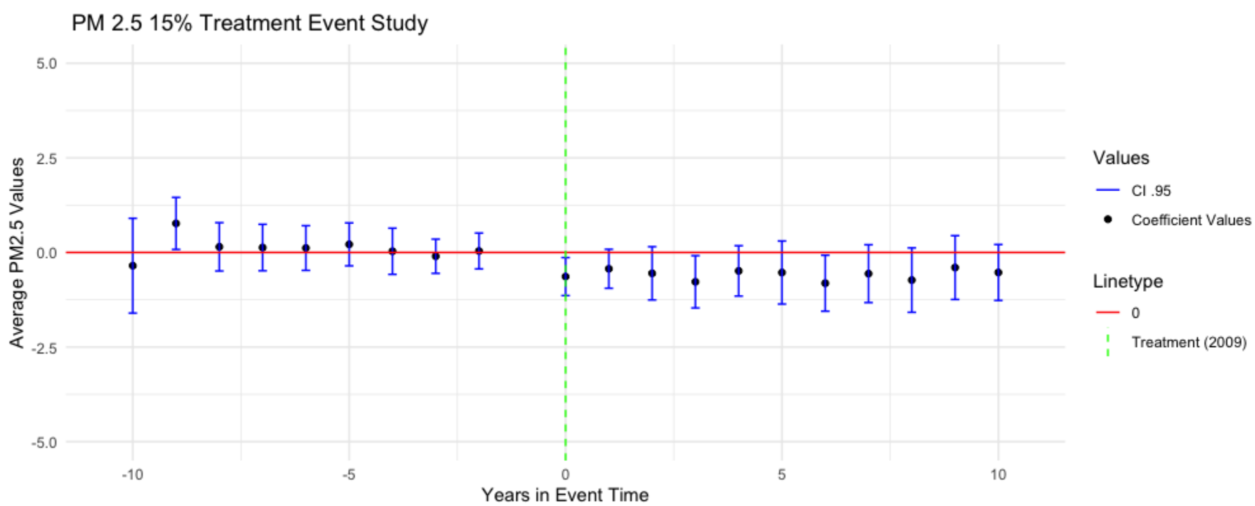

Figure 10. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level

Theoretically with a higher dosage of treatment I would assume a more pronounced effect of the treatment, and that is what is shown. It is just simply a little mis-specified, while 25% may seem like a higher dosage of treatment in this context it is a lower dosage of treatment. With 25% of total county area as treatment, this allows more counties to be classified as control, whereas with lower numbers of total county area represent higher levels of treatment, stricter adherence to any overlap as a percentage of total county area represent higher levels of treatment dosage. Table 7 presents the results for External mortality rates at the 25% level, while Table 8 presents the results for External mortality rates at the 15% level.

Tables 7 and 8 reveal no significant findings, indicating the robustness of the treatment definition in avoiding unexpected effects. However, the assessment of the RGGI policy's impact on mortality rates relies on the first stage event study, which suggests that defining treatment as 25% of the total county area violates the assumption of parallel trends in the first stage. Hence, any significant results from such a definition would be challenging to interpret reliably as attributable to the policy, or no statistically significant result would be found. Table 12 shows no significant effects on Cardiovascular mortality rates for the 15-64 age group, suggesting that the treatment definition might be too broad, leading to misidentification of treatment and control groups and, thus, inappropriate comparisons. This issue stems from the initial violation of parallel trends in the first stage event study, indicating a need to reconsider the reliability of the initial treatment definition. Nonetheless, the alignment of both stages seems to suggest the capability of the treatment definition and model to accurately capture the effect of the RGGI policy on Cardiovascular mortality rates for the 15-64 age group, albeit with cautious optimism.

	OLS 25% Level								
	(78)	(79)	(80)	(81)	(82)	(83)	(84)	(85)	(88)
	External	External	External	External (15-64)	External (15-64)	External (15-64)	External (65-85)	External (65-85)	External (65-85)
Treat x Post	-0.6	-0.6	-0.8	-0.7	-0.7	-0.8	-0.8	-0.7	-0.8
	(0.9)	(0.9)	(.85)	(1.4)	(1.4)	(1.4)	(0.8)	(0.8)	(0.8)
Observations	2907	2907	2907	1617	1617	1617	1290	1290	1290
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 7: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

	OLS 15% Level								
	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)
	External	External	External	External (15-64)	External (15-64)	External (15-64)	External (65-85)	External (65-85)	External (65-85)
Treat x Post	0.02	0.04	0.08	0.7	0.7	0.8	-1.0	-0.9	-0.9
	(0.02)	(0.9)	(0.9)	(1.5)	(1.5)	(1.4)	(0.8)	(0.8)	(0.8)
Observations	2907	2907	2907	1617	1617	1617	1290	1290	1290
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 8: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth

CONCLUSION

This study highlights the significant impact of the RGGI policy on mortality outcomes, observed in two stages. Firstly, I examine the policy's effect on yearly average PM 2.5 levels at the county level using an event study to partially check the parallel trends assumption. In the post period, I use event study results and a difference in differences model to demonstrate a statistically significant (99% confidence level) decrease in mean PM 2.5 levels between treatment and control groups over the entire post period. This represents a 40% reduction from pre period differences. With parallel trends upheld and a significant decline in PM 2.5, I transition to the second stage, analyzing health outcomes. Evidence supports parallel trends for Cardiovascular mortality rates in the 15-64 age group, showing a reduction in Cardiovascular mortality rates by approximately 553 deaths in treated counties over the post period (2009-2019) compared to control counties. By employing a model that captures initial RGGI effects on PM 2.5 levels and ensuring reliability of treatment specification, I aim to avoid misidentifying RGGI treatment effects on mortality rates. The alignment of evidence of parallel trends in the pre period for both stages suggests a meaningful relationship between PM 2.5 levels and Cardiovascular mortality for the 15-64 age cohort, warranting cautious optimism regarding the RGGI's impact on Cardiovascular mortality in this age group.

Although air dispersion modeling was not utilized in this study, I adopted an appropriate methodology drawn from health literature to accurately distinguish the immediate impact of PM 2.5 from point source emissions. By leveraging previous studies that monitor health outcomes near point source emissions and implementing 20km buffers to consider distance decay, I offer a reasonable approach to address spatial relationships between emissions sources and exposure levels. This approach is particularly beneficial for large-scale regions like the RGGI region, where existing models struggle to capture spatial dynamics accurately. While evidence of a violation of parallel trends is observed for All-Cause mortality outcomes, a stronger result indicates 553 fewer Cardiovascular-related mortalities in the 15-64 age group in treated counties during the post period. Using FEMA's 2020 value of a statistical life of $7.5 million, this equates to approximately $4.15 billion in saved human lives. Although the value of a statistical life varies, so will the total benefit. Nonetheless, this $4.15 billion figure closely aligns with the $4.7 billion value added to RGGI state economies from the sale of greenhouse gas emissions allowances during the 2009-2018 period (Hibbard, et al., 2018). Although the method for estimating this result has its limitations, it nevertheless illustrates the potential impact of a cap-and-trade policy on mortality outcomes.

The limited availability of AQI data restricted the study's scope to just over half of the counties in the entire RGGI region. This likely leads to an underestimation of the RGGI policy on mortality as it would likely increase if the whole RGGI region were considered. The PM 2.5 data only being 80% makes it difficult to conclude whether the first stage estimates are an under or overestimation. While the reasons for missing data appears random, if the effects of the RGGI policy on PM 2.5 levels is an underestimate, it would change the scope of the relationship between the reduction in PM 2.5 levels and Cardiovascular mortality rates for the 15-64 age group. An underestimation may show a far greater decrease in PM 2.5 levels is needed to be associated with the observed decline in Cardiovascular deaths for the 15-64 age group. While these may be limitations the study showcases evidences of the broader benefits of the RGGI policy beyond its strictly monetary and labor benefits, offering a foundation for future research to explore additional advantages of the RGGI policy.

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APPENDICES

APPENDIX A

ADDITIONAL FIGURES AND TABLES

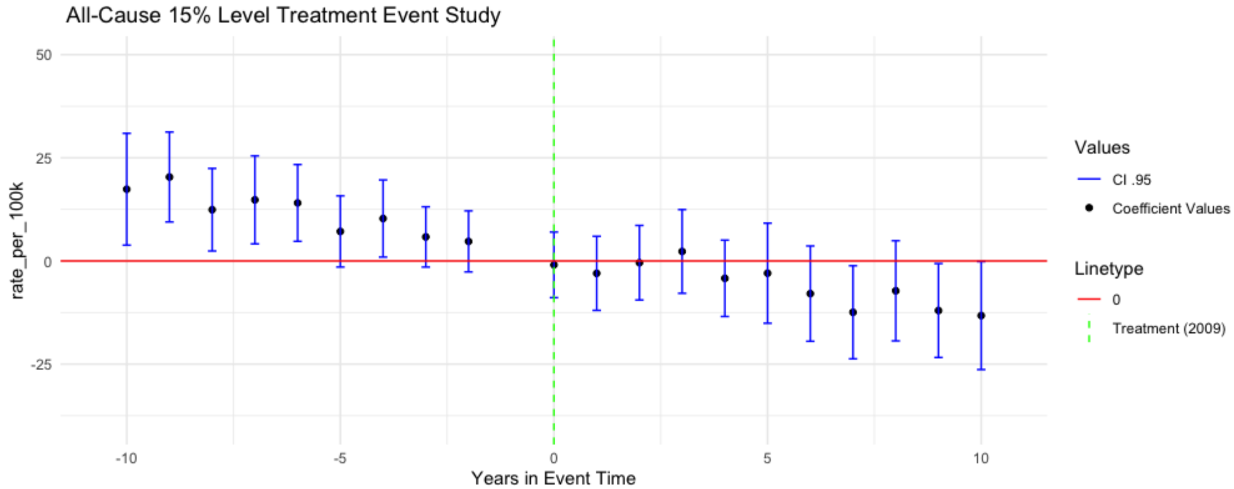

Figure 11: Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

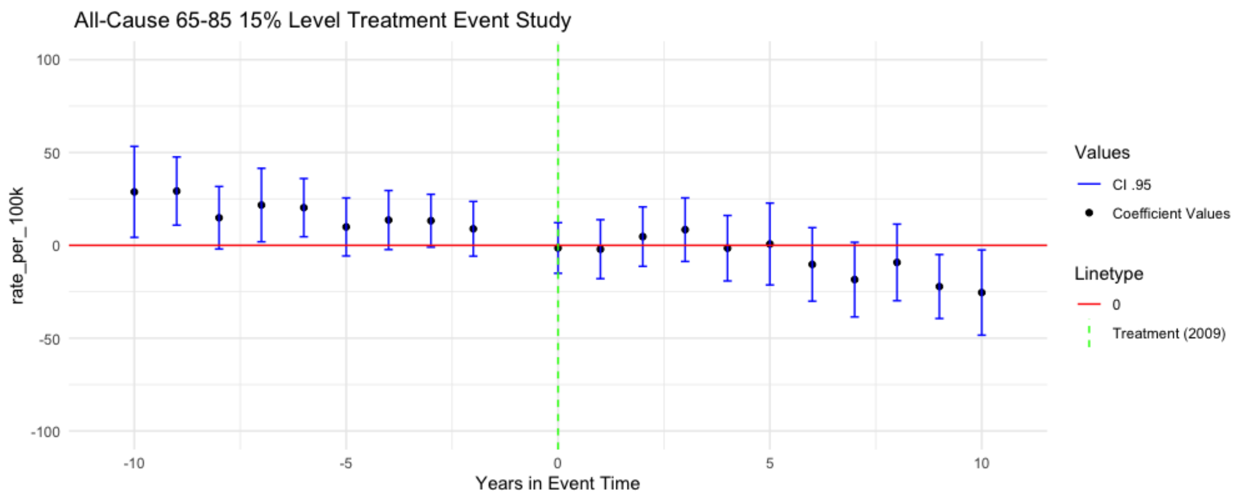

Figure 12: Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

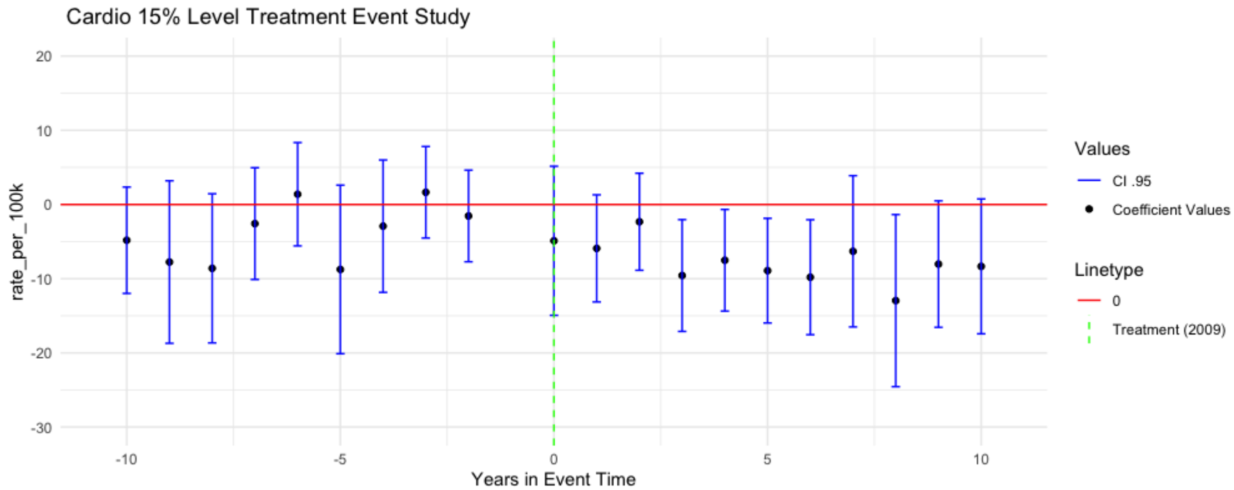

Figure 13. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

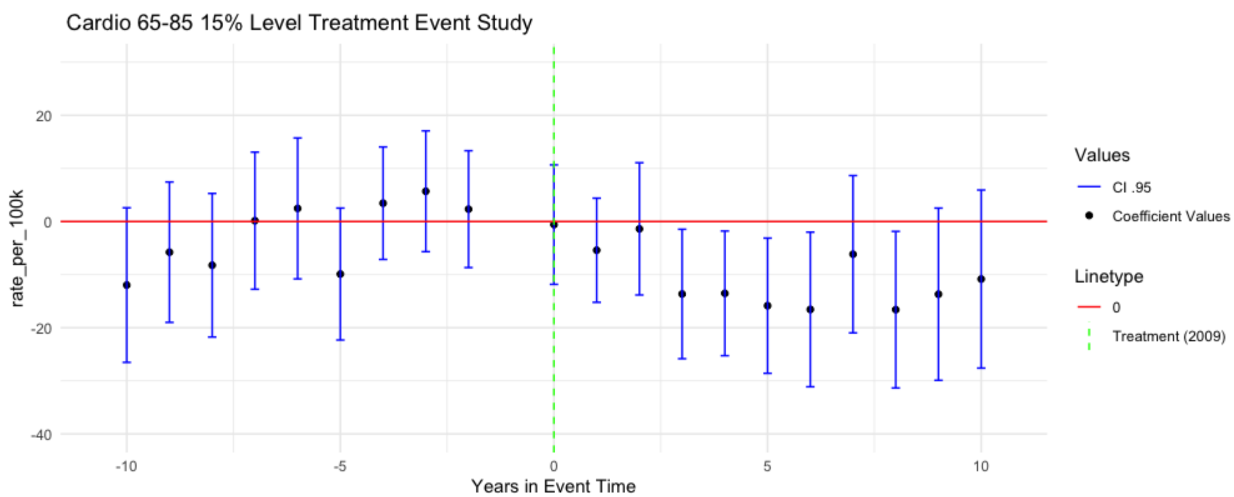

Figure 14. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

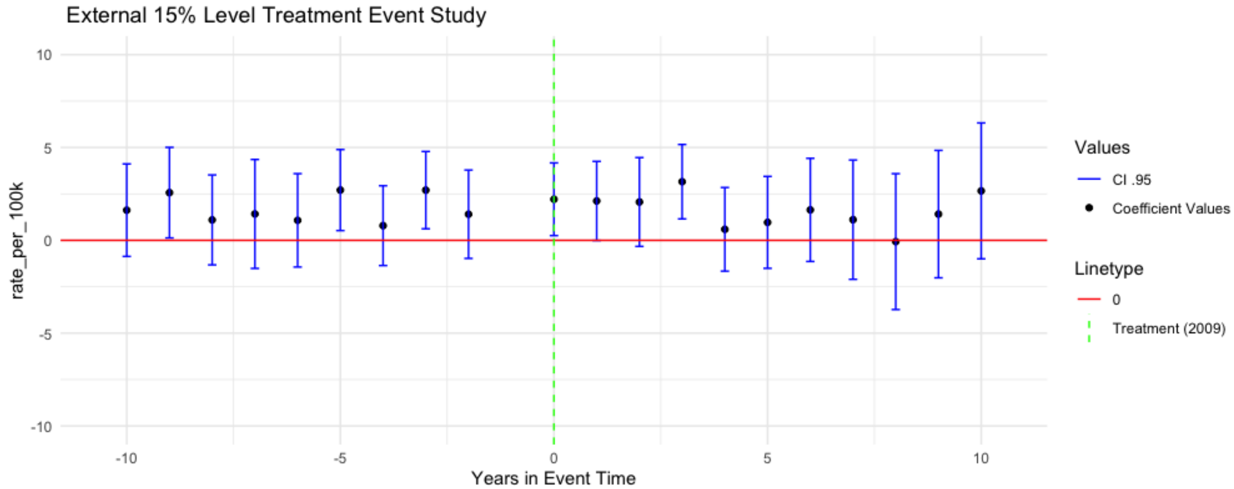

Figure 15. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

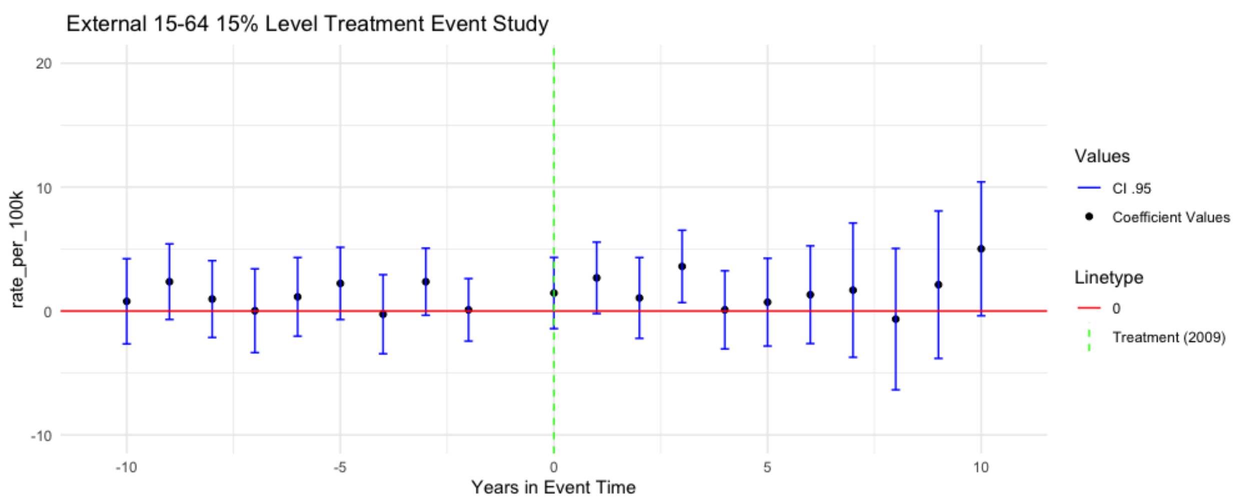

Figure 16. Note: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

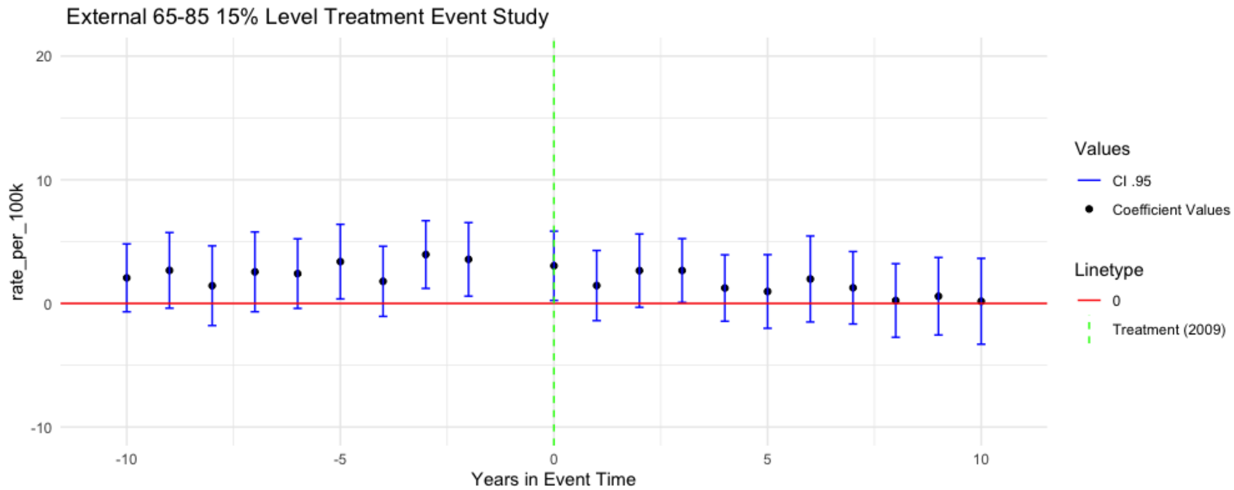

Figure 17. Notes: Event study regression was run using year and time fixed effects, controls for log population and temperature. Standard errors were clustered at the county level.

	OLS 5% Level								
	(28)	(29)	(30)	(31)	(32)	(33)	(34)	(35)	(36)
	All Cause	All Cause	All Cause	All Cause (15-64)	All Cause (15-64)	All Cause (15-64)	All Cause (65-85)	All Cause (65-85)	All Cause (65-85)
Treat x Post	-18.0**	-17.4**	-16.8**	-10.6**	-10.4**	-10.2**	-25.4**	-24.4**	-23.4**
	(5.8)	(5.1)	(4.9)	(3.5)	(3.5)	(3.6)	(8.5)	(8.5)	(8.0)
Observations	3234	3234	3234	1617	1617	1617	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 9: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

	OLS 5% Level								
	(51)	(52)	(53)	(54)	(55)	(56)	(57)	(58)	(59)
	External	External	External	External (15-64)	External (15-64)	External (15-64)	External (65-85)	External (65-85)	External (65-85)
Treat x Post	0.2	0.2	0.3	1.0	1.0	1.0	-1.2	-1.2	-1.1
	(0.9)	(1.0)	(0.9)	(1.5)	(1.5)	(1.5)	(0.9)	(0.9)	(0.9)
Observations	2907	2907	2907	1617	1617	1617	1290	1290	1290
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 10: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

	OLS 25% Level								
	(60)	(61)	(62)	(63)	(64)	(65)	(66)	(67)	(68)
	All Cause	All Cause	All Cause	All Cause (15-64)	All Cause (15-64)	All Cause (15-64)	All Cause (65-85)	All Cause (65-85)	All Cause (65-85)
Treat x Post	-16.9***	-16.7***	-17.2***	-11.8***	-11.8***	-12.0***	-22.0***	-21.6***	-22.4**
	(4.9)	(4.7)	(4.6)	(4.3)	(4.2)	(4.2)	(6.8)	(6.7)	(6.6)
Observations	3234	3234	3234	1617	1617	1617	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 11: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

	OLS 25% Level								
	(69)	(70)	(71)	(72)	(73)	(74)	(75)	(76)	(77)
	Cardio	Cardio	Cardio	Cardio (15-64)	Cardio (15-64)	Cardio (15-64)	Cardio (65-85)	Cardio (65-85)	Cardio (65-85)
Treat x Post	-4.4	-4.4	-4.2	-1.4	-1.3	-1.5	-8.5	-8.6	-8.1
	(2.8)	(2.8)	(2.8)	(1.2)	(1.2)	(1.1)	(5.3)	(5.1)	(5.1)
Observations	3220	3220	3220	1603	1603	1603	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 12: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth.

	Impute 0								
	(89)	(90)	(91)	(92)	(93)	(94)	(95)	(96)	(97)
	Cardio	Cardio	Cardio	Cardio (15-64)	Cardio (15-64)	Cardio (15-64)	Cardio (65-85)	Cardio (65-85)	Cardio (65-85)
Treat x Post	-5.2	-5.1*	-5.3**	-2.6**	-2.6**	-2.5**	-7.6	-7.6	-8.2
	(2.7)	(2.7)	(2.7)	(1.1)	(1.1)	(1.0)	(5.2)	(5.2)	(5.1)
Observations	3234	3234	3234	1617	1617	1617	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 13: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth. Table represents missing values imputed as 0.

	Impute 0								
	(98)	(99)	(100)	(101)	(102)	(103)	(104)	(105)	(106)
	External	External	External	External (15-64)	External (15-64)	External (15-64)	External (65-85)	External (65-85)	External (65-85)
Treat x Post	-1.5	-1.4	-1.3	1.1	1.1	1.2	-4.0***	-3.9***	-3.9***
	(1.1)	(1.1)	(1.1)	(1.5)	(1.5)	(1.5)	(1.2)	(1.2)	(1.2)
Observations	3234	3234	3234	1617	1617	1617	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 14: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth. Table represents missing values imputed as 0.

	Impute 9								
	(107)	(108)	(109)	(110)	(111)	(112)	(113)	(114)	(115)
	Cardio	Cardio	Cardio	Cardio (15-64)	Cardio (15-64)	Cardio (15-64)	Cardio (65-85)	Cardio (65-85)	Cardio (65-85)
Treat x Post	-5.1*	-5.1*	-5.3**	-2.6**	-2.5**	-2.4**	-7.6	-7.6	-8.2
	(2.7)	(2.7)	(2.7)	(1.1)	(1.1)	(1.0)	(5.2)	(5.2)	(5.1)
Observations	3234	3234	3234	1617	1617	1617	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 15: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth. Table represents missing values imputed as 9.

	Impute 9								
	(116)	(117)	(118)	(119)	(120)	(121)	(122)	(123)	(124)
	External	External	External	External (15-64)	External (15-64)	External (15-64)	External (65-85)	External (65-85)	External (65-85)
Treat x Post	-0.6	-0.6	-0.5	1.3	1.3	1.4	-2.5***	-2.5***	-2.4***
	(0.9)	(0.9)	(0.9)	(1.5)	(1.5)	(1.5)	(0.9)	(0.9)	(0.9)
Observations	3234	3234	3234	1617	1617	1617	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 16: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth. Table represents missing values imputed as 9.

	No spillover								
	(125)	(126)	(127)	(128)	(129)	(130)	(131)	(132)	(133)
	Cardio	Cardio	Cardio	Cardio (15-64)	Cardio (15-64)	Cardio (15-64)	Cardio (65-85)	Cardio (65-85)	Cardio (65-85)
Treat x Post	-6.8**	-6.7**	-6.7**	-3.5**	-3.6**	-3.6**	-11.1	-11.0	-11.0
	(3.2)	(3.3)	(3.3)	(1.7)	(1.7)	(1.4)	(6.8)	(6.8)	(6.7)
Observations	2597	2597	2597	1295	1295	1295	1302	1302	1302
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 17: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth. Table represents no spillover counties in control.

	BJS 15% Level								
	(134)	(135)	(136)	(137)	(138)	(139)	(140)	(141)	(142)
	All Cause	All Cause	All Cause	All Cause (15-64)	All Cause (15-64)	All Cause (15-64)	All Cause (65-85)	All Cause (65-85)	All Cause (65-85)
Treat x Post	-17.2**	-16.9**	-16.4**	-9.9**	-10**	-9.9**	-24.4**	-23.8**	-22.9**
	(4.8)	(4.7)	(4.5)	(3.5)	(3.5)	(3.6)	(7.4)	(7.4)	(6.9)
Observations	3234	3234	3234	1617	1617	1617	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 18: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth. Table represents results from method proposed by Borusyak et al. (2023).

	BJS 15% Level								
	(143)	(144)	(145)	(146)	(147)	(148)	(149)	(150)	(151)
	Cardio	Cardio	Cardio	Cardio (15-64)	Cardio (15-64)	Cardio (15-64)	Cardio (65-85)	Cardio (65-85)	Cardio (65-85)
Treat x Post	-4.1	-4.2	-4.3	-2.2**	-2.3**	-2.1**	-7.7	-7.7	-8.01*
	(2.7)	(2.7)	(2.7)	(1.1)	(1.1)	(1.0)	(5.0)	(5.0)	(4.8)
Observations	3220	3220	3220	1603	1603	1603	1617	1617	1617
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 19: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth. Table represents results from method proposed by Borusyak et al. (2023).

	BJS 15% Level								
	(152)	(153)	(154)	(155)	(156)	(157)	(158)	(159)	(160)
	External	External	External	External (15-64)	External (15-64)	External (15-64)	External (65-85)	External (65-85)	External (65-85)
Treat x Post	-4.2x10^-4	0.02	0.06	0.7	0.7	0.7	-1.0	-0.9	-0.9
	(0.9)	(0.9)	(0.9)	(1.4)	(1.4)	(1.4)	(0.8)	(0.8)	(0.8)
Observations	2907	2907	2907	2907	1617	1617	1290	1290	1290
temp	N	Y	Y	N	Y	Y	N	Y	Y
Log_Population	N	N	Y	N	N	Y	N	N	Y

Table 20: Rate_per_100k: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth. Table represents results from method proposed by Borusyak et al. (2023).

	OLS 15% PM 2.5		
	(161)	(162)	(163)
	PM 2.5	PM 2.5	PM 2.5
Treat x Post	-0.7**	-0.7**	-0.7***
	(0.3)	(0.3)	(0.2)
Observations	7762	7762	7762
temp	N	Y	Y
Log_Population	N	N	Y

Table 21: Average PM 2.5 at the yearly county level: Notes: Robust standard errors clustered at the county level are in parentheses. Regressions include controls for average yearly temperature at the county level, logged population at the yearly county level, and fixed effects for year and county. *** denotes 1% significance, ** denotes 5% significance, * denotes 10% significance. Values rounded to the nearest tenth. Table represents results of difference in differences with PM 2.5 as the outcome.

APPENDIX B

LIT REVIEW AIR DISPERSION MODELING

Emissions regulation policies require corresponding regulatory measures, often involving the use of air pollution dispersion models to assess compliance. These models play a crucial role in epidemiological studies by predicting potential exposure levels in specific areas and examining associated health outcomes. Understanding the principles and applications of air dispersion modeling is vital for comprehending the nuances discussed in this paper. This section is divided into four parts:

i. Basics of Air Pollution Dispersion

ii. Types of Air Dispersion Models

iii.Applications in Literature

iv. Land Use Regression Modeling

It's important to emphasize that the accurate and effective utilization of air dispersion models demands substantial expertise in Atmospheric Science, thorough training in model operation, and significant computational resources. These requirements entail both time and financial investments beyond the scope of this paper.

i. Basics of Air Pollution Dispersion

Air pollution comes in two forms primary and secondary, aside from natural and anthropogenic. Primary pollutants are emitted into the atmosphere from the source of the pollutant. Primary pollutants generally concern carbon compounds, nitrogen compounds, sulfuric compounds and particulates that are 2.5 micrometers in diameter. Secndary sources are pollutants that are generated after primary pollutants are emitted into the atmosphere, this would be compounds like ozone, sulfuric acid, and other particulates (El-Harbawi, 2013). These forms of pollution can come from point, line, or area sources. Point sources are stationary emitting entities such as a powerplant. Line soures are moving or entities that emit along a linear path, such as a highway. Area sources emissions are from large entities such as agricultural operations, or residential areas that have no specific emission point and their emission spreads over vast area.

When calibrating an air dispersion model to account for various types and sources of emissions, several factors influencing the dispersion of these emissions must be considered. Among these factors, wind emerges as one of the most influential forces shaping the spread of pollution from point sources. When assessing wind effects on emissions, two key factors come into play: horizontal dispersion and vertical dispersion. Horizontal dispersion pertains to the extent and breadth of pollutant spread within a given atmosphere. It is primarily driven by factors such as wind speed, direction, and topography (Wylie, 2021). Faster winds facilitate the spreading of emission plumes from point sources over greater distances but tend to maintain a more concentrated spread.

In vertical dispersion, the role of temperature, governed by thermodynamics, is paramount. Forecasters analyze the vertical temperature and wind profile of the atmosphere to accurately gauge air dispersion. Warm air tends to ascend, carrying pollutants aloft. However, this dynamic may be disrupted by instances where upper air parcels warm more rapidly than those closer to the ground, leading to inversions (Wylie, 2021). Such inversions are significant contributors to air pollution dispersion, as they can trap cooler air nearer to the surface. The peak of these inversions marks the mixing height, which serves as the boundary between the upper and lower atmosphere layers. At lower mixing heights, air pollution is trapped close to the ground, whereas when the atmosphere column is warmed and there is no inversion, pollutants can rise to mix with air higher up, leaving the ground level relatively clear. To accurately predict air pollution levels, analyzers employ atmospheric sounding to obtain the exact wind and temperature profile (Wylie, 2021).

Topographical features, such as mountains, can profoundly influence air pollution dispersion. They may serve as barriers impeding wind penetration, resulting in the trapping of pollution on the windward side. Conversely, they might disrupt windflow patterns, enhancing dispersion, particularly in the presence of strong winds.In the absence of wind, pollution trapped within valleys can undergo chemical transformations, triggered by sunlight exposure, leading to the formation of ozone. Such ozone can persist for extended periods, contributing to localized pollution issues (Cichowicz et al., 2017).

Seasonality also significantly influences air quality, with summer typically experiencing lower levels of air pollution compared to winter. During winter, the sun's lower position in the sky hinders its ability to effectively heat the atmosphere. Additionally, shorter daylight hours allow the air near the surface to cool for longer periods, exacerbating the inversion effect described earlier. This effect can be particularly pronounced in densely populated areas surrounded by mountains. Moreover, cloud cover during winter months promotes a more uniform temperature distribution, reducing the likelihood of inversions compared to clear skies. Furthermore, the presence of snow can further cool the ground, intensifying the temperature inversion effect (Wylie, 2021).

Seasonal variations in air pollution are illustrated by a study examining pollution concentrations in eastern Wielkopolska, Poland, spanning from 2009 to 2017. Daily measurements of PM10, sulfur dioxide, carbon monoxide, nitrogen dioxide, and ozone were collected alongside weather data to depict observed trends. Notably, ozone levels were higher in summer than in winter. The study confirmed this trend but also revealed a strong correlation between elevated levels of PM10, sulfur dioxide, carbon monoxide, and nitrogen dioxide, along with low ozone levels during winter months characterized by low air temperature, high humidity, and high wind speeds, particularly when these meteorological phenomena occurred simultaneously. Conversely, opposite trends were observed in summer (Cichowicz et al., 2017).[11]

ii. Types of Air Dispersion Models

The origins of air dispersion modeling date back to 1915 and have undergone continuous evolution since. An air dispersion model serves as a mathematical representation of meteorological transport and dispersion processes, utilizing source and weather parameters over a specified period (El-Harbawi, 2013). The selection of an appropriate model hinges on factors discussed in the preceding section, as well as the nature of the pollutant being measured, as gaseous and particulate pollutants entail different physical and chemical dispersion processes. Essential data inputs for an air dispersion model include pollutant emission rates, meteorological conditions, topographical and complex terrain information, and atmospheric chemistry data.

There are five main types of models. The first type is the Gaussian plume model, which originated in 1936. This model operates on the principle that the shape of an air plume from a point source typically conforms to a Gaussian normal distribution. Gaussian models consider factors such as wind direction, temperature, and emissions rate. Gaussian models are designed to estimate

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[11] Readers should be aware that these discussions only skim the surface of Atmospheric Science, as there are far more intricate and complex theories and concepts surrounding the factors influencing air dispersion, all of which are considered in the dedicated field Atmospheric Science for air dispersion modeling.

ground-level concentrations and impacts resulting from point emissions. The Gaussian plume model follows a general equation:

$$C(x,y,z) = \frac{Q}{2\pi\sigma_y\sigma_z\mu} \quad exp^{\left(\frac{-y^2}{2\sigma_y^2}\right)} exp^{\{-\frac{(z-h)^2}{2\sigma_z^2}\}} + exp^{\{-\frac{(z+h)^2}{2\sigma_z^2}\}}$$

Here c is the concentration at a given point, Q is the source of the emission, x represents downwind, y represents crosswind, μ is windspeed at height of release. $\sigma_y \& \sigma_z$ are the coefficients of lateral and vertical dispersion (Ajayi, et al. 2021).

The Gaussian plume distribution model depends on set assumptions:

I. The pollutant is coming from a point source (emission is continuous).

II. Eddy diffusion advection equation[12] (how particles move through a fluid and how they disperse in turbulent conditions).

III. Diffusion in downwind direction is negligible, consider crosswinds and vertical winds only.

IV. Atmospheric turbulence is constant.

V. Wind speed is constant, and Meteorological conditions remain constant from source to air quality receptor.

If these assumptions are violated then the measurement is invalid. Although, these conditions are for the initial model, modern models such as AERMOD can handle complex terrain, different meteorlogical conditions among other things (Ajayi, et al. 2021). Gaussian models are usually used to evaluate policy compliance in a localized setting.

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[12] Eddy diffusion equation is: $D_t \frac{\delta C}{\delta t} = D_h \frac{\delta^2 C}{\delta x^2} + D_v \frac{\delta^2 C}{\delta z^2}$

C is the concentration of the pollutant, t is time, x is horizontal distance, z is vertical distance, D_t is the total diffusion coefficient, Dh is the horizontal diffusion coefficient, D_v is the vertical diffusion coefficient.

The second type of model is the Lagrangian model, which characterizes plume parcels over time as a random walk process, sometimes spanning years. An example of a Lagrangian model is CALPUFF (California Puff Model). Lagrangian models excel at tracing pollutants back to their sources and can handle emissions from line sources. However, they cannot account for intricate chemical interactions among particles in the atmosphere or complex wind conditions. In the presence of these conditions, inaccuracies may arise regarding the pollutant's location. Neither Gaussian nor Lagrangian models are equipped to handle complex chemistry (El-Harbawi, 2013).

The third type of model is a box model, which involves dividing the atmosphere into a 3D rectangular shape, with the ground forming the bottom face of the box. The goal of this model is to approximate the concentrations of specific chemicals and compounds across the entire space, including the upper troposphere, making it suitable for initial assessments (El-Harbawi, 2013). Box models also assume a homogeneous distribution of pollutants within the defined shape, resulting in a general inability to accurately predict the true distribution within an airshed. This limitation stems from the oversimplification inherent in assuming uniformity across complex atmospheric conditions.

The Eulerian model, the fourth type of model, is designed to forecast the movement of pollutants within a fixed coordinate system. It proves particularly effective in predicting movement over complex terrain and in densely populated urban areas where various structures serve as barriers. In this model, particle movement is continuously tracked by developing conservation equations within specific volumes in the coordinate system, akin to the approach taken in fluid mechanics. Unlike the Lagrangian model, which discretely tracks individual particle movements, the Eulerian model observes particles as a continuous flow (El-Harbawi, 2013). Nonetheless, Eulerian models often encounter challenges such as numerical diffusion, leading to unintended distortions in the simulation, and they demand substantial computational resources. An example of a Eulerian grid model is CMAQ (Community Multiscale Air Quality).

The fifth type of model is the Dense-Gas model, which is specifically tailored to forecast the dispersion of dense gas plumes heavier than air, such as chlorine or ammonia. These heavier gases tend to remain closer to the ground and disperse at a slower rate compared to other pollutants. Unlike other models, the Dense-Gas model incorporates a buoyancy effect, accounting for the difference in density between the dense gas and the lighter surrounding air. Due to the inclusion of advanced mixing and turbulence algorithms and theories, Dense-Gas models typically involve more complex computations than other air dispersion models (El-Harbawi, 2013). An example of a Dense-Gas model is DEGADIS (Dense Gas Dispersion).

iii. Models Used in Literature

Thus far, we have delved into the fundamentals and broad categorizations of air dispersion models. While the research realm boasts numerous models, only a handful find widespread adoption. Among these, Gaussian puff models reign supreme, largely due to their extensive history. El-Harbawi highlights, "Point source problems are best understood, as they involve simpler mathematics and have been studied for a long period of time, dating back to 1900. These problems employ a Gaussian distribution model to predict air pollution isopleths for buoyant plumes, considering factors such as wind velocity, stack height, emission rate, and stability class" (4). Given this paper's focus on evaluating the impact of a cap-and-trade policy on point source emissions and subsequent health outcomes, it's pertinent to provide an overview of literature pertaining to Gaussian puff models. AERMOD (American Meteorological Society/Environmental Protection Agency Regulatory Model) stands out as the most widely utilized Gaussian puff model, preferred by the EPA. Subsequent papers will utilize AERMOD, with one opting for a generic Gaussian puff model, and the final paper delving into an Eulerian space model to illuminate challenges in tracking and distinguishing air pollution proportions from short transport versus far transport.

AERMOD has been utilized to examine the impact of both point and line emissions, as evidenced by the study conducted by Gibson et al. (2013). This research employed AERMOD to simulate the dispersion of nitrous oxide, sulfur dioxide, and PM 2.5 in Halifax, as well as nitrous oxide and sulfur dioxide in Sydney and Port Hawkesbury, and exclusively PM 2.5 in Pictou, all situated in Nova Scotia, Canada. Emissions data from 2004 were utilized across four 50x50 km regions to project levels over annual, monthly, and hourly periods. Subsequently, simulation results were juxtaposed with actual measurements obtained from nationally registered sensors in the respective regions to assess model accuracy. The comparison between simulated values and observed data employed metrics such as R-squared and normalized mean-square error, among others. Meteorological and wind data were sourced from local monitors at sites within the regions, supplemented by emissions data obtained from a total of seven power plants.

In summary, AERMOD simulations demonstrated satisfactory agreement for sulfur dioxide levels across all sites at yearly and monthly intervals, yet exhibited poorer performance at the hourly level. The most robust agreement was observed for annual levels in Halifax, boasting an R-squared value of 0.77. However, the model struggled to accurately predict PM 2.5 and nitrous oxide levels at the measured sites, consistently underestimating these pollutants. It notably performed reasonably well in predicting sulfur dioxide emissions from both point and line sources in the specified areas. Ultimately, this study underscores the imperative for thorough validation of the AERMOD model, highlighting its limited applicability across diverse geographical regions.

Efforts to enhance the performance of AERMOD include its combination with land use regression, as demonstrated by (Michanowicz et al., 2016). This study aimed to integrate Land Use Regression modeling alongside AERMOD in a 5 km² area surrounding a power plant in the Pittsburgh Metro area, focusing on PM 2.5 measurements. Land Use Regression modeling is a predictive approach for estimating pollution concentrations in urban areas based on the spatial relationship between local emissions and pollution concentrations. It leverages statistical relationships between pollution source covariates (such as traffic and land use) and the spatial dispersion observed at monitoring locations. However, LUR models face challenges in accurately accounting for meteorological conditions.

In addition to employing the combined LUR+AERMOD model, a separate run of a standalone LUR model was conducted and validated using data from 36 monitoring sites during the summer and winter of 2012-2013. It was noted that data from regional monitoring sites alone effectively captured the factors influencing pollution levels in the area (Michanowicz et al., 2016). Comparative analysis revealed that the hybrid model demonstrated a 2-10% improvement in accuracy compared to the LUR model alone, albeit with seasonal variations. The LUR model consistently underestimated concentrations 500-2500m downwind of industrial sources. Overall, the hybrid model may offer superior estimation of exposure in urban areas characterized by complex topography and numerous industrial facilities.

For single point sources like industrial facilities such as the Lafarge cement factory near Sagamu, Nigeria (Ajayi, et al. 2021), a straightforward Gaussian puff model suffices for monitoring emissions. The study conducted measurements for various pollutants and weather parameters over a week-long period, underscoring the significance of atmospheric stability factors like looping, coning, and fanning. Looping occurs during moderate wind speeds and heat, generating eddies that transport pollutants from the stack to the ground. Coning, on the other hand, is observed under cloud cover with high winds, resulting in a symmetrical plume carrying pollution downwind. It occurs when there is a fair amount of cloud cover with high winds, generating a vertically symmetrical plume that carries pollution downwind before reaching the ground. Fanning occurs during inversions with light winds, causing the plume to stabilize and travel parallel to the ground further downwind, up to 20 km. The researchers observed that taller stacks at the factory dispersed pollution higher into the atmosphere, resulting in lower measured concentrations at surrounding sites.

Recent studies have underscored the challenge of pinpointing precise sources of pollutants, owing to atmospheric chemical processes that transform original particles into different compounds capable of traveling long distances. To evaluate the transport distances of pollutants, chemical transport models are commonly employed, as demonstrated in a study tracking Elemental Carbon (a major component of PM 2.5) and sulfate (resulting from sulfur dioxide oxidation) across three sensors located in Pittsburgh, Atlanta, and the Great Smoky Mountains National Park (Wagstrom and Pandis, 2011). The study utilizes the CAMx (Eulerian sub-type) chemical transport model, which is a numerical computer model simulating atmospheric chemistry by solving mass-balance equations to determine the fate of atmospheric pollutants (El-Harbawi, 2013). The researchers employed this model to quantify the air composition from local secondary and primary sources across the aforementioned region (Wagstrom and Pandis, 2011).

To quantify the distances traveled, the study categorizes measurements into three categories: short-range (<100 km), mid-range (100-550 km), and long-range (>550 km). To cover the 3492 x 3240 km area, it was divided into 36 x 36 km grid cells. Air quality readings and meteorological data were collected during the summer and winter of 2001, with the air quality readings serving both as validation and input. In urban areas, over 50% of Elemental Carbon (EC) originated from local sources, while in rural areas, a majority was attributed to sources 100-550 km away, with minimal seasonal variations in both regions. More than 50% of sulfate and Volatile Organic Compound (VOC) measurements were determined to originate from over 200 km from the receptors in all areas, with higher concentrations in winter. The computational costs for these calculations amounted to approximately 30% of the total, highlighting the complexity and expense associated with implementing such models for a limited number of receptors (Wagstrom and Pandis, 2011).

iv. Land Use Regression Modeling

In a study conducted by EPA researchers (Isakov et al., 2012), the limitations of Land Use Regression (LUR) models are explored, aiming to compare their accuracy and drawbacks with air dispersion models. PM2.5, nitrous oxide, and benzene levels were evaluated using AERMOD and CMAQ (Community Multiscale Air Quality Eulerian Grid Model) in a 20x20 km area around New Haven, CT during July-August 2001, with a focus on PM2.5's predominantly regional nature.

Comparisons between LUR and air dispersion models utilized mean adjusted R-squared and mean residuals, with varying numbers of monitoring sites.

The study revealed that increasing the number of sites improved mean adjusted R-squared, plateauing at 125 sites in the training datasets, with values ranging from 0.59 to 0.79 for the measured pollutants. However, the effectiveness of LUR models is hindered by several factors outlined in this paper and the literature. These models demonstrate limited transferability between urban areas due to the need for extensive training, substantial data, and model recalibration for each location. Moreover, LUR models struggle to accurately represent the dispersion of specific emission sources based on concentrations, exacerbated by their single-pollutant focus. Additionally, model accuracy heavily relies on extensive data collection, particularly challenging and costly in highly industrialized regions, often necessitating significant manpower. Until these challenges are addressed, alternative methods are recommended for assessing concentration levels, especially in larger areas (Isakov et al., 2012).